Thomas Söderqvist

The Ecologists

y Naturalists to Saviours of the Nation

Cover: Lars-Gunnar Romell (1891-1981). Claimant of ecology and reconciler between the descriptive and experimental approaches to field studies of plant-environment relations. See Ch.2-3, 2-4, and Epilogue. Drawing by Gunnar Brusewitz.

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The Ecologists
From Merry Naturalists to Saviours of the Nation

A sociologically informed narrative survey of the ecologization of Sweden 1895-1975.

Almqvist & Wiksell International, Stockholm, Sweden.

To GUSTAF SÖDERQVIST

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Preface

»It is my own roots I am searching for: history, intellectual history, is in the last instance about myself, my own riddle«.[1]

This is an account of ecology[2] in Sweden. Why ecology? There are always some personal reasons for choosing a research topic, and in this case they go right back to my childhood. My mother and grandfather would take me out in the forest to pick mushrooms and watch the birds. Later, in *Fältbiologerna* (the Swedish Field Biology Youth Association),[3] I learned to use binoculars and field manuals, and to master a popular variety of what would later be the world-view of ecology. Many of my friends were later trained as academic ecologists. Thus naturalists and ecologists have been Important Others in my early life world.

The original idea for this study came from observing the political expansion of ecology in Sweden in the early 1970s. Suddenly, it seemed, ecology was everywhere. Becoming the catchword of the day, it sneaked into the everyday vocabulary not only of scientists and their culture, but of government bodies, political parties and social movements as well. Before one's eyes university departments translated their research activities and curricula into the new language – zoologists and botanists, geographers and even some classical scholars suddenly assumed the guise of ecologists. Brand new chairs and other research and teaching positions were created in ecology. Books and pamphlets on the new science filled the shelves of bookstores and libraries, and the gospel was spread by popular magazines and newspapers. Naturalist social movements like *Svenska Naturskyddsföreningen* (The Swedish Association for the Conservation of Nature) and *Fältbiologerna* began to present their goals and ideology in ecological terms. *Naturvetenskapliga forskningsrådet* (the Natural Science Research Council) launched a special ecology policy, and 15% of its total funds were allocated to ecological research in the mid-1970s.[4] A new kind of research organization – the large scale research project – hitherto only known in nuclear physics, was introduced with the explicit aim of saving the nation's natural resources. Even government bodies advocated a »basic ecological view« of society – in 1972 a Government Bill proposed that social planning should be pursued with due respect to ecological principles. Thus ecology was a formative influence on Swedish politics and society in the 1970s.

1. S.Lindroth 1982,p.246
2. The concept of ecology is connotatively rather unambiguous. All notions of ecology which refer to the natural world share a common connotation, viz., something like *the relation between animals (incl. man) and/or plants and their external environment (incl. other animals and plants)*. The Dictionary of History of Science defines ecology as »the branch of biology dealing with interrelations between organisms and their environment« (Bynum *et al* 1981,p.110). Note that the term ecology is also used in the social sciences, notably by sociologists. However, there is no risk of confusing the two meanings of ecology – even when »human ecologists« talk about »ecology«, they refer to ecology as a natural science, not ecology in the sociological sense.
3. Names of Scandinavian institutions, organizations etc., are given in their native language throughout. English translations are given at the first mention. A full list of translations is given in the Appendix.
4. For details, cf.4-5.

Events like these triggered my curiosity. A decade earlier we had collected beetles or watched birds. Now my former naturalist friends spoke about beetles and birds in terms of »ecosystem balance«, »population dynamics« and »energy budgets«. What had happened to them? Why did they »go ecology«? Where had they got the new outlook from?

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Making these first shaky steps towards some kind of understanding of the emergence of ecology in Sweden would not have been possible without constant support from families, friends and funds. First and foremost I am grateful to my family for their patience. My daughter Anna made the point one day, when finding me by the typewriter in the kitchen: »Do you ever do anything else than write about ecologists« she asked in mild despair, and turned on Children's hour on TV-Channel 2.

My intellectual teachers and friends have had great patience with my stubborn doctrine of self-reliance. Aant Elzinga (Göteborg) gave me the decisive kick when letting me spend two summer months in 1983 on his farm in British Columbia to write the first draft of the manuscript. He was stubborn too, but our disputes over my way of writing a sociologically informed history of science was nevertheless of utmost importance for the final result. Lars-Erik Liljelund (Stockholm) has not only given constant personal support through the whole work, but has also read the entire draft manuscript critically from an ecologist's point of view. Philip Lowe (London) critically read the final draft and took on the demanding task of adapting my Scandinavian English and view of ecology to a somewhat more international style.

Several others have commented upon the manuscript, in parts or as a whole, including Håkan Törnebohm and Lennart Svensson (Göteborg), Gunnar Eriksson and Gunnar Broberg (Uppsala), Arie Rip (Leiden), Donald Broady (Stockholm), Nils Roll-Hansen (Oslo) and Bill Coleman (Madison). Many others, among them Ron Eyerman (Lund), Andrzej Dabrowski (Roskilde) and John Law (Keele) have given me valuable advice on questions concerning sociology (of science). I am also grateful to my colleagues and friends at *Institut for biologi og kemi* (Roskilde), for having provided me with all possible working facilities even when formally not attached to the department. The librarians at *Roskilde universitetsbibliotek* have provided all possible help with interurban and international loans. The editing office of *Svenskt biografiskt lexikon* has solved a number of biographical problems.

Contacts with the ecologists' intellectual milieu in Sweden have been the alpha and omega of the whole enterprise. This applies not only for those who have given the formal interviews, but also a much larger number of ecologists who have engaged in informal discussion. Most surviving major actors in the contemporary history of ecology in Sweden have been generous in providing me with informations, and some of them have later commented upon selected parts of the draft manuscript. I must refrain from

mentioning any of my interviewees specifically.[5] I can only apologize in advance for the fact that my decision to deal with the entire history of Swedish ecology, from the Linnean natural historians to the political ecology of our days, sometimes has made the treatment of individual ecologists necessarily superficial. In addition, most of them will probably find my reconstruction biased.

A number of funds have supported various aspects of the work. *Planlægningsrådet for forskningen* (København) generously provided me with a scholarship between 1981 and 1984 for studies of »relations between research and higher education«; most of the present work was done during these three years. In addition *Statens naturvidenskabelige forskningsråd* (København), *Nordiska forskarstipendier* and *Forskningsrådsnämndens STUFO-delegation* (Stockholm) have supported grants for printing, travel and interview transcripts.

Finally I would also like to thank Rose-Marie Ekström and Alice Malmström, who transcribed most of the taped interviews, Asger Meulengracht Olsen and Ingrid Hastrup Jensen, who compiled and drew the figures, and Bente Egaa Svendsen (without her the text would still be in the word-processor) and Tina Petersen.

København, February 1986

Thomas Söderqvist

5. As a rule I have referred to my interviewees as »Interview with NN /Notetur Nomen/ 12/8 1982«. When context makes anonymity illusory, however, I have referred to initials, e.g., »Interview with HS /Hugo Sjörs/ 24/9 1981«.

Prologue

et dixerunt venite faciamus nobis civitatem et turrem cuius culmen pertingat ad caelum
et celebremus nomen nostrum antequam dividamur in universas terras.[1]

The ecologization of modern Sweden

Before the 1970s the historiography of ecology was mainly a »historical preface« to textbooks; the eminent example being the 70 page Introduction to the seminal *Principles of Animal Ecology* published in 1949.[2] The few independent contributions were written by »practitioners - turned - amateurhistorians«,[3] with Tansley's and Gleason's histories of early British and American plant ecology as the classical examples.[4] During the last decade, however, a number of professional histories has appeared, including Lowe's study of the institutional emergence of early British plant ecology,[5] Cittadino's investigation of the emergence of early American plant ecology,[6] Worster's search for the intellectual roots of the idea of ecology,[7] and Tobey's detailed account of the life cycle of the grassland school in American ecology.[8] In addition Egerton has contributed reviews and bibliographies of the literature.[9]

So far, however, no study has treated the emergence of ecology, its organization and its social impact in a long-term historical perspective. The present book attempts to give such a comprehensive picture of ecology, albeit limited to the confines of a single nation. Being the first history of ecology outside Great Britain and the United States, and covering a small nation, it should also contribute to the international comparative study of ecology.[10]

1. Vulgata, Genesis 11:4; meaning: »'Come', they said, 'let us build ourselves a city and a tower with its top in the heavens, and make a name for ourselves; or we shall be dispersed all over the earth'« (The New English Bible, Cambridge 1970).
2. Allee *et al* 1949.
3. As Graham *et al* 1983,p.xix., puts it.
4. Gleason 1936; Tansley 1947.
5. Lowe 1976.
6. Cittadino 1980.
7. Worster 1977.
8. Tobey 1981.
9. Egerton 1977a, 1977b.
10. Crosland (1977) among others, has argued for the value of a nationally confined historiography. The question of comparative studies of national scientific cultures is discussed in detail by Jamison (1982). A first attempt towards an international comparative study of the development of ecology has been made by Kormondy and McCormick (1981). Their collection of articles, although including a large number of countries, has several drawbacks, however: firstly that most contributions concentrate on the post-war period, secondly that (with one exception) it is written by practicing ecologists, thus having a strong bias towards legitimating the recent grandeur of ecology, and thirdly that many important nations are lacking, notably Finland, France and Canada.

The history of Swedish ecology is not a marginal contribution to an international comparative study. Apart from my cultural bias, the choice of Swedish ecology is not entirely arbitrary. Swedish ecologists were among the international pioneers around the turn of the century, and between the wars they were considered to occupy an international leading position (after their American and British colleagues).[11] The spectacular rise of ecology in the 1960s and 1970s was certainly a general Occidental phenomenon, but particularly conspicuous in Sweden. Hence I believe that the Swedish case might illuminate general international developments in ecology. Until now only a couple of practicioners have made preliminary contributions to the history of Swedish ecology.[12]

The emergence of ecology (and, of course, any science) can be told in different ways. Leaving aside the important issue of the legitimatory functions of the history of science,[13] we could basically distinguish between analytical and historical studies – the former utilizing the ecological source material to contribute towards a rationally coherent sociology of science, philosophy of science, psychology of science, etc;[14] and the latter understanding the emergence of ecology as a class of concrete actions and actors, both individual and collective.

The aim of this work is historical, not analytical, that is, its main purpose is to contribute to the history of learning in Sweden. As indicated in the subtitle, I have chosen to designate it as »a narrative survey« – narrative in the sense of telling a long and tortuous story, a survey in the sense of exploring a virgin historical material. The joy of writing a story however, does not free one of analytical responsibilities. All historical work involves (consciously or unconsciously) the application of analytical schemes (i.e., theories of science) in the selection of historical events. Although the bulk of the (practicioners') literature on the history of ecology pays no attention to the analytical problem, the recent studies by professional historians of ecology referred to above have all explicitly applied analytical schemes. Being contemporary with the post-Kuhnian revival of the sociology of science, it is probably no coincidence that they have been informed by sociological schemes, utilizing concepts like »scientific community« and »paradigm«, and not by schemes taken from a reconstructive philosophy of science. Thus, Lowe's study of the relation between amateurs and professionals in the institutional emergence of British plant ecology draws upon Mulkay's contribution to the study of scientific communities.[15] Cittadino, although limited in referring to sociological schemes, nevertheless interprets early American plant ecology in terms of »professionalization«.[16] And although Tobey warns his reader not to »naively jump to the conclusion that there is a sociological monograph hidden inside this historical essay«, he nevertheless resorts to a »modified

11. Cf. Ramaley 1940.
12. See A.-M. Jansson 1980 and Sjörs 1981. In addition, a few short notes on ecology are included in historiographical works on Swedish botany and zoology: Hjelmqvist 1958, Weimarck 1980, Rodhe 1973, Törje 1968, Löwegren 1968, Collander 1965 (on Finlandish botany), and Eriksson 1978.
13. Cf. Graham *et al* 1983.
14. E.g., Küppers *et al* (1978) have discussed the emergence of postwar ecology in West Germany as a contribution to the study of the relation between science and the development of science policy; a number of recent studies of ecology conducted with the aim to contribute to philosophy of science are found in the special issue of *Synthese 43* (1980); some earlier studies of ecology have likewise had philosophical aims, notably Klaauw's and Meyer's »Ökologische Studien« in the 1930's (Klaauw 1935 and 1937).
15. Lowe 1976.
16. Cittadino 1980.

Kuhnian sociological theory«.[17]

The present story is also informed by a theory of science, or, more exactly a sociological perspective. But unlike earlier histories of ecology, the one adopted here is more in accordance with Westfall's recent programmatic statement:

> *»In my vision of modern history, the growth of science plays the central role. It began by transforming the intellectual structure of the Western world. It proceeded to transform the economic system. It is now transforming life itself on the entire globe, not to mention threatening it as well. I find no way to reduce the fundamental reality of the modern world to the status of an epiphenomenon. Quite the contrary, much of the modern world appears to me as so many epiphenomena to the growth of science«.*[18]

Westfall's statement relates to the notion of rationalization, or more precisely scientification, i.e., the growth of scientific discourse as a major determinant of world affairs. This is not the place to discuss the vast literature on rationalization and scientification[19] – suffice it to say that it allows us to talk about *ecologization* as a partial aspect of scientification, in line with electrification, computerization, mathematicization, etc. Thus, the topic of the present investigation is the ecologization of modern Sweden.

Ecology as discursive consciousness

How shall scientification (including ecologization) be comprehended? Scientification is here taken to mean the establishment of a knowledge monopoly, that is, knowledge control over a certain domain of the world.[20] Knowledge control is never monolithic, however; the »knowledge society«[21] exhibits a certain patchiness. To each cognitively and institutionally discernible field of knowledge corresponds a social group which administers »a specific fund of symbolic representations serving as a means of orientation«[22] and hence exerts a knowledge monopoly. Research schools,[23] scientific specialties and scientific disciplines,[24] professions and professional segments,[25] intelligentsia factions[26] and social movements[27] are different kinds of groups exerting knowledge monopolies at different aggregational levels.

17. Tobey 1981,pp.5-6.
18. Westfall 1981.
19. The *locus classicus* for the notion of rationalization is of course Weber's oeuvre. Cf. also Habermas 1981.
20. As Elias has pointed out: »The central social function of knowledge is that as a means of orientation. As the individual orientation of every member of a society depends on the means of orientation available there, groups of people who are able to monopolize the guardianship, transmission, and development of a society's means of orientation, hold in their hands very considerable power chances, especially if the monopoly is centrally organized« (Elias 1982,p.37). (maybe the term »knowledge oligopoly« is more in accordance with the factual conditions).
21. Cf. Bell 1973.
22. Elias 1982,p.43.
23. For a critical discussion of the concept of »research school«, or just »school«, see Geison 1981.
24. What we call scientific disciplines and scientific specialties are products of the 19th century (see e.g., Swoboda 1979). The literature on the emergence of scientific disciplines is voluminous.
25. For aspects on science as a profession, see e.g., Mendelsohn 1964 and Ben-David 1972. When speaking about professionalization in the specific Swedish context, we should not forget that professions are mainly identical to bureaucratic professions, i.e., they are intimately connected to the state; on the concept of bureaucratic profession, see Fielding and Portwood 1980.
26. For a discussion of intelligentsia and intelligentsia factions, see e.g. Gouldner 1979 and Konrád and Szelényi 1978.
27. For a discussion of knowledge and social movements, see Touraine 1978.

Hence, ecologization might be seen as the establishment of any knowledge monopoly involving ecological concepts; in other words, the establishment of an intellectual closure[28] involving the separation of outsiders from insiders by means of ecological theories and concepts. Depending on the extension of this new intellectual closure we might discuss it alternatively in terms of the establishment of a scientific specialty, a scientific discipline, a profession (or professional segment), a social movement or even an intelligentsia faction. Thus, as long as the ecologization process is restricted to activities within the confines of an existing scientific discipline (e.g., botany) one can talk about it as a scientific specialty (like plant ecology), but when the process of ecologization transforms »the intellectual structure... the economic system... and life itself« it is more relevant to understand it in terms of the establishment of an intelligentsia faction or of a social movement.

How is a knowledge monopoly – a specialty, a discipline, a professional segment, an intelligentsia faction etc. – established? With respect to the formation of professional segments Bucher and Strauss assert that

> *»early in their development they carve out for themselves and proclaim unique missions. They issue a statement of the contribution that... it alone can make... The statement of mission tends to take rhetorical form, probably because it arises in the context of a battle for recognition and institutional status«.*[29]

Claiming a knowledge monopoly thus involves addressing, announcing and naming a field of scientific investigation and discourse, and dismissing other attempts to claim the field as »their« domain of discourse.

This viewpoint implies a focus on the processes of delineation of ecology vis-á-vis other intellectual discourses by means of *naming* practices. Consequently, the aim of this treatise is to reconstruct the ecologization of Sweden as the spreading of the *word* »ecology« from around the turn of the century to the present, first within academia, then throughout the wider society.

Focusing on the word might seem to be a trivial task, literally a play with words. One of the interviewees asked »How do you define ecology?« To the answer »Ecology is that kind of activity pursued by people calling themselves ecologists«, he responded with astonishment: »That's funny... but it does not say anything essential«.[30] What counts for this apprehension of a history of ecology is who investigated which »real« things »out there« when, where and why, and not under which label these investigations were pursued.

28. For a thorough discussion of the concept »social closure« and an application of it in the scholarly field, see Murphy 1983.
29. Bucher and Strauss 1961,p.326; Klegon points out that the literature on professions increasingly emphasizes the profession »as part of a political process in which knowledge and skill are claimed by a group to advance its interests« (Klegon 1978,p.269). Shapin (1982) has reviewed a number of case-studies in order to show that sciences like eugenics and phrenology should be interpreted as a social strategic professionalization practices. Bud (1978) utilizes a notion of institutional strategies in his study of American cancer research. Kohler (1972) maintains that the break-through of enzyme theory must be understood in connection with the emergence of biochemistry as a self-conscious profession.
30. Interview with NN 12/8 1982.

But playing with words is not as trivial as he thought. Words are

> *»a good place to begin, not because a conceptual language reveals the phenomena in any straightforward way, but, as Heidegger maintained, words carry the record of past perceptions, true or untrue, revelatory or distorting«.*[31]

Focusing on words, names, symbols, and labels, rather than concepts or scientific practices, is significant for a history of science since disciplinary labels are a bridge between individual research practices and the social context of science. Focusing on words, or signs, has been proposed as a solution to the classical problem of internalism vs. externalism,[32] or the problem concerning the connection between historical and structural analyses of science[33]

More important in this context is that focusing on ecology as a naming practice solves the difficulties of delineating a proper unit of historiographical analysis. Given the connotative unambiguity of the concept of ecology, all conceptual and social practices of those historical individuals and collectives having pursued studies of the relations between animals and plants and their environment might be utilized as potential events for the historical reconstruction (ecological studies in this connotative sense have in fact been pursued as far back as scholarly history can trace it).[34] But after reading through a number of histories of ecology one is bound to notice that the criteria for including events into the historical reconstruction, versus excluding them as irrelevant for a history of ecology, seem to be rather arbitrary.

That is, although the concept of ecology is connotatively rather precise, it is denotatively vague. Some investigations of the relations between animals and plants and their environment are included in the histories of ecology, whereas other, seemingly very similar investigations, are instead to be found in historical sketches of silviculture, plant geography, marine zoology etc. Some historians include studies of crop pests in a history of ecology, while others (implicitly) consider them part of a history of agronomy. The historian of ecology cannot give a satisfactory account for why studies of the effect of soil mineral factors upon the composition of plant communities should be considered an event to be selected for his historical reconstruction, although the practicing scientist himself never thought in terms of ecology and claimed them as contributions to the problem of plant geography. Should Linné be included as an ecologist because of his *Oeconomia naturae* and his landscape travels, despite the fact that he held a chair in medicine?

However, to a majority of historians of ecology this problem of delineation of the unit of analysis does not seem to be a problem at all. Some practicioners of ecology have solved the delineation problem easily. »From the point of view of a rational classification of the sciences«, said the director of fishery investigations at the British Ministry of

31. Young-Bruhl 1982,p.405. A few historians of science have likewise focused on the »word«. E.g., Christie and Golinski maintains that »it is the human activities of practising and talking about chemistry«, the specific chemical discourse, »the spreading of the word« which is the central object for a history of chemistry (Christie and Golinski 1982,p.235).
32. For a discussion of labelling as a means of dissolving the old controversy between internalist and externalist analyses of science, see Latour 1983.
33. Gunnar Eriksson, Uppsala, has drawn my attention to the possibility that a semiotic approach might be a way of bridging the two major ideal types of historiography, one where all historical matter is represented in a bewildering and unordered way (»like reality itself«), and another where all events are firmly founded in definite causes (such as marxist historiography).
34. See Egerton 1977b.

Agriculture and Fisheries in 1932,

> *»fishery research is, by reason of its methods and standpoints, simply a branch of ecology«.*[35]

To him the problem of delineation was easy to solve for the rational mind. Ecology is ecology is ecology, whether labelled as pest research, limnology, plant biology or animal geography, and whether pursued by Aristotle, Linné or the Odum brothers. This attitude also implies that the contemporary delineation of ecology sets the object of historical reconstruction. The past is mobilized as a legitimation of the present.[36] For example, when Egerton finds »ecologists« everywhere in the past, and turns Aristotle into a »population biologist«,[37] he has not only extended the existence of ecology beyond Haeckel (see Ch.1) another two millenia, but also mobilized Aristotle to the grandeur of ecology as it is known to us today. But others seem to be in trouble, not the least the practicioners. Nelkin points out that

> *»ecologists find it difficult to agree when their field became a distinct academic discipline«.*[38]

The simple solution adopted here is to delineate »ecology« to those activities considered as such by the historical actors themselves,[39] that is, how individual scientists identify, and account for, theirs and other scientists' activities as this or that science during the course of their research careers. From the point of view of research practice there is no urgent reason why anyone should use a neologism (such as »ecological«, or generally, »x-ological«) to denote his investigations of the relation between animals, plants and the environment. A number of rather specific scientific concepts as well as specific experimental procedures and equipment are of course necessary, but the actual research practice could as well be pursued under any disciplinary label, or be it »zoology«, »botany«, »hydrography«, or »natural history«, etc. In fact, much of what scientists and historians now consider as »ecological« investigations was once pursued under these different labels. During their student years most prospective »ecologists« identified themselves as actors within the sciences they were trained in. The majority of them adhered to this »label« for their entire intellectual life. Others re-identified their activities. Instead of pursuing »botany« or »zoology«, they began to conduct »ecological« work. They began to identify themselves as »ecologists«, or became identified as »ecologists« by their contemporaries.[40]

On a more sophisticated sociological view the process of ecologization as the emergence of a new naming practice can be accounted for in terms of the transition from a level of

35. Russell 1932,p.128.
36. That the historiography of science, like all historiography, serve legitimation purposes is argued for in Graham *et al* 1983.
37. See Egerton 1975.
38. Nelkin 1977,p.78.
39. Within the tradition of history of science it is natural to base the analysis at the microlevel, focusing on the individual scientist, his/her life-history, and his/her arguments and claims for ecology. Although this approach certainly is not unproblematic – individual actors operate in pre-given structured surroundings, relate to an already existing institutionalized ecology, at home or abroad, act in the linguistic web spun by the cultural beliefs of their age, and confront a diversity of institutions, departments, university boards, research councils, etc. – it is still true that »science is created by individuals, and however much it may be driven by forces from outside, these forces work through the scientist himself« (Hankins 1979).
40. Observe contemporaneity! In a recent book on scientific revolutions Cohen (1985) has utilized a somewhat similar approach: he counts a historical episode a revolution if people *say* it was a revolution. But besides contemporary scientists he also includes present day scientists, and historians of science. Such an approach to naming practices takes the force out of the argument.

»practical consciousness« to a level of »discursive consciousness«. A thorough theoretical argument for this view has recently been suggested by Anthony Giddens in his theory of structuration[41] and will not be repeated here. In Giddens' terminology »practical consciousness« stands for the kind of knowledge that is »inherent in the capability to 'go on' within the routines of social life«, whereas »discursive consciousness« means being able to put things into words.[42] That is, what concerns us here is the emergence of a discursively conscious ecology, and the subsequent authorization of an institutionalized ecological discourse.

From »proto-ecology« to »ecology«

For the reasons given above it is useful to differentiate between »proto-ecological« and »ecological« studies. »Proto-ecological« refers to studies of the relations between animals, plants and their environment which are potentially accountable as ecological, for example by recent ecologists, but which were not designated as ecological by the historical actors themselves.[43] »Ecological« studies, on the other hand, are studies being *claimed* as such by the historical actors. (Discursively conscious) claims for ecology may take many forms, from the simple use of the term »ecology« (here called »weak claims«, or just »claims«) to programmatic statements for ecology as an independent discipline (here called »strong claims« or »programmatic claims«). In this respect scientific papers might be seen as claims announcing the importance of research findings, rather than chronicles of the research procedure.[44]

Four introductory examples, all referring to surveys of the coastal waters of Sweden from the 1870s, 1890s, 1930s and the 1970s, might serve to illustrate the distinctions made above. The first example illustrates the state of marine research before the initiation of the ecologization process.[45] In 1875 a chemist, F.L.Ekman, and a zoologist, Sven Lovén, both renowned scientists and Academy members got a *Riksdag* grant of 10,000 SEK for pioneering investigations of the currents and water chemistry, and marine life respectively. The investigations were devoid of ecological reasoning, let alone informed by an explicit ecological discourse, organized by ecological institutions, or guided by an ecological research policy. In fact, the two surveys were conducted separately, as »hydrographical« and »zoological« investigations respectively. That is, the chemist and the zoologist seemingly did not even have the ambition to study the relations between water conditions and animal life, not to mention claiming such studies as a science on its own. We might call this a »pre-ecological« stage of ecologization.

The second case exemplifies what I have chosen to call »proto-ecology«.[46] In 1890 a chemical engineer, Gustaf Ekman, and a Stockholm professor in chemistry, Otto Petters-

41. Giddens 1984.
42. Ibid.,p.4 and p.45.
43. The term »historical translation« (cf. below) is used here to denote the process by which a historian of science interprets earlier historical events in terms of the scientific concepts of his own age. Hence, describing these early scientists as »ecologists« is part of the growth of the social order (cf. below) of ecology of our time.
44. See e.g., Knorr-Cetina 1981, Ch.5.
45. See biographical articles on F.L.Ekman and Sven Lovén in *Svenskt biografiskt lexikon.*
46. For details, cf.1-3.

son, secured a state grant for renewed investigations of the coastal waters of Sweden. The aim of the project was to understand the causal relation between water regimes and the occurrence of plankton and fish. They stated programmatically, that:

> *»since the biological phenomena were closely connected to the hydrographic conditions, the exploration of the sea in the future must include both«.*[47]

Subsequently, their investigations were institutionalized in the so-called *Svenska hydrografisk-biologiska kommissionen* (the Swedish Hydrographical-Biological Commission), the main goal of which was to remedy the »inadequate insight into the economy of nature«, which could only be investigated »by means of combined zoological, botanical and hydrographical research«. However, Ekman and Pettersson did not claim their studies as »ecological«. Their investigations were »ecological« only at the level of »practical consciousness« (hence »proto-ecological«).

The third example, taking us to the level of »discursively conscious« ecology, is the marine survey pursued by the Uppsala professor in zoology, Sven Ekman, and his graduate students in the 1930s.[48] Ekman had been among the first to announce his animal studies as »ecological« in the 1910s, and throughout the 1930s his students produced a number of dissertations on the relation between animals and the marine environment. These investigations were explicitly referred to as »ecological«.

Nowadays studies of the relations between animals and plants and their environment are not only generally designated »ecological«, but also authorized as such in terms of chairs, departments, curricula, etc. Ecology lays claim to be an independent science at the state universities. This takes us to the last example.[49] In the early 1970s yet another large scale coastal survey, viz., *Östersjöprojektet* (the Baltic Ecosystem Project), including physical and chemical investigations and studies of marine animal life, was formulated, not only in explicit terms of ecology, but authorized by the university and state authorities as an ecological institution. *Östersjöprojektet* was cast in the language of ecology, it was organized by a laboratory explicitly denominated as ecological, it was led by a scientist formally trained as an ecologist and later appointed professor in »marine ecology«, and finally it had been established by means of a deliberate ecological policy on the part of *Naturvetenskapliga forskningsrådet* and the *Riksdag*. In addition, the funding and accomplishment of the project, and the recruitment of personnel, were intimately related to the efforts to spread the gospel of ecology throughout educated opinion. *Östersjöprojektet* was considered part of a general strategy of saving the nation on the basis of an ecologically oriented planning policy.

This way of telling the story of ecologization poses a series of questions. For example:[50] When, where and to what extent have studies of the relation between organisms and their environment been pursued? How have these studies been claimed? And when, where and to which extent have such studies been institutionalized?

The step from »proto-ecology« to a discursively conscious ecology involves a number

47. Redogörelse 1903,p.3.
48. For a detailed discussion, cf.3-4.
49. For a detailed discussion, cf.4-5.
50. As already stated above this is a historical study, not an analytical one. So there are no specific hypotheses on test.

of questions concerning the emergence of an ecological discourse, such as: How, when and where were studies of the relations between animals, plants and the environment claimed as specifically »ecological«. What was the scientific and cultural background of scientists making claims for ecology? And how did other scientists (non-ecologists) and non-scientists (practical men, politicians, teachers, students, etc.) account for and recognize the activities of ecologists?

We will also ask questions concerning attempts to institutionalize discursively conscious claims for ecology, and the authorization of such attempts: How, when and where, and by whom, was the ecological discourse institutionalized in the form of ecological journals, ecological societies etc., and authorized by university and state authorities in the form of ecological chairs, ecological departments, other research and teaching positions, etc.? And how, when and where was a national ecological research policy adopted?

Finally we will take up questions brought to the fore by externalist historians of science, viz., the problem of the causal relation between human productive practice and scientific development. Since agriculture, forestry, fishery, etc., are domains of human practice which most conspicuously and directly interfere with the relations between animals and plants and their environment, conventional wisdom and classical externalist history of science would lead us to suggest that ecology, being a monopoly claim to a part of human knowledge taking precisely such relations as its object of study, originated as a scientification of these practical domains. But: did scientists oriented towards the solutions of practical problems of agriculture, forestry, fishery, game management, water pollution, nature conservation, etc., forward claims for ecology? And if so, did they also succeed to institutionalize them?

Enrolment into the social order of ecology

This focus on the individual claims for ecology is in accordance with Collins' suggestion that the best picture of science might be the image of

> *»an open plain with men scattered throughout it, shouting: 'Listen to me!, listen to me!'«.*[51]

That is, competition for attention is a fundamental process in science. Labelling, or denominating, one's activities as something, is certainly a most potent tool for achieving attention and recognition. But the open plain with men scattered around shouting »Listen to me, I'm an ecologist« does not constitute an ecology in the aggregate sense, that is, the emergence of ecology as a social order (a scientific discipline, a social movement, an ecological intelligentsia faction, etc.). Of course there are many ways of linking individual biographies with aggregate levels of analysis. Giddens' theory of structuration, referred to above, is one such approach. A successful attempt by a historian to join biographical details with a semistructural perspective is Daniel Kevles' *The Physicists*.[52]

However, none of these clearly addresses the issue of naming. From the view of sociology a fascinating approach to the generation of social order in terms of labeling is Duncan's notion of social drama and rhetoric.[53] Social life, social integration and social

51. Collins 1975,p.480.
52. Kevles 1978.
53. Duncan 1962 and 1968.

order are impossible without acts of naming, Duncan asserts:

> *»All social order depends on consecration through communication, or, as we have said in more specific sociological terminology, through naming...«.*[54)]

That is, »symbolic integration is achieved through naming«. Names are not fictions, or signs, but evocations of a social order; they are »goads to action«, as Duncan puts it.[55)] In the name of something »we organize our creative lives«, and »discipline our daily lives«. Or to put it in another way:

> *»Words become names, and in doing so, fix social meanings« /.../ A named thing or person exists both for the other and for me. Until it is named it has no social existence because it cannot be addressed (it has no 'reference')».*[56)]

Whoever creates names, controls the social action of others. In Duncan's theory of communication, the symbol systems governing a social order are transmitted and enacted through *dramas*,[57)] and, drawing on Burke,[58)] Duncan points to the role of rhetoric in drama. The goal of *rhetoric* is identification of the speaker with the hearer, and this is achieved by means of symbolic persuasion:

> *»The speaker persuades through stylistic identification in which he tries to identify himself with the listener's interests«.*[59)]

To pursue one's scientific activities under the name of this or that science, in this case ecology, is basically a means of creating and sustaining order in social relationships. Spreading the word ecology (or »x-ology«) is primarily a social and political act, including such elements as persuasion, social integration and power. Thus, ecology as a social order is integrated first and foremost by means of the language of ecology. By means of an ecological rhetoric, including elements such as »food-chains«, »ecosystems« and »population dynamics«, some scientists identify themselves with the interests of those not yet integrated into the social order of ecology, be they other scientists, laymen, politicians, state bureaucrats, technocratic planners, teachers, students, agronomists, naturalists, or environmental freaks. Like

> *»we love our country through the flag, our church through the cross, our neighborhood through a landmark«,*[60)]

we might say that some men (and a few women) at a specific point of time began to love nature, its animals and plants, the vegetation and the landscape through ecology.

Duncan's idea of the social order is a suggestive image having the qualities of a literary

54. Duncan 1968,p.23. The focus on naming is in also in congruence with the phenomenologists' focus on typifications. As Berger and Luckmann put it: »Social structure is the sum total of these typifications and of the recurrent patterns of interaction established by means of them« (Berger and Luckmann 1966,p.32).
55. Duncan 1962,p.431.
56. Duncan 1968,p.103.
57. As Duncan puts it: Social order...is always a resolution of struggle between superiors, inferior, and equals. This struggle takes the form of a great community drama...« (Duncan 1962,p.11). Dramas are usually tragedies and comedies. Tragedies stabilize the social order by means of generating outsiders and victims and expelling the critics and rule-breakers, while comedies stabilize the social order by means of doubt and disrespect, almost like the King's fool. The reader of the drama of Swedish ecology may decide whether he witnesses a comedy or a tragedy! Duncan's distinctions of superiors, inferiors and equals is akin to Bourdieu's distinction between dominant and dominated positions in the scientific field (Bourdieu 1975). The main difference between the two approaches to symbolic hierarchies is that Bourdieu expresses himself in a functionalist tradition, while Duncan takes an actor approach.
58. Cf. Burke 1950.
59. Duncan 1962,p.169.
60. Duncan 1962,p.431.

sociology,[61] but it lacks analytical rigour. This might be provided, however, by another approach to the phenomenon of scientification, which also takes into account the aggregation of microevents into macrostructural phenomena,[62] viz., the theory of *actor networks* proposed by Callon, Latour and Law.[63] Their point of focus is the process of scientification in terms of the dynamics of actor growth processes, viz., how microactors grow into macroactors. In this context, how insignificant, graduate students with a naturalist bent in the course of time grow into mighty ecological departments.

According to actor network theory, actors grow by means of *enrolment* processes. Whereas »recruitment« refers to social phenomena only, enrolment is a specific socio-logical relation[64] – it is impossible to differentiate between actor-actor relations as cognitive relations and social relations:

> *»Identifying a problematization postulates the existence of an actor«.*[65]

A growing actor (of any size) enrols other actors (of any size) by *identifying* and ordering them in relation to each other. Identification and ordering of actors can be described as interest *translation*, where translation stands for

> *»all the mechanisms and strategies through which an actor – whoever he may be – identifies other actors and elements and places them in relation to one another«.*[66]

Actors identify problems and interests, and translate these into an existing symbolic social order. On this view, interests are not to be seen as background factors to be discovered by the historian,[67] but as imputations made by other actors, persuading others by saying that »it is in your interest to...«.[68] Likewise actors define their own position in relation to others, for example by noting »I am interested in ecology«.[69] As a consequence an actor network is formed[70] Actor networks are in constant flux, they grow, expand, diminish, die out. Innumerable actors are involved in perpetual processes of naming, enrolling others on behalf of their interests. Simultaneously many actors bend other actors around them. The social world thus depicted is a continuous series of mutual actor-networking

61. Cf. Carroll's (1980) argument for a literary sociology.
62. Different approaches to the problem of aggregation of properties of individual actors or micrositusted social relations to macrosocial phenomena, see several articles in Knorr-Cetina and Cicourel 1981.
63. See Callon 1980, Callon and Law 1982, and Law 1984.
64. Callon 1980.
65. Ibid.,p.207.
66. Callon *et al* 1983.
67. As done by the Edinburgh school of sociology of science, see e.g. Barnes (1978) who recommends the use of »interest« as an explanatory resource, and Shapin (1982) who bases his argument on »professional vested interests«.
68. As far as possible the methodological principle has been to of analyze how the historical actors identify interests rather than imputing these interests on the actors. However, in several cases I have not been able to abstain from imputing interests, motives, desires, etc. on behalf of the individual actors.
69. Cf. Callon and Law 1982,p.622.
70. Like the actor network the research school (see above) is certainly a dynamic concept. The research school enlists graduate students, grows, stagnates and declines, and thus highlights a characteristic of science usually lacking in the literature on the emergence of scientific disciplines and specialties, viz., the relation between »mature scientists« and »advanced students«, or apprenticeship character of science. Besides the difference in abstraction level between the two concepts, the weakness of the concept of »research school« is that it confines the analysis to scientists and would-be scientists, thus making the process of scientification a causal relation between »science« and »society« instead of an inherent characteristic of the modern society (cf. Westfall 1981, op.cit.). That is, an ecological research school does not extend its socio-cognitive domain outside the sphere of science, thereby excluding naturalists, school teachers and environmental freaks from the analysis, whereas an ecological actor network in principle might extend its influence over the whole social world, as the ecologists did (cf. Ch.4). I have used the concept of school in the text when describing purely academic social orders, such as the Uppsala school of plant sociology (cf. Ch.2).

processes, enrolments and counter-enrolments.[71]

To sum up: the analytical scheme chosen here stresses ecologization as the emergence of a social order, not the progress of ecological concepts or methods. Although questions concerning conceptual and methodological development are not considered irrelevant, they are subordinated to the problem of the rise of the social order. That is, the ecologization of Sweden is analyzed in terms of a combination of Duncan's emphasis on social dramas and the growth of actor networks.[72]

Accordingly, the historiographical object for this treatise is precisely that social order which is constituted by the rhetoric[73] of ecology, as compared to other social orders, constituted by other rhetorics, whether they refer to the same »things out there« or not. For example, although the term »biology« was used frequently in the late 19th century to designate the same »things out there« which later were designated by the term ecology, we will not focus on the »biologists« any more than on the »proto-ecologists«.

Further, combining the actor network approach with Duncan's emphasis on naming implies that actors' naming practices bear witness to the importance they pay to other different actors around them. For example, a scientist writing a paper for a journal of botany, presenting his investigations as »botanical« findings and paying due respect to other well-known »botanists« in his list of references, not only considers himself a »botanist«, and is considered a »botanist« – but these accounts are the only actions that constitute him a »botanist«, that is, being enrolled into the actor network of botany. While another scientist, who reports on an identical investigation, even utilizing the same conceptual apparatus, but sends it to a journal of ecology, who quotes some internationally well-known »ecologists«, who gathers students around him for an »ecological« seminar and lectures on »plant ecology«, discloses himself as being enrolled, and simultaneously trying to enrol other actors, into a growing social order of ecology.

Methodological comments

In order to catch both the first insignificant utterances of the new word and the full-blown attempts to plan Sweden in the name of ecology the story has been extended backwards to the 18th and 19th centuries and forward as far as the 1970s. That is, starting with the first »proto-ecologists«, we end up with an institutionalized and state authorized knowledge monopoly claim, that is, a set of well-established scientific departments, chairs, societies etc., culminating in the large-scale ecology projects and the national ecology policy of the 1970s.

Wit the exception for the analytical distinction between »proto-ecology« and a discursively conscious ecology, there is no implicit attempt to adopt a stage model of scientific development hidden in this story.[74] The story follows a simple generation

71. Callon and Law 1982.
72. Cf. Latour's (1984) study of the »pasteurization« of France.
73. For a preliminary review of the literature on science and rhetorics, see Bazerman 1985.
74. Otherwise several stage models are possible, e.g., that implied in the »finalization« theory of the Starnberg group (Böhme *et al* 1973 and van den Daele *et al* 1979), and that of Clark 1972.

approach. Talking about past events we frequently express ourselves in terms of generations. This is not just a simple way of handling chaotic amounts of chronological data. Certainly many ideas are carried by a fairly distinct and limited number of age cohorts. When the word ecology came into use around the turn of the century it was adopted by scientists of widely different ages. In Sweden, the word was taken up by a fairly delimited generation of scholars trained as botanists around the turn of the century (cf.1-3). These pioneers constitute a first generation of ecologists. Similarly we might discern fairly well delimited second, third and fourth generations of Swedish ecologists. Accordingly, the reconstruction of the ecologization of Sweden has been organized in terms of the succession of generations of Swedish ecologists.

With few exceptions, the story has been limited to internal Swedish circumstances. Questions concerning the relations between Swedish ecologists (and »proto-ecologists«) and their colleagues abroad, have largely been left untouched.[75)]

Finally, a few words on the selection of sources. Scientists utilizing the rhetoric of ecology (in printed works) in the course of their careers were selected from a population consisting of all scientists (graduate students as well as post-graduates) listed in yearly reports from university departments directed towards the scientific study of animals and plants, i.e., mainly botanical and zoological departments, and all scientists employed at Swedish colleges and research institutes devoted to the practical study of animals and plants, i.e., agricultural research, forestry research, fishery research, etc. Special emphasis has been put on the subpopulation of graduate students submitting doctoral dissertations on animals and plants between 1895 and 1975.

The total population of scientists 1895-1975 scanned according to these criteria amounts to approximately 2000. Only a small proportion of these, about 300, have ever investigated the relation between animals/plants and their environment, and even fewer have ever identified themselves, or been identified, as ecologists. Rough outlines of the life-histories of these »proto-ecologists« and ecologists have been reconstructed, mainly by means of available biographical materials. Out of these, about 30 people were selected for more thorough studies, encompassing their scientific publications, popular scientific articles, applications for positions, etc.

The bulk of the written material consists of printed material available at the Swedish university libraries and public archives. The main primary sources include all relevant doctoral dissertations submitted to the faculties of natural science, to *Skogshögskolan* (the Forestry College) and to *Lantbrukshögskolan* (the Agricultural College), Swedish scientific and popular scientific journals and magazines, and yearly reports and lecture catalogues from the universities. Important secondary sources have been biographies of the main actors, and the series of *Statens offentliga utredningar* (Government Commission Reports, SOU).

The most important non-printed primary sources are *konseljakterna* (the Cabinet

75. Likewise the generation analysis has been restricted to the Swedish ecologists. No correlation to generations of ecologists in other countries is intended.

Meeting Acts) on professor appointments, which are publicly available at *Riksarkivet* (the Swedish National Archives). The acts[76] contain various non-printed documents considered in the appointment of professors and *docents*, including curriculum vitae's, lists of publications and other personal documents sent in by the applicants, the assessment reports on the applicants written by *sakkunniga* (members of the Faculty Assessment Committees), discussion minutes from meetings of *matematisk-naturvetenskapliga sektionen* (the Faculty Section) and *större akademiska konsistoriet* (the University Council), appeals and replies to appeals, and, in some cases, comments by *universitetskanslern* (the University Chancellor).

As a rule personal letters have not been consulted, partly because of the immense material available, but also because the focus of this study has been on public claims, public discourse and institutionalization.

Finally, a rather large interview material has been utilized. An extensive pilot interview programme, including 80 scientists and a few technicians, ranging from 1-3 hours discussions to short telephone calls, was carried out in the academic year 1976/77 (a few follow-ups were made in 1980-1982). All interviews were written down immediately, but only occasionally word for word. A second, and better prepared, series of deep-interviews, including 21 scientists and science policy makers, all key-actors in the institutionalization of ecology from the 1940s to the 1970s, was conducted in 1981 and 1982. These interviews, ranging from 2 to 5 hours, were, as a rule, tape-recorded, transcribed into Swedish, and used for reconstructions of biographies of the persons involved, and for reconstructions of local scientific environments. The transcribed interviews will later be deposited in a public archive under various degrees of restriction on use.

76. Referred to in the footnotes as e.g., ED 17/1 1947:1 (Ministry of Education and Ecclesiastical Affairs, Cabinet Meeting 17/1 1947, Act nr. 1), Jo 20/12 1940:19 (Ministry of Agriculture...), etc.

1 From natural history to the first claims for ecology

It is usually contended that it was Ernst Haeckel, the well-known German advocate of Darwinism, who coined the term »Oecologie« in the 1860s to denote a science that should fit into one of the pigeon-holes of his general system of the biological sciences (here quoted from his inaugural lecture in Jena):

> *»Unter **Oecologie** verstehen wir die Lehre von der Oeconomie, von dem Haushalt der thierischen Organismen. Diese hat die gesammten Beziehungen des Thieres sowohl zu seiner anorganischen, als zu seiner organischen Umgebung zu untersuchen, vor allen die freundlichen und feindlichen Beziehungen zu denjenigen Thieren und Pflanzen, mit denen es in directe oder indirecte Berührung kommt; oder mit einem Worte alle diejenigen verwickelten Wechselbeziehungen, welche DARWIN als die Bedingungen des Kampfe's um's Dasein bezeichnet«.*[1]

Haeckel's notion of the new science went largely unnoticed for another quarter of a century. The term was again, and seemingly independently, reintroduced, now as a new botanical specialty, by Reiter in 1885. By »Oekologie« oder »Haushaltslehre« Reiter understood a branch of science, also within an essentially Darwinian research program, which should study

> *»die Erscheinungen der Anpassung, die der Connex zwischen der Variabilität und den natürlichen Bedingungen der Existenz etabliert«.*[2]

It was only in the 1890s that the term began to spread among academic botanists. In 1893, a group of botanists meeting in Madison, Wisconsin, decided to adopt the word »ecology« to apply to this new field.[3]

In Europe the Dane Eugenius Warming stands out as the baptizer of the new science, by publishing the influential *Plantesamfund; Grundtræk af den økologiske Plantegeografi* in 1895 (a German translation, *Lehrbuch der ökologischen Pflanzengeographie* was published the following year).[4] By then ecology was mainly identified with the search for

1. Haeckel 1869,p.365. Haeckel's first mention of the term was in *Generelle Morphologie der Organismen* of 1866. He repeated it several times, e.g., in *Natürliche Schöpfungsgeschichte* of 1868. Haeckel introduced the word as a substitute for one of the meanings of the equivocal term »biology«, designating either the study of the living organisms in general, or in a more restricted sense as a synonym to ecology. Most students of animals and plants used to talk about the »biology« of the organism when referring to its relation to the environment. The term »biology« was used in this way far into the 20th century. Haeckel argued against the use of the term »Biologie«: »Diese Oecologie (oft auch unpassend als Biologie im engsten Sinne bezeichnet)« (ibid.,p.365). »Biologie« should, he said, denote the overall study of living beings, as suggested by, e.g., Lamarck and Geoffroy St. Hilaire in the early 19th century.
2. Reiter 1885,p.4.
3. Cittadino 1980,p.171; the circumstances are described in detail in Proceedings of the Madison botanical congress, Madison 1894,pp.35-38.
4. Warming 1895 and 1896. Altogether it appeared in four German editions, besides the 1896-edition, in 1902, 1918 and 1933. An English edition was published in 1909 (Warming 1909). See also Goodland 1975.

causal explanation of the distribution of plants on the earth's surface.[5] Zoologists also adopted the new word, though with greater caution. An editorial article in Science defined ecology as

> *»the branch of zoology or botany that is concerned with the dwelling place or distribution of animals or plants«.*[6]

As in America, from the turn of the century onwards, a growing number of Swedish scientists, trained as botanists and zoologists, began to announce their investigations in terms of ecology.[7] Ecology was increasingly identified as a special branch of the existing academic disciplines of botany and zoology. Already by 1902, some of the works assessed in the competition for a chair of botany were characterized as »ecological«.[8] The 1907 edition of the national encyclopedia defined ecology as:

> *»...a term formulated by H.Reiter in 1885, to cover knowledge about the adaptation and dependence of animals and plants to external circumstances (such as climate, nourishment, soil etc.), the totality of their relations to their organic and inorganic environment«.*[9]

But, of course, »ecology« was a term for the specialists. To the well-educated citizen it was one out of hundreds of technical terms used by professional botanists and zoologists. To the man in the street »ecology« was devoid of meaning.

The aim of this chapter is to present the background of this nascent ecological rhetoric in Sweden, by reviewing how studies of animals and plants were denoted and organized during the two centuries preceding the pioneer ecologists. This naturally brings us back to Carl von Linné and the natural historians.

Thus, section 1-1 outlines the rise and decline of the social order of natural history (including what in hindsight might be called a »proto-ecology«) both in the universities and within the realm of natural resource management. These »proto-ecological« practices did not constitute the immediate background for the first claims for ecology, however. The pioneer ecologists saw animals and plants from a Darwinian viewpoint. But Darwinism was a double-sided enterprise. The theory of descent lay behind the emergence of a »new German« laboratory-oriented botany and zoology. Section 1-2 describes the rise of the new disciplines of botany and zoology in the latter part of the nineteenth century, and the largely negative attitudes of professional botanists and zoologists to naturalist studies of animals and plants. Finally, section 1-3 presents the renewal of field studies under the impact of the Darwinist theory of adaptation, and the claims for animal/plant biology and animal/plant geography. The first animal and plant ecologists and their attempts to institutionalize a new scientific specialty are taken up in this context.

5. E.g., MacMillan writes: »That branch of biology which concerns itself with the adaptations of organisms to their surroundings is, by the modern school, termed ecology, this name having first been applied by Haeckel... Four main divisions of the subject are not difficult to indicate... Ecological morphology... Ecological physiology... Ecological embryology... and Ecological distribution... The essay in hand lies quite within the field of ecological distribution...« (MacMillan 1897,pp.950-51).
6. Editorial, *Science 15*,p.511 (1902), quoted by Klaauw 1937.
7. Some of the first outspoken claims for an independent science of ecology are mentioned below (1-3).
8. ED 12/8 1902:4 (cf.1-3).
9. *Nordisk familjebok 7*,p.115, Stockholm 1907.

1.1 The natural historians as »proto-ecologists«[10]

The pioneer ecologist at the turn of the century was typically a man on his way to make a regular Swedish academic career. He was aged between 25 and 30, training as a botanist at the university, writing his doctoral dissertation to construct his own version of the natural order of plants and vegetation. He probably would be looking to a public position as a secondary school lecturer, or as a research officer at an agricultural or forestry research institute; or even aspiring to be appointed a professor of botany.

Let it be said at once that the mere possibility of characterizing the typical pioneer ecology actor as »a prospective civil servant trained in botany at the university« is in itself a product of the late 19th century. Clear distinctions between zoologists and botanists, between state employed and amateur scientists, and between scientists having a practical rather than an academic orientation only emerged in the course of the 19th century.[11] Previously, studies of animal-plant-environment relationships had been an integrated part of the tasks of all-round natural historians. On the view taken here, the natural historians were the first »proto-ecologists«.

Kungl. Vetenskapsakademien (the Royal Academy of Science), the center of 18th century natural history, illustrates the all-encompassing activities of the natural historians. Founded in 1739 with the Royal Society in London and the Académie des Sciences as its models,[12] the Academy paid great attention to natural history from the outset. Although many of its members specialized in insects, flowers or birds, there were no formal distinctions between zoologists and botanists. When the Academy was divided into classes in 1798, one was established to deal with »external natural knowledge and natural history«,[13] encompassing all kinds of studies of »natural things«. Nor did the Academy make any clear distinction between amateurs and state employed investigators; in fact, the dilettanti constituted almost half of its membership.[14] Moreover, practical and pure investigations were juxtaposed in the Academy proceedings. Reports with no evident utility on the occurence of birds were published side by side with technical papers on the construction of ploughs, and observations on climate, soil conditions and plant growth.[15]

An Academy natural historian typical of his time was the parson Clas Bjerkander (1735-1795), elected in 1778. Making systematic notes on all kinds of events in nature, including weather conditions, the flowering of plants and observations of insect pests, he

10. This section is mainly based on secondary sources (except quotations).
11. These three dichotomic features – botanist vs. zoologist, state official/professional vs. amateur, and academically vs. practically oriented – have frequently been discussed in connection with the historiography of ecology. E.g., Lowe has discussed the interrelation between amateur naturalists and professional botanists in the early years of British plant ecology (Lowe 1976). Others have discussed ecology's roots in relation to practical/applied interests vs. academic interests (Kormondy and McCormick 1981,pp.xxix-xxvii). Finally, virtually all historians of ecology have pointed to the fact that although the term »ecology« was first invented by Haeckel, a trained zoologist, the first outspoken ecological claims were made by botanists, while animal ecology dragged behind for decades.
12. The history of *Vetenskapsakademien* during the age of the natural historians has been treated by Sten Lindroth (1967).
13. S.Lindroth 1981,p.21.
14. S.Lindroth 1978,p.51.
15. See S.Lindroth 1967.

contributed 49 articles for publication in the proceedings of the Academy.[16] Bjerkander studied animals as well as plants; although being an academically trained civil servant he was an amateur with regard to the study of animals and plants; and he combined the utilitarian programme of the age with a pure interest in natural history.

As in other European countries natural history was also established as a social order and learned discourse at the state universities during the 18th century.[17] The state authorization of natural history was largely made with reference to medical interests. By the late 17th century one of the two professors at the medical faculty in Uppsala, the leading national university (founded in 1477) was responsible for natural history, botany, chemistry and anatomy.[18] Olof Rudbeck and his son of the same name embodied late 17th and early 18th century natural history. The former founded a botanical garden, while the son conducted an expedition to Lapland, then an unknown and remote wilderness, and became known as a pioneer in the scientific study of animals.[19] After Rudbeck junior, Carl von Linné, already famed as a natural historian all over learned Europe, was appointed to one of the vacant chairs at the medical faculty in 1741, and entrusted with »natural history, pharmacology, semiotics and dietary science«.[20] For more than a century this chair continued to be designated a combined medical and natural historical chair attached to the medical faculty. The three subsequent holders[21] mainly devoted themselves to the study of plants, upholding the medical obligations to varying degrees.[22]

Although the capital had no university (its relation to Uppsala was akin to that of London to Oxford or Cambridge in 17th century England), it housed not only *Vetenskapsakademien* but also the *Collegium Medicum*, a hybrid of medical academy and government office. In the 1750s and 1760s three medical professorships were created, forming the basis of what in hindsight came to constitute *Karolinska institutet* (the Stockholm Medical College). On the suggestion of another of Linné's students, Peter Jonas Bergius, a professorship in »natural history and pharmacy« was created for him in 1761. The holders seem to have spent more time studying plants or birds than on their other medical obligations,[23] until the 1830s when the chair became a pure medical chair.[24]

Natural history was also authorized with reference to interests in natural resources. By

16. For a detailed treatment, see ibid.
17. The standard intellectual history of the 18th and 19th century natural history is Lepenies 1976.
18. S.Lindroth 1975.
19. S.Lindroth 1978,p.155.
20. G.Eriksson 1982.
21. The chair was held by Carl von Linné junior, an insignificant figure, between 1777 and 1783, and then by Carl Peter Thunberg, another Linné student from 1784 to 1828. Thunberg, a diligent collector and systematician, was the internationally best known Uppsala professor of his age (see S.Lindroth, 1981,pp.34-39.
22. The last of them, Georg (Göran) Wahlenberg, besides being known in the history of botany as one of the pioneers in discussing the climatic distribution of plants (see below), actually spent most of his time on homeopathy; hence practicioners of homeopathic medicine in Sweden also enlist him into their history (Sjögren 1918).
23. Bergius mainly devoted himself to plants, and donated money to *Vetenskapsakademien* for the creation of a botanical garden and a »professor Bergianus« in Stockholm. Both still exist (cf.4-4). Later Anders Sparrman, holding the chair during the 1790s (then called »medicine and pharmacy«), spent most of his time on birds and published an illustrated bird fauna of Sweden; Olof Swartz combined his position as profesor Bergianus with the Institute chair between 1813 and 1818; finally Carl Stenhammar, professor between 1818 and 1827, was considered one of the leading botanists of his age.
24. Lennmalm 1910.

means of a private donation, a chair in »practical economy« was created at the medical faculty in Uppsala in 1759, to assist the improvement of agriculture.[25] For the next half a century or more its holders were concerned both with floristic studies and with practical agricultural problems. Thus, Samuel Liljeblad's new and very popular Swedish flora of 1792, which for long remained the standard reference, also contained a lot of information on useful plants.

Likewise one of Linné's students and »apostles« was appointed to a newly created chair in »economy« at the provincial university in Åbo (founded in 1640) in 1747.[26] At the other provincial university in Lund (founded in 1666-68),[27] natural history was also connected to practical economy and agriculture, although some natural history had been taught by the medical professors. In 1756 a chair in »natural history«, financed by *Manufakturfonden* (the Manufacture Foundation) was created. Its first holder, Erik Gustaf Lidbeck, had to teach about animals, plants and minerals, and how to lay out plantations. Most of his time was, in fact, devoted to the introduction of mulberry trees for silk worm production. His successor Anders Jahan Retzius covered the whole of natural history, including studies of animals, plants, minerals, rural economy and chemistry.[28] After Retzius's retirement in 1812, the chair was divided into three; two of them being designated »botany and economy« and »natural history« respectively.

With the establishment of the social order of natural history, not only knowledge of »natural things« in themselves, but also of the »things« in their natural surroundings was advanced. Bjerkander, for example, did not restrict his notes to single species, but extended them to the environment as well. Cultivating mullberry trees or observing noxious insects fostered an insight into the life habits of plants and animals, including their dependence upon the environment – hitherto an experience transmitted between generations of peasants and hunters only. Thus, many of the activities within the confines of the social order of natural history might be designed as a kind of »proto-ecology«.

Most of these »proto-ecological« insights into the relations between animals, plants and their surroundings are concealed in diaries, travel journals and species descriptions. For example, Linné's accounts of his landscape travels during the 1730s and 1740s are filled with notes on animal life habits and descriptions of plant growth conditions.[29] This descriptive and occasional literature on the relations between animals, plants and the environment remained scattered and unsystematic, however – with one notable exception, viz., Linné's highly speculative works *Oeconomia naturae* and *Politia naturae* from 1749 and 1760 respectively.[30]

25. S.Lindroth 1978,p.30,102.
26. Pehr Kalm (See S.Lindroth 1978,pp.244-49). Neither Åbo university, nor its new existence as Helsingfors (Helsinki) university from 1827, is treated further here. This is a rather severe limitation since the cultural and scientific elite in Finland to a large extent were of Swedish nationality, and furthermore since the contacts between Finlandish and Swedish natural history, zoology, botany and emerging ecology were rather intimate during the whole historic period covered in this book (cf. Ragnar Hult below, 1-3).
27. Lund university was founded after the Swedish conquest of the former Danish provinces in 1658 as a means to maintain administrative power over the new dominions.
28. See Löwegren 1968,pp.29-30; the details of succession were rather complicated, involving the temporary creation of a combined chair in economy and natural history.
29. For example Linné 1969,1974 and 1978a.
30. Originally published in Latin. In Swedish transl. in Linné 1906 and in Linné 1978b; see also Broberg 1975 for a recent date treatment of Linné's work with special emphasis on his view of nature.

It is easy to find cognitive precursors to modern ecological concepts like biogeochemical cycle, food-chain and niche in *Oeconomia Naturae* and *Politia naturae*. For this reason some historians of ecology have taken Linné's works as classics of the science of ecology. Egerton, for example, has considered Linné the most important ecologist of his age, the first to realize the need for a unified science of ecology;[31] Worster identified Linné as one of the founding fathers of the idea of ecology, together with Gilbert White, the parson-diarist of Selbourne;[32] and the Swedish historian of science Gunnar Broberg, has similarly sought to update the reputation of the great systematist.[33] It is nevertheless a mistake to proclaim Linné as an ecologist.[34] Linné's holistic model of the relations between animals, plants and environment was in accord with his physico-theological world-view and the prevailing economic utilitarian view of society: that is, the Creator, the great economist, had instituted the household of nature as a counterpart to that of the nation or of the family.[35] Linné, of course, never made any claims for a new science of ecology – but that should not diminish his importance as a »proto-ecologist«.

The most conspicuous natural historian »proto-ecologist« in the early 19th century was the last successor to the Linnean chair in Uppsala, Georg (Göran) Wahlenberg (1780-1851)[36] – a Scandinavian contemporary of, and counterpart to, Alexander von Humboldt. Being an even more diligent traveller than Linné, Wahlenberg made extensive notes not only on the occurrence of plant species, but also on the occurence of minerals, on soil conditions and bedrock features, as well as meterological and geographical observations. For example, he noted that the occurrence of some southern plants on the islands of Gotland and Öland was due to

> *»the longer summer heat without considerable change, and a later autumn without early frost nights, by means of which the plants here could also ripen their seeds«.*[37]

Thus Wahlenberg approached what three quarters of a century later would be known as ecological plant geography. From 1814 onwards he gave lectures on plant distribution in relation to geological features. One of Wahlenberg's favourite schemes was to measure the soil mean temperatures as a criterion of the climate because »they stand in the closest relation to the plants«, an idea extended in the *Flora Lapponica* from 1812. Besides presenting a list of the families and species of plants, a review of their distribution and a history of the botanical exploration of Lapland, he also dealt with the division of Lapland into plant regions, the altitude of »Lappish earth«, the air temperature, the earth/soil temperature and the climate and its »growth-force«. This was much more in accordance with the Linnean landscape travels, than with the pigeon-holed activity of Linnés followers, and, by any measure, the first extensive »proto-ecological« work in Sweden.

31. Egerton 1977; Egerton 1975 actually translates Aristotle into a »population biologist«.
32. Worster 1977.
33. Broberg 1977 and 1978.
34. Cf. the argument in the Prologue.
35. The idea of the household of nature was not taken up by his followers. A majority of the academic Linnean successors turned to systematical problems rather than deepening the studies of animals and plants in their natural surroundings. This is true, for example, of Elias Fries, the leading Swedish botanist in the period from the 1830s to the 1850s, who devoted his life to profound systematical studies of lichens and fungi, while relegating his joyful field observations of the Swedish flora and vegetation to popular writings. Fries 1843-64; G.Eriksson 1962 has given a detailed account of Fries' scientific life's work in relation to the romantic ideas of the early 19th century.
36. For biographical details on Wahlenberg, see Sjögren 1918 and Anon. 1853.
37. In »Utkast till Gottlands flora«, *Kungl. Vet.akad.Handl. 28*, 1807; quoted by Anon. 1853,p.441.

Forestry and fishery investigations as a continuation of the practice of the natural historians

The utilization of forest resources posed few problems to the 18th century natural historians.[38] During the 19th century forestry investigations gradually emerged. *Skogsinstitutet* (the Forestry Institute) was founded in Stockholm in 1828 after the German model.[39] Being primarily intended for higher education of forest managers, it was also supposed to develop scientific forestry investigations, but little was achieved in this direction for over half a century. It is true that the foundation of a journal of forest management, *Tidskrift för skogshushållning* in 1850,[40] was of great importance in the propagating of the idea of »rational forestry«, but neither this private initiative, nor *Skogsstyrelsen* (the National Board of Forestry) established in 1859 (reorganized in *Domänstyrelsen*, the National Board of Crown Forests and Lands, in 1882), had any research objectives. In 1860 *Skogsinstitutet* was again enjoined to support »the development and distribution of forestry science within the country«,[41] but seemingly did not get the necessary resources, and a decade later the director was proposing a comprehensive research organization for studies of silviculture, logging, forest yield, growth, seed germination, fertilization and pruning, together with physiological and metereological investigations.[42] A permanent organization for scientific forestry investigation was not established until the end of the century (cf.1-3).

Although not incorporated within the social order of natural history the few forestry investigators were closely related to the natural history tradition. The teacher in »natural history«(!) at *Skogsinstitutet*, appointed in 1859 was obliged to teach not only:

> *»those parts of physics, chemistry and mineralogy, necessary for a forest climate science and soil science... forestry botany... and zoology, as far as this science had connection to forestry matters«,*

but also:

> *»to guide the pupils in the application of herbaria, stuffing of birds and mammals and keeping of higher animals and insects, to undertake mineralogical and botanical excursions together with the pupils during which to pursue soil investigations and examinations of plants, to visit the museum of the Academy of Science together with the pupils, and to be responsible for the Institute's collections of natural objects«.*[43]

According to the statutes a decade later, in 1871, this versatile teacher should also teach knowledge of hunting and

> *»imbue the pupils with dexterity in the art of shooting on the Institute's shooting range«.*[44]

38. E.g., the growing problem of forest devastation was countered with legislation, not with science.
39. The emergence of the »rational forestry«-movement and the forestry intelligentsia has not been the subject of any comprehensive historical reconstruction. Whereas forestry legislation goes back to the 14th century, and an administration for the crown forests was established in the 17th century, the forest counterpart to the agriculture intelligentsia emerged only slowly during the 18th century. *Skogsinstitutet* was mainly the result of one man's initiative, viz., I.A. af Ström (1778-1856) who wrote »Förslag till en förbättrad skogshushållning i Sverige« (Suggestions towards an improved forest economy in Sweden) in 1822. Earlier only a couple of elementary forest schools existed.
40. It was published by the director of a private forest school in 1850-1854 and in 1856, later by state employed forest managers from 1873 and onwards.
41. A.Wahlgren 1917,p.74.
42. Ibid.
43. Ibid,p.25.
44. Ibid,p.95.

Actually, August Holmgren (1829-1888), holding the position from 1859 to his death, had that versatility. He published a series of entomological articles, a work on Scandinavian mammals (1865) and one on Scandinavian birds (1867-71), he wrote on noxious insects, on harmful and useful birds, and on the importance of soil and climate on forestry. His biographer emphasizes his »sharpt talent for nature and notable capacity to attach interesting and original comments to even the most common phenomena in flora and fauna«,[45] developed during the excursions. His followers were kindred spirits, and most 19th century forestry investigations and higher forestry education largely retained its natural history and »proto-ecological« character, focusing on excursions and field study of plant-environment (soil, climate etc.) relations.

What has been said about forestry investigations so far is largely true for fishery investigations as well.[46] With a few notable exceptions,[47] 18th century natural historians paid almost no attention to fishery problems either. Lakes and rivers did not constitute a major economic resource; fishing was mainly a household activity and yielded limited surplus compared to agriculture. By the mid-19th century, however, general complaints about deteriorating fisheries led to recurrent demands for counter-measures,[48] although nothing comparable to the agriculture or forestry investigation organizations (cf.1-2 and 1-3) was established. During the following decades Academy members took occasional initiatives. *Lantbruksakademien* (the Academy of Agriculture), for example, appointed a teacher in fish culture in 1855, and in the 1860s the germ of a fishery administration and of an institutionalized fishery science was established when Sven Lovén, professor in zoology at *Naturhistoriska riksmuséet* (the National Museum of Natural History), suggested to *Vetenskapsakademien* that the former sporadic investigations should be placed on a permanent footing and led by a *fiskeriintendent* (fishery manager). The fishery programme proposed by Lovén was a task for a true polymath; the fishery manager was expected:

> *»to conduct scientific investigations of the fisheries, to suggest and make arrangements for the improvement of fishery, especially by means of fish culture, together with the protection of fish by means of putting out or transferring valuable kinds of fish into suitable waters, by means of the establishment of fish parks and fish ponds and by introducing improved fishery-tackles and tools«.*[49]

The *Riksdag* approved, a manager and two assistants were engaged, but did not succeed, however, in complying with this grand program.[50] The intended scientific fishery investigations were never started, and the extent of »proto-ecological« investigations within the realm of fisheries remained rather limited. In 1878 a *Riksdag* Bill proposing a regular state research institute, including a scientific experimental station and a fishery school, was rejected. A regular organization for freshwater fishery science investigations was not established until around the turn of the century (cf.1-3).

45. Ibid,p.116.
46. The following is mainly adopted from Svärdsson and Nilsson 1964,pp.9-10,37-41; Betänkande 1918,pp.94-102; and Betänkande 1920,p.122ff.
47. Linné's student companion Petrus Artedi (1705-1735) is sometimes considered the founder of modern fishery research not only in Sweden, but internationally as well (see Nybelin 1935; the archipelago parson C.U.Ekström (cf.below note 57) was the main advocate of fish studies in the early 19th century.
48. Stockhaus-Åberg 1980,p.5.
49. Quoted from Betänkande 1918,pp.95-96.
50. The first manager, Hjalmar Widegren (1838-1878), a student of Vilhelm Lilljeborg in Uppsala, engaged himself deeply in practical fishery problems (Widegren was the model for the fishery inspector Borg in August Strindberg's Nietzsche'an novel *I havsbandet* from 1890).

The amateur »proto-ecologists«

Of course, Linné's more than thirty years' reign in Uppsala had an immense impact on the establishment of natural history as a scientific social order in Sweden in the 18th and early 19th centuries. According to the late Sten Lindroth, the leading historian of 18th century Swedish natural science:

> *»Under Linné's banner natural history seemingly became an academic popular movement«.*[51]

In his life-time Linné exhibited great magnetism; his lectures usually filled the university auditorium in Uppsala. His *herbationes Upsaliensis*, or field excursions, provided the foundation for the coming naturalist movement. They are colourfully described by Lindroth:

> *»Dressed in prescribed uniform, sweater and jumpers, with botanical tins, insect nets and pins as weapons, the merry army marched in the footsteps of their master, listening to his comments on plants, insects and stones, and finally marched back home through the town gate in military order. As propaganda instruments the botanizing expeditions were unsurpassed, the participants numbered in the hundreds«.*[52]

The number of merry soldiers in Linné's flower army grew steadily. Few of them were engaged at the universities; after all there were only four chairs in natural historical subjects at the universities. Few could, like Bergius, manage to create a new chair for themselves. Some were engaged as secondary school lecturers – from the 1750s to the 1870s six such lectureships were created, and the new subject was gradually introduced as part of the school curriculum. At least in one case the lecturer combined his teaching duties with a district medical office.[53] Other Linneans held a state office like Carl Clerck, or were proprietors like Charles de Geer.[54] Probably the bulk of them were medical doctors in provincial towns or parsons at various parishes throughout the country.

The group of 18th century natural historians that most captures the imagination was the Linné »apostles« – the fifteen or so Linné students who for forty years travelled abroad far and wide. This collective phenomenon was evidently »a unique Swedish contribution to the history of science«,[55] not only for its scientific results, but also for serving as a model for natural historical explorers in the 19th century. Many of the »apostles« were from modest backgrounds; some may have been fortune hunters, some died in foreign lands; others returned home with eagerly awaited specimens to fill the blank spaces in the classificatory system. With the help of the »apostles« the Swedish social order of natural history was established as one of the leading ones in Europe.

Most of those attracted into natural history by Linné and his followers were amateurs, and they were at least as important for the development of the Linnean natural history tradition in 19th century Sweden as were the few, like Wahlenberg, who occupied natural history offices. This is especially true with regard to outdoor and »proto-ecological«

51. S.Lindroth 1978,p.180.
52. Ibid.,p.180.
53. Ibid.,pp.70-71,271.
54. The tax-collector Carl Clerck, for example, is said to have been inspired by a Linné-lecture to devote all his spare-time to the cataloguing of spiders and rare butterflies; and the foundry proprietor and marshal at the royal court, Charles de Geer, published a monumental 5000 page catalogue of insects in seven volumes between 1752 and 1778. See S.Lindroth 1978,pp.273-79 for details.
55. Ibid.,p.240.

studies. Although some amateurs pursued technical taxonomic work within the pigeon-hole »accountant« aspect of the Linnean tradition, others were among the most keen and diligent observers of animals and plants in their natural surroundings. Samuel Ödmann and C.U.Ekström, both serving as archipelago parsons for many years of their lives, are best known for their detailed observations of the life habits of birds, insects and fishes.[56] As Ekström commented, »few natural scientists possess the time necessary to observe fishes in free nature«.[57] In other words, the dilettant natural historian could spare the time for intense and prolonged observation, while his university colleague was tied up with academic intrigues, medical lectures or university council meetings.[58]

The most advanced amateur »proto-ecological« contribution during the age of natural history was made by Hampus von Post (1822-1911).[59] He was trained as a military officer, served with the cavalry until the age of 30, and then spent more than fifteen years of his life as a glassworks manager in Reijmyre in southern Sweden, before being appointed teacher in chemistry and geology at *Ultuna lantbruksinstitut*. He got his botanical training by personal contacts with Wahlenberg and Fries in Uppsala, but never took an academic degree, and never held a university position. Von Post was the keen nature observer, an empiricist who hated speculations:

> *»Everything which is assumed at a guess or by chance is a step backwards and must sooner or later be redone«.*[60]

These words he uttered at the age of nineteen in an address titled »a few words to the younger botanists of my native country«. He never abandoned this anti-speculative attitude running through his many-sided life's work.[61] His empirical studies of moraines served as one of the major foundations of the glaciation theory; he studied the genesis of organic sediments, oozes, muds and peat; at Ultuna he investigated different varieties of potatoes and the effects of fertilizers on various crops. All kinds of observations of »natural things« filled his note-books.

Von Post's »proto-ecological« awareness of the relations between plants and their environment is best expressed in his program for the investigation of plant communities. In his youthful address mentioned above he had suggested that »plant geographic localities« should be denominated as such, and he pleaded for plants to be catalogued according to locality as well as their systematic relationship, a suggestion later considered as a precursor to the synecological and plant sociological programme of the Uppsala school three quarters of a century later (cf.2-2 and 3-2).[62] The prime task for plant geography, he said, should be to

56. Brusewitz, himself an amateur naturalist, has written some very insightful portraits of the two men (Brusewitz 1968); otherwise, see S.Lindroth 1967.
57. Quoted from Brusewitz 1968,p.73.
58. It should be added that field observations probably did not have very high status either; it is probably significant that Wahlenberg's field investigations, although popular among the students, remained rather unnoticed by his contemporaries, while his systematist colleagues won much higher recognition.
59. For biographical details on von Post, see Sernander 1912 and Hesselman 1942.
60. Quoted from Sernander 1912,p.173.
61. Von Post's anti-speculative attitude was closely connected with his anti-darwinism and anti-materialistic outlook; in these respects he sided with his former teacher Elias Fries (cf.note 35) – see also G.Eriksson 1962,pp.448-56.
62. Accordingly Sernander and the Uppsala school saw him as one of their ancestors; but it is worth noting that *both* Hesselman and Sernander, the pioneer claimants of plant ecology in Sweden, wrote a biography on von Post.

> *»investigate those associations of several plant species which together occupy a similar place covering or settling on the earth's surface«.*[63]

This programme, first put forward in a lecture to a student's association in Uppsala in 1846 (but not published until 1862) included the following elements: an analysis of plant species growing together under similar physical and chemical conditions; the relations between these plant species (e.g., domination by means of shading); the periodicity in relative numbers of plants from year to year and under different climatic conditions; the physical and chemical character of the growth media; and finally the development of different plant species under different natural conditions.[64]

Von Post's program was astonishingly advanced and could serve as a basic research programme for any ecological institution to this day; the object of his research is the object of modern ecology. The only, and great difference, is that von Post's programme was devoid of ecological concepts, i.e., concepts developed within an explicit ecological discourse. Firstly, because, true to his own empiricist ideals, his observations never led to anything like a theory, or even theoretical concepts. Secondly, because von Post never thought of ecology as an independent science, and never claimed his programme as a new domain of study. Nevertheless his life's work was surely guided by a clear insight into the interrelationships between the different kinds of »natural things«. But his proposal for methodical empirical studies of the distribution of plants did not strike root. The natural philosophic (and romantic) attitude then prevalent among scholars was unreceptive to von Post's empirical, observational and methodical program.[65] The editor of the recently created *Botaniska Notiser*, although responsive to the young man's initiative, considered lists of plant species growing together at similar localities to be »unnecessary« on the presumption that growth conditions were invariable throughout the country.[66]

Another outdoor activity lending itself to amateur observations is, of course, hunting. From early times hunting was a free activity in Sweden, but was gradually restricted to noble privilege in the 17th century. The French revolution was a major reason why the peasants regained their hunting rights in the late 18th century, but the consequence was a veritable slaughter of all kinds of game. This state of affairs is said to have been one of the causes behind the foundation of *Svenska Jägareförbundet* (the Swedish Hunters' Association) in 1830 with one of its outspoken goals to establish »an appropriate game management«. After a couple of years the Association counted more than a thousand members. Its journal, *Tidskrift för jägare och naturforskare* (Journal for hunters and nature investigators), published in 1832-34, and again from 1863 (now as *Svenska Jägareförbundets nya tidskrift*), abounds with lively reports, often of a high literary standard, indicating the extent of knowledge about animals and their habitats among the growing number of members (approx. 6000 in 1918).[67] The art of hunting perpetuated the »proto-ecological« knowledge of the natural history tradition. One of the Associations's most prominent scientific members said in the early 1900s:

63. In von Post »Försök till en systematisk uppställning av vextställena i mellersta Sverige«, Stockholm 1862; quoted in Sernander 1912,p.157.
64. Sernander 1912,p.158.
65. Ibid.,p.159.
66. Ibid.,p.159.
67. A popular magazine, *Från jaktmarker och fiskevatten* (From hunting grounds and fishing waters), was initiated in 1913.

> *»To the art of hunting belongs ... first of all, an accurate knowledge of the appearance, way of life, reproduction and habits of the huntable animals... the natural history of the animals«.* [68)]

The practice of hunting was mainly an amateur achievement and was only partially incorporated into the social order of natural history, though the last professor in natural history in Lund, Sven Nilsson (1787-1883), was one of the founding members of *Svenska Jägareförbundet*. Otherwise, only a minority of academic natural historians, and later zoologists, paid attention to hunting and game management problems. During the 19th century investigations and education concerning game management became an integral part of the tasks of *Skogsinstitutet*, and later, in the early 20th century, *Skogshögskolan* (the College of Forestry).[69)] The Association was mainly an interest association of hunters, its administrative responsibilities were negligible, and neither separate research positions nor an independent educational organization was suggested.

Hunting was one of the outstanding sources for »proto-ecological« awareness during the 19th century. However, it would be misleading to assert that the emergence of ecology drew on hunting practices. Although the above-mentioned journals abound with notices of a »proto-ecological« character, no claims for ecology can be found among them (in fact, not even claims for a separate game science). On the other hand, it is true that Sven Ekman, a pioneer claimant of ecological animal geography (cf.1-3), paid great attention to hunting – observations of game animals evidently provided a basis for his translations of studies of animal distribution into ecological animal geography.

The legacy of the natural historians

Although the first claims for ecology as an independent scientific specialty in Sweden were not put forward until around the turn of the century, studies of the relation between the organisms and their environment was an inherent part of the activities of 18th and 19th century natural historians. Thus, even though it is misguided to interpret Carl von Linné as an ecologist, the natural historians exhibited an extensive »proto-ecological« awareness.

The ideal-typical natural historian and »proto-ecologist« was a kind of renaissance man. He knew a lot about the animals, their habits and how to shoot them. He knew many hundreds of flowering plants, including which were edible and which were poisonous. He was interested in the progress of agriculture. As a parson, a medical doctor or a proprietor he transcended everyday knowledge by acquaintance with accumulated wisdom of generations of peasants and the refined scholarly attentiveness to species descriptions and species distribution taught by the Linneans at the universities. In that sense early 19th century Sweden had plenty of »proto-ecologists«. On the genealogical viewpoint adopted here the activities of the »proto-ecological« natural historians were an important background for the first claims for ecology. As will be shown in the following

68. Grönberg 1910.
69. August Holmgren has already been mentioned above; later Gösta Grönberg (1871-1934), who was trained as a zoologist in Stockholm, and was appointed teacher at *Skogsinstitutet* in 1901, took care of game management.

sections, however, ecology was not a simple further extension of the activities of the »proto-ecological« natural historians. The first claims for ecology were made from a Darwinian position, which, in turn, was the result of a fundamental reorientation of animal and plant studies.

On another view, however, the natural historians contributed to

> *»turn natural history into a fashionable movement which flourished in full vigour into this* /the 20th/ *century«.*[70)]

This popular movement, albeit originally restricted to the well-educated strata, laid the foundation of the naturalist mass movement of the 20th century (cf.3-1 and 4-1). So, even though none of the 18th or 19th century »proto-ecologists« claimed their pursuits as an independent science (and least of all as ecology), and even though their activities were not the immediate precursors to the nascent social order of ecology in Sweden, the steadily growing naturalist movement was of ultimate importance as an enrolment base for later generations of ecologists.

1.2 The rise of new professional scientific social orders: a turn-away from field studies

Like in other European countries, the social order of natural history in Sweden, originating with the Rudbeckians in the late 17th century and expanded by Linné and the Linneans, came to a climax during the first decades of the 19th century. The following desintegration of the social order of natural history was accompanied by the establishment of new social orders for the study of animals and plants. To a large extent these new social orders implied a turn-away from the study of living nature, and hence from »proto-ecology«.

Commencing professionalization of animal and plant studies

With regard to the practical study of animals and plants and rational natural resource management this turn-away from »proto-ecology« is illustrated by the emergence of an agricultural science organization.[71)] Countywise agricultural economic associations (*hushållningssällskap*) were organized during the first decades of the century. Having been proposed already during the days of mercantilism[72)] they forwarded the physiocratic

70. S.Lindroth 1978,p.146.
71. The emergence and development of the Swedish agricultural intelligentsia has not been the subject of any comprehensive historical investigation (cf. however, Juhlin Dannfelt 1899 and Frykholm 1949). Hence it is beyond the scope of this treatise to analyze the power relations involved in the creation of the agricultural science organization during the course of the 19th century. Of course the organizations of the peasants and the nobility were important background policy groups supporting the growing national agricultural science organization.
72. Beginning with an organization in Gotland in 1791, a number of *hushållningssällskap* were created, mainly during the 1810s; for a historical treatise of the organizations, see Kempe 1923.

idea of a »rational agriculture«,[73] organized local field trials and were the prime movers behind the establishment of a number of regional and local agricultural stations for routine investigations.[74] *Svenska mosskulturföreningen* (the Swedish Peat Culture Association, established in 1886/88 for the purpose of cultivating mosses, mires and peat forest land), and *Sveriges utsädesförening* (the Swedish Seed Association, established in 1886/94) for plant breeding,[75] were also largely initiated by the county economic associations.

The state counterpart to these regional initiatives, *Lantbruksakademien* (the Academy of Agricultural Science), founded in 1811 with the aim to contribute to Swedish self-sufficiency after the Napoleonic wars, rapidly became the national center for the rationalization movement in agriculture, founded a large library, and published a series of Annals. In 1820 an Experimental Field (*Experimentalfältet*) was established, constituting the germ of the later central agricultural research organization.[76] Most important for the reproduction of this growing agricultural intelligentsia was the establishment of schools of agricultural education from 1834, and later two *lantbruksinstitut* (schools for secondary agricultural education) at Ultuna outside Uppsala (1848), and at Alnarp outside Lund (1862).[77]

With these steps, including the creation of *Entomologiska anstalten* (the Entomological Institute) in Stockholm in 1897 for studies of pest damage on crops[78] an independent agricultural science organization was established by the turn of the century. In 1907 most investigation and research institutions were incorporated in *Centralanstalten för försöksväsendet på jordbruksområdet* (the Central Institute for Agricultural Research).[79] As a sign of its scientific status the department directors were titled professors, and

73. The idea of a »rational agriculture« was effectively spread by the series of national agricultural meetings, held approx. every five years from 1846 onwards. An historical treatise of the »rational agriculture« movement, as well as its »rational forestry« and »rational fishery« counterparts is badly needed in order to give the background to the accelerating 20th century rationalization of society.
74. A number of regional agricultural chemical stations for routine soil analysis were created from 1876 onwards; likewise a number of seed control stations and milk control stations were established.
75. See Osvald 1951.
76. Another event signalling the emergence of an agricultural intelligentsia was the positions of »agricultural engineers« from the 1840s/50s, enlarged to a nation-wide county organization from 1886
77. Several university trained botanists of the age were attached to them. For example, Elias Fries, the leader of Uppsala botany in the 1840s and 1850s (cf.note 35) was a member of the board of the Ultuna school in its early years. But often the teachers, such as Hampus von Post, had no academic degrees – a situation prevailing until the late 19th century when the staff was almost without exception university trained and authorized as professional botanists. For a detailed history of the Ultuna school, see Frykholm 1949.
78. *Entomologiska anstalten* (on its foundation, see Butovitsch 1952) was initiated by members of *Entomologiska föreningen* (the Entomological Society) in Stockholm, particularly Sven Lampa (1839-1914). Lampa, the son of a prominent land owner, never finished high school or ever pursued academic studies. Having a strong naturalist interest he was employed as preparator at *Naturhistoriska riksmuséet*. Gradually he turned his attention to economically important insects and published a large number of articles on practical entomological problems in *Entomologisk tidskrift*, the journal of the Society. Lampa soon became the leading Swedish entomologist; he was one of the founders of *Entomologiska föreningen* in 1879; from 1887 he was commissioned by *Lantbruksakademien* to make nation-wide investigations of noxious insects; and in 1897 he was summoned as the first director of *Entomologiska anstalten*.
79. *Centralanstalten* had departments for agriculture (incl. plant husbandry), agricultural chemistry, animal husbandry, agricultural botany and agricultural entomology (the former *Entomologiska anstalten*, cf.note 78). Later a bacteriological department was added. *Svenska mosskulturföreningen* remained independent, however, for several decades. The prime mover behind this centralization of agricultural research was an individual and mainly self-taught scientist and administrator, Herman Juhlin Dannfelt (1852-1937), an early student of Hampus von Post at Ultuna, who became lecturer at Ultuna at the age of 40 and ten years later secretary of *Lantbruksakademien* – Dannfelt was the central figure in Swedish agricultural science for almost half a century.

selected by means of assessment committees, as at the universities; in 1912 the teaching at the two schools in Ultuna and Alnarp was rearranged on a more scientific basis, and in 1918 the teaching positions at Ultuna were changed into professorships.[80] In 1909 a Riksdag bill proposed the establishment of an agricultural college.[81] Thus, with respect to agricultural investigations, the social order of natural history had eventually been replaced by that of an independent and state authorized agricultural science.

Agricultural investigations rarely involved »proto-ecological« investigations, however, not to mention discursively conscious ecological studies.[82] For example, after Hampus von Post had been attached to Ultuna he spent the next 23 years on extensive fertilization trials, and never returned to the study of plant-environment relations (cf.1-1). His early »proto-ecological« ideas, fostered by his natural historian approach were translated into problems of agricultural science, particularly agricultural chemistry, which was the main scientific discipline within agriculture. In 1856/64 *Experimentalfältet* was supplemented by a chemical department which pursued agrochemical investigations in the wake of Liebig. The study of plants was also translated into the »new German« laboratory botany including plant physiology. In 1876 *Lantbruksakademien* engaged a »botanist«(!) for »microscopic and plant physiological« investigations, and a decade later a department for plant physiology was established, although in practice it restricted its work to investigations of plant diseases.[83] The work at *frökontrollanstalterna* (the Seed Control Institutes) was closely attached to the emergence of plant physiology as a scientific specialty.[84]

Independent science organizations were established for forestry and fishery investigations too. The reorganization of *Skogsinstitutet* in Stockholm into *Skogshögskolan* in 1912, the creation of *Forstliga försöksanstalten* (the State Forestry Research Institute) in 1902, and the creation of an independent organization for marine fishery investigation in 1901, *Svenska hydrografisk-biologiska kommissionen*, will be discussed below (1-3) in connection with the pioneer ecologists. But in contrast to the non-»proto-ecological« character of agricultural research the establishment of these independent scientific social orders of forestry and fishery research did not involve a turn-away from »proto-ecology«, as will also be demonstrated below.

80. Frykholm 1949,p.37.
81. Utredning 1913; yet another step in the development of agricultural science was taken in 1932 with the foundation of *Lantbrukshögskolan* (the Royal College of Agriculture, cf.2-1).
82. Exceptions like Robert Tolf's extensive field investigations for *Svenska mosskulturföreningen*, based on the assumption that »climatic conditions and the chemical and physical composition of the soil assert a highly important influence upon the result of the struggle for life of the plant species« (Tolf 1891,p.2; see also Tolf 1896,p.162), and Ernst Henning's »agronomical« inventory of the flora in Jämtland county pursued in 1887-88 for *Sveriges geologiska undersökning* (the Swedish Geological Survey), made in order to: »investigate, on which sites good forage-plants appear in mass or «stand», further the influence of different rocks and soils on the composition of the vegetation...« (Henning 1889a,p.3; cf. Henning 1887 and 1889b), do not change the general impression that 19th century field investigations of animals and plants aiming to contribute to the problems of agriculture were only marginally of a »proto-ecological« character. Neither were the »proto-ecological« investigations denoted as ecological, although »a summary course in ecology (knowledge of adaptations) and plant physiognomy (knowledge of plant communities)« was planned to be incorporated as a subordinate element in the teaching of plant geography at the Alnarp and Ultuna schools and at the proposed new agricultural college (Henning 1912). For biographical details on Tolf and Henning, see von Feilitzen (1904) and Lindfors (1944), respectively.
83. Jakob Eriksson 1913.
84. Cf. Areschoug in Lund, below.

The gradual disintegration of the social order of natural history implied a turn-away from »proto-ecology« at the universities too. Academic studies of animals and plants were increasingly claimed as the autonomous scientific disciplines of botany and zoology respectively. Chairs and institutions devoted to the study of botany and zoology were created; gradually specific training programmes for zoologists and botanists were established.[85]

The establishment of the new specialized academic disciplines of botany and zoology, parallel with developments in Germany, was led by the academies. Already in 1821 the former class of natural history at *Vetenskapsakademien* was superseded by a class of »zoology and botany«.[86] The Academy also created chairs for the custody and enlargement of its collections. Already in 1823 the curator of the plant collections was called professor. Five years later a zoologist was engaged to organize the animal collections, and in 1831 this curator too was titled professor. Likewise, the provincial *Göteborgs kungl. vetenskaps- och vitterhetssamhälle* (The Göteborg Royal Academy of Science and Letters), having had one single class for the whole of science at the time of its foundation in 1778, instituted a special class for »zoology and botany« in 1832.[87]

Somewhat later the natural history chairs at the universities in Uppsala and Lund were replaced by chairs in botany and zoology. The Borgström endowed chair in Uppsala was transferred to the philosophical faculty around 1850 and denoted »botany and practical economy«, though in practice it became a pure botanical chair.[88] After Wahlenberg's death in 1851, the old Linnean natural history chair was transformed to a medical chair, and instead an entirely new chair in zoology was created, thus instituting animal studies as an academic enterprise for the first time in Uppsala. In Lund the chair in botany and economy created in 1812 was soon restricted to botany only;[89] the chair in natural history was denoted as a zoology chair in 1857.[90]

Thus by the 1850s studies of plants and animals at *Vetenskapsakademien* and at the universities were unambiguously defined as botany and zoology and legitimized by the state authorities. From 1871 graduate students could get a specialist competence (*fil.lic.*-degree)[91] in botany or zoology and hence be authorized as such. From then on, one was not only a nature investigator or natural historian, but a professional botanist or zoologist. The two professions spread their message by creating a number of local and later national botanical and zoological associations and journals; the first was established

85. The terms »botany« and »zoology« are, of course, much older, but studies of plants and animals were not institutionalized as such until the mid-19th century.
86. E.W.Dahlgren 1915; in 1905 the class of »zoology and botany« was divided further into two separate classes.
87. Beckman 1928,pp.14,28.
88. S.Lindroth 1976,p.157; the addition »practical economy« did not disappear until the 1940s.
89. Törje 1968,p.36.
90. Löwegren 1968,pp.69ff.
91. Largely corresponding to a Master's degree; cf. below.

already in 1838, and the last additions came in the inter-war period.[92]

We may distinguish between two significant institutional phases in the establishment of botany and zoology as independent scientific social orders – first the rise of the museums as the nuclei of botany and zoology towards the mid-19th century, and second the establishment of anatomical, cytological and physiological laboratories towards the end of the century.

Refinement of systematic botany and zoology - the museums

The »proto-ecological« character of 18th century natural history should not conceal the fact that most Linneans were first of all systematicians. Ever since Linné the species was the central object of study. The core of the Linnean program was the inventory of fauna and flora –to find out what kinds of animals and plants existed and where to find them. The species should be differentiated, described, named and assorted into their positions in the classificatory system. Collecting, describing, and classifying were the three main activities of the Linnean natural historians. The results of the large faunistic and floristic inventories of the country had been published throughout the first half of the 19th century, in large multivolume works, often with beautiful colored illustrations, such as Anders Sparrman's *Svensk Ornithologie* (1805-1816), Sven Nilsson's *Skandinavisk fauna* in four parts and up to three editions between 1820 and 1860, J.W.Zetterstedt's *Diptera Scandinaviae* (1842-1860) just to mention a few zoological examples,[93] or Carl and Carl Johan Hartman's *Handbok i Skandinaviens flora* which came in 11 editions between 1820 and 1879.[94]

To begin with, the establishment of the new and independent academic social orders of botany and zoology did not involve any substantial change of direction in animal and plant studies. Until the 1880s and 1890s a majority of the zoologists and botanists holding leading positions devoted most of their research and teaching efforts to systematical problems, including morphological descriptions. Under the heading of »Botany and Zoology (incl. Palaeontology)« the Swedish Book Catalogue Index 1866-1875 almost exclusively lists systematical works, advanced floristics and faunistics and descriptive morphology. Of course, the object of research was increasingly sophisticated. Systematical problems became more and more intricate, demanding finer and finer morphological investigations, utilizing microscopes and large collections of material from across the

92. The first botanical journal, *Botaniska notiser*, was initiated in Lund by Elias Fries in 1838 and two decades later it was followed by a local botanical association, *Lunds botaniska förening* (for a historical review, see Weimarck 1980). Uppsala students could enrol in a botanical student association from 1865, and the students at the newly established *Stockholms högskola* (the University College in Stockholm) founded their botanical society, *Botaniska sällskapet*, in 1882. The nation-wide *Svenska botaniska föreningen* (the Swedish Botanical Association) and its journal, *Svensk botanisk tidskrift*, was not founded until 1907, however; somewhat earlier *Vetenskapsakademien* had began to publish *Arkiv för botanik*. Zoological counterparts to these botanical associations and journals came too, although somewhat later. The Uppsala students created a zoological association in 1865; in Lund and Stockholm local zoology societies were formed in 1866 and 1893 respectively. No zoological counterpart to *Svensk botanisk tidskrift* was established, however. The first zoological journal was launched by *Vetenskapsakademien* in 1903 (*Arkiv för zoologi*); later followed *Zoologiska bidrag från Uppsala* (1912) and *Acta zoologica* (Stockholm 1920).
93. 18th and 19th century Swedish zoological literature has been thoroughly reviewed by Dal 1974.
94. For bibliographical references, see Krok 1925.

world for making comparisons. It was generally the privilege of the specialist to approach animal and plant groups which needed a microscope for their discrimination, i.e., algae, lichens or fungi, or marine animals which were available by means of ships and expensive dredges only. Hence marine animal invertebrates (»worms«) and cryptogams gradually became the favourite topics of the specialists, gradually replacing flowering plants, vertebrate animals and insects as the object of research.[95)]

As a consequence the rift between the professional investigator and the dilettant deepened. Amateurs could in principle specialize too, maybe even buy a microscope and make use of the large Academy or university collections, but the merry dilettanti usually restricted themselves to land-living macroscopic animals and flowering plants, including aesthetically more appealing groups like butterflies, beetles, birds, and flowering plants. This tendency became even more accentuated after the Darwinian revolution. The systematic work of museum specialists was increasingly guided by the theory of descent, turning individual findings into data used for the construction of phylogenetic trees, while the naturalist amateurs were guided more by aesthetic motives or simply enjoyment.[96)]

Descriptive systematics and morphological studies were at their height around 1840 to 1880, i.e., during the establishment of the two social orders of botany and zoology. It had an enormous impact on university training. Many generations of scholars were trained mainly in morphological and systematical botany and zoology. Far into the 20th century studies of the morphology and taxonomical variation of conserved animals preserved in spirit or formalin, or cut to slices with the microtome, and of stick herbarium sheets and flower diagrams constituted one of the two pillars of university training in botany and zoology. As late as the 1940s the professor in zoology in Lund is said to have demanded knowledge of »600 crustaceans« for a first-year examination.[97)] Likewise the herbarium collections became the inflexible axis around which academic studies revolved.

The systematical and morphological practice of botany and zoology had an immense impact on secondary school education as well. Botany and zoology students trained as cryptogram or »worm« systematicians and morphologists, and having published systematical and morphological dissertations, were employed as lecturers in »natural history« in the secondary schools. Bengt Lidforss, the social radical and later professor in plant physiology in Lund, commented upon what he called »the futile educational value« of his school years in the early 1880s, when being trained, year after year, in outer morphology and classification of plants:

> *»The botanical teaching is mainly aimed at enabling the pupils to differentiate and name a number of wild plants, the more the better, and this in turn demands knowledge of the meaning of some botanical terms, a knowledge which nowadays is indicated by the proud word organography«.*[98)]

The institutional counterpart to the professional craftmanship of descriptive systematics and morphology was the museums, mostly established during the first half of the

95. For details concerning the development of academic botany and zoology in Sweden during the 19th century, see the sources given in the footnotes below.
96. More than anyone else, Gunnar Brusewitz has contributed to our understanding of the amateur naturalist joy in Sweden; see Brusewitz 1982.
97. Interview with NN and NN 8/9 1981.
98. Quoted from R.Karlsson 1983,p.18.

19th century as a kind of joint venture between the bureaucratic state and botanists and zoologists seeking office. The genealogy of the museums stretches back into the 17th century. Natural historians had gathered their »natural objects« into cabinets,[99] and the Linné-apostles contributed considerably to their expansion; Solander, taking part in Captain Cook's sailing round the world, Kalm, visiting North America, and Thunberg, collecting in Japan, were followed by hundreds of others. With the age of steam ships and large scale Northwest European colonialism almost every young botanist and zoologist aspired to make a long expedition to other continents. Expeditions to South America, the Far East, Africa, etc., were part of the standard post-graduate apprenticeship for zoologists and botanists far into the 20th century. As a consequence of these travels the natural cabinet collections grew from private cabinets to large, state-supported museums.

At the university in Lund the leading faunist of the early 19th century, Sven Nilsson (1787-1883) created a zoological museum based upon Retzius' old natural cabinet. Likewise, Vilhelm Liljeborg (1816-1908), the leading faunist after Sven Nilsson and the first holder of the zoology chair in Uppsala saw as one of his first duties to create a separate zoological museum in Uppsala. A botanical museum was established there in 1856, and a decade later Jacob G. Agardh raised funds for the erection of »a house for collections and a gardener's home« in Lund.[100] The museums expanded, presumably reflecting the national prestige and enrolling power of the new social orders of botany and zoology.[101] The establishment of university museums culminated around the First World War, when the Uppsala and Lund zoologists got huge buildings housing the large scientific and public collections.

The leading museum, exhibiting the most precious treasures, however, was created by *Vetenskapsakademien* in Stockholm. The formerly disparate Academy collections of natural things were gathered into *Naturhistoriska riksmuséet* (a name symbolizing the climaxing social order of natural history) in Stockholm in 1841. The zoological collections were further specialized into invertebrate and vertebrate departments, both having a professor as a superintendent.[102] From the 1840s and for approximately half a century onwards the professors at *Riksmuséet* were considered the most prestigious in Swedish botany and zoology, as reflected by the support they got for a new museum building – a monumental edifice, 20.000 sq.m. floor area in national romantic architecture, completed in 1916 at a cost of three million SEK.[103]

The huge new museum buildings mark the high-point of the age of Linnean systematics and morphology. New approaches to the study of animals and plants soon succeeded

99. For a thorough inventory of natural cabinets in Sweden, see Löwegren 1952.
100. Törje 1968,p.64.
101. Gunnar Eriksson has suggested that the emergence of systematic botany and zoology, being part and parcel of the great inventory of Sweden, was a consequence mainly of national interests (G.Eriksson 1978,p.202). That is, while it is true that 18th century faunistics and floristics and Linnean systematics was supported for productive reasons, it seems reasonable to impute a national interest behind the late 19th century state support for descriptive systematics and morphology as necessary tools for the great national inventory. It is rather unlikely that the museum Linneans, and hence the institutionalization and extension of mid 19th century botany and zoology, had anything to do with any industrialist interests; the industrial revolution rather coincided with the decline of the systematists in the 1880s and 1890s, and the rise of the laboratory botany and zoology (i.e., what below is called the »new German« botany and zoology).
102. Later special departments were created for paleozoology and paleobotany. For a historical review, see Naturhistoriska Riksmuséets historia 1916.
103. Naturhistoriska riksmuséets historia 1916.

systematics and morphology. The last institutional expression of systematic botany was the establishment of a botanical garden and an independent natural history museum in Göteborg by the end of the First World War. When the fifth university was founded in Umeå in northern Sweden in the 1960s, a museum was not even suggested.[104]

The decline of the museums was largely a post-First World War phenomenon, however. For the period considered here, i.e., the late 19th century, the botanical and zoological museums constituted the nuclei of the first phase of the establishment of the new scientific social orders of botany and zoology. To be true, many museum botanists and zoologists were ardent naturalists as well. In addition, a few individual museum scientists had a rather great impact on the popular naturalist movement after the turn of the century. But these »museum naturalists« subordinated their naturalist studies to the aims of the museums: field observations were a means for providing specimens to the collections, but rarely a scientific end in itself. In that sense it is no exaggeration to conclude that the rise of the museums and the systematical research tradition by and large discouraged and marginalized the outdoor »proto-ecological« tradition of natural history.

The emergence of the modern laboratory

This turn-away from outdoor »proto-ecology« was even more pronounced during the second phase in the establishment of botany and zoology – the emergence of the modern laboratory.

The last three decades of the 19th century was the period of the modernization of Sweden. The old agricultural society quickly disintegrated. Iron and wood production were industrialized in the 1870s and 1880s; in the 1890s manufacturing industry and the production of consumption goods finally smashed household production. Although the landed nobility and state officials still constituted the leading elite, a new technical and economic elite emerged; the factory owner, the wholesaler and the engineer signified the rapid elite circulation in late 19th century Swedish society. A working class began to organize itself, culminating in the foundation of *Socialdemokratiska arbetarepartiet* (the Social Democratic Workers Party) in 1889. The bourgeoisie and the working class formed a contradictory opposition against the old bureaucratic state.

August Strindberg, the literary avant-garde figure of modernism, had attended lectures at Uppsala for a short period, and ridiculed academic life in several of his novels and stories. During the long ages of romanticism and post-romanticism, i.e., the first three quarters of the century, the two university cities had been sleepy and conservative idyllic spots, dominated by classical scholarship, the faculties of law, and the particular brand of idealist philosophy taught by Boström.[105] But the new age slowly approached Uppsala and Lund too. According to the new university statutes of 1852 the doctoral dissertation should be evaluated as an original contribution from the graduate student,

104. SOU 1963:76; cf.4-4.
105. For a general review of Swedish 19th century philosophical history with an emphasis on Boström, see Nordin 1981.

and post-doctoral (*docent*) research scholarships were introduced. The new intermediate scientific degree, the *licentiate*, was introduced in 1870, and finally it was decided that all applicants to chairs should undergo peer review assessment (*sakkunnigförfarande)*.

Modernizing reforms like these, favouring innovative research rather than traditional scholarship, were followed by a revival of the natural sciences. The romantic age had been a period of decline for the natural sciences in Sweden (with the possible exception of botany), in contrast with the flowering of natural history in the 18th century. There is certainly a connection between this revival and the appearance of new class actors on the economical and political scene: the new bourgeoisie and the new worker's movement were united in their belief in the natural sciences and technology as one of the main levers of modernization. This »eminent progress of the natural sciences« in Sweden during the period 1870-1914 has been documented elsewhere,[106] and need not be reviewed here. One significant event should be mentioned however – the foundation of the privately and communally financed *Stockholms högskola* (the University College in Stockholm). Initiated largely by progressive and bourgeois groups in 1878, it embodied the new spirit – the new university was devoted to natural scientific subjects,[107] and most of its professors were not only scientifically, but also politically radical; in addition quite a few of its early students, and even one of its first professors, were women.[108]

The general vogue for natural sciences benefited botany and zoology as well. The four existing chairs at the universities around 1860 were multiplied threefold by the turn of the century. Botany and zoology not only underwent quantitative growth, but a profound qualitative change as well. The focus on Linnean systematics and morphology that had characterized their constitution as new disciplines gave way to a new and revolutionary focus on laboratory studies. The new botanists and zoologists were united by a common concern for the causal, mechanical development of the organic world, either the long-term historical unfolding of the taxa, or the causal unfolding of the individual organism from egg cell to embryo and from embryo to full-grown. By means of the microscope, perhaps complemented by chemical analysis or physiological experiments and measurements, the new zoologists and botanists revealed the secrets of the mechanisms behind the emergence of organic structure. More than anything else Roux's radical concept of »Entwicklungsmechanik« epitomized the change towards new basic problems within the botanical and zoological disciplines.

Industrialism provided the general, though diffuse, social setting for this modernization. The Darwinian theories and the new materialistic and secular world-views were important ideological preconditions. Laboratory life and work with the microscope provided its practical basis. In addition, it all came from Germany. It is true that Darwinism was born in England, but it got its home in Germany,[109] and the German interpretation of Darwinism focused on the theory of descent, not on the selection theory. By comparative anatomical studies one might be able to reconstruct the evolutionary history

106. G.Eriksson 1978 is a pioneer review of Swedish natural sciences and technology during the age of industrialization.
107. For a history of *Stockholms högskola*, see Bedoire and Thullberg 1978.
108. See S.Johansson 1983 for a short discussion of female students and scientists in the early history of the university.
109. Nordenskiöld 1924,p.287.

of the organisms. By embryologial studies one might also be able to reconstruct the developmental history of the individual organism/species. »Homologies« were the password of the age. Haeckel's phrase »ontogeny recapitulates phylogeny« was a popular summary of this focus upon two kinds of unfolding mechanisms of the organic life. The modern microscope and the laboratory were German inventions too. Adding to this the fact that Germany was a symbol of industrial and national progress, a star shining even brighter after the 1871 war, it is understandable that the scientists introducing laboratory life, microscopic investigations and the search for the unfolding mechanisms of the living matter oriented themselves to Germany. Numbers of young enthusiastic botanists and zoologists went on pilgrimages to Gegenbaur's, Strasburger's and other laboratories in Germany during the last decades of the century.[110] Therefore it is appropriate to refer to this qualitative change of orientation within the social orders of botany and zoology as the emergence of a »new German« botany and zoology.[111]

This distinct qualitative change towards the »new German« botany and zoology during the last three decades of the 19th century is reflected in the denomination of the new chairs at the universities:[112]

1878	botany with plant anatomy	(Uppsala)
1882	botany	(Stockholm)
1883	botany	(Lund)
1884	zoology	(Stockholm)
1889	comparative (zoological) anatomy	(Uppsala)
1894	comparative (zoological) anatomy	(Lund)
1897	plant biology	(Uppsala)
1911	plant anatomy and cell science (personal chair)	(Stockholm)

The impact of the »new German« zoology and botany on animal and plant studies, and its consequences for »proto-ecology« and the emergence of ecology becomes even more pronounced when we go into some details in the scientific practices at the three universities.

The »new German« zoology

The »new German« zoology was slow to get a foothold in Lund.[113] It is true that comparative anatomy was introduced to Lund zoologists by Fredrik Wahlgren (1819-1877), who succeeded Sven Nilsson, the great Linnean faunist, to the zoological chair in Lund in

110. The leading German animal comparative anatomist was Carl Gegenbaur , who enrolled great numbers of students and colleagues around him (including Ernst Haeckel), first in Jena 1855-1872, then in Heidelberg during the last three decades of the century. The leading plant cytologist was Eduard Strasburger who also worked in Jena, but later in Bonn; see Nordenskiöld 1924,pp.289-95,340-43 and Jahn *et al* 1985.
111. Tobey (1981) likewise makes a point out of »the new botany« without stressing its German origin, however.
112. The chair in plant biology was the only exception to this transformation to the »new German« botany and zoology (cf.1-3).
113. Secondary sources for late 19th century Lund zoology include Danielsson 1965 and 1967, G.Eriksson 1978, Löwegren 1968, A.Wahlgren 1865, and biographies of Lund zoologists in *Svenskt biografiskt lexikon (SBL)* and *Svenska män och kvinnor (SMK).*

1856/57.[114] Wahlgren had written a medical dissertation on the reproductive organs in man and some other mammals as early as in 1849;[115] he procured a microscope for the zoology courses in 1860. But Wahlgren seems to have been too old to be attracted by the Darwinian siren calls,[116] and never succeeded in attracting sufficient students to transform zoology in Lund – a traditional zoologist and declared anti-Darwinian was appointed his successor.[117]

It was only with the extra ordinary professorship in 1893/94 that the »new German« zoology was reintroduced in Lund. The pioneer, David Bergendal (1855-1908), had originally learnt the »new German« botany from F.W.C. Areschoug (see below) in the 1870s, before turning to animals and studies with Gegenbaur and other great German comparative anatomists. Bergendal took over the ordinary chair in 1904,[118] and with it took charge of the department. Immediately he institutionalized the new zoology by getting authorization by the faculty to redefine the terms of reference for the zoological chairs in Lund. The one chair should take care of »general zoology and systematics with embryology and animal geography«, embryology being added to provide against »systematics reclining into mere species descriptions«. The other chair was to be for »comparative anatomy with histology and general physiology«.[119]

Bergendal died prematurely. He did not get the time to enrol many students (in fact only four graduate students submitted their dissertations between 1890 and 1909), but his successors were nevertheless chosen to continue the new tradition. One of the chairs was filled by a devoted comparative anatomist in 1912.[120] To the other chair was appointed Hans Wallengren, a student of morphology and faunistics, who had taken up experimental physiology in Jena and Göttingen in 1900-01 and who was later counted as »a pioneer in animal physiological research« in Sweden.[121] Thus, by the first decade of the 1900s the »new German« zoology was strongly established in Lund.

In contrast to Lund, the translation of animal studies into the new zoology was almost total at the newly founded university in Stockholm.[122] Zoology was among the pioneer subjects, and the Board of the university was probably not unaware of the fact that they

114. Simultaneously, the denomination of the chair in Lund was changed from »natural history« to »zoology«.

115. As in Germany, the forerunners of the new zoology were associated with the medical faculty. Wahlgren was a student of Anders Retzius (1796-1860; not to be confused with his father Anders Jahan Retzius, i.e., the former professor in natural history in Lund, cf.1-1), who served as professor in anatomy at *Karolinska Institutet* from 1824. Retzius wrote the first comparative anatomical dissertation for the medical degree, he studied microscope techniques with Purkynje in Breslau, and was the first Swede to acquire a microscope in 1833. Later, comparative anatomy became one of the foundations of the basic medical training in Sweden, and several medical doctors wrote comparative anatomical dissertations in the second half of the century. In addition, until the early 20th century prospective medical doctors could take some courses, including zoology, at the universities, and hence much of the comparative anatomy teaching at zoology departments was arranged for prospective medical students.

116. See Danielsson 1965,p.191.

117. August Quennerstedt (1837-1926), professor in zoology in Lund 1880-1903; Quennerstedt not only remained the only anti-evolutionist among late 19th century Swedish zoologists, but little by little he abandoned zoology and devoted his energy to Swedish history.

118. ED 15/7 1904:15.

119. According to minutes of meeting with *matematisk-naturvetenskapliga sektionen*, Lund university, 24/10 1904 and established in *Kungl.brev* 31/12 1904.

120. Oskar Carlgren (1865-1954), a student of Leche in Stockholm; for details, see ED 6/6 1912:31.

121. Wallengren was summoned to the chair in 1908 (ED 23/12 1908:30).

122. Secondary sources for late 19th century Stockholm zoology include Danielsson 1965 and 1967, G.Eriksson 1978, Pehrsson n.y., Tunberg 1957, and *SBL* and *SMK* biographies of Stockholm zoologists.

summoned one of the politically and scientifically most radical young zoologists in Sweden.[123] Vilhelm Leche (1850-1927), who was born of German parents and educated in German schools,[124] was one of the few of Wahlgren's students in Lund who had taken comparative anatomy seriously; after his dissertation in 1876 he travelled to Gegenbaur in Heidelberg to learn the new zoology directly from its most prominent source. Leche was full of contempt for descriptive zoology; like Bergendal he considered systematics and faunistics uninteresting and unscientific. Zoologists ought to solve theoretical problems, and the overshadowing problem for Leche was the evolutionary history of the animal world, particularly the higher vertebrates. He coupled this attitude with a materialistic »Weltanschauung«; by studying the descent of man, Leche thought, religion could eventually be surpassed by scientific thinking. In addition he was politically radical, believing that a »step towards the left always could be defended«,[125] and consequently he attached himself to Anton Nyström's *Stockholms arbetarinstitut* (the Stockholm Workers' Institute), known as a center for Comtean positivism and anti-religious propaganda in the 1880/90s, and a persistent source of irritation for the old political elite.[126]

Embodying the spirit of modernization, the social optimism and the evolutionary world-view of the late 19th century, Leche gathered enthusiastic students around him to develop a zoological (zootomical) laboratory which would remain the leading center for the reconstruction of vertebrate phylogeny in Scandinavia until this day. Between 1880 and 1915 alone some 70 larger scientific works were published, almost all of them devoted to the anatomy of different mammalian organs.

In Uppsala, finally, the translation of animal studies to the new zoology was also pronounced, although not as total as in Stockholm.[127] In the 1860s and 1870s Uppsala was a center of Linnean faunistics through the efforts of Vilhelm Lilljeborg; his huge Scandinavian fauna was a climax of the Linnean descriptive faunist tradition. But the »new German« zoology had already announced itself by the 1860s and 1870s with several

123. The professors at *Vetenskapsakademien* had the factual scientific influence in the Board.
124. For biographical details of Leche, see Leche-Löfgren 1934 and Franzén 1979.
125. Franzén 1979,p.416.
126. On the conflicts around *Stockholms arbetarinstitut*, see Richardsson 1963,pp.262-65.
127. Secondary sources for late 19th century Uppsala zoology include Danielsson 1965 and 1967, G.Eriksson 1978, Jägerskiöld 1943, S.Lindroth 1976, Tullberg 1897, Wirén 1907, and *SBL* and *SMK* biographies of Uppsala zoologists.

of the younger Uppsala zoologists taking up Darwinism and phylogenetic problems;[128] the most prominent spokesman for Darwinism and the new zoology was Tycho Tullberg (1842-1920), who began as a student of Lilljeborg, but soon turned to phylogenetic studies;[129] by 1873 he had organized a discussion group on comparative anatomy and by 1876 had established a small anatomical laboratory, probably the first in a Nordic country. In 1882 Tullberg was summoned to the chair in zoology to succeed Lilljeborg and chose a younger colleague, Hjalmar Théel (1848-1937), who had also oriented himself towards embryological and cytological problems, as *prosektor* (later extra ordinary professor) in comparative anatomy. During the following decade the two men together fostered a number of graduate students in the new zoology and procured their succession. When Théel left Uppsala[130] yet another Uppsala student of comparative anatomical problems, Axel Wirén (1860-1925), took over; he is significantly described as having been of

> *»great importance as a school founder in zoological research; he was surrounded by several students, all of whom successfully but one-sidedly studied different groups of worms«.*[131]

Yet another adherent to the »new German« zoology was appointed to the other chair in 1910.[132] Hence, although Lilljeborg's faunistic orientation still prevailed until the turn of the century (cf.1-3), the »new German« zoology was hegemonic in Uppsala from the 1890s and onwards; accordingly more than two thirds of all doctoral dissertations in Uppsala from the 1890s up to the 1940s treated comparative anatomical or systematical anatomical problems, mainly using marine invertebrates as the preferred object of study.

The »new German« botany

The new botany was most forcefully introduced by F.W.C. Areschoug (1830-1908), professor in botany in Lund[133] 1879-1898.[134] He started by writing a dissertation on algal systematics, but soon after travelled to the Promised Land on the other side of the Baltic where he was trained in plant anatomy and microscopic techniques by von Mohl in Tübingen.[135] He organized a small botanical laboratory, the first of its kind in Sweden, and in 1874 he succeeded in getting some financial support to mount plant anatomical courses. Some 15 years later the *Riksdag* granted a new institute building, with accom-

128. Before him one of Lilljeborg's *docents*, Fredrik Smitt, made phylogenetic studies based on the theory of descent in the 1860s, and likewise did Lilljeborg's *adjunkt* T.T. Thorell and others; see Danielsson 1965,pp.192-93,202 for details.
129. For biographical details on Tullberg, see Holmgren 1943; see also Danielsson 1965,pp.204-06.
130. To succeed Lovén as professor in invertebrate zoology at *Riksmuséet* in 1892.
131. Biography on Wirén in *Svenska Män och Kvinnor*.
132. Adolf Appellöf (1857-1921); ED 28/10 1910:28; Appellöf was not without importance for early Swedish animal ecology (cf.2-5).
133. Secondary sources for late 19th century Lund botany include Danielsson 1965 and 1967, G.Eriksson 1978, Almborn 1980, R.E.Fries 1950, Weimarck 1980, Hjelmqvist 1958, Håkansson 1958, Karlsson 1983, Törje 1968, and *SBL* and *SMK* biographies of Lund botanists.
134. Just as the new zoologists had a forerunner in Anders Retzius, so the new botanists in Sweden had one in Carl A. Agardh, professor in Lund in the 1820s, although his plant anatomical studies were strongly influenced by the romantic natural philosophy (see G.Eriksson 1962 for details).
135. For biographical details on F.W.C. Areschoug, see G.Andersson 1920.

modation not only for anatomical investigations, but also for plant physiology. Plant physiology was closely associated with practical agricultural interests (cf. above). Areschoug had been teacher at the Alnarp agricultural school and considered plant physiology and seed control as two necessary means for a rational agriculture. The new botanical department in Lund actually housed the Malmö county seed control laboratory as well.[136]

Areschoug also became one of the first adherents to the Darwinian doctrines in Lund;[137] his introductory lecture as professor in 1879 was dedicated to Darwin. Shortly before his retirement, Areschoug pushed through the proposition that the two botanical professorships should be specified in order to »secure plant physiology«. The one in systematic botany (systematics, morphology and plant geography) should be responsible for the botanical garden and the herbaria, while the other, in plant physiology (anatomy, physiology and biology) should take care of the laboratories and the biological and morphological collections.[138]

All sources depict Areschoug as a most successful enroller for anatomical studies. It is said that »it was hardly advisable to choose a subject for a dissertation outside anatomy«,[139] and others have remarked on the »long row of rather dry and schematic plant anatomical investigations presented by Areschoug's students«.[140] In fact, all but one wrote anatomical dissertations. Hence »a notable plant anatomical school was established in Lund during the latter part of the 19th century«.[141]

Those who subsequently occupied the two chairs in Lund were all students or close associates of Areschoug. Sven Berggren (1837-1917), who had followed Areschoug in the transformation of plant studies to the new botany by devoting himself to studies of the anatomy, embryology and reproduction of mosses, was summoned to the newly created extra ordinary chair in 1883, and eventually succeeded his senior colleague in 1898. His successor in turn, Svante Murbeck (1859-1946), worked mainly on systematical problems, but like several others he turned against purely descriptive systematics based on outer morphological inspection, and supported systematic studies based on thorough anatomical and cytological investigations. In fact it was Murbeck who introduced microtome techniques »and hence the modern embryology«[142] to Lund.

The successors to the physiological chair likewise turned out to be distinct and independent actors for the new botany in Lund. Bengt Jönsson (1849-1911), who wrote his dissertation on the embryonic development of angiosperms, had learnt the new anatomy under Strasburger in Jena. But he also studied physiology with Frank in Berlin. Being a pioneer of plant physiological research and teaching in Sweden, he was sure to get the physiological chair in 1902.[143] The third of Areschoug's prominent new botany students was Bengt Lidforss (1868-1913), one of the best known politically radical natural scientists ever in Swedish history. Being an outspoken Social Democrat, a devoted

136. See Törje 1968,pp.87-89.
137. See Danielsson 1965,p.179ff.
138. *Kungl.brev* 8/3 1895 (ED EI:2667,nr 13 1895).
139. G.Samuelsson 1918,p.738.
140. Franzén 1975,p.543.
141. Hjelmqvist 1958,p.13.
142. Ibid.,p.16.
143. ED 24/10 1902:8.

Darwinian and an experimental physiologist, Lidforss, like Leche in zoology, embodied the close connection between the new botany and the modern break-through in Sweden.[144] However, after succeeding Jönsson to the chair in 1911, he died soon after, burnt out by a hectic life. Finally Herman Nilsson-Ehle, a pioneer in plant hereditary research, should be mentioned. His breeding experiments, submitted as a dissertation in 1909, was a logical outcome of the new focus on cell structure; it also anticipated his later claim for genetics as an independent scientific discipline.[145]

The transformation to the new botany was delayed in Uppsala.[146] After all Uppsala was the center not only for botany but for Linnean systematics as well. The two successors to the chair after Elias Fries, J.E. Areschoug and Thore M. Fries, had approached anatomical issues and microscopic investigations, but only as an aid to the systematic study of cryptogams;[147] phylogenetic and embryological problems did not interest them. Frans Kjellman (1846-1907), who succeeded Fries as ordinary professor in 1899 started as a systematician, although with an all-round outlook (cf.1-3).

Bergendal had introduced the new zoology in Lund after having been trained in the new botany – conversely the new botany was introduced to Uppsala via the new zoologists. Hans Oscar Juel (1863-1931) could not get the desired training in microscopy and thin sectioning from Fries. Only after having written his doctoral dissertation on the comparative anatomy of a group of flatworms did he turn to the study of flowers. Like so many others of his generation he got his advanced training in cytological and embryological techniques at Strasburger's laboratory in Bonn; back home he pursued investigations of parthenogenesis in *Antennaria* species which soon became classical papers in the plant cytological literature. Accordingly Juel was appointed extra ordinary professor in 1902,[148] and a few years later, in 1907, he was summoned to the chair to succeed Kjellman.[149]

With Juel Uppsala botany was entirely reoriented. Linnean systematics disappeared. After a short tenure by Bengt Lidforss in 1910-11,[150] yet another student of cell structures, Nils Svedelius, was appointed to the other botanical chair in 1914.[151] Svedelius's career epitomizes the break-through of the new botany. Starting with rather broad-ranging studies of algae (systematics, biology, plant geography etc.) he soon changed his scientific work to cell structure investigations, and managed to produce enough embryological and cytological articles to win the competition for the Uppsala chair.

144. For a scientific biography, see R.Karlsson 1983; the extensive secondary literature on Lidforss' social radicalism is listed there.
145. Cf.2-1, note 26.
146. Secondary sources for late 19th century Uppsala botany include Danielsson 1965 and 1967, G.Eriksson 1978, R.E.Fries 1950, Th.M.Fries 1897, S.Lindroth 1976, Svedelius 1940, and *SBL* and *SMK* biographies of Uppsala botanists.
147. E.g., one of J.E. Areschoug's first tasks as new professor was to publish a textbook in general botany. Both the book, and his introduction of microscopical exercises in the botanical curriculum was in part dictated by the demands of the new botany, however: »systematic botany becomes a loose, hardly coherent edifice without scientific value if it isn't founded in knowledge of the anatomical and morphological conditions of plants« (Svedelius 1919,p.147).
148. See ED 12/8 1902:4; cf. below and note 167-168.
149. ED 15/11 1907:23.
150. ED 28/10 1910:39.
151. ED 21/2 1914:24.

At *Stockholms högskola* finally, botany as well as zoology was among the first subjects.[152] Veit Wittrock, one of the leaders of the Darwinian plant biology movement in Uppsala in the 1860/70s (cf.1-3), and professor at *Riksmuséet* from 1879, gave a series of lectures over »the plant system, founded on the evolutionary theory« in the 1880s. After a short appointment of the Dane Eugenius Warming in the early 1880s (cf.1-3) the university could not afford to pay a new professor until 1895,[153] when Gustaf Lagerheim (1860-1926) was appointed. Lagerheim had many botanical irons in the fire, but the microscope was always in the center of his activities: »he was a pronounced microscopist«.[154]

Lagerheim laid the foundation for the laboratory praxis which would come to characterize Stockholm botany, and accordingly he, together with Leche, took an initiative to create a personal chair for one of the leading young exponents of the »new German« botany, Otto Rosenberg (1872-1948), son of a professor at *Tekniska högskolan* (the College of Technology) in Stockholm. Rosenberg got his undergraduate training in Uppsala but is said to have disliked the old-fashioned (read systematical-morphological) atmosphere there and travelled to Strasburger in Bonn to learn cytology. Actually he even wrote his dissertation there, and was awarded a German doctoral degree, before returning to Stockholm to continue his studies of cell chromosomes. He lectured on cytology and plant physiology, in 1904 he was appointed extra ordinary professor, and in 1911 he was installed in the personal chair in »plant anatomy and cell science«.[155] Thereby the so called Stockholm school of new botany was founded, one of the scientifically and politically most radical local scientific groups in the history of 20th century Swedish natural science.

The modernization of botany and zoology and its impact on »proto-ecology«.

Around the turn of the century 1900 the »new German« laboratory botanists and zoologists had taken the lead. In Bourdieu's terminology they occupied the »dominant positions« in the field of botany and zoology. Their hegemonic pretensions were not met with enthusiasm by the remaining natural historians and the traditional Linnean systematists. The naturalists and systematists on the one hand and the new zoologists on the other forwarded two altogether different claims to animal studies; they referred to quite different social worlds and conceptions of life – traditional versus modern – which hardly kept any contact with each other.

With respect to the situation in animal studies Gustaf Kolthoff, the creator of the biological museums in Uppsala and Stockholm (cf.1-3), is said to have exclaimed:

> *»Here they cast worms in tallow candles and then they slice them in small pieces by means of a planing machine, and then they call this a science - when we do have so much left to learn about the birds«.*[156]

152. Secondary sources for late 19th century Stockholm botany include Danielsson 1965 and 1967, G.Eriksson 1978, R.E.Fries 1950, Tunberg 1957, and *SBL* and *SMK* biographies of Stockholm botanists.
153. In the mean-time anatomical and physiological problems were taken up by temporarily attached teachers, particularly by J.E.F.Klercker (1866-1930) who had studied plant physiology and anatomy with Pfeffer in Leipzig; Klercker acted as *docent* in botany in the early 1890s.
154. O.Rosenberg 1927.
155. ED 30/12 1911:377.
156. Jägerskiöld 1943,p.179.

On the other hand there is no mistake about the enthusiasm of the new zoologists. The biographies of Leche, Tullberg, Théel and others abound with examples of their fervour. Although predominantly a professional activity it occasionally spread to a few amateurs as well. For example, we are told about the primary school teacher Albertina Carlsson, who was attached to Leche's zootomical laboratory in Stockholm for more than 40 years:

> *»She never aimed at a degree and never received any pecuniary remuneration for her activities, neither did she demand any... She spent all her spare hours at the dissection tray, leaned over an alcohol soaked carcass, and after her retirement she sat there regularly every day«.*[157]

Kolthoff's Sweden was a country of elk-hunts or early morning capercaillie courtships, and his work days resembled those of the noble land owner. The new zoologists unfolded themselves in a milieu resembling the factory workers' and small industrialists' more than that of the old elite:

> *»With the kerosene lamp in one hand and Leunis-Ludwig Synopsis der Thierkunde in the other they had to place the kerosene lamp on the floor when turning the pages«.*[158]

Their Sweden has been portrayed by the Swedish novelist Sven Delblanc as that of:

> *»the ore that was broken from the mountains, the forests that came crashing down, the praying towers of the factory chimneys, smoking incense to the God of gold«.*[159]

Their »Weltanschauung« was not the remains of post-romanticism, but a new radical, materialistic and secular world-view. One of the Uppsala comparative anatomists emphasized the intimate relation between scientific and political radicalism. The real goal of his studies in the 1890s was to acquire a »conception of life«:

> *»The conception of life searched for was a natural scientific conception of the world... It was also quite natural... that it was politically and socially radical... There were conditions in the society of that time which made it natural for idealistically inclined young people to join progressive people in the struggle for political and social reforms«.*[160]

The new zoologists could at best consider their naturalist colleagues as a kind of service personnel, as illustrated by the activities at *Kristinebergs zoologiska station* (the Kristineberg Zoological Station), established by *Vetenskapsakademien* (mainly on the initiative of Sven Lovén) in 1877 for studies of marine animals.[161] Although very little was known about the West coast fauna, and almost nothing was known about the distribution of animals or their relation to the marine environment, most zoologists used the station to collect material for studying comparative anatomical problems or (later) physiological problems. It is true that when Hjalmar Théel described the activities at the station during its first thirty years of existence he paid attention to the fauna and its life conditions[162] – but the aim of this »proto-ecological« report was to serve as a basis for the collection of animals for comparative anatomical and physiological studies.

157. Pehrson, n.y.,p.126. Until the age of 78 she produced 24 minor and major comparative anatomical papers.
158. Ibid.,p.15.
159. Delblanc 1982.
160. N.von Hofsten 1938.
161. A number of marine station were founded in Europe by the mid-19th century, e.g., the French stations in Concarneau (1859), Arachon (1863) and Roscoff (1872), the German stations in Naples (1872), at Helgoland (1893) etc. For details on the Kristineberg station, see Théel 1907 and Holmgren 1953.
162. Théel 1907.

The situation was quite similar in botany. The traditional Linneans either developed into »accountants« of animals and plants,[163] or continued the kind of »proto-ecological« studies of the geographical distribution of plants which had been pursued by Linné and Wahlenberg, i.e., species descriptions accompanied by notes on the place of finding (site notations). But Wahlenberg had not won any adherents, and the Linnean florists had been bypassed by the museum systematists and the new laboratory botany. With the new botanists' triumph not only Linnean systematics but also the whole narrative botanical tradition, which had been identical with botany as a scientific discipline only fifty years earlier, largely disappeared. It was relegated to a leisure time amusement or to the solemn memorial meetings of the newly founded *Svenska Linnésällskapet* (the Swedish Linné Society).

It is true that amateur floristics remained immensely popular, as witnessed by the widely spread *Handbok i Skandinaviens flora* which went through 11 editions between the 1820s and the 1880s. But floristics was hardly an affair for university botanists:

> *»The leading botanists at our universities flung themselves into new areas of research, it was no longer fashionable to work with special floristics, at least not if it did not treat critical genera or lower plants, and 'the endless plant locality lists' were not particularly welcomed where botanical literature was printed«.*[164]

Although »interested men«, including secondary school teachers in »natural history«, continued to collect plants and make site notations »irrespective of the fashion of the day«,[165] they had no influence upon university botany. Only a few professional botanists upheld the naturalist tradition (cf.1-3).

No event could illustrate the new botanists' hegemony over the traditional botanists better than the competition over the extra ordinary professorship in botany in Uppsala in 1902. Carl A.M. Lindman (cf.1-3), a field botanist, florist and systematist from Uppsala, South American traveller and painter, who had spent several years as a private instructor for the young royal princes was criticized for his »disregard of the inner construction of plants«.[166] Four applicants were laconically dismissed as being too descriptive. Their work, it was said:

> *»would have gained in value and been on much the same lines as recent research, if their investigations had been more thorough and if the discussion had rested on broader, more secure comparative morphological, anatomical and physiological foundation«.*[167]

163. See e.g., the discussion by Broberg 1978.
164. Andersson and Birger 1912,p.4.
165. »Among the foremost during the 1860s-1880s were S.Almqvist, Fl.Behm, K.F.Dusén, V.F.Holm, P.Olsson, C.Melander, L.M.Neuman and others« (Ibid.,p.4); none of these were university botanists.
166. Areschoug's assessment in ED 12/8 1902:4.
167. Kjellman's assessment in ED 12/8 1902:4. Among those dismissed were Rutger Sernander, the coming founder of the Uppsala school of plant geography and plant sociology, the institutional center for Swedish plant ecology from the 1930s to the 1960s (cf.2-2 and 3-2). One assessor thought that Sernander's contributions »almost exclusively concern one of the peripheral areas of botany«, another that Sernander was verbose and drowned himself in unnecessary details (Kjellman's and Areschoug's assessments respectively, in ED 12/8 1902:4). A decade later the two lines clashed again. In the capacity of being professor in plant biology Sernander was set to assess the applicants to the other botanical chair in Uppsala. Choosing between the systematist and florist Robert Fries and the cytologist and embryologist Nils Svedelius, Sernander wrote: »Cytology is considered more central for modern botany at our universities than systematics« (Sernander's assessment in ED 21/2 1914:24), and found Fries more scientifically competent. However, being in minority Sernander could not stop Svedelius from getting the chair and hence establish cytology as the dominant research specialty within the confines of university botany in Sweden for the next 30 years to come.

In contrast, the new botanists, viz., Juel, Lidforss and Murbeck were rated highly. The panegyric to their mastery is worth quoting *in extenso*:

> *»By means of the supremacy they have acquired over the recent driven microscopic technique they have been able to set about investigations demanding all aids of this technique and penetrate into the most difficult issues as deeply as it is possible at the moment. Consequently their research is, with regard to subject as well as method, fully modern and has, in their most distinguished works, concerned questions which hold a prominent position within recent botanical research, as for example the build-up and life of the cell nucleus, the fertilization and embryonic development of phanerogams, finer and more complicated histological and histogenetical conditions, and so forth«.*[168]

1.3 The pioneer ecologists

To sum up: During the course of the 19th century, the dissolution of the social order of natural history and the subsequent rise of the social orders of museum botany and zoology, and later of the »new German« laboratory botany and zoology, implied the marginalization of outdoor naturalist investigations, including the »proto-ecological« practice latent in the natural history tradition. Observation of the life habits of animals or the growth localities of plants collected during landscape travels or long-term outdoor wanderings were gradually abandoned when new claims for the institutionalized study of »natural things« emerged. Studies of animals and plants in their natural environment were no larger considered a truly scientific activity. This was reflected in university and secondary school curricula too. Nobody demanded from a young zoology student that he should be able to recognize a flying hazel-hen or a school of perch in the creek. Secondary school pupils had to go out in the bush and collect herbaria specimens, but few stimulated them to pursue observations of living plants in their natural settings.

But even though the »new German« practices were attaining a hegemonic position, a few academic botanists and zoologists nevertheless began to claim field studies of animals and plants as an independent academic realm. Indeed, the pioneer claims for ecology were made at the height of the influence of the »new German« botany and zoology. In this section we shall review the background to these claims for ecology, first within the field of academic plant studies, then within the field of academic animal studies.

Our point of departure is that these first claims for ecology should be interpreted as outcomes of the scientification of naturalist studies, which proceeded along two main paths. Firstly, although classical naturalist studies had largely been abhorred as unscientific by the »new German« botanists and zoologists, they could be raised to scientific status by being translated into animal and plant geographical problems. Haeckel was of the opinion that Darwin's theory of descent made it possible to inquire into »die mechanischen Ursachen« of the geographical distribution of animals and plants.

Comparative anatomical and animal geographic studies shared a common goal, viz., to contribute to the historical reconstruction of organic evolution. For example, David

168. Kjellman's assessment in ED 12/8 1902:4.

Bergendal in Lund, one of the most enthusiastic Darwinists and comparative anatomists, gave lectures in animal geography, e.g., his inaugural lecture was entitled »Neogäa, a decisive proof for the scientific justification of animal geographic regions«.[169]

Secondly, and more importantly, the Darwinian revolution, not only triggered off the laboratory botany and zoology, but also provided a new impetus to field studies. It is often forgotten that the late 19th century Darwinian revolution was two-pronged. The search for phylogenies in terms of descent theory was unambiguously and rapidly accepted by German and Scandinavian zoologists. The other aspect of Darwinism – the study of adaptations and »the struggle for survival«, including the study of the interrelationships between organisms and environment[170] – although never prevailing, was nevertheless adopted by a handfull of botanists and zoologists towards the end of the 19th century. Those who devoted themselves to field studies of plant morphological adaptations, or animal life habits and behaviour, often called their studies »biological« in the restricted sense of the word.[171]

Studies of plant and animal geography and studies of plant and animal adaptations were in practice often interwoven with simple naturalist observations. On the following pages we will give a cursory sketch of the most important Swedish academic scientists who, in the late 19th century, devoted themselves to field studies of plants and animals, calling their studies plant geography, plant biology, animal biology and animal geography. Finally we will pay special attention to those who began to utilize the term ecology around 1900, i.e., the pioneer ecologists.

Studies of plant distribution: from floristics to plant geography

The naturalist approach to the study of the distribution of plants was turned into an academic task along two lines. One was the study of post-glacial plant immigration. The Norwegian botanist Axel Blytt's theory on the relation between large scale climatic changes and the post-glacial immigration history of Scandinavian vegetation, being founded on peat bog analyses,[172] became a point of departure for two young Swedish botany students in the late 1880s – Gunnar Andersson in Lund and Rutger Sernander in Uppsala. Both were busy and versatile men. They were active in the emerging nature conservation movement, they were both engaged in practical forestry problems,[173] and both were pioneers in breaking with the prevailing direction of 19th century museum and laboratory botany. Sernander (1866-1944)[174] submitted his dissertation on the vegetatio-

169. Löwegren 1968.
170. Darwin's works, and particularly *The Origin of Species* (1859), is full of »proto-ecological« observations and reasoning. Darwin never claimed ecology an independent science, however, but designated these topics as the »economy of nature« or »polity of nature« (the resemblance of these concepts with Linné's *Oeconomia naturae* and *Politia naturae* (see Linné 1906 and 1978b) are striking, as also Stauffer 1957,p.139 has pointed out.
171. The term »biology« in the restricted sense was introduced by Delpino (1867) as the knowledge of the outer life conditions of the plants.
172. See the article on Blytt in *Norsk biografisk leksikon*.
173. Andersson was one of the initiators behind the creation of *Forstliga försöksanstalten* in 1902 (cf.below), and Sernander made some investigations for *Mo och Domsjö* forest company.
174. For biographical details on Sernander, see Heribert-Nilsson 1945 and Skottsberg 1945.

nal history of the island of Gotland in 1894.[175] Andersson (1865-1928),[176] a student of Areschoug in Lund, had to choose an anatomical topic for his dissertation, but from 1890 onwards concentrated upon vegetational history and published a series of articles on peat-bog analysis and migration history.[177] His most important and mature work in this field was a study of the immigration of hazel in Sweden.[178]

The other line of scientifying the classical floristic plant lists and site notations was taken by studies of physiognomy of vegetation, and particularly by the study of plant communities. The problem of plant physiognomy was reiterated in a number of vegetational studies during the 1880s, 1890s and early 1900s:

> *»A viewpoint of vegetation which seems to have forced its way through during the last decade the 1880s is that in order to understand the life of the individual species it is necessary to consider it in relation to those species which occupy the surrounding places. Thus the study of vegetation becomes the study of the plant community«.*[179]

The success of such studies was largely a consequence of the progress of analytical methods. In this respect the Finlandish botanist Ragnar Hult (1857-1899) became an intellectual model through his vegetation analyses.[180] For example, Sernander, after having attended a summer course in plant geography with Hult in 1886, is said to have »adopted Hult's ideas with great enthusiasm«;[181] later Sernander's improvement of Hult's method for vegetation analysis became internationally known as the »Hult-Sernander cover scale«.

Gunnar Andersson and Rutger Sernander were among the pioneers of plant geographical studies along these new lines – they also claimed their studies as plant geography. In addition they were the first to be able to institutionalize their claims, at least temporarily. Andersson was appointed *docent* in the new specialty in Stockholm in 1893, and two years later Sernander was appointed *docent* in Uppsala. From these positions they spread the gospel. Largely thanks to their achievements plant geography was rapidly considered a respectable academic botanical line of pursuit along with the »new German« laboratory botany from the mid-1890s and onwards. In botanical circles in Uppsala

> *»plant geographical and above all immigration historical viewpoints were increasingly forwarded in the 1890s«.*[182]

175. Sernander 1894; Sernander concentrated on the fossil record, while only paying succinct attention to recent plant formations and site notations. Vegetational history was not alien to the other Uppsala botanists. E.g., Kjellman gave lectures on the developmental history of the Scandinavian phanerogram flora during the spring of 1886.
176. For biographical details on Andersson, see Samuelsson 1918 and Trägårdh 1929.
177. E.g., G.Andersson 1892 on the plant geographical and paleontological support for the assumption of climate changes during the Quarternary.
178. G.Andersson 1902.
179. G.Andersson 1890,p.492.
180. For details on Hult, see Collander 1965,pp.74-77; Hult's most important work was probably his detailed description of the succession of the plant communities of the Blekinge county, thereby being almost two decades ahead of Clements in the USA (R.Hult 1885).
181. Collander 1965,p.77.
182. Svedelius 1940,p.195.

Darwinian plant biology in Uppsala

The original conception of plant geography as the search for historical explanations for the distribution of plant species[183] was soon supplemented, however, by an approach searching for the environmental causes of the distribution of plants. This approach, having its origin in the problems of Darwinian plant biology, was almost without exception an Uppsala affair. It is true that F.W.C. Areschoug in Lund, although making his greatest achievements in institutionalizing the »new German« botany, was later interpreted as an immediate fore-runner to the first claims for ecology:

> *»it can probably not be denied that he was one of the pioneers among those biological-morphological scientists which in the 1860s and 1870s laid the ground to the ecology of our days* /i.e. 1920/, *and that he had a clear and broad-minded grasp of ... how form, distribution and mass of the different tissues of the plant most probably are connected to the changing ecological demands«.*[184]

But to call Areschoug an ecologist is an anachronism, a »historical translation«. He does not seem to have claimed ecology as an independent botanical specialty, and none of his students made any substantial contributions to the field study of flowers known as plant biology. Lund botany remained a stronghold for the new laboratory fashion.

The biological approach to plant studies was introduced in Uppsala during Thore M. Fries' reign in the 1870s. While Fries was sceptical of evolutionary theory, a younger generation of Uppsala botanists, being in their twenties in the late 1860s and early 1870s, were seemingly fascinated by Darwin's investigations of plant biology.[185] Severin Axell's 1869 dissertation on phanerogam reproduction was in full accordance with the Darwinian biological approach,[186] and for more than two decades problems concerning the morphological adaptations of plants were one of the most widely discussed issues at the meetings of *Botaniska sektionen* (the Botanical Student Association) in Uppsala;[187] hence several of the botanists who would later hold high scientific offices grew up with plant biological problems.

The seniors of this generation, Veit Wittrock (1839-1914), Axel Lundström (1847-1905) and Frans Kjellman (1846-1907), started their scientific careers writing systematic dissertations, but gradually turned to plant biological problems. Wittrock published morphological and biological studies of seed-leafs and ferns around 1880. Lundström wrote two applauded and prize-winning articles, *Pflanzenbiologische Studien* in 1884 and 1887, on morphological and anatomical adaptations to the external environment, arguing that a number of morphological features like hair cover, secretions, leaf arrangements etc., were well adapted to their purpose, and not an expression of the

183. The historicist approach was evidently the core of the early plant geography of Andersson and Sernander. Andersson wrote in 1903: »Everything has its history, it may be part of human life or nature. Hundreds of years of collected experience has told us that it is often of decisive importance for the conditions that can be observed today« (G.Andersson 1903,p.3).
184. G.Andersson 1920,pp.153-154; Hjelmqvist likewise asserts that Areschoug »adopted...ecological points of view« (Hjelmqvist 1958,p.9).
185. See Danielsson 1965,pp.182ff,205-206 for details.
186. See Ibid., p.184.
187. Svedelius 1940,p.201.

capriciousness of nature only.[188] Kjellman finally, when summarizing his many arctic expeditions in the 1870s, discussed not only the anatomy, morphology and systematics of arctic algae, but also biological problems; when taking over the teaching responsibilities in Uppsala in 1883 he turned more and more to problems concerning plant biology and functional anatomy and supported younger students in the field.[189]

Among the junior plant biologists might be mentioned Carl A.M. Lindman (1856-1928) and Lars Albert Nilsson (1860-1906).[190] Lindman's dissertation of 1884 contained a multitude of examples to show that the purpose of the so called post-floration was to protect the rudimentary fruit. Nilsson's dissertation of 1887 was the result of field studies of the morphological development and differentiation of the stem with regard to its purpose as an assimilatory organ. A typical title of Upsalian plant biology was Jungner's dissertation in 1890, *Anpassungen der Pflanzen an das Klima in der Gegenden der regenreichen Kamerungebirge*.[191]

The first claims for ecology and ecological plant geography

The term ecology began to spread in Swedish botanical circles around 1900 as a synonym for biology in the restricted sense of the word. Bengt Lidforss, for example, used it in passing in an article in 1901,[192] and some of the applicants for a chair at Uppsala in 1902 were considered by their peers to pursue ecological investigations.[193] In an article the same year Lars Albert Nilsson classified the Swedish land vegetation into four »oekologische Serien« with regard to their nutrient economy.[194] The Lund botanist Thorild Wulff used the term »ökologie« in passing as a synonym for biological and physiological studies of the floristic and geographical relations of plants.[195]

The most conspicuous use of the new term was made by a couple of graduate students of Kjellman who started their dissertation works during the years 1900-1904. All three utilized the term »ecology« in their titles. In 1900 Hernfrid Witte (1877-1945) planned

> *»an exhaustive floristic, physiognomical and ecological treatise of all Swedish lime heath vegetation«,*

and although having to restrict his work, he gave a detailed report of field studies of

> *»the most important ecological peculiarities of the limestone heath plants, particularly concerning nanism, i.e., dwarf growth, transpiration protection, formation of a subsurface system etc.«,*[196]

under the title *Till de svenska alfvarväxternas ekologi* (On the ecology of the Swedish limestone heath plants). Likewise, in his 1905 dissertation *Ur de nordiska vedväxternas*

188. Lundström 1884 and 1887; his investigations of plant-animal relations were internationally well-known; the term »domaties«, i.e., outgrowths looking like small houses and containing animals believed to work in the service of the host-plant, has found its way into the botanical dictionaries.
189. For biographical details on Kjellman, see Franzén 1977 and Svedelius 1940.
190. For biographical details on Lindman, see Hesselman 1929; for details on Nilsson, see Hesselman 1907.
191. For a characterization of Jungner's work, see the assessments in ED 28/1 1910:39.
192. R.Karlsson 1983,p.112.
193. Lundström said of Lindman that he had studied »characteristic plant formations and their dependence upon outer factors (ecology)«, and Areschoug said that Lindman had published »ecological-biological« works (ED 12/8 1902:4).
194. L.A.Nilsson 1902.
195. Wulff 1902,p.1.
196. Witte 1906,p.3.

ekologi (From the ecology of the Nordic ligneous plants) Emil Haglund demonstrated how the anatomy and morphology of woody plants was an adaptation to the outer environment. Finally, a third Kjellman student, Sven G. Blomqvist, »began ecological investigations« in the summer of 1904[197] and discussed plant morphology and anatomy as an adaptation to the environment in a dissertation titled *Till högbuskformationens ekologi* (On the ecology of the high bush formation) submitted in 1911.

Where did they catch the new term from? In practice they had continued the same kind of investigations as Axel Lundström and their elderly colleagues had pursued under the heading of »plant biology« for more than thirty years. Their contemporaneous and seemingly independent choice of the term »ecology« was obviously an import from Copenhagen. They had found the new term in the works of the Danish botanist Eugenius Warming, by that time professor of botany at Copenhagen. Warming's textbook, *Plantesamfund,* had appeared in its first, Danish, edition in 1895 and had become an immediate success. Warming's detailed studies of morphological and anatomical adaptations to different environments were considered exemplary, and his notion of life-forms was a conceptual innovation in the tradition of plant biological studies. Hence, when accepting Warming's leading position in the biological studies of plants his junior Swedish followers adopted his terminology too – »ecology« was one of the key terms in Warming's theoretical system.

The adoption of Warming's ideas was facilitated by the fact that he was already well-known as an authority on plant biological problems to Swedish botanists. It was probably one of the first generation of plant biologist in Uppsala, Veit Wittrock, who had taken the initiative to summon Warming as professor in botany in Stockholm in 1882,[198] and also to recommend Warming to engage Axel Lundström as his first assistant. Though Warming stayed only a few years in Stockholm before returning to Copenhagen,[199] the personal contacts he made at the time with a new generation of students and young researchers (some of whom were to become ecological pioneers), were probably of importance for the later acceptance of his ecological thought.

With his emphasis on life-forms and the ecological adaptation of plants to their environment, Warming laid claim to a new botanical specialty.[200] In adopting Warming's ecological rhetoric his junior Swedish readers were enrolled into a miniscule new scientific social order. To begin with, however, none of them claimed ecology as an independent specialty of botany, not to mention a separate science. But soon others did. The claim for plant ecology as a synonym for plant biology was fully established a decade later, as witnessed by its use in a popular booklet by Rutger Sernander and another Uppsala botanist. Plant ecology, as identical to plant biology was, according to them:

»the science of adaptations to the outer world«,

or

197. »With the aim of studying the constitution of the bush type... on the basis of naturally growing material« (Blomqvist 1911,p.1).
198. See Stockholms högskola 1878-1898, pp.124-158; Wittrock had been appointed to the botany chair at *Riksmuséet* in 1879, and had tried to introduce plant biology at *Stockholms högskola* when giving lecturers in plant anatomy, phylogeny and biology for a couple of years around 1880.
199. For bibliographical notes on Warming, see Christensen 1924-26.
200. For an assessment of Warming's contribution to early ecology see Goodland 1975 and Coleman *in press*.

> *»the whole array of outer conditions or factors under which the plants live«.*[201]

Thus, the notion of plant ecology, as an independent botanical specialty, gained ground during the first decade of the 1900s. A nascent scientific social order of plant ecology had been established.

Neither Warming's emphasis on *»plantesamfund«*, i.e., plant communities, nor his approach to the study of adaptations was particularly new for a Swedish audience. His true innovation was the fusion of the practice of plant biology and the practice of community studies, as indicated by the Danish subtitle to *Plantesamfund*, viz., *Grundtræk af den økologiske Plantegeografi* (Foundations of ecological plant geography). In the popular booklet referred to above, Sernander described the goal of »ecological plant geography«:

> *»to show in which relation the different members of the sitting («matlag») stand to each other and to the factors which determine the existence of the community as such«* [202]

Warming's ecological approach evidently gave an apparent scientific foundation to the study of problems of plant geography. As demonstrated above (1-1) already Georg Wahlenberg and Hampus von Post had investigated the environmental conditions for plant distribution, and here and there botanists had approached the problem of plant communities and plant distribution. But these studies were still pursued essentially within the scope of the natural historical tradition. Likewise, when Gunnar Andersson should describe the relation between plant communities and their environment he wrote rather cryptically that

> *»since the outer circumstances... largely determine the biological peculiarities of the individual, it is by observing these peculiarities one should be able to form a picture of the plant communities which the species is capable of participating in«.*[203]

Warming's concepts made it possible to approach the problem of vegetation-environmental relations in a much more precise way, and hence his textbook from 1895 swiftly changed the attitude to these kinds of investigations. Thus, the study of »ecological factors« behind the distribution of plants became rather popular among Swedish botanists during the first decades of the 1900s. At the meetings of *Botaniska sektionen* in Uppsala »plant geographical problems« were taken up in connection with »ecological questions«.[204] For example, the dissertation of Nils Svedelius on a plant geographical problem,[205] was considered by his contemporaries to be, at least partly, an »ecological« study.[206]

Although many botanists only referred to the problem of ecological plant geography in passing,[207] others made it the main topic for their research. For example, John Frödin (1879-1960), who devoted most of his professional efforts to geographical problems (he was appointed professor in geography in Uppsala in 1929), was a well-known adherent of ecological plant geography. In an article on the relation between climate and plant

201. Sernander and Skottsberg 1915,p.21.
202. Ibid.,p.16.
203. G.Andersson 1890,p.492.
204. Svedelius 1940,p.196.
205. Svedelius 1901.
206. When assessing Svedelius for the chair in Uppsala 1914 (cf.1-2), Juel considered some of his work as »ecological« (ED 21/2 1914:24).
207. For example, Harald Kylin in his dissertation from 1907 mentioned »ökologischen Faktoren« in passing in his discussion of the regionalization of the algal flora and vegetation.

distribution he maintained that it was not enough to observe the relation between on the one hand plant distribution and vegetation types and climatic circumstances on the other; it was also necessary to analyze the climate types

> *»in their meteorological components and investigate the influence of each of them on plant life or plant species«.*[208]

This could be done, he thought, either by means of climatic experiments or by means of comparative studies where all climatic components, except for one, were common to two study areas. Frödin continued to publish occasional plant geographic articles, and undoubtedly exerted a certain influence on Swedish plant geographical discourse for several decades to come.[209]

Another well-known contribution was the vast inventory of the flora of northern Sweden conducted by Gunnar Andersson and his brother Selim Birger combining the immigration historical problem with extensive discussions of the ecological factors responsible for the geographical distribution of plants.[210] Ljungqvist's dissertation, *Mästermyr; en växtekologisk studie* (Mäster mire; a plant ecological study) of 1914, although by no means a scientific masterpiece, epitomized the basic idea of the ecological plant geographical approach. Ljungqvist wanted to inquire into the causal relation between the character of the site and the appearance of the vegetation:

> *»to demonstrate the postulated relation between cause and effect between 'the site' and its vegetation, and the ideal should be, so to say, to predict what kind of plant community must occur on a certain physiographically defined site«.*[211]

Each vegetational change, thought Ljungqvist, »corresponds to a site change«. This was to be a reciprocal relation:

> *»the placing together of plant communities... and... corresponding, known* /site/ *factors«* /constitute/ *»formation ecological units«,*[212]

i.e., what Ljungqvist called »a periodic system of reversible reaction products«.

To sum up – Warming's works evidently served as the impetus for the widely spread notion of ecological plant geography in the first decade of the 1900s. That does not mean, however, that his approach was always accepted in all its consequences. Particularly, his teleological metaphysical view of adaptation was contested. Sernander, who also had turned to the problem of finding biological explanations to the appearance of vegetation in the late 1890s, rejected all preconceived ideas concerning the relation between the site and the vegetation, including taking the adaptability of traits for given. Instead he presented a strict empirical observational approach in line with Hampus von Post. Accurate empirical investigations of biological phenomena might be a useful tool for

208. Frödin 1912,p.2.
209. Frödin 1916; Frödin's authoritative role among Swedish plant geographers is indicated by the following anecdote: Du Rietz (cf.2-2) had called a seminar in the mid 1930s on the vegetation on the Kullen peninsula which Frödin had worked on earlier: »And we gathered ... some 20 - 25 persons, and someone looked out through the window and discovered to his horror that Frödin was coming. And Du Rietz was totally... he was so frightened out of his senses that he rushed around...:'What shall we say, what shall we say'. He was afraid that Frödin should criticize the quality of his seminar, and unmask plant biology as a damned bluff. And I had... 'You must not say anything that offends Frödin' he said. And I had written rather devilish... I had written that we had realized that Frödin's description was inaccurate ... but I had to cut that out of my account« (Interview with BP 7/2 1982). Furthermore, in 1933, when the faculty should decide on the orientation of the plant biology chair in Uppsala, Frödin took a unambiguous stand in favour of an ecological interpretation of plant biology (cf.2-4).
210. Andersson and Birger 1912.
211. Ljungqvist 1914,p.10.
212. Ibid.,p.11.

elucidating the immigration history and the physiognomy of the vegetation, i.e., what he called plant biology:

> *»Die Pflanzenbiologie der Zukunft hat keine wichtigere Aufgabe als die, ihre Forschung durch direkte Beobachtungen und Experimente auf die Basis einer rationellen Entwicklungsgeschichte zu stellen«.*[213)]

By literally crawling on his knees, Sernander inquired into how certain plants spread their seeds by means of ants. Two great monographs came out of it: *Den skandinaviska vegetationens spridningsbiologi* (The spreading biology of the Scandinavian vegetation) in 1901 and *Entwurf einer Monographie der europäischen Myrmekochoren* in 1906.

The authorization of botanical field studies and plant »proto-ecology«

Axel Lundström was generally considered the leading plant biologist in Uppsala, and in 1891 Kjellman persuaded the faculty to propose a personal chair for him. The proposal was dismissed, however, on the grounds that it would be preposterous to have as many as three botanical chairs. Lundström took a lectureship at *Ultuna lantbruksinstitut*, but he did not develop agricultural research. Instead he intensified his contacts with an old student friend, Frans Kempe, who for several years had been managing director of *Mo och Domsjö*, the leading forest product company and a large forest owner. Kempe, who was considered one of the most progressive among his generation of entrepreneurs, believed in rational forest management and hired several botanists and geologists to work out management plans for the company's forests. In 1895 he helped Lundström to establish a small, private, forest biology research station in order to:

> *»investigate, by means of trials, the life conditions of the forest trees and the laws for the development of the forests... and to investigate the influence of mode of logging, soil treatment, degree of moisture, ditching, fire, grazing etc. on different modes of regeneration and growth«.*[214)]

The same year Lundström published a booklet on forest management, *Om våra skogar och skogsfrågorna*, which caused a great stir.[215)]

To be sure, Lundström was not the only professional botanist to approach practical forestry problems. During the 1880s and 1890s at least a dozen people »now and then« pursued »forest botanical investigations«, indicating »that the necessity for forest research was generally acknowledged«.[216)] But there was no institutionalized forestry research, and no forestry science organization (*Skogsinstitutet* in Stockholm did not pursue any research worth mentioning). There were plans to create one: in 1895 *Domänstyrelsen* proposed the establishment of a »forestry investigation organization«, but not until 1902 was a state forest research organization created. This indecision with regard to a state initiative may have been one of the reasons why Kempe in 1897 donated a large sum of money to *Uppsala universitet* for the creation of a chair in plant biology on the condition that Lundström should be its first incumbent.[217)] Thus, thanks to a private

213. Sernander 1906,p.394.
214. Lundström 1895.
215. Ibid.
216. Maass 1904,p.4(60); for a bibliography of Swedish forestry literature before 1900, see Malmström and Malmström 1959. Among the more proliferate forest investigators was Lars Albert Nilsson, who also agitated for experimental forest investigations conducted by a permanent forest investigation institute.
217. Anon.1963,pp.10-11.

initiative field studies of plants, and more specifically plant biological research, was authorized at Uppsala.

With Axel Lundström's untimely death in 1905 the chair became vacant. As a leading plant biologist and field botanist of a younger generation, Rutger Sernander was, of course, one of the top applicants. The outcome was not inevitable, however. Some of the leading younger »new German« botanists, such as Nils Svedelius and Bengt Lidforss, also applied for the chair. The laboratory botanists again saw a possibility to extend their sphere of influence. In a letter to the faculty Kjellman suggested that »plant biology« should be interpreted as »botany in its whole extension«, including plant physiology;[218] but Kempe, who was asked to comment upon the issue, was of the opposite opinion. He argued for

»the need for a special professorship in plant biology, distinct from physiology«.[219]

The majority of the assessors and the faculty respected the donor's intentions – Sernander was eventually appointed, and hence the authorization of field studies of plants and plant biology was upheld.[220] In addition a majority of the assessors and the faculty considered the chair to be directed towards »plant community biology or plant ecology«. However, on the view taken here, while this was surely a claim for ecology, strictly speaking it was not an authorization of ecology. »Ecology« was not a rhetorical key-word in the conflict, and the chair was officially still named »plant biology«.

Immediately after his appointment Sernander acquired a small suite of rooms for his Plant biology department, his life-form collections and the vestiges of a library. He also established a seminar, a kind of academic activity which was quite unusual among natural scientists at the time. Seminars were typically forums for arts students. »Real« natural scientists would gather in laboratories. But Sernander was not a typical natural scientist. He was as much a scholar in the arts and a patriot, as he was a natural scientist.[221] That does not mean that he was conservative or supercilious. On the contrary, being politically radical as a young student he played an important role in the liberal *Verdandi* association in Uppsala, and remained a *Verdandi* member to his death, never losing the attitude of an active and lively student.

Accordingly, Sernander's seminar was a popular and patriotic enterprise. It was entitled *Svenska växtsamhällen* (Swedish plant communities),[222] and from 1908 to the end of the 1910s it was devoted to a grand scale survey of Swedish vegetation, a parallel to the *British Vegetational Survey.*[223] The flora of Sweden was well-known since the days of Linné, but knowledge of vegetation and plant communities was rather limited. Sernander and his seminar proceeded methodically: first they spent two years on the wetlands, then several years on the forests,[224] and finally the lake-, sea-, and mountain vegetation was

218. Ibid.,p.11.
219. Ibid.,p.13.
220. At approximately the same time the old Uppsala plant biology network rapidly broke down. Lundström, Kjellman and Nilsson died within a few years time. Their students graduated and dispersed.
221. One of his biographers maintains that »he loved our history and its great cultural figures, and he himself was a well-informed and very well-read historian of personalities and scholarship, and a good classical scholar« (Skottsberg 1945,pp,.6-7).
222. For details on the activities of the seminar, see Sernander 1929.
223. See Tansley 1947.
224. The studies of forest vegetation was partly made in cooperation with Henrik Hesselman at *Statens skogsförsöksanstalt*; that was before the notorious feud between the two men.

taken up.

From the first day Sernander gathered around him dozens of students, not to mention curious amateurs.[225] Their mentor stressed that everybody, even freshmen, should contribute actively – an expression of his popular educational ideal. Consequently, the plant biology seminar became a tremendously lively gathering point for botanical field work and discussions. During the first ten years of its existence 43 different lecturers gave 126 lectures in all, and about a hundred articles were published. Even more important were the innumerable excursions; officially Sernander himself led 84 excursions – to all this must be added all the unofficial excursions and those led by his senior students. They were immensely popular; one of his biographers (and students) wrote:

> *»Never before had any academic teacher been more dearly loved, indeed adored. I suppose that such an enthusiasm had never been seen since Linné lead the way for his famous herbationes«.*[226]

Thus, Sernander established a most successful group of young field botanists directed to the study of Swedish vegetation.[227] It is not difficult to understand why: as a consequence of the laboratory and museum revolution in academic botany, field excursions had been disregarded as a serious academic activity even in Linné's country. Would-be botanists were trained already in school how to collect and classify species, and when entering university they were first of all introduced to the microscope. Sernander could secure a great number of students, with a latent or open interest in field studies of plants.

What did he translate this (and other) interests into? Actually, Sernander was the last great scientific naturalist in Sweden. If Hampus von Post was the greatest all-round Swedish naturalist of the 19th century, Sernander was his equal in the 20th.[228] Sernander wrote on bogs and their vegetation, on species formation, on adaptation in ants, on lichen biology, on nitrogen plants, on leaf litter, etc., but his main area was quarternary vegetational history – a theme explored already in his doctoral dissertation of 1894. And while on excursions he collected life-form, demonstrated environmental adaptations and soil profiles. He was a true polymath. For him, plant biology was a broad approach to the study of Swedish vegetation in all its aspects.

Hence Sernander translated his students naturalist interests into a broad »scientific natural history«. If ecology equals »scientific natural history«, Sernander was surely considered the most outstanding ecologist of his age. Though rarely using the word the idea was not at all alien to him. For example, in 1914/15 he gave a series of lectures on »soil science from the plant ecological point of view«.[229] Several of his doctoral students

225. The percentage of young undergraduates at the seminar during the first years is striking. As the years passed, the numbers of graduate students and post-graduates increased – »the men of the 1910s« grew old with the seminar; it was not rejuvenated until »the men of the 1930s« were enrolled (see 3-2).
226. Skottsberg 1945,p.8.
227. Among them were Fredrik Hård af Segerstad, Elias Melin and H. Smith, Sten Selander, Rikard Sterner and Carl Malmström, Gunnar Booberg, Åke Tengwall, Hugo Osvald and Erik Almquist, and finally Einar Du Rietz, the »youngster« of the group of young men recruited during the first enthusiastic years of the seminar and publishing their doctoral dissertation during the following decades. The last dissertation came in 1950(!), when Sten Selander, by then a famous novelist and essayist, published his investigations of the flora of the South-western Lule Lapland (see 3-2,note 89).
228. It is no coincidence that Sernander wrote the biography of Hampus von Post (Sernander 1912; cf.1-1).
229. Sernander 1929.

conducted ecological, or what would later be considered ecological, studies.[230]

In retrospect this donation chair might be interpreted as an early institutionalization and authorization of the first claims for plant ecology, since, as we have seen above, the terms »plant biology« and »plant ecology« were increasingly used as synonyms in the literature from around 1900. Strictly speaking, however, it was not.

An ecophysiological approach to plant distribution: Henrik Hesselman's field experiments

Henrik Hesselman (1874-1943) could not accept Warming's version of ecology either. But instead of developing a strict empirical field research program in keeping with Hampus von Post, he adopted the »new German« laboratory botany. As a young student in Stockholm Hesselman was closely attached to Gunnar Andersson, by then *docent* in plant geography at the university, and followed him as his assistant on Nathorst's Spitsbergen expedition in 1898.[231] He took his undergraduate degree in Uppsala, and by writing his licentiate thesis with Kjellman he was also trained in the ideas of the plant biologists, including the work of Warming. Among his contemporaries in Uppsala were Emil Haglund and Hernfrid Witte. Hence, his direction of thought in the mid 1890s was a mixture of plant geography and plant biology, or as he himself expressed it, »plant physiognomic studies and biological observations«.[232]

However, in 1899-1901 Hesselman started on what he called »physiological-ecological studies«, viz., investigations of the influence of different site factors on the life processes of plants. It was most probably his contacts with Rosenberg, who had just returned to Stockholm from Germany, that had set him off on the new trail:

> *»Beim Studium der Laubwiesen kam ich auf den Gedanken, es wäre vielleicht eine lohnende Aufgabe, direkt in der Natur die Lebensvorgänge der Pflanzen zu verfolgen, sich eine, wenn auch sehr dürftige und unvollständige, so doch empirisch gewonnene Vorstellung von den wechselnden, äusseren Faktoren under der variierenden Tätigkeit des Pflanzenlebens zu verschaffen«.*[233]

This was an innovation. Almost nothing of this kind had been done before, neither in Sweden nor abroad. The few plant physiologists were all busy studying plant reactions under artificial conditions: they were laboratory men. But Hesselman dragged the few measurement devices in Lagerheim's laboratory out into the park meadows in the

230. We have already mentioned Ljungqvist. Another was Carl Malmström (1891-1971) who attached himself to Sernander in 1910, before working as an assistant to Hesselman on the water-logging problem (cf.2-3). Malmström translated this practical forestry problem into an academic problem very close to the core of Sernander's main stream of research practice, and submitted a dissertation in 1923, dealing with the hydrology and developmental history of a northern Swedish mire complex. (Malmström 1923). Actually Malmström, not Du Rietz, became Sernander's true intellectual inheritor; he wrote extensive forest historical treatises, e.g., *Hallands skogar under de senaste 300 åren* (1939). Before finishing his dissertation he became permanently employed by Hesselman, however, and never took an active part in the later development of Sernander's seminar. Neither did he make any claim for ecology, though his work certainly would be interpreted as ecological by contemporary standards.
231. For biographical details on Hesselman, see Malmström 1971, and ED 31/12 1912:53.
232. Jo 31/12 1912:53.
233. Hesselman 1904,p.346.

archipelago of Stockholm. His doctoral dissertation of 1904, *Zur Kenntnis der Pflanzenlebens schwedischer Laubwiesen*, was filled with tabulated measurements of environmental factors, assimilation- and respiration intensities, and field trials of plant transpiration under different light conditions, including investigations of the accompanying changes of the leaf tissues. This was certainly something entirely new in Swedish botany!

Hesselman claimed his work as »ökologische Studien«. As a *docent* in botany at Stockholm he gave a series of lectures in 1906 on »Ecological questions with regard to the vegetation of Sweden«; he also reviewed what he called Swedish »ecological« literature for *Botanische Zeitung*.[234] It is important to note, however, that he did so in direct opposition to Warming's conception of ecology. He criticized Warming (and implicitly his Uppsala contemporaries) not only for being speculative and adopting a teleological methodology, but also for restricting the scope of ecology:

> *»Bei ökologischen Studien und Forschungen hat man in erste Linie den äusseren Bau der Pflanzen berücksichtigt«.*[235]

But this was not enough:

> *»wie diese verschiedenen Organisationstypen tatsächlich im Leben der Pflanzen wirken, wie sich die Lebensprozesse unter verschiedene Bedingungen abspielen, darüber liegen bloss wenige oder in vielen Fällen gar keine Untersuchungen vor«,*[236]

he added. Thus Hesselman claimed ecology as a science pursuing physiological investigations as an aid to inquire into the *mechanisms* of adaptation. Henrik Hesselman's claim for ecology was a claim founded on the advent of the »new German« laboratory botany.

Hesselman was assessed only as number three in the Uppsala competition in 1907-08.[237] However, he succeeded in getting his approach to plant studies authorized in another way. In fact, he too benefited from the emerging institutionalization of forestry science. In 1902 *Forstliga försöksanstalten* was created; a »botanist« was to be employed to take care of the scientific investigation, and the investigations were to be published in a separate report series.[238] The official aim of the Institute was:

> *»to contribute to the solution of biological and forestry questions fundamental for a rational forestry management by means of investigations and comparative trials. In this regard it should make close investigations of the particular forest types of the country, their occurrence and development, and finally the conditions of the forest trees within these types«.*[239]

This aim, it was thought, would be best fulfilled by means of »experimental investigations« for which special »trial areas« should be selected in different parts of the country.

To begin with Gunnar Andersson, by then *docent* in plant geography at Stockholm, and considered to be one of the »leading forces« behind the establishment of the Institute,[240] was employed as botanist and Hesselman became his assistant. When Andersson

234. See *Botanische Zeitung* in the early 1900s.
235. Hesselman 1904,p.345.
236. Ibid.,p.346.
237. ED 13/6 1908:21.
238. *Meddelanden från forstliga försöksanstalten* (later *Meddelanden från statens skogsförsöksanstalt* and *Meddelanden från statens skogsforskningsinstitut)*. Simultaneously the nation-wide *Föreningen för skogsvård* (the Association for Forest Management) was founded in 1902 (from 1913 *Svenska skogsvårdsföreningen*), publishing its own journal, *Skogsvårdsföreningens tidskrift*, from 1903.
239. Address from *Domänstyrelsen* to the Government 3/12 1901 (in Jo 9/5 1902:8); the final instruction for the Institute is of the same wording (see *Bih.SFS* nr 45, 9/5 1902).
240. G.Samuelsson 1918,p.739.

left the Institute (and botanical research) in 1906,[241] Hesselman was appointed his successor and soon concentrated full-time on rational forest management problems. By 1907 he edited *Skogsvårdsföreningens tidskrift*, and in 1912 he was appointed professor at the Institute, by now called *Statens skogsförsöksanstalt*.[242] His forestry science career did not put an end to his mode of approaching problems concerning the relations between plants and environment by means of physiological analysis. But it did in practice put an end to his tentative claim for ecology as a new botanical specialty.

Thus, in spite of the hegemony of the »new German« botany in Sweden around the turn of the century, field studies of plants, being claimed alternately as plant biology, plant geography or occasionally plant ecology, were authorized at two local sites by the first decade of the 20th century – the endowed chair in plant biology in Uppsala and *Statens skogsförsöksanstalt* in Stockholm. Sernander's and Hesselman's local departments would turn out as the main centers for ecological research during the following decades. Chapters 2 and 3 will review the further development of field studies of plants and vegetation at the two local sites, focusing on the emergence of renewed claims for plant ecology as an independent specialty.

Practical animal »proto-ecology«: marine fisheries and Svenska hydrografisk-biologiska kommissionen

Like the new approach to field studies of plants, the new academic approach to field studies of animals proceeded along two lines – animal biology and animal geography, analytically distinct in principle, but often interwoven in practice and persons. While the first plant biologists appeared in Uppsala in the 1870s, and the first claims for an independent specialty of plant geography came in the 1890s, animal biologists and animal geographers appeared somewhat later. Likewise the claims for an ecological animal geography and animal ecology came later, and were not as extensive and as strong as for plant ecology.

In the following passages we will examine the immediate background of the emergence of the first claims for animal ecology made by academic scientists trained as zoologists in Sweden around the turn of the century 1900. But before reviewing these we shall briefly discuss a case of »proto-ecological« investigations related to the practical problem of marine fishery.

Like freshwater fishery, marine fishery was a lacuna for the natural historians, and the later establishment of a marine fishery organization was as slow. A temporary administrative position as a herring fishery manager on the West coast had been set up during the decades of good herring fishery around 1800, and a fishery inspector was appointed for the West coast fisheries in 1855. Otherwise very little happened until *Lantbruksstyrelsen* (the Board of Agriculture) was established in 1889 and some of its fishery inspectors and assistants occasionally took up marine fishery problems. The few zoologists hired by

241. Andersson actually turned more and more to practical and social problems; after a short tenure as lecturer in botany at *Skogsinstitutet* he was appointed professor in economic geography at *Handelshögskolan* (the College of Commerce) in Stockholm in 1909.
242. Jo 31/12 1912:53.

Lantbruksstyrelsen to pursue irregular marine fishery studies mainly continued the »proto-ecological« approach inherent in the practice of the natural history tradition.

The principal initiative for a marine fishery research organization, and a more explicit »proto-ecological« research programme was not formulated by zoologists, however, but by two chemists, Otto Pettersson, professor in chemistry at Stockholm and his old student friend Gustaf Ekman, by then chief engineer in Göteborg.[243] In the winter of 1890 they resumed Sven Lovén's and F.L.Ekman's zoological and hydrographical survey of Swedish coastal waters of 15 years earlier (cf. the Prologue), and oriented themselves to the emerging problems of a rational marine fishery. Overfishing was considered a serious problem, exacerbated by new techniques (e.g. steam ships), new trawling methods, and the advent of new fishing nations (e.g. Germany).

The immediate motive for the 1890 expedition was to follow up a long standing idea of Ekman's; twenty years earlier, when commissioned to make hydrographic investigations in connection with an anticipated herring glut, he had suggested that the mass occurrence of herring depended on the type of water that was prevalent off the coast. According to Pettersson it was this finding that

> *»directed my interest to a deeper study of the inner movements of the sea and their importance for the climate and the marine fisheries«.*[244]

The winter expedition of 1890 verified Ekman's hypothesis, and added other similar findings on the connection between the occurrence of fish and the state of the marine environment. It also taught them that:

> *»since the biological phenomena were closely connected to the hydrographic conditions, the exploration of the sea in the future must include both«.*[245]

In order to conduct such a program a permanent research effort was necessary; in 1892 Pettersson and four other members of *Vetenskapsakademien* proposed organized explorations of the coastal waters. They succeeded in gaining the support of the *Riksdag* for the project, and it was launched shortly after under the name of *Hydrografiska kommissionen*.[246] The biological investigations were confined to plankton,[247] since these organisms »more than others must be dependent on hydrographical changes and water changes«.[248] A large number of technical reports was the result – 33 reports were published on plankton only between 1893 and 1901.[249]

The successful *Hydrografiska kommissionen* was followed up by other initiatives. In the shadow of the pre-war naval rearmament in the North Sea Ekman and Pettersson gathered representatives of the coastal states to a conference in København in 1902. This was the start of a permanent international marine research cooperation and organization.

243. For biographical details of Pettersson, see O.Pettersson 1908; on Ekman see Hubendick 1950.
244. O.Pettersson 1908,p.57.
245. Redogörelse 1903,p.3.
246. A history of the Commission's work is found in Redogörelse 1903.
247. The plankton investigations were pursued by an Uppsala zoologist, Carl Aurivillius (cf. below) and the well-known chemist and plankton specialist P.T.Cleve.
248. Redogörelse 1903.
249. On the other hand they did not focus on the relation between hydrographic conditions and the occurrence of bottom animals. Ekman and Pettersson expected that »professional zoologists« should continue Lovén's pioneer investigations of bottom fauna from 1877-79. The only attempt to do this was Lönnberg's investigations (note 255).

The aim was, of course,

> *»eine rationelle Bewirtschaftung des Meeres auf wissenschaftlicher Grundlage vorzubereiten«.*[250]

Over again the two comrades-in-arms emphasized that different opinions regarding the over fishing problem had their ground in

> *»inadequate insight into nature's economy«,*

i.e.,

> *»the life cycle of fishes and migrations and the production capacity of the sea could only be inquired by means of combined zoological, botanical and hydrographical research«.*[251]

In 1901 Ekman and Pettersson reorganized *Hydrografiska kommissionen* as a Swedish branch of the new international marine fishery organization: the new commission, *Svenska hydrografisk-biologiska kommissionen* (the Swedish Hydrographical-Biological Commission) was enlarged with among others a representative of the fishery administration. They engaged two assistants to work on plankton production, the sea-bed, and the biology of economically important fish species; in 1905 they acquired a survey ship; and finally they erected a counterpart to *Kristinebergs zoologiska station*, viz., the *Bornö station* further up the Gullmar Fjord, a

> *»marine investigation station for scientific investigations of the life and development of useful sea animals«.*[252]

Thus Ekman and Pettersson had institutionalized a »proto-ecological« research programme focusing on the relations between organisms (fish, plankton etc.) and environment.[253] They sidestepped the »proto-ecological« legacy from the natural historians – instead they built their investigation programme on the new specialized disciplines of »zoology«, »botany« and »hydrography«. But they never claimed the programme as a new and independent scientific discipline. Problems concerning the causal relations between water regimes and plankton and fish occurrence were translated either into »hydrography« or into »fishery biology«, but never into »ecology«.[254] Thus, the late 19th century marine fishery scientists never turned into ecologists.[255]

250. G. Ekman *et.al.* 1907.
251. Redogörelse 1903,p.3
252. Anon. 1903. Later, however, the unity between hydrographers and fishery biologists was broken up. The hydrographers took over *Bornö station*, while the fishery biologists were temporarily accommodated at *Kristineberg* and other places.
253. *Svenska hydrografisk-biologiska kommissionen* existed until 1948 when it was incorporated into *Fiskeristyrelsen* (the Fishery Board). Its results were published as *Svenska hydrografisk-biologiska kommissionens skrifter* nr 1(1903)-nr 7(1922) and *idem. NS Biology 1* nr 1-7 (1925-35) and *2* nr 1-10 (1937-1948).
254. Others claimed similar research as new sciences. A good decade later, as shown below (2-1), a young Lund botanist, Einar Naumann, claimed studies of the relation between lake water and bottom regimes, and the occurence of microorganisms and fish as a new science: »limnology«.
255. Lönnberg at Uppsala (cf. below) made some »proto-ecological« investigations when commissioned by *Lantbruksstyrelsen* to »investigate animal life in Öresund, and to make collections of marine animals to be found there« (Lönnberg 1898, p.6). His surveys in the 1890s resulted in a species list but also »a list of their common appearance in the main types of the different sea-bed formations«, i.e., a late parallel to what Wahlenberg and von Post had done on flora and vegetation. Lönnberg concluded that: »almost every animal species prefers some special part of the sea-bed. In many cases this is due to their being adapted to a certain way of life, and hence if the larvae, after spawning sink down to a place with an unsuitable bed or where other conditions of life are wanting, the animals soon die« (ibid.,p.62; see also Lönnberg 1899), a notion which reflected the general »proto-ecological« view of organism-environment relations at the time. Studies like these were not claimed as ecology, however.

Animal biology in Stockholm

In spite of the fact that the university in Lund had been the center for faunistic studies in Sweden during Sven Nilsson's reign from the 1830s to 1850s, no Lund zoologists made claims for ecology during the first decades of the 20th century. The Lund zoologists were above all microscopists – David Bergendal had introduced comparative anatomy and Hans Wallengren had turned to physiological studies. Except for the rather limited animal field studies made by Hans Wallengren and his students in the Öresund sound, «proto-ecological» studies did not have any prominent position among Lund zoologists during the period considered here.[256] And consequently no Lund zoologists claimed ecology.

The Department of Zoology at Stockholm was a center for comparative anatomical studies too, and field zoology was not a favoured pursuit. Leche abhorred simple descriptive faunistics. Nevertheless some of the Stockholm zoologists were among the first to speak about their studies in terms of ecology. One of the best known pioneers of field animal studies in Sweden during the late 19th century, Gottfrid Adlerz (1858-1918), was closely associated with Leche. His dissertation, worked out in the early 1880s, was devoted to ants;[257] besides a detailed morphological, anatomical and systematical review of Scandinavian ant species, he made very detailed observations of ant behaviour, care of eggs, larvae, and cocoons, on the the distribution of work in the anthill, and the relation between separate ant-societies, and even on the importance of the outer temperature for the activity level of ants. His later works were devoted to the study of instinctive behaviour in insects, but he also touched upon the problem of animal distribution, making a small investigation of the immigration of animal species on an island,[258] hence foreshadowing Shelford's classical succession studies.

Leche most probably considered Adlerz's studies a way of making faunistics scientific. Later one of Leche's students, doing field studies of insects, proclaimed in the spirit of Leche:

256. After having been appointed professor in zoology in 1908, Hans Wallengren spent part of his summers together with a couple of students investigating the bottom fauna of the Öresund sound. In 1914, he started a report series, *Undersökningar över Öresund*, procured a boat (called «Sven Nilsson»!) and decided that all zoology students should devote a month to the study of the marine fauna (Löwegren 1968, pp.97-99). One of his graduate students was Wilhelm Björck (1888-1975), who could well have institutionalized a claim for ecological animal geographical tradition in Lund. Björck (for a biographical note on Björck, see Sandberg 1923) early oriented himself towards animal biological and fishery problems. He was one of the first students to visit Nordqvist's laboratory at Aneboda (cf.2-1), and for two summers he worked as an assistant to *Svenska hydrografisk-biologiska kommissionen.* On Wallengren's initiative he took up animal geographical problems and tried to group the fauna of Öresund into animal geographic regions. During this work he also touched upon something akin to ecological animal geographical problems when discussing «the successive changes of the sea bottom... /and/ the causality and conformity to law of the distribution of marine animals» (W.Björck 1915) in relation to bottom and hydrographic conditions and vegetation, and the reciprocal influence of animals upon the environmental conditions. Björck did not continue his academic career, however. Being one of the few academically trained Social Democrats of his age, his political affiliations got the better of him. In 1917 he took his seat in the *Riksdag* and later made a career in the school administration. What might had turned into a claim for marine ecology in Lund came to nothing. The tradition for Öresund investigations did not result in any substantial ecological animal geographical work until the late 1930s (cf.3-4).
257. Adlerz 1886.
258. Adlerz 1893.

> *»Only to know the name of an animal and knowing the outer appearance is in any case a superficial and dry knowledge only... Only by combining more deep and detailed questions - whether anatomical, ontogenetic, physiological, or biological - the knowledge is deepened«.*[259]

Adlerz's approach to animal field studies was in full accordance with this programme. Having been interested in field observations from very early years he seems to have been heavily influenced by Darwin's work, through which

> *»I got the idea of how natural scientific research ought to be pursued and that dry systematical studies by no means are the final goal of biological science«.*[260]

Hence Adlerz's field animal studies did not threat the hegemony of the »new German« zoology. On the contrary he was apparently encouraged by Leche to continue his biological studies – his scientific career coincided with the founding years of zoology in Stockholm, and he taught zoology there between 1884 and 1894, before ending up as a secondary school teacher.

Despite his support from Leche, Adlerz was a lone wolf and never had any research students in Stockholm. In addition, Adlerz never mentioned the term ecology, and never made any claim for it as an independent zoological specialty. But several Stockholm zoologists took up animal field studies again, eventually even deciding to »pursue excursions for a faunistic investigation of the Stockholm area«.[261] One of them, Eric Mjöberg (1882-1938) published a number of systematical, biological and faunistic observations on insects – the best known of them is his inventory of the insect fauna of the Baltic island Fårö, in which he tried to classify the fauna in »biosynoecier« in order to give a picture of the composition of the fauna in different localities, like deciduous forests, sand dunes etc.[262] Mjöberg never claimed these studies as ecological, however. For example, when, in 1905, discovering a rich insect fauna in wrack, »dieses sharf begrenzten Faunengebietes«, Mjöberg focused on the developmental stages of the wrack species, not on the relationships between insects and the wrack environment (while thirty years later a third generation ecologist would make the study of the ecology of the wrack fauna the topic for a whole dissertation).[263]

An older colleague of Mjöberg, Nils Holmgren (1877-1954), making profound contributions to the anatomy of insects, took up the cudgels for biological observations. As a youth he had had a deep interest in nature,[264] and subsequently got a chance to join a South American expedition which furnished him with material for his dissertation on termites. Besides making faunistic and systematic notes, Holmgren also made »biologischen« observations of termite habits, and described the swarming and formation of new termite colonies, the function of soldiers in the »Termitstaate«, and the symbiosis between different species.[265]

259. Mjöberg 1905,p.4.
260. Quoted from Anon. 1928,pp.376-77.
261. Quoted from *Stockholms högskolas årsredogörelse* 1907-09. Some of them published papers with faunistic or biological contents. For example, John Runnström, later a pioneer experimental zoologist (cf.2-5), chose a faunistic subject for an undergraduate thesis (J.Runnström 1909).
262. Mjöberg 1905.
263. See Helge Backlund below (3-4).
264. »Already as a school-boy he collected natural things of different kinds and wrote his findings and observations down in small note-books« one of his biographers points out (Pehrson n.y.).
265. Holmgren 1906.

In fact, Holmgren presented his findings of termite life habits under the heading »Öcologie«, obviously using it as a synonym for biology, and in contrast to »Faunistik«. Thereby Holmgren was the first zoologist at Stockholm, who presented his findings in terms of ecology. The Stockholm zoologists seem to have considered ecology a decent scientific enterprise. Leche, following Haeckel and the Darwinian tradition, was later to define ecology as a kind of relational physiology:

> *»Ecology, which is often quite improperly designated as biology, is the science of the household of the animals and all external conditions of life, i.e., the total sum of relations to their inorganic and organic environment, in one word all those complicated circumstances which Darwin called 'struggle for life'«.*[266]

On the other hand, these attempts at animal field studies, although accepted by Leche, were never far-reaching. The Darwinian impetus for a claim for ecology was never pursued.

Holmgren continued his, mainly systematical, termite studies for another decade, interleaved with reports on field observations. They were not supposed to take the major share of his attention however. When discussing the relations between ant community and vegetation in an article a few years later[267] he apologized for the

> *»ziemlich flüchtigen Untersuchungen welche ich in meinem Mussestunden vorgenommen hat«.*[268]

Thus, animal field studies in Stockholm was mainly considered an activity for spare-time enjoyment. Holmgren returned to the main stream of the »new German« zoology, and devoted the rest of his academic life to comparative anatomical and phylogenetic problems, even switching from insects to vertebrates, probably in order to secure appointment to the chair in zoology after Leche[269] in 1920. From then on his students remember him best for his scathing critique of all kinds of faunistics.[270] Stockholm zoology remained a strong-hold for comparative anatomical studies of vertebrates until the early 1950s. The few scattered notions of ecology were never institutionalized, no ecological discourse was established.

Studies of animal distribution in Uppsala

The »new German« zoology was almost hegemonic in Lund and Stockholm. In Uppsala, however, where Lilljeborg had been the leading Swedish faunist through the 1870s and 1880s, field studies of animals prevailed an important undercurrent practice. As with their colleagues at the botanical department, the Uppsala zoologists did not remain satisfied with faunistic surveys. They translated field studies of animals partly into problems of animal biology, under the impact of Darwinism, but mainly into problems of animal geography. A pioneer with regard to animal biological investigations was Carl Aurivillius (1854-1899), who made field studies of morphological adaptations, e.g., masking,

266. Leche 1922,p.832.
267. Holmgren 1904; in which he reached the conclusion that »die Ameisen bei der Hügelbildung in der fraglichen Sümpfen eine grosse Rolle spielen, indem ihre Haufen als Ansatzpunkte der Moos- und Torfvegetation dienen« (p.369).
268. Holmgren 1904.
269. See ED 31/12 1920:2; the assessors did not mention Holmgren's ecological studies.
270. All interviewees having personal knowledge of Holmgren mention it.

symbiosis, and the relation between the anatomical build-up of sense organs and life habits of crustaceans;[271] Aurivillius, who was retrospectively considered »an ecologist«,[272] died young, however, and had no research students. Other field studies of animal life habits were made in connection with practical fishery problems, and sponsored by the emerging fishery administration. As shown above (1-1) fishery investigations had been rarely pursued before the 1880s. It was not until the 1890s that a new and nation-wide fishery administration was established under *Lantbruksstyrelsen*.[273] The prime mover in late 19th century fishery research was Filip Trybom (1850-1913), a student of Lilljeborg. Joining the fishery administration in 1878, and establishing himself as the leading fishery investigator in the 1890s and 1900s.[274] he made investigations of the biology, predation and growth of crayfish and investigated stomach contents of different fish species in order to decide their potential harm to salmon fry. He also devoted much of his energies to marine fishery investigations.

It is easy to understand the popular appeal of studies of animal biology. This was particularly evident from the biological museums created by Gustaf Kolthoff and Bruno Liljefors in the 1880s and 1890s. Kolthoff, a self-made zoologist, with hardly any formal education, devoted most of his time to hunting, and it is said that he possessed an unusual capacity for observing game habits. He was trained as a taxidermist, owned his own enterprise, and in the 1870s he created a small biological museum in his home village, biological in the sense that the animals were exhibited in their natural environment instead of being displayed in the normal monotonous systematical order. Lilljeborg in Uppsala offered him the curatorship of the zoological museum in 1878, and here he was encouraged, both in words and by funds, to carry out his plans. The young artist Bruno Liljefors made the settings and Kolthoff prepared the animals. All the exhibits were completed between 1887 and 1890.

Kolthoff's and Liljefors' small biological museum in Uppsala was such an immense success, that they raised private funds for another and much enlarged project in Stockholm, a circular diorama on a huge scale. *Biologiska muséet*, as it was called, opened in 1893, displaying most of the Scandinavian macrofauna in its natural surroundings and installed in a spiral-circular three-storey building. Mainly thanks to Liljefors' artistry the illusion of forest and shore-meadows, cliffs and mire was bewitching, and the museum became a model whose techniques were widely copied.

Kolthoff continued to produce small dioramas. Liljefors developed his art into a long series of paintings of animals in their natural environment which eventually established

271. Lönnberg 1920.
272. Jägerskiöld 1943.
273. A first small fishery investigation institute was created in Finspång around 1890 (it languished and soon faded away, however). More important from the point of view of fishery research was the creation of a fishery scholarship in 1891, and yet another one in 1904. In addition a regional fishery manager organization was established in 1904-1905 – thus the basis for a »rational fishery« had been laid, albeit not comparable in size to the organizations for »rational agriculture« and »rational forestry«. E.g., the fishery scientists and administration had a very low degree of autonomy, being part of *Lantbruksstyrelsen*, and most important they had no independent educational system of their own (most fishery investigators and administrators were trained as zoologists).
274. Trybom was responsible for more than half of the reports on Swedish fisheries published by *Lantbruksstyrelsen* (in the series *Meddelanden från Kungl.Lantbruksstyrelsen*) in the 1890s and 1900s; for a biography of Trybom, see Lönnberg 1913.

his status among the masters of naturalist art: »he did not add the animal to the terrain but let them grow out of the ground in an organic way«.[275] One of his favourite subjects was birds in protective disguise, and beasts and bird of prey in action, showing that his art was heavily influenced by the Darwinist idea of »struggle for life«. Yet it is not true that Liljefors »in the 1890s consciously began to aim at an animal description with ecological signatures«, as a Swedish historian of art maintains.[276] He was a trained hunter, a keen observer of animal habits, but his view of animals and nature was the Darwinian biologist's. He had no ambitions of delineate his studies of nature as a new specialty.

Thus, Darwinian animal biology had adherents in Uppsala, but even more important from the point of emerging animal ecology was the vibrant faunistic tradition in Uppsala. After Sven Nilsson's student Vilhelm Lilljeborg had been appointed professor in zoology in Uppsala in 1854, the center for faunistics in Sweden moved there from Lund. Lilljeborg exerted a considerable enrolling power on Uppsala students even after his retirement (he did not die until 1908), and in contrast to the conditions in Stockholm, where faunistics was abhorred, his successor Tycho Tullberg did not directly reject faunistic studies; on the other hand there was a strong tendency to translate animal field studies into animal geography. The best known researchers, who had the greatest influence upon the course of Swedish faunistics and animal geography, were Einar Lönnberg (1865-1942) and Leonard Jägerskiöld (1867-1945).[277] They studied zoology together in Uppsala in the late 1880s and wrote the almost obligatory dissertation on the anatomy of »worms« under Tullberg's supervision in the early 1890s. Otherwise they spent most of their energies on field-trips and hunting, travelling extensively: Lönnberg went to Florida, to the Caspian Sea and to East Africa; Jägerskiöld to Egypt and Sudan. Both published a string of faunistic, systematical and animal geographic papers, and hence contributed to the maintenance of the considerable faunistic and field zoological practice established at Uppsala by Lilljeborg.

Neither Lönnberg nor Jägerskiöld could obtain a university tenure and both left Uppsala in the early 1900s[278] for museum appointments. As museum zoologists they made further substantial contributions to animal field studies, particularly through their popular publishing activities. For example, in 1906, Lönnberg created a popular journal, *Fauna och Flora*, containing numerous naturalist, faunistic and floristic articles and short communications. It was started in order to »create a connection between the general public searching for knowledge and pure scientific researchers«, and later became an important input to the naturalist revival to follow from the late 1920s onwards (cf.3-1). Lönnberg was also a leading member of *Svenska jägareförbundet* and *Svenska naturskyddsföreningen* for many years, and thus likewise prepared for the naturalist revival in the inter-war period.

275. Ellenius 1981,p.178.
276. Ibid.p.178.
277. For biographical details on Lönnberg, see Franzén 1984, Hanström 1943, and Wibeck 1943. For biographical details on Jägerskiöld, see Hanström 1945 and Franzén 1975.
278. Lönnberg was appointed professor at *Riksmuséet*; Jägerskiöld director at *Göteborgs naturhistoriska museum* (the Museum of Natural History in Gothenburg).

The first claims for an ecological animal geography

We saw above how the two approaches to a scientification of field studies of plants – plant biology and plant geography – were united in the notion of an ecological plant geography. The idea of coupling animal biology and animal geography into a »biological« or »ecological animal geography« began to take shape in the minds of the generation of zoologists recruited to the universities in the late 1890s, most probably inspired by the example of the ecological plant geographers.[279] An apparent example is Nils von Hofsten's zoological program for the Swedish Spitsbergen expedition in 1908. Von Hofsten (1881-1967), the leading figure in Uppsala zoology in the inter-war years, devoted himself to a number of faunistic investigations in the early 1900s[280] besides doing comparative anatomical work for Tullberg. When assigned as the zoologist on Gerhard de Geer's last great Spitsbergen expedition in 1908 his aim was not only systematical, but:

> *»ein ökologisch-tiergeographischer: auf die Verteilung der Organismen, auf die Zusammensetzung der Tiergesellschaften, über die Abhängigkéit von den den wechselnden äusseren Bedingungen war Licht zu werfen«,*[281]

a task which he considered

> *»restricted and maybe also modest, but at the same time a new one«.*[282]

The plan was for the ship's officers to make depth measurements, while other participants of de Geer's expedition were to make hydrographic, planktonological etc. investigations, and von Hofsten and his assistant were to survey the bottom fauna. The correlations between water conditions and distribution of animals should be published in order to give:

> *»ein Gesamtbild der Bodenfauna des Eisfjords... von ökologischen und tiergeographischen Gesichtspunkt«.*[283]

However, the systematical interest was stronger. Most of the animal groups collected were worked up by museum specialists, but the »zusammenfassende Darstellung« never appeared. The notion of ecology was incorporated into the plan, but was never put into practice. Von Hofsten again concentrated on his anatomical work and was eventually summoned to the comparative anatomical zoology chair in 1921.[284]

Another Uppsala student recruited in the late 1890s went a step further in unifying the geographical and biological approaches to field studies of animals,[285] and in addition translated this approach to the first persistent claim for ecology and ecological animal geography as a zoological specialty in Sweden. Sven Ekman (1876-1964) is said to have uttered that he

279. The connection between ecological/biological plant geography and ecological/biological animal geography around the turn of the century has not been investigated in detail here; although the professional limits between botanists and zoologists were well established, there are many indications that, at least among field botanists and field zoologists, the personal contacts and intellectual interchange was quite lively.
280. For example N.von Hofsten 1909.
281. N.von Hofsten and Bock 1910,p.1.
282. Ibid.,p.1.
283. Ibid.,p.2.
284. ED 29/4 1921:84.
285. Later several Uppsala zoologists devoted themselves to a combination of systematical, animal geographic and biological problems – a common type of Uppsala dissertation for several decades was an exhaustive monographical treatise of a restricted systematical group, their taxonomy, morphology, sometimes anatomy, life habits, distribution etc. (cf.2-5).

> *»became a biologist in order to be able to be outdoors as much as possible, especially during the summer«.*[286]

Already by his early student years he had written articles in this naturalistic spirit, including observations of birds in the mountain region, and popular essays on mountain animal and plant life and the changing animal life in upper part of the province of Dalarna (including a notice on the mating call of the brown bear!). Ekman continued this approach and it recurred in his dissertation of 1904, which was an all-round (systematical, biological and animal geographic) treatise of north Swedish crustaceans, focusing on the faunistic composition, but also including a description of the life cycle of the species, and discussing what he called »Die äusseren Existenzbedingungen der Tiere« in relation to the composition and distribution of the fauna.[287]

These »biologisch-tiergeographische« investigations were, of course, very similar to what his botanical colleagues in Uppsala called »ecological plant geography«, i.e., an attempt to explain the distribution and immigration history of the species with reference to their life conditions. Ekman and Sernander knew each other[288] and it is quite possible that Ekman simply translated Sernander's approach to animal studies. Anyway, Ekman continued to orient himself towards combined biological (adaptational) explanations of animal geographic problems. In a popular booklet on lake animal life from 1909 he gave an outline of animal communities resembling the »deductive« approach to plant communities (cf.2-4), i.e.,

> *»such animals, which are adapted to the same surroundings, constitute together what one calls an animal community«.*[289]

Like Lönnberg before him, Ekman for several years made his living by holding a fishery scholarship, and utilized this position to continue his investigations, for example on relict faunas. His best known and most extensive study along these lines was the Lake Vättern investigations, pursued while he served as a secondary school lecturer in Jönköping in 1909-16. Securing financial support from the county agricultural economic associations around Lake Vättern he studied the composition of bottom animal communities and their relations to bottom types. Probably without knowledge of Petersen's bottom sampler, which had been constructed the same year,[290] Ekman constructed a bottom sampler to take quantitative samples over the whole lake, down to 120 meters depth. He was able to demonstrate that the distribution of the bottom fauna was patterned, that the »animal communities«

> *»are not at all irregular, but parallell to the variation in bottom material in a very fixed way«.*[291]

After having spent almost a decade as a secondary school teacher in biology in Jönköping, Ekman was endowed with a post-doctoral *docent* scholarship in Uppsala in 1916. Once more he threw himself into the problems of animal geography, and spent six long years summarizing his rich faunistic experience and the whole Scandinavian faunistic

286. Rodhe 1976,p.5. For biographical details on Ekman, see ED 6/6 1912:31, ED 20/7 1927:2, Lundblad 1964 and Rodhe 1976.
287. Ekman 1904,pp.72-80,158.
288. Ekman acknowledges Sernander's help with »pflanzengeographische Angaben« (S.Ekman 1904,p.4).
289. S.Ekman 1908,p.3.
290. See Petersen 1911-1918.
291. S.Ekman 1914,p.3; the full results were published in S.Ekman 1915.

and animal geographic literature. The result was *Den skandinaviska djurvärldens invandringshistoria*, a treatise in excess of 600 pages on the immigration history of the Scandinavian fauna. Although largely a compilation of faunistic and geographic information, Ekman nevertheless gave it a distinct ecological twist. Already on the first page he defined ecology as »the knowledge of the way of life of animals and plants«.[292] Thus, Ekman considered ecology primarily to be a science referring to relation between the environment and the single individuals/species, not the relation between environment and animal communities.[293] Further down the text, he introduced a distinction between »existence ecology« and »distribution ecology«, the former referring to the relation between environmental factors and the existential demands of the species, the latter referring to the species' faculty for colonizing a certain geographical area. Hence, although Ekman's book dealt with the fauna and its distribution, his approach was deliberately ecological. In other words, Ekman tried to ecologize faunistics and animal geography.

In contrast to Holmgren and von Hofsten who had used the term ecology in passing, Ekman actually claimed ecology as an independent scientific specialty. In a popular booklet from 1917 he designated studies of the conditions of existence of animals as ecology, and even considered it one of the major branches of zoology. Ecology was, said Ekman:

> *»the study of the way of life of separate species... the science of the household, viz., with the outer conditions of life, the relation to light, heat, nutrition, other organisms etc., in short: the science of the status of the organisms in the outer world«.*[294]

From his literature recommendations we can see what Ekman understood as model ecological investigations: Kolthoff's *Ur djurens liv* (From the life of animals), Hemberg's *Jaktbara däggdjurs gångarter* (The paces and tracks of game), and Palméen's *Om fåglarnas flyttningsvägar* (On the migration routes of birds). That is, Ekman's animal ecology was a synonym for animal biology in the restricted Darwinian sense, but unlike other Swedish zoologists of his age he claimed it as an independent specialty worthy of serious and sustained academic attention.

On the other hand one gets the impression that Ekman's claim for ecology was intended more for the layman than for the professional zoologist.[295] For one thing he considered ecology to be one of the few branches of zoology in which the layman could »deliver important contributions to the progress of science«.[296] A particularly suitable

292. S.Ekman 1922,p.1,note 1.
293. Cf. however, his work on marine animal communities in the 1930s and 1940s (3-4).
294. S.Ekman 1917,p.4.
295. In fact, a few amateurs utilizing the term »ecology«, were probably enrolled by Ekman. Most important was Einar Wahlgren (1874-1963), a contemporary of Ekman, who wrote a systematic-morphological dissertation in Uppsala on insects in 1906. Like Ekman, Wahlgren took a position as secondary school lecturer, where he continued his insect studies, mainly writing systematical, animal geographic and faunistic works. A significant contribution in this context is his description of the fauna of the island Öland (Wahlgren 1915, 1917). Besides a faunistic inventory, Wahlgren discussed the »ecological distribution« (part iv) of the »biocoenoses« of the calcareous heath, and the »effect of calcareous heath environment on the ecology of the fauna« (part vi), that is, a qualitative discussion of the chemical composition of the ground, climatic factors, the plant cover, etc. as ecological factors. Wahlgren conducted all his researches as a spare-time pursuit, and, unlike Ekman, who returned to the university (see below), he had no possibility to recruit research students. He enrolled another amateur, however, viz., Anton Jansson, who quoted Wahlgren's studies, when making reference to insect sites in »ecological respect« (A.Jansson 1922).
296. Ekman 1917, pp.26-27.

area of study for the layman, he said, was field studies of birds: the periodicity in their appearance, the character of their breeding terrain, the materials, methods and duration of nest-building, the intervals between the eggs, the time of brooding, the growth of fledglings, etc.[297] He also promoted ecology, as a central element in secondary school education, believing that the »biological (ecological) and animal geographical parts of the subject« should be emphasized. »There is hardly«, he said:

> *»any part of zoology which arouses the interest of half-grown adolescents like the biological«.*[298]

In many respects Sven Ekman stands out as an all-round natural historian. He was closely attached to the aged Vilhelm Lilljeborg, and his popular writings are part and parcel of the old Linnean faunistic tradition. His work as a teacher and popularist evidently aimed at raising the pupils and laymen's interest in living animals in their natural environment. In all these respects he continued an old »proto-ecological« tradition. But he also tried to scientificize this tradition. He translated his naturalist observations into animal geographical, animal biological and eventually ecological problems. Thus his claim for ecology as an independent scientific specialty was a scientification of »proto-ecology« to respectable academic status. In that respect Sven Ekman stands out as the founding father of the nascent social order of animal ecology in Sweden.

1.4 The first claims for ecology: concluding remarks

In this chapter we have traced the fate of field studies of animals and plants in Sweden from the 18th century to the emergence of the first claims for ecology as an independent botanical and zoological specialty by the turn of the 20th century. Our point of departure was the »proto-ecological« activities of the social order of natural history, stretching from the Rudbeckians around the turn of the 18th century, through Carl von Linné to Elias Fries and Sven Nilsson in the mid-19th century.

The social order of natural history was continously broken up throughout the 19th century. The renaming of the secondary school subject of »natural history« to »biology« in 1905 symbolizes the end of natural history. The practical aspects of the social order of natural history were replaced by new knowledge monopolies. In the establishment of independent agencies, research organizations and proscribed educational training, scientists got their special mastery over certain areas of nature management authorized and symbolized by slogans such as »rational agriculture«, »rational forestry«, »rational fisheries«, etc. An agricultural science organization was already established around 1850, while a forestry science organization did not emerge until around 1900. Likewise freshwater and especially marine fishery administrations and science organizations were institutionalized around 1900.

297. Ibid.,p.30.
298. He did not want to cut in morphological and systematical details, however. His ecological strategy against the museum and laboratory zoologists was much more subtle: »It is not my intention to decrease the systematical and morphological detail material... It should only be organized in another way« (S.Ekman 1911,p.500), i.e., to a large extent the systematical knowledge should be arranged around biological (ecological) and animal geographic themes, instead of the other way around.

Field studies of animals and plants were an integral part of the drama of natural history, being essentially an outdoor narrative enterprise, integrating academic and amateur, economic and theoretical perspectives. But nobody conceived of field studies of animals and plants as a distinct realm of the learning. Linné's *Oeconomia Naturae* and *Politia Naturae*, for example, did not result in any claims for a new scientific social order.

Within the realms of the new social orders of forestry science and fishery science of the late 19th century the »proto-ecological« practices of the natural historians were continued to different extents. While scientists working on agricultural problems only made occasional »proto-ecological« investigations, forestry and marine fishery scientists focused on investigations of the relations between the organisms and their environment. In that respect the activities of 19th century scientists working on forestry and fishery problems were largely »proto-ecological« and provided an important background experience for the first claimants of ecology around the turn of the century. On the other hand, it is noteworthy that no 19th century scientist working in any of these applied fields was among the pioneer claimants of ecology as an independent science.

At the universities, field studies of animals and plants were gradually marginalized during the course of the later part of the 19th century. Collecting, describing and systematizing animal and plant species and the curation of museum collections superseded proper field studies. Studies of cell structures and comparative anatomical investigations followed. At the turn of the century the »new German« laboratory-oriented botany and zoology was the vogue, while observations of animals and plants under natural conditions held little esteem. The naturalist tradition inherent in the 18th century social order of natural history was marginalized from the centers of science.

But although field studies of animals and plants had a weak status among academic botanists and zoologists around 1900, it was nevertheless taken up anew, as a translation of naturalist studies into problems of animal geography and plant geography, or into problems of animal biology and plant biology. Field studies of plants were authorized in Uppsala, by the creation of a chair in »plant biology« in 1897, and at the new forestry research institute in Stockholm in 1902. This renewal of field studies at a time when the »new German« laboratory-oriented botany and zoology was becoming hegemonic, was partly a continuation of the natural history tradition, but also an application of the Darwinian viewpoint of the »new German« botany and zoology.

The first hesitant, but conscious claims for ecology during the first decade of the 1900s were not forwarded by amateurs, forestry scientists or fishery biologists, but by academic botanists and zoologists translating animal/plant geographical and animal/plant biological studies into the language of ecology. Adopting the signals from Eugenius Warming in Danmark, the new ecological rhetoric was quickly taken up by Swedish academic field botanists. Within a few years between 1900 and 1905 a number of graduate students, formerly considering themselves as plant biologists, began to claim their studies as ecological. Likewise a few zoologists claimed their field studies of animals as ecology. During the early 1900s a growing number of scientists trained as botanists and zoologists recognized ecology, or ecological plant geography, as an independent specialty of botany and zoology. By the First World War, ecology denoted a topic of discourse, and was recognized as a research specialty, particularly by academic botanists.

We have also demonstrated the existence of a duality in the claims for ecology. The claims for plant ecology emanated on the one hand from the field practices of the plant biologists and plant geographers in Uppsala. Rutger Sernander stands out as the main figure in this field approach to ecology. On the other hand, the claim for ecology forwarded by Henrik Hesselman emanated from the laboratory practices of the Stockholm botanists. Although hardly visible at the time, this duality between field claims for ecology and laboratory claims for ecology would be a significant theme in the growth of the social order of ecology throughout the 20th century.

However, the handful of men utilizing the new word in an academic context in the early 1900s did so in passing only. The rhetoric of ecology had not turned into a scientific drama, not to mention a drama of society at large. Compared to other contemporary botanical and zoological specialties, such as physiology and cytology, ecology was an insignificant one.[299] Ecological view-points, for example, were not taken up in the secondary school curricula.[300] Moreover, the emerging social order of ecology was not authorized. There were no ecological chairs or other positions, no specific ecological societies or any ecological journal, no training programs for undergraduate or graduate students. By 1914 the extent of ecologization was hardly visible as one of the pieces of the patchwork of Swedish scientific life.

299. Colliander's directory of articles published by *Vetenskapsakademien* between 1826 and 1917 (Colliander 1917) does not list the word »ecology«, i.e., Colliander seems not to have found ecology significant enough to warrant a separate entrance.
300. For example in Knut Bohlin's book on plant life conditions, written for the lower high school classes, plant sociology, plant physiology and biology were mentioned, but not ecology (Bohlin 1909).

2 The Great Polemic: sociologists versus experimentalists

This chapter treats the period from the first claims for ecology as an independent scientific specialty in the early 1900s to the establishment of a national ecological discourse around 1930.

Internationally, the term ecology spread quickly. Ecology had originally been demarcated as an distinct field of inquiry by the German zoologist Ernst Haeckel, but few claims for ecology as an independent science were made in Germany during the 1910s and the 1920s. One important exception was Richard Hesse's book *Tiergeographie auf ökologischer Grundlage*, published in 1924, which had considerable influence, not only in Germany but also in Scandinavia, as well as in the United States and Great Britain after its appearance in translation in 1937. Hesse's main contribution to the establishment of ecology (ecological animal geography) was to emphasize the importance of ecological factors in the geographical distribution of animals. Noticing that most animal geographers of the age were museum zoologists, Hesse advocated the need for a field study approach:

> *»Reiseausbeuten in Tierbälgen und Alkoholmaterial haben wir zunächst genug; was uns fehlt, sind Beobachtungen über den Zusammenhang zwischen Tier und Umwelt«.*[1]

However, his call and the few other claims for plant and animal ecology were not authorized in Germany. There were neither chairs nor departments of ecology, nor journals nor textbooks. On the other hand, studies of freshwater organisms and their environment were claimed as »Limnologie«, i.e., the scientific study of lakes and their organisms, and limnology was recognized as an independent scientific specialty, as exemplified by textbooks by Thienemann (*Limnologie* 1926) and Brehm (*Einführung in die Limnologie* 1930).[2] The ecological basis for silviculture was acknowledged in a work such as Dengler's *Waldbau auf ökologischer Grundlage* (1930).

No other European country provided much opportunity for the wider diffusion of claims for ecology; not even Warming's Denmark, where the development of the subject fell far behind international developments. Warming was professor in botany in Copenhagen 1885-1910, and the first ecological chair (in ecological botany) was not created until 1967.[3]

The recognition of ecology as an independent science was mainly an American and British enterprise. From the turn of the century individual ecologists began to enrol

1. Hesse 1924,p.vi
2. Also claimed as »Hydrobiologie«, as exemplified in Hentschel's *Grundzüge der Hydrobiologie* of 1923.
3. Slottved 1978.

students into groups and schools. The most successful ecological school was the group of plant ecologists gathered around and inspired by Frederic Clements in the United States.[4] Though his ideas were beginning to be questioned by the late 1920s, it is no exaggeration to say that in both countries, plant ecology was largely Clementsian through the 1910s and the 1920s. Arthur Tansley, the doyen of British plant ecology in the inter-war period, was of the opinion that:

> *»Clements's great book on Plant succession* /from 1916/... *probably influenced British ecology more than any other publication since the foundation works of Warming and Schimper«,*[5]

and that:

> *»it was his* /Clements's/ *broad and thorough treatment of the subject that made us realize the universal significance of succession as a key process involved in all the phenomena of the communities of vegetation«.*

As a consequence of this effective enrolment into the Clementsian actor network, two historians of British ecology conclude that

> *»plant ecologists were largely occupied with studies of the dynamics of vegetation, attempting to elucidate the causal relations between the plant community and its habitat«.*[6]

While it is true that Clements and other plant ecologists operated with a number of seminal key concepts, like »ecological factors«, »succession«, »community«, and »food relations«, it would be wrong to assume that the plant ecology of the 1910s and 1920s reflected any deeper theoretical maturity. The same applies to animal ecology. Allee *et al* point out that animal ecology studies in the 1920s were mostly focussed on feeding relations as the main factor behind the integration of animal communities, but add, with reference to American animal ecology, that the 1920s

> *»was an era when ideas were just starting to emerge into a broader ecological framework and when ecological research ceased being helter-skelter and started to acquire focus«.*[7]

For example, the American animal ecologist A.S.Pearse presented a heap of theoretically rather unrelated themes in his book *Animal ecology* published in 1926: physical and chemical factors, biological factors, succession, animals of the ocean, freshwater animals, terrestrial animals, relationships of animals to plants, relations of animals to color, interspecific relations and economic aspects of ecology.[8] Likewise Charles Elton's pioneer work *Animal Ecology* of 1927 introduced a rather broad concept of ecology: although he considered its essence to be the study of populations and communities, not so much studies of environmental factors, he nevertheless covered the distribution of animal communities, ecological succession, environmental factors, parasites, the number of animals and their variation, ecological methodology, and ecology and evolution.

Despite its theoretical immaturity, the new specialty institutionalized quickly. Scientific societies are important indicators of the degree of institutionalization. The *British Ecological Society* (in reality, almost all plant ecologists) was established in 1913, followed, two years later, by the *American Ecological Society*.

4. The evolution of »the Clementsian paradigm« in ecology has been documented in detail by Tobey 1981.
5. Tansley 1947,p.137.
6. Lowe and Duff 1981,p.143.
7. Allee *et al*, 1949,p.62.
8. Pearse 1926.

The appearance of textbooks is another indicator of the institutionalization of a new scientific social order. The first standard textbooks of ecology appeared simultaneously in Britain and the United States in the mid-1920s. Elton's and Pearse's books have already been mentioned. In the United States came McDougall's *Plant Ecology* in 1927, followed by Weaver's and Clements' *Plant Ecology* of 1929 and Chapman's *Animal Ecology* of 1931. These were mainly descriptive works. In 1929 Shelford published *Laboratory and Field Ecology*, a methodological manual. Shelford's work instituted a trend towards a division between, on the one hand, claims for an experimental and laboratory ecology (including, for example, experimental studies of insect populations, identifying and translating agricultural and forestry interests into the language of ecology), and, on the other, claims for descriptive field studies, seemingly identifying and translating a general naturalist interest. The ecological basis for silviculture was reflected in a textbook of 1928.[9]

In conclusion: the first three decades of the 20th century was a period of frequent claims for ecology as an independent science in Britain and the United States. McIntosh's summary of the state of ecology in the United States at that time would apply to British ecology also:

> *»By 1920 self-conscious ecology was reasonably well established and recognized as an academic discipline... although it was hardly known to the general public... It had also established its own professional society and publication outlet and had a number of notable spokesmen in the biological community«.*[10]

— — —

What about the Swedish ecologists? The first Swedish claimants of ecology around the turn of the century had been few and were isolated voices, and only one or two of them had made programmatic claims for ecology as an independent science. But during the 1910s, and especially in the 1920s, a number of scientists trained in botany and zoology began seriously to promote ecology as an autonomous realm of natural scientific knowledge. Among them were Thore Fries, Henrik Lundegårdh, Lars-Gunnar Romell, Elias Melin, Göte Turesson, and G. Einar Du Rietz. They were all men,[11] and they were all born around 1890. They matriculated at the universities around 1910 and formulated their claims for ecology during the 1910s. If the pioneers, Rutger Sernander, Henrik Hesselman and Sven Ekman, constitute a first generation, »the men of the 1910s« constitute a second generation of ecologists in Sweden, a generation responsible for the growing acceptance of ecology as an independent scientific social order.

In this chapter we will follow the work of the second generation of ecologists from around 1910 to the early 1930s. Since a major issue in Chapter 1 was the establishment of the social orders of botany and zoology and the marginalization of field studies of

9. Toumey 1928.
10. McIntosh 1977,p.356.
11. Actually no female scientists devoted themselves to ecology until the 1960s. On the other hand there were quite a few female zoologists at Stockholm university during the decades around the turn of the century (cf.1-2, note 108).

animals and plants, we will shortly discuss (in Section 2-1) the state of botany and zoology and the preconditions for new claims for field studies of animals and plants, and also examine another claim for an independent science of the relations between organisms and their environment, viz., limnology.

In the following sections (2-2 to 2-5) we shall turn to the actual claims for ecology. These were by no means identical, having different departures and led to different results. A look back to the first generation of ecologists might serve as a key to the analysis of the second generation's knowledge claims. Firstly, the distinction between botanists' and zoologists' claims for ecology still prevailed in the 1910s and 1920s; consequently we will treat the plant ecologists and the animal ecologists separately (Sections 2-2 to 2-4, and 2-5 respectively).

Secondly, while the first generation of ecologists referred to Warming, the second generation interpreted the problems of ecological plant geography in two different ways. Already Witte (cf.1-3) distinguished between two kinds of ecological studies – autecology and synecology, i.e., studies of adaptations of single species to the environment, and descriptions of vegetational formations and the external factors determining them, respectively. Hesselman, for example, advocated the translation of ecological plant geographical problems into experimental problems of ecological physiology. Others focused their interest on observed correlations between environmental factors, such as soil and climate, and the distribution and composition of the vegetation. As a consequence we shall also treat separately the plant »ecophysiologists« and the »synecologists« (Section 2-2 and 2-3 respectively).

Finally, we will consider the conflict between the »ecophysiologists« and the »synecologists«. From the 1920s these two approaches, rooted in quite different academic cultures, became involved in a polemical disagreement concerning the nature of ecology, and this factional struggle amongst the plant ecologists will be covered in Section 2-4.

2.1 Commencing specialization of botany and zoology: the case of limnology

As shown in Chapter 1, ecology began to be recognized as a botanical and zoological specialty around the turn of the century. To be accepted as an independent knowledge monopoly claim, however, required a weakening of the enrolment power of the social orders of botany and zoology.

The three last decades of the 19th century had been a great period of growth for the natural sciences at Swedish universities, including the authorization of botany and zoology as nationally prestigious scientific disciplines, symbolized by the magnificent museum buildings erected during the First World War (cf.1-2). The botany and zoology elites, readily identifiable through their membership of the sections for botany and zoology of *Kungl. Vetenskapsakademien*, supervised the demarcation of these disciplines, into the specialized fields of morphology, anatomy, phylogeny, embryology, cytology etc. (cf.1-2).

The enrolment power of botany and zoology began to show itself also in agricultural and forestry research and education. During the later part of the 19th century the natural history tradition gave way to a definition of animal and plant studies in terms of botany and zoology. The immediate successor to August Holmgren, the last natural historian in forestry research at *Skogsinstitutet*, was trained as a university botanist.[12] At *Skogshögskolan*, founded in 1914 by raising the academic standards of *Skogsinstitutet*, botany was represented by a chair in »forest botany with general botany« and zoology by a lecturer in »general and vertebrate zoology together with game- and fishery management«; likewise a department of forest entomology was created at *Statens skogsforskningsinstitut*.[13] The new botanical and zoological museums were proudly described as the core of *Skogshögskolans* new botanical and zoological departments.[14] Academically trained botanists and zoologists exercised the professional monopoly over *Centralanstalten*, and at *Lantbrukshögskolan*, founded in 1932, one of the chairs was designated »systematic botany and hereditary science«.[15]

The first generation of ecologists, i.e., »the men of the 1890s«, had been among the offspring of this huge build-up. Thus, »a man of the 1910s« entering the university with an interest in field studies of animals and plants had first to train in the laboratory with microscopes and microtomes in order to establish his professional identity as a botanist or a zoologist. His area of monopolized knowledge was fairly well delimited. He also had a fairly well defined group of clients, being trained primarily to become a secondary school teacher in biology.[16]

But while it is true that the generation considered here, »the men of the 1910s«, were trained under the auspices of the cytological and comparative anatomical elites, they nevertheless experienced the initial weakening of the enrolment power of the social orders of botany and zoology. The expansion of the natural sciences had come to an end in the

12. Lars-Albert Nilsson was lecturer at *Skogsinstitutet* after Holmgren between 1890 and 1906. Being trained as a botanist in Uppsala (cf.1-3), he substituted the natural historical attitude of Holmgren with the new Darwinian plant biology: »he gave the students new and widened points of view with regard to biological phenomena in nature. On the other hand he had no sense for museum collections« (Wahlgren 1917,p.144).
13. See Butovitsch 1952 and Skogshögskolan 1917; The department was headed by a professor from 1922. The first holder was Ivar Trägårdh (Jo 2/12 1921:27) (cf.2-5).
14. Skogshögskolan 1917.
15. This was the last chair at *Lantbrukshögskolan* to be denominated as zoological or botanical; the last new chair at *Skogshögskolan* having a zoological/botanical denomination was the chair in »forest zoology ...« created in the late 1940s (cf.3-4).
16. Until 1905 secondary school studies of animals and plants were conducted under the subject »natural history«, a legacy of the enrollment power of 18th and 19th century natural history. After 1905 studies of animals and plants were conducted under the subject »biology« (See *Kungl.Maj:t Stadga för Rikets allmänna läroverk* 18/2 1905).

universities.[17] The further build-up of the natural scientific and technical intelligentsia was, for several decades to come, channelled to the »applied« colleges and research institutes, such as *Skogshögskolan* and *Lantbrukshögskolan*, and the technical colleges: *Tekniska högskolan* and *Chalmers tekniska institut* (Chalmers' Technical Institute in Göteborg).[18] With regard to new tenured positions the expansion of the natural sciences had already ended by the turn of the century. The last significant contribution for many years to come was the universal promotion of *»extra ordinarie«* (associate) professors to full professors in 1909. However, this was a qualitative strengthening only. Until the post-Second World War expansion of the universities, only a small number of personal chairs in the natural sciences were created. Though mainly offered to particularly outstanding biologists,[19] they were only a tiny addition to the total number of available career positions. Likewise, the expansion of natural sciences in the secondary school system came to a stand still. During the two decades from 1910 to 1930 only five new positions as secondary school lecturers were added to the existing thirty,[20] another factor increasing the competition among university graduates. Having increased steadily since the 1870s, the number of university students in the natural sciences even began to fall and reached a very low level in the 1920s, before rising again.

Zoology and botany were no exceptions to the general stagnation of the natural sciences in the universities. In the academic year 1908/09 twenty students followed undergraduate courses in zoology in Uppsala; in Lund half as many. Ten years later, both figures were halved.[21] To this quantitative stagnation should be added the qualitative. With the exception of yet another zoology journal, *Acta zoologica*, founded by Nils Holmgren in Stockholm in 1920, the growth of the social orders of botany and zoology came to a stand-still in the first decades of the 20th century, and with one exception no new chairs in botany or zoology were created.[22] Likewise the zoologization and botanization of agriculture and forestry stagnated. The accelerating scientification and rationalization of the handling of living organisms in agriculture and forestry was increasingly

17. On the other hand, the faculties of arts expanded during the first decades of the century. A number of chairs in the languages, in history, geography, archeology, national economy and statistics etc., were created. *Göteborgs högskola* (the University College of Göteborg, founded in 1887-91 and financed by the municipality) which only had arts subjects on its programme, had its seven chairs almost tripled by 1926 (see L.Svensson 1980). Thus, the general scientific milieu at the universities during the 1910s and 1920s was dominated more by a classical ideal of cultivation of culture and a growing concern of knowledge for social control, than by natural scientific ideals.
18. E.g., *Tekniska högskolan* in Stockholm and *Chalmers tekniska institut* in Göteborg conspicuously increased the number of chairs in 1912 and 1920 respectively. A number of industrial branch research institutes were founded in the 1920s and 1930s; see e.g., G.Eriksson 1978, Stevrin 1978 and L.Svensson 1980 for details.
19. See below, note 25-26.
20. *Statskalendern* (the State Directory) 1910-1930.
21. During the autumn of 1918 three(!) students followed the zoology courses in Lund. A student from the 1920s recalls how the huge redbrick zoology building at Helgonavägen accommodated fewer students than staff members. The low number of students probably had some connection with the bad future prospects for secondary school lecturers: »it was so difficult to obtain a teacher's position that you had to go the whole way up to Kiruna /i.e., 2000 kms away/ for one week's temporary position only, and still you had to pay for your own ticket« (interview with NN 2/9 1981).
22. The one exception was the creation of botanical and zoological chairs at the university in Göteborg in the early 1960s; a few years later, however, the new chairs for animal and plant studies at the university in Umeå, founded in the 1960s, were designated as »ecological botany«, »physiological zoology« etc. (cf.4-4).

claimed as independent agricultural and forestry sciences.[23] Entirely new kinds of positions were created, e.g., »plant husbandry« and »silviculture«. This tendency towards the creation of new professional segments within the realms of agricultural and forestry research was manifested in the 1940s and 1950s when independent doctoral degrees were instituted in agricultural and forestry science respectively.[24]

A corollary to the stagnating enrolment power of the social orders of botany and zoology was a trend towards claiming new botanical and zoological specialties as independent sciences. Already in the 1890s the two botany chairs in Lund had been specialized into »systematical botany« and» physiological botany«.[25] The authorization of claims for scientific independency continued in the 1910s, 1920s and early 1930s when studies of fossil animals, of plant heredity, of freshwater organisms, and experimental animal studies, were endowed with personal chairs – in paleontology in Uppsala in 1910, in hereditary science in Lund in 1917, in limnology in Lund in 1928, and in experimental zoology and cell research in Stockholm in 1932.[26]

»Proto-ecological« studies had been authorized by the creation of the endowed chair in plant biology in Uppsala in 1897 (cf.1-3). With the endowment of the personal chair in limnology in 1928 »proto-ecological« studies were once again not only claimed but also authorized as an independent science. A closer look at the story behind the limnology chair in Lund might shed some light upon the general conditions for claiming »proto-ecological« studies as an independent scientific enterprise.

23. Fishery research positions have long been denominated as »fishery biologists« (and always filled with people trained as zoologists). Likewise the first position at *Södra Sveriges fiskeriförening*'s (the Fishery Association of Southern Sweden) laboratory in Aneboda (cf.below) was a »biologist«. The scientification of marine fisheries was also continued in terms of »fishery biology«. The scientists employed by *Svenska hydrografisk-biologiska kommissionen* (cf.1-3) were trained as zoologists but the research activities were continually defined as »fishery biology« (cf.2-5).
24. The first dissertation at *Lantbrukshögskolan* was submitted in 1942; the first at *Skogshögskolan* in 1957.
25. All scientists »specialize« their work, but not all »specialties« are claimed as such. E.g. Nilsson-Ehle specialized in studies of the chromosomes and plant heredity, but was nevertheless appointed to the chair in »botany« in Lund in 1914. A few years later he was awarded the personal chair in »hereditary science«. The reason for this »specialization« was political, not cognitive, that is, the creation of the chair in »hereditary science« was not primarily the consequence of cognitive specialization, but the outcome of a specific process of negotiations between claims and counterclaims for independency. In this particular case the botanical elite did not want Nilsson-Ehle on a »botany« chair (see ED 18/6 1915:19); although Nilsson-Ehle was as much (or little) »specialized« as any other »botanist« in Sweden at the time, he was nevertheless considered too »specialized« relative to the demarcation criteria set up by the botanical elite.
26. Karl Wiman, Hermann Nilsson-Ehle, Einar Naumann (cf. below) and John Runnström (cf.2-5) respectively. It is true that these emerging new claims for independency of certain research efforts only had marginal influence on the enrolment power of the zoology and botany networks, at least during the period considered in this chapter. Only Nilsson-Ehle in Lund enrolled sufficient numbers of students to have any possible effect on botany in Lund. Naumann's work on lakes did not begin to attract students until the very late 1920s; likewise Runnström's laboratory in Stockholm was not really established until the early 1930s. Hence these cuttings, though planted in a nourishable soil, did not tap any significant amounts of sap from the botany and zoology trees. But they nevertheless signalled the eventual end of the powerful and prestigious scientific social orders of botany and zoology.

The Aneboda Laboratory

The claim for limnology as an independent science cannot be considered in isolation from fishery research. As we recall from Chapter 1, proposals for an organized fishery research in Sweden had been submitted several times during the final three decades of the 19th century. The basis for a »rational fishery« organization under the supervision of *Lantbruksstyrelsen* was eventually laid around the turn of the century, albeit not comparable to its agriculture and forestry counterparts. Six regional fishery managers were appointed in 1904; the following year a fishery assistant, a fishery engineer, and yet another fishery scholarship were established. After the failure of the Finspång fishery institute in the 1890s, however, there was still lacking a national freshwater fishery investigation institute. With the newly established national institutes for agricultural research and forestry research as models, the question of a central fishery research institute was again placed on the political agenda towards the end of the First World War by Oscar Nordqvist (1858-1925), an exiled Finlandish zoologist and former inspector of fisheries in Finland.

Nordqvist was a main proponent for the institutionalization of fishery research in the 1910s and 1920s; succeeding Trybom as head of *Lantbruksstyrelsens fiskeribyrå* (the Fishery Division of the Agricultural Board) in 1913, and besides being a member of *Svenska hydrografisk-biologiska kommissionen* from 1915, he was the leading figure in Swedish fishery research administration for the following decade. On his arrival from Finland Nordqvist took a new, regional, initiative towards the institutionalization of freshwater fishery science in 1906; procuring economic support from a number of *hushållningssällskap* in southern Sweden, and later also securing state support, he organized *Södra Sveriges fiskeriförening* (the Fishery Association of Southern Sweden) and a laboratory at Aneboda in the southern part of the province of Småland in 1907/08. Being a scientifically trained zoologist, Nordqvist spoke for a rational fishery management on a secure scientific basis. The aim was:

> *»to develop rational methods of culture for freshwater and pond culture through the establishment of planned fishing and field culture trials in leased or owned waters«.*[27]

The Aneboda laboratory would come to function as a brood base for the new science of limnology.

With regard to the scientific content behind the new fishery research policy initiatives in the 1910s and 1920s, however, Nordqvist relied heavily on his junior assistant, Einar Naumann (1891-1934).[28] Naumann received his first training in lake investigations from Nordqvist at the Aneboda laboratory in the summer of 1910, when he was only 19 years old. Making use of bottom scrapers and plankton nets as a starting point to his professional career, he continued to improve his knowledge of all possible aspects of lakes, and published his first papers while attending undergraduate courses in Lund. Already by 1913 Nordqvist was employing his young protegé in a new position as »biologist« at Aneboda. In his capacity as an influential civil servant at *Lantbruksstyrelsen*, Nordqvist also employed the young Naumann to pursue investigations of »plankton biological and bottom conditions in certain tarns« in the Kloten crown forest park. The result of this study together with those from another on »the formation biology of recent Lake Tåkern

27. Södra Sveriges Fiskeriförening 1906-1955 .
28. For biographical details on Naumann, see ED 3/6 1927:22, G.Lundqvist 1934, Wallengren 1935, Gislén 1935, and Thienemann 1938.

ooze« (made on behalf of *Sveriges geologiska undersökning*), formed the basis of Naumann's dissertation on plankton and ooze formation submitted in 1917. Its aim was:

> *»primarily to shed light on the relation between the nature of the surroundings and the biology of plankton on the one hand, and on the other hand the relation between the biology of plankton and the bottom sediments«.*[29]

At that time Naumann was fairly closely involved in Nordqvist's fishery research programme. This was a version of the kind of studies made by Sven Ekman in Lake Vättern and by the *Svenska hydrografisk-biologiska kommissionen* (cf.1-3), i.e., making correlations between bottom conditions, the bottom fauna and the fish. It was also the kind of investigations pursued by another of Nordqvist's protegés, Gunnar Alm (1891-1962), who started his professional career at Aneboda in the summer of 1910:

> *»Alternating with microscopic studies we* /Alm and Naumann/ *made excursions in the vicinities, tried different bottom scrapers and plankton nets, and gathered our first personal experiences regarding the animal- and plant world of the waters«.*[30]

Alm[31] was trained as a zoologist in Uppsala in the 1910s, and wrote a doctoral dissertation in 1915, which although being more in accordance with Uppsala zoology tradition,[32] nevertheless served as an admission ticket to the fishery administration. In 1914-16 he transferred his experiences from Aneboda to a series of faunistic and fishery biological investigations in Central Swedish lakes, e.g., in Lake Hjälmaren[33] where he studied the character of shores and bottoms, plants and animals, the occurrence and biology of fish, and discussed the relation between organisms and their environment, including nutrient production and fish growth. Although restricting himself to qualitative findings like

> *»mehrere der sublitoral vorkommenden Formen sind in hohem Grade von den edaphischen Faktoren beeinflusst, weswegen sie nur an beschränkten Lokalen vorkommen«,*[34]

already these early works were »proto-ecological«. Holding a fishery scholarship for some years, he continued on a large investigation of the bottom fauna and the biology of fish of the Lake Yxtasjön, now making quantitative studies of the bottom fauna and the relation between bottom fauna and the fish production.[35] However, though continuing this »proto-ecological« approach to the study of lakes and their organisms, he never claimed it as ecology; since he did not contribute to the emerging social order of ecology[36] we will not count him as a second generation ecologist.

29. Naumann 1917,p.3.
30. Alm 1956,p.52.
31. For biographical details on Alm, see Olofsson 1962; his bibliography is printed separately in *Institute of Freshwater Research, Report* nr 44, 1962.
32. The dissertation (Alm 1915) was a systematical treatise of Swedish fresh water ostracodes, including some notes on collection sites, and the reproduction cycle (Ch.3: »Biologie der Süsswasser-Ostracoden«), thus within the theme laid down by Ekman in his dissertation of 1904.
33. Alm 1916 and 1917. His work was supported by *Vetenskapsakademien* and the local Uppsala students' natural science society.
34. Alm 1916,p.39.
35. Alm 1922b; his similar investigations of the bottom fauna of the Lake Mälaren (Alm 1927) was a reiteration of this paper.
36. In an article on »the organic existence conditions of fish« Alm discusses the relation between »the different animal- and plant communities of the waters« and »these as organic factors being decisive for the existence of fish« (Alm 1922a,p.162). His insight into »the organic existence conditions« is an echo of Sven Ekman's discernment of »existence ecology« put forward in his vast treatise on the Scandinavian animal immigration history published the same year (S.Ekman 1922, cf.2-5). However, while Ekman claimed such studies as ecological, Alm did not.

Claiming a new fishery administration and a new science - limnology

In 1915 the Jönköping County *hushållningssällskap* sent the *Riksdag* an appeal for »a state fishery research institute«, emphasizing that the fisheries were now on the brink of a new era, the culture stage. Attempts to »lay lakes under culture« had to be founded upon a combination of scientific research and practical trials.[37] The Riksdag commissioned Nordqvist to deal with the question, and after a couple of years he returned with a proposal for a »central fishery board«, i.e., a state office of his own, separate from *Lantbruksstyrelsen*.[38] The rest of *Lantbruksstyrelsen* did not accept such a solution, but after another couple of years of commissioned work, Nordqvist succeeded in enlisting the rest of the Board to support the creation of two central fishery research institutes, one for marine fishery with a programme very similar to *Svenska hydrografisk-biologiska kommissionen*,[39] the other for freshwater fishery. The latter should:

> *»primarily have as its task to lay the foundation for a rational fishery management, by means of investigations concerning the life of fish, and the nature of the surrounding environment«.*[40]

The task included studies of »general metabolism of fresh waters and the future possibilities for regulating it«,[41] that is, a clear-cut »proto-ecological« research programme.

The proposed tasks of the fresh-water fishery institute deviated considerably from that of its marine counterpart, and also from what had hitherto been proposed in terms of lake and fishery investigations. The only conceivable explanation for these programmatic differences is that Nordqvist was now in turn being enrolled into his protegé's, Einar Naumann's, ever more independent scientific development. How was that?

While Nordqvist devoted himself to fishery research policy questions, Naumann extended his academic contacts. Already as a doctoral student, he had complemented his employment at the Aneboda laboratory with jobs as a teaching assistant at the Department of Zoology and at the Department of Botany in Lund. Lund botany in those days was totally dominated by Murbeck's systematical and cytological approach (cf.1-2). But in 1915 (when Naumann was still a graduate student) an experimental physiologist from Stockholm, Henrik Lundegårdh, came to hold a *docent* scholarship in Lund. As we shall see below (2-3), between 1915 and 1917 Lundegårdh was establishing his subsequently world-famous ecological station on the island of Hallands Väderö, and was beginning his decade-long campaign for an experimental ecophysiological approach to plant geography. Lundegårdh's presence seems to have been of the utmost importance for Nau-

37. The appeal was signed 30/9 1915 (see Nordqvist 1918,pp.187-215); note that it was the Jönköping County *hushållningssällskap* that had supported Ekman's bottom fauna investigations in the Lake Vättern some years before (cf.1-3).
38. Nordqvist 1918,pp.187-215.
39. It should be located in Göteborg, and include a »biological« and a »technical« division. The tasks of the »biological« division should be, thought Nordqvist, to investigate: »partly the life, growth, food, reproduction, migration etc. of fish and other marine organisms, partly also the nature of the surrounding environment, such as water temperature, percentage of oxygen and other gases« (Betänkande II:1, 1920,p.186), evidently a research programme very similar to that of *Svenska hydrografisk-biologiska kommissionen*.
40. Betänkande II:1,1920,p.178.
41. Or as they said: »investigations concerning the natural character of water, its level of temperature, gases and minerals, and the influence of these factors on the animal- and plant life« (Ibid).

mann's later development.[42] It was evidently Lundegårdh's ideas which were reproduced in Naumann's dissertation section on an ecological lake geography – remarks which have later been called the first step towards freshwater ecology, and the so called ecological lake type system. Dividing the investigated lakes into two types, the oligotrophic (poor in nutrients) and the eutrophic (rich in nutrients), he wrote:

> *»In this context – as in several places earlier – we have emphasized that the fundamental distinction between the lakes investigated is by no means simply geographical, but rather primarily must be dependent on ecological-nutritional biological factors«.*[43]

After his dissertation Naumann was employed as an assistant teacher in plant physiology by Lundegårdh, then holding the vacant chair in plant physiology in 1918-1920. During these three close years together with Lundegårdh, Naumann developed his physiological view of lakes, summarized in the notion of their »metabolism«. In a 1918 booklet entitled *Sötvattnets produktionsbiologi* (Freshwater production biology), he outlined »theoretical guiding principles for rational water culture«, employing physiological analogues. The lake has a »total metabolism« and a »general circulation«; he distinguishes between »consumers« and »producers«; and without suggesting any particular terminology, he constructed the idea of nutrition pyramids and food chains, suggesting that the material losses upwards in a food chain could be investigated and quantified by experimental means.[44]

Thus, Naumann, unlike Alm, disengaged himself from Nordqvist's influence, and was instead enrolled in the experimental ecophysiological rhetoric of Lundegårdh, thereby in turn switching Nordqvist's orientation. One could imagine how the two men saw themselves as director general of a future state fishery board and scientific director of a national fishery research institute, respectively.

However, Naumann once again changed his scientific rhetoric, and made a claim for freshwater research as an independent science, with its own theory and its own methodology. During the years 1918-1919 he began to employ the word »limnology«[45] for:

42. The close contact between the two men is shown *inter alia* by the fact that Naumann witnessed Lundegårdh's application papers for the chair in physiological botany, and that Lundegårdh employed Naumann as his assistant (see below). It is remarkable that the close contact between the two men has been totally neglected by Naumann's contemporary biographers. Henrik Lundegårdh was not *persona grata* in Swedish botanical circles; his unpopularity may have made these biographers unwilling to credit his importance for the ecological orientation of the young Naumann.
43. Naumann 1917,pp.116-17.
44. His discussion on this point is worth quoting *in extenso*: »The algae are not eaten directly by the fish, but only after being 'refined' by lower organisms - the important natural food. It is clear that under such circumstances, a considerable large share of the initial production must get lost, before eventually - through the intermediary hosts - being transformed into fish meat production. Curiously enough, however, one does not find any detailed treatment of this important question in the literature. KNAUTHE /Die Karpfenzucht, Neudamm 1901 and Das Süsswasser, Neudamm 1907/, however, has on occasions given it a cursory treatment, coming to the surprising result that during the course of metabolism so large amounts get 'lost' that only 1 kg of fish meat is produced from 100 kg algal dry weight. However, this estimate should be accepted with the utmost reservation and seems hardly acceptable. Therefore to clarify this important question new, purely experimental, investigations are needed« (Naumann 1918,pp.105-06). Naumann, like so many others, was probably unaware of Karl Semper's findings almost forty years earlier that »the proportion of the whole mass of plants produced by the soil is to the animals which can subsist on them... as ten to one« (Semper 1881,p.52).
45. The term »limnology« was originally proposed by the Swiss Forel in the 1890s, but was not widely spread at the time.

> *»a wholly new and independent science, which attacks its problems from such specific assumptions that there should be no doubt as to its independent right of existence among the older natural scientific disciplines«.*[46]

Together with August Thienemann at the Plön laboratory in northern Germany he proposed the foundation of an international association of theoretical and applied limnology.[47]

To be sure, he sometimes considered this new science of limnology to be primarily ecology, both autecology and synecology,[48] but he never made any serious claims for ecology. Whether to denote his knowledge monopoly claim as »freshwater natural science«, »freshwater ecology« or »limnology« was seemingly a subordinate question for him. What was of primary importance, however, was the scientific independence of the studies of lakes and their organisms. Thus, while Oscar Nordqvist made a *political* claim for freshwater research as a new administrative field, Naumann claimed a *science*, an independent theory and a specific methodology.[49]

While still keeping in touch with both fishery research and the ecophysiological view of the lake, Naumann now began to develop a new set of core problems for limnology. He continued with the idea of lake-types already touched upon in the dissertation, identifying the classification of lakes as the central problem in limnology,[50] and spent most of the 1920s developing a highly sophisticated lake-type system. His article »einige Grundlinien der regionalen Limnologie« of 1921 became a programmatic text for limnology in a way comparable to Du Rietz's program for plant sociology (cf.2-2). Actually Naumann's regional limnological research programme was very close to the plant sociological programme of the Uppsala school. They were both internationally renowned for their classificatory contributions. In terms of terminological inventiveness both went to extremes. An eye witness tells about Naumann's:

> *»When I cleaned up in the lab and in waste baskets, I sometimes found pieces of paper and envelopes with different drafts to new words and concepts. For example, once I read 'Meioeutrof – may be a useful cliché'«.*[51]

Naumann's lake-type doctrines differed from Du Rietz's sociological doctrines in one important respect, however. While Du Rietz programmatically avoided delimiting the plant communities with reference to the site (the so called »inductivist« approach), Naumann was a »deductivist«: lake types should be delimited on ecological grounds, i.e., trophic characters. In this respect Naumann was closer to the early ecological plant geographers, and to some extent to his contemporary experimental ecophysiologists, than

46. Naumann 1919.
47. For a history of *Internationale Vereinigung für theoretische und angewandte Limnologie*, see Rodhe 1974.
48. Naumann and Thienemann 1922.
49. From the time horizon of the period considered here Alm describes the construction of limnology in the following way: »Until Ekman's first appearance in 1905 you can say that most Swedish scientists working with freshwater problems were either zoologists or botanists, geologists or chemists, even though particularly Trybom and O.Nordqvist had paid attention to the general natural character of lakes in their investigations made from a fishery point of view. However, it was only with Ekman's, v.Hofsten's and Naumann's works that the new line of research called *limnology*... was introduced« (Alm 1931)
50. The traditional approach to lake classification was founded on purely bio-geographical notions, e.g., a classification into subalpine or baltic lake types. This was the approach taken, for example, by the Danish freshwater scientist Wesenberg-Lund whom Naumann polemizied against (they later developed a harsh hostility towards each other). Naumann's, and simultaneously Thienemann's in Germany, most important contribution to early limnology was that they lay an ecological foundation for the classification of lake types. Naumann summarized his lake type classification in »Grundzüge der regionalen Limnologie« (1932).
51. Rodhe 1973,p.27.

to Du Rietz and the Uppsala school. This is exemplified by the work of one of his students in the late 1920s. Karl Lang (1901-1976), a primary school teacher who began to study zoology in the 1920s, was associated with Naumann at the Aneboda laboratory. In 1928 he began »faunistisch-ökologische Untersuchungen« in order to clarify the relation between environmental factors and the depth distribution of the fauna in different lake types, i.e.:

> *»...auf Grund ökologischer Untersuchungen von den Faktoren Kenntnis zu bekommen, die die Verbreitung der verschiedenen Tiere bedingen«.*[52]

Hence Lang, seemingly heavily influenced by Naumann, took an ecological animal geographic problem as his point of departure. Actually, Lang's dissertation was the first large scale quantitative ecological animal geographic study since Ekman's Lake Vättern bottom investigations 20 years earlier (cf.1-3). Lang continued studies of lake fauna and environmental factors for some years in the early 1930s, gave a few lectures in Lund, but soon turned to systematics. Making a career as a scientific officer and later professor in invertebrate zoology at *Riksmuséet*, he left studies of animal-environment relations altogether, and never made any further claims for ecology.[53]

As pointed out above, Nordqvist's and Naumann's claims presupposed each other. Naumann's formulation of Nordqvist's fishery research program was reciprocated by Nordqvist's proposal that Naumann should be appointed to a personal chair in limnology. This proposal gained support from a number of influential Swedish scientists and international freshwater scientists.[54] By now, Naumann was extending his international contacts, particularly through the international association for limnological research. The Aneboda laboratory began to be a gathering place for domestic and foreign visitors.

For different reasons, however, this mutual aid project came to a temporary end. Nordqvist did not succeed in enlisting the support of the *Riksdag* for his proposal.[55] Likewise the proposal for a personal chair for Naumann came to nothing, mainly due to disagreement among Swedish zoologists and botanists over which university should get the chair.[56] Thus, by the end of 1924, the whole joint enterprise seemed to be doomed.

However, during the latter part of the 1920s things changed, after a number of interventions by private donors, a press campaign, and several *Riksdag* bills. First, Einar Naumann lost the competition for the chair in systematical botany at Lund in 1925-27,[57] but this initiated an extensive public debate, in which he deployed all his rhetorical powers

52. Lang 1931,p.3.
53. Cf. his competition with Per Brinck in 1957-58 (4-3).
54. Kolkwitz in Berlin and Thienemann in Plön wrote very favourable appeals for a personal chair for Naumann (ED 3/6 1927:22).
55. His plans had been worked out under conservative governments, which very probably would have been favourable inclined (since only wealthy land owners could afford the fishery management methods of the kind proposed by Nordqvist), but they were rejected by Branting's social democratic government, referring to the severe state financial situation (*Proposition* nr 1,*IX*,p.120,1923). A year later Nordqvist died.
56. The Uppsala zoology professor Axel Wirén wanted to locate the chair to Uppsala.
57. The chair was still denominated »systematical« botany according to *Kungl.brev* 18/3 1895 (cf.1-2). Besides Naumann, Thore Fries and Einar Du Rietz from Uppsala and Göte Turesson from Lund applied for the chair. None of them had made any substantial contributions to systematics or morphology, and hence the assessment committee (the resigning Murbeck, Svedelius from Uppsala, and the Dane Ostenfeld) evaluated the applicants according to the amount of systematical work in their total scientific productions. This comparison went in Fries' favour, while disfavouring Naumann (ED 3/6 1927:22).

to claim limnology as an urgent national affair of great importance for the future of fisheries and freshwater management.[58] Again, he received substantial support for his case. While several *Riksdag* bills pleaded for measures against the degradation of lakes and streams and some for organized fishery research,[59] Lund university applied for a personal chair for Naumann. Eventually, but only after a private donation had been made for the laboratory buildings did Naumann get his personal chair, in limnology, in 1929.[60]

A few years later Nordqvist's plans for a fishery institute eventually materialized as well, largely by the efforts of Gunnar Alm. After having taken over *Lantbruksstyrelsens fiskeribyrå*, Alm was commissioned to find a cheap solution to the *Riksdag* appeals for a fishery institute.

> *»It seems to me as if it is about time to bring about a solution to the question of organizing a rational investigation- and experimental activity in the area of fresh water fishery«,*

the Minister of Agriculture said in March 1930.[61]

Eventually, and after the private *Wallenbergfonden* had proffered 300,000 SEK to set up a »fully up-to-date central laboratory«, *Undersöknings- och försöksanstalten för sötvattensfisket* (the Investigation Institut for Freshwater Fishery, later *Sötvattenslaboratoriet*, the Freshwater Laboratory) was established in 1932, with two posts of scientific officers.[62]

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Einar Naumann claimed limnology as an independent science of the lake and its organisms. Although sometimes considering limnology to be primarily an ecological science, he never publicly claimed it as such. Hence, on the analytical view taken here, that is, to restrict the domain of ecology to discursively conscious investigations of the relations between animals, plants and the environment, Naumann's limnological laboratory, and the subsequent authorization of limnology in Sweden (cf.3-2, 3-3 and 4-2) cannot be

58. See, e.g., *Sydsvenska Dagbladet Snällposten* 13/10 1926-22/2 1927, *Lunds Dagblad* 13/10 1926-5/2 1927, *Dagens Nyheter* 14/10-21/10 1926, *Arbetet* 15/10 1926-4/2 1927, etc.
59. See the following *motioner*: nr II:57 (1925) on overfishing in the seas and the consequences of lake impoundments and water power constructions; nr II:137 (1926) which referred to the »urgent need for all-round investigations concerning the influence of industrial plants and lake impoundments on the fisheries«; nr I:69 and II:115 (both 1927) which pointed to the importance of raising the yield of freshwater fisheries and to restrict industrial damage on lakes and streams. It is interesting to note that the two latter bills were rather moderate in their demands for a research institute; the bill writers emphasized that the work of the biologists ought to »go hand in hand with practical experiments«, and not be so protracted as to set aside immediate practical needs. Thus, some *Riksdag* members were enrolled in the scientification process, others stressed the need for practical measures.
60. *Proposition* nr 31, 25/1 1929.
61. *Proposition* nr 264, 14/3 1930,p.43.
62. See addresses from *Lantbruksstyrelsen* of 29/10 1929 and 21/11 1929 and *proposition* nr 264, 14/3 1930. The two first positions at the laboratory were filled with the Lund zoologist Sten Vallin and the Uppsala zoologist Orvar Nybelin. A few years later Nybelin succeeded Jägerskiöld as museum director in Göteborg and Vallin was appointed *Fisketillsynsmyndighet* (Inspector of the Fisheries). They were replaced by another couple of zoologists, both from Lund: Sven Runnström, who had written his dissertation in 1925, and Lars Brundin, who had written the first insect synecological dissertation (cf.3-4).

considered a part of the ecologization process of Sweden. Given the history of Naumann's career, however, nothing in principle speaks against the possibility that he could have stopped half-way in his translation of fishery investigations to limnology, that is, at ecology, rather than going on to limnology. On the other hand, given the specific circumstances of the time, that is, the possibility to impute an interest in limnology on behalf of the emerging fishery research administration, the stubborn refusal of the botanists to accept his work as »botanical«, and his locally and historically determined co-operation with Nordqvist, his claim for limnology seems the more logical outcome. At that time, ecology was a claim mainly made by people trained as botanists and utilizing physiologial or descriptive geographical methods. To claim his activities and conceptual constructs as ecology would probably have diminished his chances of establishing himself as an independent scientific actor.

Thus, the only serious attempt during the first decades of the 20th century to translate fishery research into a new scientific claim was in effect a dismissal of ecology as an independent scientific social order. With regard to scientific activities, however, the authorization of limnology under the charismatic leadership of Naumann turned out to have profound indirect effects for later enrolment to the social order of ecology. Already by the late 1920s Naumann's laboratory at Aneboda had become a popular place for visitors, and as we shall see below (3-2, 4-2), his successors, both in Lund and later in Uppsala, drew large numbers of students to their courses. This educational work was part of the naturalist revival of the inter-war period. Many fourth generation ecologists received their first scientific training in limnology, and only later translated it into the language of ecology.

2.2 The Uppsala school of plant sociology

Thore Fries, »the gang«, and a claim for synecology

Rutger Sernander's all-embracing approach to vegetation studies, the Swedish plant communities seminar in Uppsala, attracted students in great numbers but it was also criticized by them. Sernander's vaguely ecological plant geographical approach to the history of vegetation was the point of departure for a new, and more precise, claim for ecology.

We recall that in his early days Sernander had been a student of Ragnar Hult, the Finlandish botanist, who had taught him always to distinguish carefully between vegetational and site analyses. The latter was not allowed to influence the former. Sernander stuck to the rule, but never went out of his way to advertise it. He paid a lot of attention to vegetational analysis, e.g., turning Hult's analytical method into the so-called Hult-Sernander cover scale. But he always considered vegetational analysis (or ecology, or soil science etc.) an *aid*, never a goal in itself.

A group of younger graduate scientists attending his seminar, however, went back to the roots, revived Hult's principle, and began to accentuate vegetational analysis as a problem in itself. The survey seminars were gradually replaced by more and more elaborated methodological discussions, which »generated a lot of debate«, as one of the participants euphemistically stated.[63] The new generation seemingly wanted to raise the status of their science. They were probably too well aware of the fact that Sernander had been rejected by the botanical elite in an earlier professorial competition,[64] and that his seminar and excursions, though attractive, were neither particularly academic nor rigorously scientific in the prevalent meaning.

The leader of this quest for scientific status was one of Sernander's first doctoral students, viz., Thore Fries (1886-1930), the son of the former Uppsala botany professor Thore Magnus Fries, and hence the third generation of the well-known Fries-family of Swedish botanists. His biographer maintains that Fries' understanding of the relation between vegetation and site began while commissioned by *Civildepartementet* (the Ministry for Civil Service Affairs) to make an investigation of the prospects for reindeer-grazing in Lapland.[65] After four years of work on a vegetation monography of northern Torne Lapland, he submitted his dissertation in 1913.

Fries' dissertation was a practical critique of the prevailing assumptions among European plant geographers, including some Swedish practitioners,[66] i.e., the correspondence between site and vegetation, and the presumption of uniform environmental conditions within an association. In opposition to this, Fries stated that:

> *»die Vegetationstypen in der Natur sozusagen direkte und ohne weiteres greifbare Fakta sind, während der Standort und alle damit verknüpften Faktoren gerade die unbekannten Grössen sind«.*[67]

This was the so called »inductive method«. Fries employed the term »synecology« for studies of the mutual relations between the plant communities and the establishment of the exact relations between the plant species in the plant communities. When employed as director of *Abisko naturvetenskapliga station* (the Abisko Natural Science Field Station) in northern Lapland in 1917,[68] he developed his own ecological method, the so called »synecological line survey method«, and by means of it he refined his investigations of the correlation between snow and vegetational conditions in mountain areas. In 1918 he addressed the Uppsala seminar, emphasizing that the »synecological line survey method« made it possible to

> *»intimately investigate both the single species and the vegetation as a whole, by pure ecological means«.*[69]

»By pure ecological means« he meant the inductive and comparative method – certainly not ecology in Warming's sense. One was not allowed to introduce any *a priori* hypotheses, such as Warming's hypothesis of Darwinian adaptation.

63. Almquist 1929,p.vi.
64. In the professorial competition in Uppsala in 1902 (ED 12/8 1902:4) Sernander had been dismissed as an observational field botanist (cf.1-3).
65. For biographical details on Fries, see ED 3/6 1927:22 and Du Rietz 1931.
66. A clear-cut example was, of course, Ljungqvist's dissertation *Mästermyr* of 1914 (cf.1-3).
67. Th.C.E.Fries 1913,p.49.
68. Originally created as *Vassijauri naturvetenskapliga station* in 1904/05 and financed by *Vetenskapsakademien*, it was moved to Abisko in 1912.
69. Quoted from Sernander 1929,p.4.

To be sure, Fries did not reject environmental analysis. But he thought that an unprejudiced inductive analysis of vegetation was a prerequisite, that is, first map the vegetation units, then establish the correlation between vegetation and site. His dissertation, for example, contained the most detailed analysis of plant communities, followed by a discussion of the influence of snow distribution on the distribution of plant communities. By means of the exact »inductive« method, synecology no longer had to:

> *»be the last in the row of branches of botany; on the contrary it will probably soon be able to occupy an eminent position as one of its foremost«.*[70]

Thus, Fries wanted to raise the scientific level of the prevailing ecological plant geography, not get rid of ecological questions.

The inductive approach soon won adherents in Uppsala. Thore Fries is said to have had

> *»an unusual capacity of carrying his fellows and students with him and making them enthusiastic in the struggle for common aims«.*[71]

In other words, he was successful in enrolling younger colleagues for his cause. Among his keenest followers were Hugo Osvald (1892-1970), Åke Tengwall (1892) and Einar Du Rietz (1895-1967) – together they constituted »the gang«.

»The gang's« main adversary at Sernander's seminar was an outsider, Gunnar Samuelsson (1884-1944), who had written a cytological dissertation for Juel in Uppsala.[72] Having also a spare time floristic interest, Samuelsson had approached the problems of plant geography, focusing on the same investigation object as Fries: snow distribution and tree limits in the mountain areas. But while Fries insisted that plant communities were natural units, which could be revealed as direct facts of nature by means of the »inductive« approach, Samuelsson again took up and defended the classical ecological plant geographical argument (even though he too refuted Warming's approach). Fries' plant communities, he said, were »artificial groupings«, precisely because of the method employed. Besides, he maintained, since we cannot deny the causal relation between vegetation and site:

> *»One of the leading principles of* /modern synecology/ *is the idea, that the vegetation is mainly a product of the ecological character of the site«,*[73]

there was nothing inherently unsound in letting the site determine the grouping of plant communities. Precisely because of the »mutual interaction« between vegetation and site, a »natural« classification of plant communities demanded knowledge of the ecological relation between the two.[74]

Einar Du Rietz and the claim for plant sociology

»The gang«, striving for »exactitude« in vegetation analysis could not accept Samuelsson's opinion that the plant communities were »artificial«, and hence Samuelsson's clear and stringent arguments created a stir. As a result of these prolonged seminar discussions the principles and methods of what later would be known as the Uppsala school emerged.

70. Th.C.E.Fries 1919,p.4.
71. Du Rietz 1931,p.451.
72. Biographical data on Samuelsson are found in Skottsberg 1944.
73. G.Samuelsson 1916,p.401.
74. Cf. G.Samuelsson 1916.

Except for the inductive and empirical principle the school emphasized that an association can be characterized by the occurrence of a group of so-called constant species. They could demonstrate[75] that both the number of these constant species and the total number of species increase with increasing size of the sample area up to a limit, which they called »the minimum area«. Although accepting that species occurred together because they share a common reaction to the site (but also because of mutual interaction, i.e., so called secondary site factors), they saw no need to demand uniform environmental conditions within an association. As a consequence the Uppsala school gave up the claim for synecology in practice. Instead they made an independent claim for a science devoted to the »inductivist« analysis of »natural« plant communities. This new science they called plant sociology.

The leading spokesman for the new claim for plant sociology was G. Einar Du Rietz,[76] an ardent amateur botanist and son of a large Stockholm businessman. He moved to Uppsala in 1912 to join Sernander's seminar, and soon became one of its most active participants. Fries' dissertation seems to have been a moment of salvation for the young Du Rietz:

> *»Perhaps one need to have experienced this dissertation as a young student to understand fully what a mighty influence it came to exert upon the newer Swedish plant geography during the decades to follow. For my own part I will probably never forget that day, when first finding Thore Fries' dissertation at my students' society and for a couple of days totally forgetting both chemical exercises and many other things in order to finish reading it«.*[77]

What impressed Du Rietz most was the »inductive« principle:

> *»Only the composition of the vegetation, not in the least way the character of the site, may influence the division of associations«.*[78]

which stood in contrast to Samuelsson's less principled opinion that

> *»a careful community description... on the whole should precede the site analysis«.*[79]

Four years later, only 26 years old, Du Rietz submitted the most theoretically conscious dissertation in the history of Swedish vegetation research up to *this* day. *Zur methodologischen Grundlage der modernen Pflanzensoziologie* (1921) was not only a brilliant dissertation;[80] it was also Du Rietz's claim to a new science. Accordingly he devoted a lot of space to tracing the historical roots of plant geography and vegetation analysis, and, in true systematical Haeckelian fashion, he discussed the place of plant sociology among the branches of botany.

Du Rietz's claim for plant sociology was extremely successful. In 1923, he and his supporters founded *Växtsociologiska sällskapet* (the Plant Sociological Society) and began to issue a series of publications (from 1929 *Acta Phytogeographica Suecica*),[81] and gradually they began to make themselves known internationally. Du Rietz advocated his methodology in a series of conceptual papers, and in 1925 he headed the 4th International

75. »The gang's« ideas were first set forth in Du Rietz *et.al.* 1920.
76. There is nothing but a short obituary on Du Rietz (Osvald 1967); his correspondence is still in private hands. What follows on Du Rietz is taken from his own articles, some of the interviews, yearly department reports, and ED 9/2 1934:44.
77. Du Rietz 1931,p.443.
78. Du Rietz 1917,p.54.
79. Kylin and Samuelsson 1918,p.402.
80. His empirical material for the dissertation consisted of a sociological analysis of lichen communities.
81. Published 1923-26 under the title *Svenska Växtgeografiska Sällskapets Handlingar.*

Plant Geographic Excursion through Norway and Sweden. The result was an international break-through for the Uppsala school, which now became established as one of the major international schools of vegetation analysis alongside the Zürich-Montpellier school headed by Braun-Blanquet, and the Clementsian system in the United States and Great Britain.[82]

Du Rietz's enrolment power was impressive. Most of his contemporaries and students (»the men of the 1930s«, cf.3-2) followed his methodology until the 1950s. The 1920s and the 1930s were the golden decades of the plant sociological Uppsala school. All 15 doctoral dissertations produced at the department after Fries' in 1913 up to 1940 took as their main theme either floristic plant geographical or plant sociological problems.[83] The most significant dissertation alongside Du Rietz's was that of Hugo Osvald, *Die Vegetation des Hochmoores Komosse*, of 1923, a pure sociologial analysis of the huge bog in southern Sweden.[84] Another sociological treatise produced on the periphery of »the gang« was Booberg's sociological analysis of the Gisselås mire in central Sweden.[85]

These claims for plant sociology in Sweden were an exclusive Uppsala affair. The reason for this is quite clear: plant sociology was formulated as a critique of the prevalent ecological plant geography and Darwinian studies of morphological adaptations to the environment, which had been advocated by Axel Lundström, Lars Albert Nilsson, Frans Kjellman and others in Uppsala for several decades. Furthermore, had it not been for the donation of the chair in plant biology in 1897, the claims for plant sociology would probably not have been forwarded in Uppsala. Sernander's institutionalization of field botany and vegetation studies in terms of plant geography were unique in the country. The two other botanical chairs in Uppsala were occupied by cytologists;[86] and with one single exception (Samuelsson) none of their students devoted themselves to the study of vegetation.[87]

Stockholm and Lund botany had also been established on a laboratory basis from the turn of the century, and thus vegetation studies and plant sociology did not have good prospects there. Stockholm and Lund botanists had no tradition in either Darwinian plant biology or vegetation studies, they had no donation chair like that in Uppsala, no specialized seminar, and no institutionalized excursions to attract students to vegetation

82. For a review of the international schools of vegetation analysis, see Whittaker 1962.
83. Although a few took up plant-environment problems as well: Ljungqvist 1914, Elias Melin 1917 (cf.2-3), Carl Malmström 1923, and above all Bertil Lindquist in 1931 (cf.3-2).
84. Osvald 1923 (cf.below).
85. Booberg 1930; under the heading »On the ecology of sociations« Booberg actually tried to make some pH-measurements »in order to investigate the relation between the hydrogen ion concentration and the sociations at a certain point of time at the Gisselåsmyren« (Booberg 1930,p.151). Compared to what »the men of the 1930s« did, however, Booberg's attempts were pathetic, and his results were inconsequential.
86. Juel had been summoned to the one botanical chair (»botany and practical economy«) in Uppsala in 1907 (see ED 15/11 1907:23) which he held until his retirement in 1928 when he was succeeded by Elias Melin in 1930. After Bengt Lidforss short sojourn in the other botanical chair in 1910-1911, Svedelius was appointed in 1914 (see ED 21/2 1914:24) and held the position until his retirement in 1938. Both ruled the Department of Botany with an iron hand; Juel's biographer emphasizes his closed nature and lack of spontaneity; Svedelius was rather unpopular both among colleagues and students.
87. After the proliferation of Uppsala dissertations on anatomical and plant biological problems in the decades around the turn of the century, the number of graduate students fell drastically. In the two decades 1915-1935 only 11 dissertations written under the auspices of Juel and Svedelius were submitted (compared to 28 dissertations submitted between 1890 and 1915).

studies[88]. It is true that Svante Murbeck, professor in »systematical botany, i.e., systematics, morphology and plant geography« in Lund between 1909 and 1924, pursued rather extensive systematical and floristic plant geographical work himself. But otherwise he mainly enrolled graduate students to do systematical, cytological and embryological dissertations.

Disclaiming synecology

Du Rietz's claim for plant sociology was accompanied by a refutation of the adaptionist ideas of Darwinism, and of

> *»the so called biology or ecology, which in everything tried to demonstrate and explain the fitness, demanded by the Darwinian system«.*[89]

As a consequence, Du Rietz considered the ecological plant geography of Warming, and the plant biology of Kjellman, Lundström and others in Sweden as speculative and rooted in a teleological nature philosophy. In this respect Du Rietz joined Hesselman. But Du Rietz never dreamt of going into the laboratory. He advocated »calm empirical research work«, or rather calm empirical field work.[90] His research programme can be summarized in one simple goal – search for the natural vegetation units:

> *»to learn to know the vital species combinations in nature, the plant communities, is the main task of plant sociology«.*[91]

When refuting plant biology or ecology á la Warming, he did not refute ecology in general, however. A substantial part of the general sections of his dissertation was devoted to a discussion of ecological analysis.[92] But the point, of course, was that only after completing the sociological analysis was one allowed to move on to ecology, that is,

> *»to explain why precisely the real existing communities have developed, why they are composed precisely according to the laws one has found, and why they grow where they actually grow. Further /we/ have got to establish their interaction with the sites and their changes in past times and present time«.*[93]

Consequently Du Rietz warned against botanists who »take unpermitted shortcuts«, trying to study site-vegetation relations without thorough familiarity with the natural plant communities. He compared it to »writing books without knowing the letters of the alphabet«,[94] or studying botany without knowing your species. In fact, Du Rietz general-

88. After Bengt Lidforss' premature death in 1913, the one (»physiological«) botanical chair in Lund was held by Hermann Nilsson-Ehle for two years only (1915-1917), and then, after Nilsson-Ehle's appointment to the personal chair in »hereditary science« (cf.2-1), by Harald Kylin for a quarter of a century (1920-1944) (after the heart-rending conflict between Lundegårdh and Kylin, cf.2-3 and 2-4). The other (»systematical«) botanical chair in Lund was held by Svante Murbeck from 1909 to 1924, and by Thore Fries from 1927 to 1930 (after a similarly frenzied competition between him and Naumann. Kylin did not succeed in recruiting many students during his reign, Murbeck recruited some. On the other hand, to write a genetical dissertation under the auspices of Nilsson-Ehle was the fashion among botany students during the 1920s. The only dissertation taking up vegetation problems was Vallin's study of the vegetation at Hallands Väderö published in 1925 (cf.2-3); Vallin had started his work during the interregnum years 1917-1920 when Henrik Lundegårdh upheld the chair. Otherwise the exceptions to the cytological-genetical dominance were an insignificant floristic plant geographical treatise by Heintze in 1913, Naumann's dissertation in 1917 (cf.2-1), and much later a floristic plant geographical treatise by Hultén in 1937.
89. Du Rietz 1924.
90. Ibid.
91. Ibid.
92. E.g., Du Rietz 1921a,p.243ff.
93. Du Rietz 1924.
94. Ibid.

ly thought in terms of analogies between species and plant societies. Plant communities were:

> *»firm sociological units to some extent analogous to the species in systematics and worth a detailed study in themselves, fully independent of... studies of the history of their development and site ecological studies«.*[95]

Thus Du Rietz did not rule out synecological studies in principle. But whereas Fries had viewed »inductive« vegetation analysis as a means to a synecological end, Du Rietz turned these priorities upside down: synecological analysis was a means to plant sociological ends. And in practice, the demands put on vegetation analysis by Du Rietz were so strict, that the ecological analysis fell by the wayside. Consequently Du Rietz only exceptionally made any synecological analysis, and usually restricted himself to the classification of plant communities and to species systematics; in fact, lichen systematics occupy a large part of his collected output.

With the publication of Du Rietz's programmatic claim for plant sociology the heated methodological discussions at Sernander's seminar came to an end, and the claim for (syn)ecology was given up. The main proponent of the ecological plant geographical viewpoint, Gunnar Samuelsson, withdrew from the arena. After publishing two larger articles on the ecological geography of water plants,[96] he abandoned problems of plant geography and ecology, transformed himself into »a gifted systematician«,[97] and was eventually appointed to the chair in systematical botany at *Riksmuséet*. Thore Fries continued to publish a few articles with synecological analyses but gradually became more and more sceptical of the claim for ecology. In the beginning, he said, ecology denoted the study of primary physical and chemical factors. However, many authors had come to talk about ecology in a much broader and more vague manner, making the concept obsolete:

> *»Welchen Zweck hat es überhaupt, die Frage zu erörtern, ob die Pflanzengesellschaften ökologisch bedingt sind oder nicht, wenn man sich nicht klar gemacht hat, was man damit eigentlich meint, und wenn sich die recht dunklen Begriffe der verschiedenen Verfasser gar nicht decken?«*[98]

Fries concluded:

> *»Das Wort Ökologie ist abgenützt und muss entweder restauriert oder verworfen werden«.*[99]

Instead Fries turned to systematics and floristic analysis. After an African journey in 1921/22 he published a series of floristic geographical and systematical articles, and in 1927 succeeded Murbeck to the chair in systematical botany at Lund.[100] None of the other seminar members took up the claim for synecology again.

To claim studies of the composition of plant communities as a separate science of plant sociology was a specific Swedish phenomenon, not to be found, for example, in

95. Du Rietz 1931,p.443; Gunnar Samuelsson also made analogies between species and plant communites, but with the addition that *both* were »artificial«. Du Rietz's holistic view of the plant community was not without parallels to Clements', but even more interesting is the fact that they echoed the leading political scientist in Uppsala of the age, Rudolf Kjellén. Kjellén considered the nation to be »a personality in its own respect« in which »the individual is included like the cell in the body« (Kjellén 1906,p.135). Kjelléns nationalistic sentiments coincided with those of the young Du Rietz – both were members of the conservative and nationalistic cultural counter-movement (»the young right«) from the 1890s onwards (cf.2-4).
96. G.Samuelsson 1925.
97. Skottsberg 1944,p.455.
98. Th.C.E.Fries 1925,p.61.
99. Ibid.,p.61.
100. ED 30/6 1927:22; in competition with Naumann (cf. above, 2-1).

Britain, where the claim for plant ecology actually included descriptions of plant communities. Pearsall, one of the leading British plant ecologists of the time, made the following comment on some of the basic works of the Uppsala school:

> *»some highly interesting work on the composition of vegetation and the description of plant communities has been developed by the Upsala school of plant ecologists (or sociologists as they prefer to call themselves)«.*[101]

Tansley also commented upon the distinction between claims for plant sociology and plant ecology:

> *»I am in agreement with various modern writers that the term plant-sociology has a greater logical claim than plant-ecology to describe the study of vegetation as such. The terms ecology and synecology are however retained here for the general study, rather than for the study of the habitat alone, partly because they are firmly established in England and America, and partly because it is of advantage to have a word for the whole study which brings into prominence the fundamental nature of the habitat as a determining factor of vegetation«.*[102]

To bring into prominence »the fundamental nature of the habit as a determining factor« was exactly what the Uppsala school struggled against – and hence they avoided the term ecology. And, as we shall see in the following section, they claimed plant sociology as a demarcation against another claim for plant ecology, viz., the experimental-physiological claim originating in Stockholm in the 1910s.

Thus, before 1930 the few claims for synecology as an independent scientific activity were rather confined; they were a local Uppsala affair, they were restricted to a short period of time around the 1910s, and they were only made as a corollary to a much stronger claim for scientific independence – that of plant sociology. And consequently, in terms of scientification the Uppsala school's claim for plant sociology was actually a process of *de-ecologization,* that is, a weakening of the ecological rhetoric, and a corresponding destabilization of ecology as a social order.

It should be noticed that this de-ecologization coincided with a turn-away from practical concerns, and a corresponding increasing academization of the whole enterprise. Sernander was a practical man indeed, embedded in the larger society with thousands of ties. Among his students were Carl Malmström and Elias Melin, who both approached practical forestry problems in their dissertations (cf.2-3). In addition Sernander kept close contacts with Hesselman at *Statens skogsförsöksanstalt* during the first decade of the seminar's existence. Thore Fries had some practical assignments – but Du Rietz was a through-going ivory tower scientist (It is true that Du Rietz was engaged in nature conservation in the 1940s (cf.3-2), but his engagement was a manifestation of his concern for the *untouched* vegetation).

Without proposing a direct causal link between this de-ecologization and the corresponding academization, it is nevertheless striking that those members of Sernander's seminar who investigated plant-environment relations were also attached to practical forestry, while the plant sociologists were full-fledged academics. Hugo Osvald makes the point. Osvald joined the seminar in 1912,[103] and soon became one of the »gang« against

101. Pearsall 1924,p.135 regularly; the *Journal of Ecology* took descriptive articles on plant community analysis.
102. Tansley 1920,p.118, note 1.
103. For biographical details on Osvald, see Jo 21/12 1933:1.

Samuelsson. He made no references to ecology whatsoever in his dissertation. Beside , however, he was employed as a »botanist« at *Svenska mosskulturföreningen* in Jönköping for long periods between 1915 and 1919, and again as its director from 1925. His practical work furnished material (and the financial support) for the dissertation. Thus he translated his practical work into pure plant sociology. But after having finished his dissertation he gave lectures on the »soil ecology of plant societies«, probably following Fries' idea of synecology, and two years later he travelled to the U.S. for plant sociological and soil science studies. A decade later he stated, that,

> *»agricultural science and plant husbandry* /are/...*practically and economically directed applications of plant biology (ecology)«,* [104]

thus actually making a plea for ecology in agricultural science.

2.3 The Stockholm school of experimental plant ecology

Neither Stockholm nor Lund botanists pursued vegetation studies in the Uppsala fashion. But they did make other kinds of claims for ecology. Particularly scientists trained at the Department of Botany at Stockholm, but also some Uppsala and Lund laboratory botanists, forwarded claims for an experimental and/or laboratory oriented plant ecology during the 1910s and 1920s. In this section we shall focus on the emergence of the Stockholm school of ecophysiology.

The second generation of experimental and/or laboratory oriented plant ecologists, born around 1890, differed considerably from the Uppsala school of synecologists and plant sociologists. Following Rutger Sernander, the Uppsala school had elaborated an essentially narrative, descriptive enterprise. The Stockholm school of ecophysiologists continued along the track laid down by Henrik Hesselman around the turn of the century. Lars-Gunnar Romell, the most persistent claimant of ecology as an independent science during the 1920s was a close assistant to Hesselman for many years. Henrik Lundegårdh, who founded the Hallands Väderö ecological station was probably intellectually heavily indebted to Hesselman, and Gottfrid Stålfelt, Nils Johansson, Hervid Vallin and Göte Turesson were in turn indebted to Lundegårdh. Elias Melin, finally, got his basic ecophysiological training from a close co-worker of Hesselman.

Henrik Lundegårdh and the laboratory in nature: the Hallands Väderö field station

J.W.C. Areschough in Lund had been the first to initiate physiological studies of plants in the late 19th century. But from the turn of the century onwards it was the Department of Botany in Stockholm that grew into a center for cytological and physiological experimental research in Sweden. The ordinary professor, Gustaf Lagerheim, had founded a botanical laboratory. Though he continued to contribute to its proceedings, it was Otto Rosenberg, who had been awarded a personal chair in plant anatomy and cell science in

104. Osvald 1933,p.3.

1911 (cf.1-2), who turned the laboratory into a gathering point for experimentally devoted students of botany. There were connections with Hesselman and *Statens skogsförsöksanstalt*; indeed being a former student of Lagerheim and a friend of Rosenberg, Hesselman actually belonged to the same circle of laboratory botanists, viz., the Stockholm school.

A botany student in Stockholm was not only trained as an experimentalist, however. Lagerheim was also a keen systematician and a field botanist; his excursions together with the students, though not as extensive as Sernander's in Uppsala, were nevertheless said to »comprise one of the most beautiful parts of his teaching activity«.[105] Stockholm was also the seat of the newly founded *Svenska botaniska föreningen*, and students attending its sessions could regularly meet field and museum botanists from *Riksmuséet*, such as C.A.Lindman, as well as the grand old man of Swedish ecological plant geography, Gunnar Andersson (cf.1-3). Hence botany students in Stockholm encountered a combination of field botany, ecological plant geography and laboratory cytology and physiology, and a local botanical milieu much more institutionally and intellectually diverse than that of either Lund or Uppsala.

During the 1910s a group of graduate students devoting themselves to ecophysiological problems emerged out of this fruitful tension between laboratory and field botany milieux.[106] They constituted the core of the local *Botanistklubben* (the Botanist's Club). Although Lagerheim and Rosenberg fostered the students' general laboratory approach to botany, it was one of their senior students, Henrik Lundegårdh (1888-1969), the son of a master tailor in Stockholm, who took the lead in formulating the ecophysiological approach.[107] Lundegårdh was undoubtedly the most brilliant student at Rosenberg's laboratory during the first decades of the century - publishing a series a cytological articles in a short time, he was able to defend his doctoral dissertation in 1912, not yet 24 years old.[108] In addition he possessed literary qualities, being a prolific author of natural philosophic essays. Lundegårdh's meteoric career should have landed him a chair in mainstream botany sooner or later, but for unknown reasons he changed in research field. After having visited the great plant physiologist Pfeffer in Leipzig in 1912/13 he wrote a series of papers on the tropisms of plants, which soon brought him a reputation for being an outstanding plant physiologist; in 1915 he was ranked as number two for the vacant chair in plant physiology in Lund, only surpassed by the internationally well-known Nilsson-Ehle.[109]

But plant tropisms were a transitional topic in Lundegårdh's swiftly changing career. In 1914, just before the outbreak of the European war, he pursued »ecological-physiological studies in Stockholm archipelago and Uppland«.[110] Lundegårdh burnt all his papers

105. O.Rosenberg 1927.
106. In a roman a clef with easily identifiable figures (e.g., Romell is called Morell), Sörlin (1931) gives a vivid picture of the daily life and attitudes of the members of *Botanistklubben* in Stockholm in the 1920s.
107. For details of Lundegårdh's biography, see Söderqvist 1984, ED 31/12 1920:107, and Jo 19/6 1926:52; Lundegårdh is said to have burnt his manuscripts and letters before his death (oral comm., Kraka Lundegårdh).
108. His dissertation was a collection of articles, not a monograph, a rather unusual form of publication at the time.
109. ED 18/6 1915:19.
110. ED 31/12 1920:107.

before he died, and we will probably never find out why he embarked on this new course. He must have made the decision during 1913. A qualified guess is that he was triggered off by the same event that sparked the young Einar Du Rietz, that is, Thore Fries' dissertation of 1913. Being well aware of Hesselman's doubts about a physiological plant geography, and having spent a winter with Pfeffer and been exposed to the very latest in physiology, Lundegårdh probably wanted to do what Hesselman had declared impossible, and what Fries now agressively declared to be unscientific. In any case Hesselman's ecophysiological investigations in the archipelago of Stockholm a decade earlier certainly stood out as a model for Lundegårdh.[111]

Having been awarded a post-doctoral *docent* scholarship in Lund he was able to spend almost a year travelling around the coasts of Zealand and Scania. In retrospect we know that he was searching for a suitable location for a field laboratory. Finally he decided on the small island of Hallands Väderö, probably fascinated, like so many before him, by its rich, unusual and varied flora and vegetation.[112] By the help of private donors, he erected the first field station in the country definitely intended for ecological investigations, or as he put it:

> *»For the solution of several ecological questions, the access to a laboratory immediately near to the investigation area is very desirable ... Some questions can, of course, be solved in an artificial culture, but usually it is not good enough to move the plants into the laboratory, but you have to move the laboratory out to the site«.*[113]

Step by step the station expanded; seven years later it included a laboratory room, several bedrooms and a kitchen, a greenhouse with separate laboratory, and a director's residence. In scale it was unsurpassed until the 1960s.

The scope of Lundegårdh's new field station at Hallands Väderö was to »analyze the life conditions of the plant communities«.[114] Over and over again he repeated his key ideas: to combine plant geography and an experimental-physiological method. Like Hesselman before him, Lundegårdh too was sceptical of Warming's life forms for being too anthropomorphic and speculative. Hence his goal was to develop »eine experimentelle ökologie«, i.e., how plants reacted to a given set of constellation of ecological factors. Because:

> *»die Probleme der Ökologie und der kausalen Pflanzengeographie nur auf experimentellen Wege mit Erfolg bearbeitet werden können«.*[115]

Or as he put it:

> *»My ecology is... physiology«.*[116]

Accordingly Lundegårdh continued the series of papers on plant tropisms as well as a large work on the assimilation of forest and shore plants, considered by his contemporaries to be his best ecological work.[117]

111. E.g., Lundegårdh says that: »An instructive example for the performance of such investigations is HESSELMAN's well-known studies on the Skabbholmen island in the archipelago of Stockholm« (Lundegårdh 1920a,p.244).
112. The vegetation of Hallands Väderö was later studied in detail by Hervid Vallin (1925); see also Vallin's popular booklet of 1949.
113. Lundegårdh 1920a,p.244.
114. Ibid.,p.244.
115. Lundegårdh 1925,p.iii.
116. Lundegårdh 1920b,p.31, note 2.
117. Lundegårdh 1921.

More important than his own ecological work was the fact that he invited other scientists to the comfortable field station. A large number of foreign and Swedish botanists were his guests for longer and shorter periods.[118] Of all the visitors to the Hallands Väderö station, Gottfrid Stålfelt (1891-1968) was probably the one most closely associated with Lundegårdh's line of thought. Stålfelt, the son of a small farmer, was also a student of Rosenberg;[119] in 1921 he defended a dissertation on how light changes affect the rhythm of cell division. At Hallands Väderö he utilized Lundegårdh's methodological skills and chose a more ecophysiological problem for his later research, viz., the ecological conditions for carbon dioxide assimilation of conifers under natural conditions.[120] His contemporaries applauded both the accurateness of the anatomical-physiological work and his ecological inclination,[121] manifested in his teaching in physiological ecology at the Department of Botany in Stockholm and at *Skogshögskolan.*[122] In fact, he revived Lundegårdh's »laboratory in nature« program many years later (cf.4-2).

Another close associate of both Lundegårdh and Stålfelt was Nils Johansson (1893-), who was also a graduate of Rosenberg's laboratory in Stockholm. He started his work on the gaseous exchange of plants at Hallands Väderö in 1922, considering it as »ein pflanzenphysiologisches Problem von ökologischen Standpunkt«.[123] A fourth member of the inner circle at Hallands Väderö was Hervid Vallin (1893-1980). Vallin started as an undergraduate student of Murbeck and Nilsson-Ehle in Lund, but

> *»Im Sommer 1919 hielt ich mich auf der Väderö einige Wochen auf und wurde durch Dozenten H. LUNDEGÅRDH für pflanzenökologische Untersuchungen interessiert, worauf ich 1920 mit dem Studium des Ulagapet (Ugglegapet), einem der grössten und interessantestens Erlensümpfe, begann«.*[124]

Vallin's dissertation work on the forest and shore vegetation of the island epitomizes the scope of Lundegårdh's ecophysiological programme. Firstly he utilized a number of methods for plant community analysis, including line- and quadrat assessment and photography. Secondly, he made a very detailed mapping of all possible ecological factors, including measurements of light intensity, air and soil temperature, wind velocity, air and soil humidity, pH, hygroscopicity and nutrient salt concentration measurements, and even attempted to evaluate the effects of competition between plants and of animal consumption. However, the correlation between the ecological factors and community structure was not successful. Vallin ended up with a heap of elaborate measurements, but no generalized results. Romell would probably have said that he lacked an hypothesis! (cf.2-4).

118. These and other details of the history of the Hallands Väderö field station can be found in Lundegårdh 1927.
119. For details of Stålfelt's biography, see ED 9/2 1934:44 and ED 21/12 1941:21.
120. Stålfelt 1924.
121. Later Lundegårdh emphasized Stålfelt's »highly 'botanical' attitude to the problems«: »he always understands to shed light on them in a diversified way, giving a really living insight into the mode of function of the plant« (in ED 12/12 1941:21), a verdict biased by the fact that Stålfelt was one of the few botanists still a friend of Lundegårdh; nevertheless it expressed the general opinion regarding Stålfelt's scientific contributions.
122. Besides he taught physiology at *Skogshögskolan*, and in the 1930s he was appointed teacher in »plant physiology, anatomy and ecology« at *Lantbrukshögskolan*. For Stålfelt's contributions to ecology in the period from the 1930s to the 1950s, see below 3-3 and 4-2.
123. N.Johansson 1926.
124. Vallin 1925,p.3.

Thus Lundegårdh became the central figure for a new and seemingly vigorous claim for ecology in the late 1910s and early 1920s. Like the Uppsala school's claim for plant sociology and Naumann's claim for limnology, Lundegårdh's claim for ecophysiology was internationally renowned too. After his *docent* scholarship had expired, he was invited by the young state of Czecho-Slovakia to establish a station for experimental ecology at the university of Brno;[125] while lecturing there he completed his main programmatic work: *Klima und Boden* (1925), in fact the first Swedish textbook on ecology. It was very positively received abroad,[126] but abused by Einar Du Rietz and the Uppsala school.[127]

But unlike both the Uppsala school and Naumann's limnological laboratory at Aneboda, however, Lundegårdh's claim was only temporarily institutionalized in terms of a field station. When applying for the chair in physiological botany in Lund in 1917, Lundegårdh was expected to be the front-runner.[128] After a long and furious competition, however, both the assessors and the academic authorities downgraded him, arguing that his work was »speculative« (cf.2-4), and advocating a more traditional and secure scientist instead. As a consequence Lundegårdh redirected his efforts to the chair in agricultural botany at *Centralanstalten*. This was to fall vacant in 1925,[129] and during the early 1920s he published some soil physiological work and was finally appointed 1926. Subsequently he embarked on pure physiological and analytical chemical problems, and left problems of ecological plant geography altogether. The laboratory at Hallands Väderö declined. A tenth anniversary article in the German journal *Flora* in fact became its epitaph.[130]

Thus Lundegårdh's attempt to institutionalize his claim for ecology, the first serious claim for an experimental ecology in Sweden, ended as a failure. The reason was undoubtedly that when assessed in the late 1910s by the Scandinavian botanical elite in the professorial competition, Lundegårdh was evaluated by criteria favouring his competitor's more traditional and secure research; no consideration was given to Lundegårdh's success as a scientific innovator and institution builder.

Elias Melin: Bringing nature into the laboratory

Lundegårdh's claim for ecology was an attempt to establish the laboratory out in the wilds. Others claimed ecology by moving the wilds into the laboratory. Elias Melin (1889-1979) was also characterized by the tension between laboratory botany and the

125. See Jo 19/6 1926:52.
126. It came in five editions (1925,1930,1949,1954 and 1957) and in English translation (Environment and plant development, London 1931).
127. See Du Rietz 1926 for a devastating critique.
128. The applicants for the chair in 1917 were the same as those in 1915 (except for Nilsson-Ehle); Lundegårdh had been ranked as a clear number two in 1915 (see ED 18/6 1915:19), hence it was expected that he should to be top-ranked in the 1917-competition as well. See further 2-4.
129. See Jo 11/6 1926:52. Lundegårdh succeeded Ernst Henning, who had been director of the Department for Agricultural Botany at *Centralanstalten* in 1914-1923. Despite his interest in environmental-plant relations in the 1880s (cf.1-2, note 82), Henning made no contributions to the ecological plant geographical discussion during the 1910s and 1920s.
130. Lundegårdh 1927.

tradition of scientific natural history.[131] His first small paper was an attempt to give an ecological explanation to the distribution of *Sphagnum* species in the Tiveden area; he classified the species found in ecological groups according to their assumed need for nutrients. Still utilizing *Sphagnum* as his object of study he was trained as a laboratory botanist, studied cytology with Juel in Uppsala, and wrote a licentiate thesis on *Sphagnum* cell division. He also participated in the lively discussions at the Uppsala seminar, without taking a clear stand, however.[132] His dissertation, initiated by Sernander in 1911 in order »to try to find out how different mires functioned as forest ground«[133] was mainly devoted to pure vegetation analysis.

In a concluding chapter, however, he took up an ecological question, viz., the conditions for mires to function as forest ground, and furthermore he added a short discussion on the mykorrhiza problem. Being trained as a laboratory botanist, he evidently realized the potential possibilities in studying the mykorrhiza and its significance for forest soils. Hence, soon after having defended his dissertation at Uppsala he was attached to Torsten Lagerberg who had just been appointed to the chair in forest botany at *Skogshögskolan* in Stockholm after having served as an assistant to Henrik Hesselman for ten years.[134] Thus, through Lagerberg, Melin was rapidly introduced to the thinking and methods of the Stockholm school.

After further training in micro-organism culture techniques with German and Dutch microbiologists, Melin introduced an entirely new approach to the »proto-ecological« problem of the mykorrhiza. By means of a series of culture experiments he was eventually able to prove that the mykorrhiza is a symbiosis between forest tree roots and the most common of the edible fungi. Melin's laboratory studies of the mykorrhiza and its importance for the nutrient uptake of forest trees were highly esteemed both by his botanical colleagues, and by practical foresters. When his *docent* scholarship expired he was awarded a personal grant from the *Riksdag* to continue his research – a rather unusual science policy measure at that time – and this arrangement was maintained until his appointment in 1930 to the chair of botany in Uppsala vacated by Juel.

Melin's singular career was a parallel to that of Lundegårdh and the other ecophysiologists in Stockholm; like them Melin considered his work as ecological as witnessed by the titles of several article and book titles: »Experimentelle Untersuchungen über die Konstitution und Ökologie der Mykorrhiza von...«[135] and *Untersuchungen über die Bedeutung der Baummykorrhiza. Eine ökologisch-physiologische Studie* (1925). He also chose to publish some of his results in the *Journal of Ecology*.

131. For details on Elias Melin, see Söderqvist 1986, and ED 14/8 1930:8.
132. In an introductory remark to the dissertation he stated that: »every association theoretically corresponds to a certain type of site. Parallel to a change of the site goes mainly one of vegetation... It is certainly desirable... to try to characterize the corresponding site type... but this demands a much more detailed study and must not anticipate but only supplement the former« (E.Melin 1917,p.5).
133. E.Melin 1917,p.ix.
134. Lagerberg served as Hesselman's assistant at *Skogsförsöksanstalten* between 1909 and 1919 before being appointed professor in forest botany at *Skogshögskolan* in 1918 (Jo 31/5 1918:38); Lagerberg worked closely together with Hesselman, focusing on the problem of parasitic fungi, but never translated his »proto-ecological« work into claims for ecology.
135. E.Melin 1923.

Thus Melin's appointment to one of the two botanical chairs in Uppsala in 1930[136] could be interpreted as a successful step towards the institutionalization of the Stockholm school of experimental plant ecology. In retrospect it turned out to be one – Melin fought on the side of the experimental ecologists in the final round of the Great Polemic (cf.2-4), and later he trained several third generation ecologists who in turn became important actors in the further institutionalization of ecology in Sweden (cf.3-3). Melin's appointment was by no means a conscious act of ecologization, however. Though utilizing the term ecology to describe some of his work, Melin never made any programmatic statement for ecology. Furthermore he was hardly recognized as an ecologist by his contemporaries. When applying for the chair his ecological inclination was only mentioned in passing. The botanical elite, viz., the leaders of the cytological revolution, appointed Melin in his capacity as a very competent laboratory botanist. Therefore the circumstance that by this an ecologically inclined scientist came to hold yet another botany chair in Uppsala was not intended, but only a matter of coincidence.

Göte Turesson and genecology

Yet another experimentalist's claim for ecology to be considered here was made by a man caught between the Hallands Väderö ecophysiologists and the rapidly growing genetics group headed by Hermann Nilsson-Ehle in Lund. Nilsson-Ehle, who had been appointed to a personal chair in hereditary science in 1917, rapidly enrolled a corps of students. One of them was Göte Turesson (1892-1970). As a school boy, Turesson was considered too independent-minded to fit into the rigid Swedish educational system, and he acquired his basic academic training at Washington State university in Seattle,[137] where he came in contact with American ecological plant geography and published a couple of plant geographical and plant biological papers.[138] Back in Sweden he soon took up field- and culture studies of shore plants with a creeping growth form. Partly working together with Lundegårdh at Hallands Väderö, partly with Nilsson-Ehle, he wrote his doctoral dissertation, *The genotypical response of the plant species to the habitat*, in 1922. Here he introduced the concept »ecotype«, meaning:

> *»that the plant species are composed by different ecological units with different appearance and different physiological characteristics, and that these so called ecotypes are hereditarily adapted to the sites or climate areas, in which they occur«.* [139]

Turesson immediately realized that by studying species differentiation into ecotypes he had opened up an internationally new field of research.[140] Taking plant geographical, genetical and physiological problems as his point of departure, he claimed a new scientific specialty of genecology, and considered this a major contribution to ecology. Concluding his dissertation he noted that:

> *»The importance of this line of research for the understanding of bio-geographical and bio-sociological questions is also evident«,* [141]

136. ED 14/8 1930:8.
137. For biographical details on Turesson, see Müntzing 1971 and 1972, ED 3/6 1927:22, ED 9/2 1934:44 and ED 23/2 1934:31 and Jo 30/3 1935:1.
138. Turesson 1914 and 1916.
139. Turesson 1922c (engl.orig.)
140. Turesson 1922b.
141. Turesson 1922c,p.347; for a historical evaluation of the concept of genecology in a longer time perspective, see Langlet 1964.

and in an historical review of plant sociology he pointed out that it was necessary to recognize the fact that species are composed of genetical races with different ecological relationships.[142]

Göte Turesson addressed deaf ears, however. His claim for genecology was not even countered; both the Stockholm experimental ecologists and the Uppsala school simply ignored it. Neither did he enrol any students to his genecological programme. During his *docent* scholarship years he made an attempt to rejuvenate botanical discussions along new lines: in 1924 he founded a botanical discussion club, arguing that the venerable *Lunds botaniska förening* (Lund Botanical Association) did not give room for modern experimental botany, including genetics. Lundegårdh, of course, supported him,[143] but otherwise his revolt remained a failure. The club soon merged with the Association again. Symptomatically it was up to a geneticist to express the greatest estimation of Turesson's ecological contributions:

> *»In my opinion Docent Turesson has taken up a group of problems of utmost importance in his genecological research, and taking exact genetics as his point of departure he has considered ecological, sociological and geobotanical problems from a common point of view, and hence promoted the development of these fields of research in a most fruitful way«.*[144]

Probably because of lack of response Turesson went further into the genetical and systematical aspects of the genotypes. He erected an experimental garden in Åkarp outside Lund, which remained his scientific base during the 1920s and 1930s. After losing four university professor competitions between 1925 and 1934,[145] he was eventually appointed to the chair in systematical botany and hereditary science at the newly established *Lantbrukshögskolan* in 1935.[146]

Lars-Gunnar Romell: nature as a laboratory

The last claim for ecology made by an experimentally oriented botanist to be considered here was made in the 1920s by Lars-Gunnar Romell (1891-1981), son of a secondary school teacher in natural history turned inventor.[147] If the novelist Ivar Lo-Johansson is a reliable eye-witness, the father Romell had »a son whom he wanted to bring up to be an unromantic scholar in some exact science, and who was already on his way to become

142. Turesson 1922a.
143. Lundegårdh was among the lecturers; on one occasion he talked on »the plant community and the ecological factors« (Håkanson 1958,p.40).
144. Heribert-Nilsson's assessment in Jo 30/3 1935:1.
145. In Lund 1927 (Th.C.E.Fries was appointed), in Uppsala 1930 (Elias Melin), again in Lund in 1934 (Heribert-Nilsson) and again in Uppsala 1934 (Du Rietz).
146. Turesson's claim for genecology did not win adherence among contemporary ecologists abroad either; but in the long run his thoughts are said to have gained a wide influence in international plant ecology and systematics: »These studies by Turesson were an important factor in bringing about the major revolution in plant taxonomy which gave the ecologic viewpoint the consideration it deserves in that subscience«, writes an authoritative textbook author (Daubenmire 1974,p.363).
147. For further biographical details on Romell, see the roll of *Lantbruksakademien*, Jo 11/6 1926:52, ED 9/2 1934:44, Jo 20/12 1940:16 and Sörlin 1931. Romell's correspondence and book collections are still family property.

one«.[148] Romell, who came to Rosenberg's laboratory in 1912, was a true child of Stockholm's diverse botanical milieu. While finishing his undergraduate education, he wrote some floristic and plant geographical papers – results of summer travels in the archipelago of Stockholm. His first true scientific work was a plant physiological article – like his licentiate thesis, a result of a study trip to Strasbourg in 1916.[149] Besides these scientific endeavours he showed an early interest in nature conservation.

Romell's main claim for ecology was made in connection with his work together with Henrik Hesselman at *Statens skogsförsöksanstalt.* It will be recalled that Hesselman was one of the pioneers in utilizing the concept of ecology (cf.1-3). Although continuing to work on plant-environment relations, and although his contemporaries considered his work as ecological,[150] however, Hesselman rarely translated his »proto-ecological« research programme at *Skogsförsöksanstalten* into the language of ecology. His department nevertheless provided a favourable intellectual environment for studies of plant-environment relations. Hesselman himself concentrated more and more upon what would become his work's guiding problem: the relation between forest trees and soil chemistry.[151] Beginning with the humus question and the problem of nitrogen nutrition he worked almost single-handedly for a decade, save for a couple of temporary botanical assistants to work on noxious fungi.[152]

Hesselman's orientation towards practical forestry problems made him repudiate traditional descriptive botany. In a lecture given to Swedish foresters in 1919 Hesselman said that theoretical knowledge in botany was still too underdeveloped to be of any use for a rational forest management. And the main reason why botanists had so little to contribute was, he said, to be found in:

> *»the more morphological or descriptive direction, which for a long time has dominated... botany...* /with the consequence that/ *botanists have mainly directed their energies towards descriptive knowledge of single species and discussed the outer and inner morphology of their organs ...*/while/ *the study of the life phenomena of the flora and vegetation, and of the plant as a living being in its manifold reactions to external influences, has had a subordinate position«.*[153]

Emphasizing that the study of the »life phenomena of the flora and vegetation« ought to take the forest as an organic whole as its object, Hesselman explained that a forest is:

> *»not only a collection of trees of a certain age or certain character, but a kind of organism, being built up from individuals of different species, but intimately connected to each other and influencing each other's living conditions«.*[154]

Not only the forest plants and their communities, but also the site (and soil) were included

148. Lo-Johansson 1954. Lars Romell is imperfectly camouflaged under the name »Grottius« (see the chapter »Evighetsmaskinen«); his son Lars-Gunnar was a latin student, but achieved top grades in physics and natural history at high school.
149. A tour which also resulted in a French-born wife, and a good knowledge of the French language, which he utilized in the Great Polemic a few years later (cf.2-4).
150. Eg., most of Hesselman's works were included under the heading »forest ecological conditions« in a contemporary bibliography of forestry literature (Linder 1920).
151. The end-point of Hesselman's research on humus was the finding that nitrogen nutrition is of decisive importance to the regeneration and growth of forests, results which later became the scientific foundation for modern large area clearings, a method widely criticized by today's ecology movement.
152. His assistants were Torsten Lagerberg (cf. note 134), and Nils Sylvén, who lost the competition over the same chair (see Jo 31/5 1918:38).
153. Hesselman 1919,p.5.
154. Ibid.,p.5.

in this »organism«:

»Forest and ground are almost knit together, they form a kind of organic whole«.[155]

Consequently Hesselman hoped that a further development of soil science »primarily in connection with plant physiology and plant community studies«[156] would be the best contribution to a future rational forest management.[157]

Thanks to the general build-up of forestry science during the war, Hesselman got financial resources to expand his research on soil-forest relationships, and recruited three young assistants. To Carl Malmström (cf.1-3) he entrusted the water-logging question. Olof Tamm was entrusted with mineral soil problems and wrote a dissertation on quarternary geological problems. The third assistant was Lars-Gunnar Romell who was entrusted with the problem of soil ventilation. Hence Romell was enrolled into the forestry research network: his dissertation of 1922 showed that almost all gaseous exchange in forest soil was dependent on diffusion processes and that the soil water content had a decisive influence on diffusion. These were findings of rather important practical consequence. It was also a theoretically very sophisticated work. None of his academic teachers, says one of his near friends, »would have contemplated using differential equations in a doctoral dissertation«.[158]

Romell early expressed his familiarity with ecological problems;[159] for example, he published a paper on voles as an ecological factor.[160] It was only Romell at *Skogsförsöksanstalten* who translated forestry research problems into ecological problems. In the introduction to the dissertation, entitled *Luftväxlingen i marken som ekologisk faktor* (Gaseous soil exchange as an ecological factor) he emphasized that:

»the gaseous exchange between the atmosphere and the upper soil layer should...be of ecological interest«.[161]

Romell continued along the trail chosen in the dissertation, and during his ten years together with Hesselman he wrote on several matters, mainly carbon dioxide turnover, pertaining both to ecological and to forestry research problems.

Unlike Lundegårdh, Romell did not try to move the laboratory out into the wilds, and unlike Melin, he did not try to replicate nature in the laboratory. His approach was to use nature as a laboratory, and to test hypotheses by elaborate field measurements. Taking a middle position between Lundegårdh and Melin, Romell demonstrated the essence of the independent ecological claim. In fact, Romell was the only botanist in the 1920s who published articles aimed entirely at solving ecological problems. That is, unlike both Melin's and Lundegårdh's claims for ecology which were by-products of their basic physiological orientation to the problems of plant life, Romell claimed ecology for its own

155. Hesselman 1922,p.139; his conception of the forest as an organic whole might very well have been taken from Clements, but it was probably as much his own idea, constructed out of his unique knowledge of soil chemistry *and* plant communities, a rather unusual combination at the time.
156. Hesselman 1919,p.11.
157. In addition, Hesselman meant that only a group of scientific specialists, each working on a different part of the forest-as-a-whole, could constitute the scientific organization necessary for a rational forest management. The terminological expression of this synthetic approach was not made until 1946, when his successor Carl Malmström renamed the department »Botany and soil science«, but in practice this had been its character since the end of the First World War.
158. C.O.Tamm 1981.
159. See e.g., Romell and Teiling 1912.
160. Romell 1921.
161. Romell 1922,p.1.

sake, hence transcending the problems of ecological plant geography to problems of ecology.[162] That is, Romell was open to experimental natural scientific studies, but also to the subject matter of the new science of ecology – the manifoldness of animal and plant species and their disparate relations to the environment.

One of Romell's close junior colleagues manifested these two aspects of the new science of ecology in his dissertation. Olof Arrhenius (1895-1977), a son of Svante Arrhenius, the Nobel Prize winner,[163] took the new signals from Uppsala seriously, and took up methods for statistical analysis of plant communities;[164] but he was also of the opinion that the Uppsala approach was a little too comfortable for the scientist. A more difficult, but also more attractive task, would be to learn to know the factors influencing the life of the plants and the plant societies, »d.h. die Feldphysiologie oder Öcologie«,[165] since it hints at deeper connections. Arrhenius's dissertation, *Öcologische Studien in den Stockholmer Schären* (1920), was originally planned as a immigration historical study á la Thore Fries. But Arrhenius was gradually convinced about

> *»der starke Zusammenhang zwischen den Pflanzengenossenschaften und dem Standort«,*

and echoing Romell he incorporated the »deductive« approach, because

> *»die eine* /inductive approach/ *sucht ein künstliches System aufzubauen, in der Art des Sexualsystems, die andere* /deductive approach/ *ein natürliches System«.*[166]

Romell and Arrhenius faded from the scene in the mid 1920s. Both lost a competition for a chair in 1926;[167] Arrhenius was attached to an agricultural research station on Java, and when returning home he settled as a private scientist. Romell went abroad too. After studies of soil microbiology with Vinogradskij at the Pasteur Institute in Paris in 1926-1927, he was summoned to a newly established research professorship in forestry soil science at Cornell university. Thereby Romell was seemingly also lost to Swedish ecology. Yet another claim for an experimental plant ecology seemed to have failed to become institutionalized.

2.4 The Great Polemic among the plant ecologists

The claims for plant ecology made by the second generation of ecologists around 1920 all referred back, directly or indirectly, to the claim for ecological plant geography advanced by Warming and adopted by the pioneer generation of ecologists around the turn of the century. Thore Fries and Einar Du Rietz in Uppsala took the problems of ecological plant geography as their point of departure, as did Henrik Lundegårdh and colleagues in Stockholm.

162. In his introductory article to the history of botany in the multi-volume *Växternas liv* (Plant life) edited by Carl Skottsberg (1932), Romell considered ecology to be a major branch of botany (»Nowadays those research branches which deal with the adaptation of the plants to the outer conditions are often called ecology« (pp.21-22).
163. For biographical details on Olof Arrhenius, see Jo 11/6 1926:52.
164. See Arrhenius 1919 and 1923.
165. Arrhenius 1920.
166. Arrhenius 1920,p.92 and p.3 resp.; the available data hardly convinced a contemporary reader, however, in spite of the elaborate soil chemical and soil physical analyses.
167. Arrhenius, Lundegårdh, Elias Melin and Romell applied for the chair in agricultural botany at *Centralanstalten* (see Jo 11/6 1926:52); Lundegårdh, of course, was top-ranked.

As indicated above, the second generation of ecologists translated their field studies of plants into entirely new problems, however. The Uppsala school, gathered at Rutger Sernander's seminar and excursions, translated their field studies into synecology, and above all plant sociology, focusing on the analysis of plant communities. Those trained in laboratory experimental methods, on the other hand, most of them associated with *Botanistklubben* in Stockholm, translated their field studies into different experimental approaches, focusing on the influence of site factors on the life and distribution of the plant.

Thus, already around 1915-1920 an observer of Swedish botany could discern two qualitatively different approaches to the field study of plant-environment relations, i.e., the Uppsala school and the Stockholm school. In this section we shall take a closer look at the emerging conflict between the two schools during the 1920s and the early 1930s.

An immediate consequence of these two departures was that the original focus on ecological plant geography hardly survived the early 1920s as an inspiration for academic scientific research. Furthermore, while the first decade of the 1900s had been a period for formulating ecological claims, the 1920s was a period of struggle between these two main ecological social orders. The peaceful scientific discourse between the first generation of ecologists now gave way to internecine combat. From 1920 onwards, a majority of Swedish plant ecologists became involved in a Great Polemic which was not settled until 1934, when the main representatives of the different versions of ecology applied to succeed Rutger Sernander to the chair in plant biology in Uppsala, the only tenured position then existing suitable for a scientist studying plant-environment relations.

Sernander, the doyen of ecological plant geography in Sweden, never joined in the polemic, neither in its prelude at his own seminar in 1915-1920, nor as an emeritus during the competition to choose his successor.[168] The Great Polemic was entirely an affair for the second generation of ecologists – »the men of the 1910s«.

Which were the issues? What was at stake?

Henrik Lundegårdh versus Einar Du Rietz: laboratory experimentalism versus descriptive empiricism

It will be recalled that Lundegårdh's scientific programme was that of »the laboratory in nature«. Utilizing apparatus and measuring instruments to perform physiological experiments was to him the key to the problems of plant geography.[169] Lundegårdh occupied one extreme standpoint in the polemic. At the other was Du Rietz, who thought that nothing sensible could be said about the possible environmental influence upon the distribution of plants without a thorough descriptive classification of vegetational units. Accusing each other of »ökologischer Dilettantismus«, Lundegårdh thought Du Rietz's

168. Contrary to academic tradition he did not act as an assessor; legally he was disqualified since one of the applicants, Einar Du Rietz, had become his son-in-law.
169. »One will not proceed very far in analyzing the life conditions of the plant communities without a suitable equipment of instruments and apparatus« (Lundegårdh 1920,p.244.

plant sociology had stiffened into »unfruchtbaren Klassifikationsversuche«,[170] while Du Rietz, on the other hand, found Lundegårdh »speculative« and »careless«, not even knowing the names of the species he worked with.[171]

Henrik Lundegårdh's ideal of science[172] was never presented in direct argument with Du Rietz, but he made it explicit in his earlier polemic in connection with the competition for the Lund chair in plant physiology in 1917-1920.[173] The assessors, known as strictly empirical botanists,[174] considered Lundegårdh to be an original, independent and imaginative scientist, but they also warned against his tendency to speculation and »freely floating flight«. Instead they recommended his fellow competitor, a rather limited and traditional botanist who was thought to express a »solid« and »accurate« scientific attitude. Considering himself unjustly and even abusively treated by the assessors, Lundegårdh countered with a combat pamphlet, *Modern experimentell växtfysiologi och traditionell botanik*, arguing for experimental botany, including »modern physiology and ecology« to be a »naturally delimited discipline«[175] opposed to the traditional, purely empirical and descriptive botany.[176]

The Uppsala school, on the other hand, stands out as a model of an empirical, inductive science. Rutger Sernander had advocated strict empiricism; his attitude was followed up by Thore Fries and Einar Du Rietz. Conclusions regarding the composition of plant communities were to be drawn only after a critical examination of the facts of nature. No hypotheses regarding ecological relations between site and vegetation were allowed to sneak into the vegetation analysis. Only after mapping the vegetational units might one establish correlations concerning a possible ecological law-like relation between the two.

The difference between the experimental and laboratory ecologists, on the one hand, and the Uppsala school on the other, was particularly obvious in their attitude to the role of experiment in the scientific enterprise. Du Rietz talked about »experiments« too, but these were of an entirely different kind from those recognized by the Stockholm school. When accused of not making experiments, Du Rietz coined the formula »nature's own experiment«, which became one of the leading arguments in his polemic arsenal against the experimentalists. For example, when Göte Turesson, who had spent a decade performing long-term field experiments in his experimental garden at Åkarp, pointed out that the kind of observations Du Rietz pursued »of course never can lead to an elucidation of causal connections in nature«,[177] Du Rietz retorted:

170. Lundegårdh 1925,p.5.
171. Du Rietz 1926.
172. For a discussion of the concept »ideal of science«, see Törnebohm 1983.
173. See ED 31/12 1920:107 for details.
174. Both the Dane Johannsen and the Norwegian Wille were known as empiricists. Johannsen's biographer points out that he »criticizes...colleagues who get on the wrong track by poor speculation and imperfect experiments« (*Dansk Biografisk Leksikon*) and Wille is said to have preferred »a solid basis of facts to high edifices of thought« (*Norsk Biografisk Leksikon*).
175. Lundegårdh 1920b,p.31.
176. His main competitor, Harald Kylin, countered with a critique of Lundegårdh's »philosophical« attitude to botanical research, presenting himself as a thoroughly empirical working botanist (Kylin 1920).
177. Turesson 1933,p.16.

»The comparative study of nature's own great experiment can, of course, as well as the study of the experiments which we ourselves in specially favourable circumstances and to a limited extent can set up, lead to the elucidation of causal connections in nature«,[178]

and when almost two decades later he should assess an applicant to the chair in limnology in Lund, he said that:

»/the applicant/ *has preferred comparative studies of nature's own great experiment to experimental-physiological laboratory investigations«.*[179]

Lars-Gunnar Romell on ecology as a hypothesis-testing science

Lundegårdh's critique of the Uppsala school had not been particularly sophisticated. First and foremost a craftsman, he designed his experiments by intuition; he never subscribed to any outspoken meta-theoretical position. Lars-Gunnar Romell, not Lundegårdh, was the intellectual center of the ecophysiological discussions in Stockholm during the early 1920s. Some designated Romell the

»triumph of the /Stockholm/ *botanists, the genius who knew everything without reading anything«,*

others thought he was

»a scientific snob, who avoided Swedish language as much as possible«.

But nobody was unaffected by his intellectual manners:

»He was an aesthetician, to his finger-tips. One day he took /everybody/ *by surprise with a beautiful essay on the importance of nature to the creative artist, published in the leading cultural journal of the country, and after two years academic work he published a long article in French about carbon dioxide assimilation in conifers, which could have been accepted as a dissertation and awarded a prize by Svenska Akademien* /the Swedish Academy/«.[180]

So, it was well in accord with his reputation that Romell eventually made an ostentatious public claim for ecology – and in addition a methodologically sophisticated one, criticizing Du Rietz in a much more principled way than Lundegårdh had done. Thus the polemic between Romell and Du Rietz attained a higher level than the mutual accusations of »ökologischer Dilettantismus« exchanged between Lundegårdh and Du Rietz. Romell's declaration was even more pronounced by the fact that it was published in the national botanical journal – and in French! *Svensk botanisk tidskrift*'s first thirteen volumes had been characterized by moderation and descriptive articles on floristics, floristic plant geography, cytology and systematics. Suddenly, in its 14th volume, the scholarly calm was broken by Romell's general attack on the emerging Uppsala school. Under the title »Physionomistique et écologie raisonnée« Romell argued that plant sociology probably had some value as »a kind of description from a scientific journey«, but it would be of permanent value only if combined with ecophysiological investigations. Only by means of ecophysiology could plant geography become an exact science. The plant geographers and -sociologists in Uppsala reminded Romell of nit-picking scholastics:

178. Du Rietz 1933,p.12.
179. See his assessment of Gunnar Lohammar in ED 30/12 1948:7 (cf.3-2).
180. Sörlin 1931,pp.75-6.

»On est presque forcé de penser a ce professeur allemand dans le Simplicissimus qui enseigne l'histoire d'art: 'Erst durch die Zusammensetzung des tetrastylen Tempels A plus dem pyknostylen Tempels B erfand der hellenische Geist jene herrliche Spirale, die wir mit rg bezeichnen'«.[181)]

It was these harsh words directed implicitly, if not explicitly, at Einar Du Rietz that instigated the Great Polemic.

Romell's criticism against the Uppsala school's inductivism was accompanied by a sophisticated statistical argument, developed in co-operation with trained statisticians.[182)] They argued that the concepts of constant species and minimum area was weakly founded in statistical reasoning. In addition they criticized Du Rietz for his tendency to attach super-individual status to the plant communities. All in all, they did not believe in the existence of clear boundaries between the associations – the variation in vegetation was far more continuous due to gradually changing environmental conditions.[183)]

On the other hand, while refuting the Uppsala school's inductivism and belief in the plant communities as independent natural units, Romell nevertheless sided with Du Rietz's critique of Lundegårdh. Romell had spent a summer with Lundegårdh at Hallands Väderö, and his translation of naturalist curiosity into ecological problems seems to have been stimulated by Lundegårdh's achievements; but he did not follow Lundegårdh through thick and thin. In fact, the two men never engaged in any deeper cooperation; Romell particularly criticized Lundegårdh's fixation with apparatus. Making experiments with nature did not have to involve a lot of equipment and instruments, he thought. During the climax of the Great Polemic in the early 1930s Romell warned against Lundegårdh's programme, arguing that it was urgent to give:

»a place of refuge for a both affectionate and intelligent open-air study of Swedish flora and vegetation in a good old Swedish and Nordic spirit. If the matter is managed in such a way, that downright geneticists or nature-blind Pfeffer types might get on Rutger Sernander's chair, I incline to the opinion that this would be a treachery, not only against Frans Kempe /i.e., the donor of the chair/, *who probably never had anything like that in mind, but also against a distinguished Swedish and Uppsalian tradition«.*[184)]

This quotation illustrates Romell's mediating standpoint between an old narrative, essentially descriptive ideal of floristic and systematic botany and plant geography, and a pure laboratory ideal, which reduced botany to test-tubes, electric cables and measurement instruments.

Thus Romell openly criticized Lundegårdh's »nature blindness« and sided with Du Rietz in the criticism against Lundegårdh's »carelessness«.[185)] But he did not ride on the

181. Romell 1920,p.136.
182. Harald Kylin (i.e., Lundegårdh's competitor for the physiolological botanical chair in Lund, who was also skilled in probability calculus), the chemist The Svedberg, and the professional statistician S.D. Wicksell.
183. See Romell 1925 and Kylin 1926 for a summary of the arguments from the side of the »statisticians« and a bibliography of the significant articles in the discussion so far. The Great Polemic of the 1920s included over 25 articles, most of them written by the main actors identified in the two preceding sections, but also a few contributions from amateurs who nevertheless found the dispute to be of great moment, e.g., Svedberg 1922. We have not had the ambition to review all aspects of the polemic here, but have focused on the questions having direct significance to the ecological discourse. However, the Great Polemic would certainly be well worth an in-depth study of the rhetorics of a scientific dispute.
184. Romell 1934,p.14; the »downright geneticist« was a reference to Nils Heribert-Nilsson who got the chair in botany in Lund in 1934 (ED 23/2 1934:31), while the »nature-blind Pfeffer type« of course referred to Lundegårdh, who had studied with Pfeffer in Leipzig in his early years (cf.2-3).
185. See Romell 1934.

last point, like Du Rietz did. After all, »carelessness« is not a question of philosophical principle. But Du Rietz's empirical inductive method was very much a matter of philosophical principle. Referring to Poincaré, Romell emphasized the ultimate importance of creating hypotheses, as long as they were subsequently subjected to critical empirical test. Romell admitted that, in a sense, it is speculative to put forward a hypothesis about ecological adaptation as the basis for plant community analysis, but that kind of hypothesis was at least amenable to testing. Du Rietz surely had a hypothesis too, only it was implicit and unconscious, and hence so much more dangerous. Du Rietz's hypothesis was, according to Romell, that the cause of the seeming constancy of composition of plant communities (and hence their »natural« character) is that they have evolved by means of »a slow community selection- and establishment process«. But such an hypothesis, Romell continued, is impossible to test, since »the cause is located somewhere in the beginning of time«.[186] Du Rietz had not shown that this was the only possible hypothesis; he had not even shown that this hypothesis really could explain the constancy of the plant communities. He just took it for granted, believing that it was more scientific to gather an ever growing empirical material:

> *»Du Rietz thinks that he serves science more by remaining silent about his 'speculations' and investigating more quadrats instead«.*[187]

Two academic cultures

Evidently a methodological abyss divided the Uppsala school and the experimental and laboratory ecologists, most of them trained in Stockholm. The latter approached plant-environment relations and the problem of plant distribution by means of experiments in the conventional sense (testing hypotheses under controlled circumstances). The experiment, either in the laboratory, or under strictly controlled outdoor conditions, was the means to unravel the causal laws of nature. Hence when claiming ecology, they claimed a causal, experimental science, a science elucidating the mechanisms of the relations between organisms and their environment. The Uppsala school, on the other hand, approached plant-environment relations and the problem of plant distribution by means of accurate descriptive field work and historical comparisons. For them, the excursion took the place of the laboratory. And when claiming ecology (as synecology), they only claimed it as a law-like induction. Du Rietz refuted the »deductivist« point of view as a vicious circle.[188] To the experimentalists, ecological relations were real, material and »out-there«, canonized in what Romell called »l'hypothese fondamentale de l'écologie«,[189] while to the Uppsala school it was only the species and their historically formed combinations into organism-like communities that had any real existence. To them it was unacceptable to delineate plant communities on ecological grounds.[190]

These contradictory standpoints to nature express a more fundamental intellectual contradiction between two different ideals of science.[191] On the one hand a descriptive

186. Romell 1923,pp.4-5.
187. Ibid.,p.6
188. Du Rietz 1921b,p.118.
189. Romell 1920.
190. See Du Rietz 1921b for a counter-critique of Romell 1920.
191. Cf. Törnebohm 1983.

and empirical ideal centered in Uppsala, and on the other a hypothetical and experimental ideal centered in Stockholm. These ideals of science were in turn anchored in two different local academic cultures. It was not restricted to the botanists and plant ecologists. The Great Polemic had its parallels in other academic disputes. The best known example from the history of Swedish science is the conflict over Svante Arrhenius, the physical chemist and Nobel Prize winner. Arrhenius was decidedly a man of bold ideas, even speculations;[192] his doctoral dissertation in Uppsala was almost refuted, and despite his internationally valued work on the electrolyte theory, his election to *Vetenskapsakademien* in 1901 and his Nobel Prize in 1904, both supported by scientific circles in Stockholm, were strongly opposed by natural science circles in Uppsala.

The conflicts between the second generation of ecologists might be evaluated against this larger background. We should not forget that »the men of the 1910s« were intellectually socialized during the early decades of the 20th century, the »hesitant and limping marching-up period«[193] of the welfare state, a political and ideological transition period. They saw the old society, dominated by great landowners and state officials, eventually giving way to a modern society, politically and culturally dominated by capital owners and an oligarchy of working class leaders. The entrepreneur and the industrialist were the new social heroes. »The men of the 1910s« attended school when the workers' movement, with its optimism for the future and its internationalism, broke through. Trade unions had united nationally in 1898, and the Social Democratic Party won its first parliamentary seat in 1897. Even though the General Strike of 1909 failed, it could not put an end to the social optimism of the industrial classes. Though opposed to each other in industrial conflict, capital owners and workers had a common enemy: the old political ruling elite, the remnants of the old nobility and the central state bureaucracy. Industrialists and workers joined in the struggle for democracy. The battle for universal suffrage was won in 1909 for men, and for women in 1918. Thus, within a few decades, a backward rural periphery of Europe had entered the circle of civilized nations. »The men of the 1910s« were among the spectators.

Though not so civilized as to join the battlefields of the great European war, Sweden was nevertheless caught in the general social, political and ideological turmoil that swept the continent in its wake. The first government with Social Democrats was formed in 1917, followed by a decade and a half of shifting liberal, conservative, and Social Democratic governments. The last conservative government held power in 1928-1930. Then the Social Democrats took over. The modern welfare state was born in the 1930s.

From the point of view of culture and leading ideas the transition period was characterized by a clash between nationalist romanticism and a more cosmopolitan modernism, the former socially associated with the older ruling elite, the latter with the progressive industrialist strata. The distinction between August Strindberg and the progressive literary 1880s, on the one hand, and Verner von Heidenstam and the nationalistic literary 1890s, on the other, is a well-known theme in Swedish literary history. The dissolution of the old agricultural society was accompanied by the cultural radicalism[194] referred to above (1-2), but also by the rise of the modern sentiment for nature and patriotism.

192. See Svedberg 1920 for a biography of Svante Arrhenius.
193. K.Samuelsson 1968,p.246.
194. See e.g. Richardsson 1963,pp.89-101.

Svenska turistföreningen (the Swedish Tourist Association) was founded in 1885 under the motto »Know your country!«; the painters discovered Sweden as a motif; the foundation of *Nordiska muséet* (the Nordic Museum) in 1872 and particularly its later world-famous open-air annex *Skansen* in 1891 epitomizes the growing concern for Swedish values.[195]

The second generation of ecologists were cast into these main cultural conflicts of the transition period, and the clash between the claims for ecology and ideals of science formulated during the 1910s and the 1920s echoed this general pattern of cultural conflict. Lundegårdh's, Turesson's, Romell's and Stålfelt's radical modernism and experimental laboratory approach to plant geography was not without connection to their social and intellectual biographies. Lundegårdh was the son of a master tailor in Stockholm, the center of industry and commerce, with its radical university. An anecdote tells us that the young Henrik had to follow his father to the tailor shop every morning; when passing by the bust of the famous Swedish inventor John Ericsson he was supposed to stop, take his cap off and bow.[196] Romell's father was a bourgeois radical; originally a secondary school teacher, he had resigned after a conflict with the local school authorities,[197] and continued his life as a patent agent; Lars-Gunnar did not hesitate to publish articles in a Social Democratic newspaper.[198] Turesson, as we have seen, was a rebellious type. Stålfelt, finally, the son of a small farmer, sympathized with the Communist Party.[199] A contemporary has attested to the radical mix of Socialist, Communist and early Fascist ideas prevalent in the Botanical Club in Stockholm.[200] And we should not forget their internationalistic orientation: Lundegårdh established a laboratory in Brno, Romell took a position at Ithaca, Melin learnt his methods in Holland and Germany, Turesson got his undergraduate training in Seattle etc. Thus, the laboratory and experimental ecologists were socially and intellectually connected to the modern break-through.[201]

Their adversaries in Uppsala, on the other hand, had their social and intellectual background in the small-town university environment. Lundegårdh's competitor for the Lund chair had his intellectual background in academic Uppsala. Thore Fries was member of the old academic Fries-family. Einar Du Rietz, the son of an established company owner, was also intellectually formed in Uppsala, as were Hugo Osvald and the others. And as we saw above (2-2), the whole Uppsala seminar was immersed in the Swedish patriotism, sentiment for the nature, and traditionalism so characteristic of the counter-modern cultural movements of the 1890s.

Having noted this, it is probably no coincidence that Lars-Gunnar Romell and Einar

195. S.Björck 1946, Ch.2.
196. Oral comm., Lundegårdh's widow, Kraka Lundegårdh, March 1982.
197. See Lo-Johansson 1954.
198. He published several articles in the newspaper *Social-Demokraten* from 1920 onwards; later he also published in the liberal *Dagens Nyheter*.
199. Stålfelt's communist sympathies are mentioned by Sörlin 1931.
200. Sörlin 1931 writes: »/they/ had been born during the triumphal progress of Darwinism, imbibed naturalism's brute love of truth, and were infected by the ideas of socialism. Many of them had gone to socialist meetings, been members of radical societies and gone to rallies. They had discussed Branting and Wicksell /i.e., leading Social Democrats/, fancied Bengt Lidforss /cf.1-2/ and together they had read his criticism of Christianity. They had been atheists and idealists, dreamt about social wonderlands and races of supermen. To a pronounced degree they had been children of their time« (p.132).
201. Melin was the exception, being brought up in a clergyman's home.

Du Rietz also had contrary views on nature conservation. Romell advocated a rational management of natural resources, focusing on the general public's access to nature, and he engaged himself in the preservation of natural recreation areas around Stockholm. Du Rietz, on the other hand, like Sernander, thought in terms of protection of distant mountain areas and desolate mires. For the Uppsala school, nature conservation was essentially a museum enterprise.

The climax of the Great Polemic 1932-1934

The Great Polemic climaxed in the early 1930s with the competition for the Uppsala chair in plant biology. All the main actors referred to above were involved, either as applicants or assessors. Du Rietz, Lundegårdh, Romell, Turesson and Stålfelt (and, as the dark horse, Nils-Heribert Nilsson, the geneticist from Lund) applied for the chair. Finding assessors was not easy; first, the faculty appointed Hesselman, Melin and Samuelsson (as well as Nilsson-Ehle) as assessors, a decision which in practice would have excluded Du Rietz from the chair in advance. Then after a number of withdrawals, new appointments and renewed withdrawals a quite different assessment committee was appointed, viz., Melin together with two Norwegian descriptive botanists, Nordhagen and Holmboe, and a Danish geneticist, Winge.

After two years delay the assessment procedure started in 1933. The assessors were deeply divided along the same lines as the applicants. Melin, who defined the chair as an »ecological« one, considered Lundegårdh's, Romell's, Stålfelt's and Turesson's production to be mainly »ecological« and praised Lundegårdh as the leading figure of Swedish experimental-physiological plant ecology:

> *»he has in a skilled way introduced new methods, through which he has been a pioneer in the field of ecological research«.*[202]

Winge, likewise considering ecology to be the central subject of the chair, also top-ranked Lundegårdh.

Holmboe and Nordhagen, on the other hand, hardly mentioned ecology at all, and were reluctant to accept physiological work as a relevant qualification for the chair. E.g., both reduced Stålfelt's production to pure plant physiology. Nordhagen stated his position quite clearly – while merely observing that

> *»Du Rietz's attitude towards... modern experimental approaches* /is/ *rather cold«,*[203]

he ridiculed the experimentalists:

> *»There is plenty of scientific snobbery in the cry for 'more causal research' heard from several younger Swedish botanists«.*[204]

This dead heat between Lundegårdh and Du Rietz, could have been difficult for the faculty to handle, had it not been for Nordhagen's impeachment of Lundegårdh's scientific honour. Nordhagen bluntly declared him incompetent as an academic teacher. Enumerating a number of factual errors in two marginal review papers, while ignoring Lundegårdh's original production, he found it

202. ED 9/2 1934:44, Melin's assessment.
203. Ibid., Nordhagen's assessment.
204. Ibid., Nordhagen's assessment.

> *«clearly and evidently proven that* /Lundegårdh/ *is incompetent to hold an academical teaching position in botany or plant biology»,*

because:

> *»After all a university is a temple for learning and knowledge!«*[205]

Facing this harsh dismissal Lundegårdh's well-known temper claimed its due; he withdrew his application in anger.

Lundegårdh's dramatic withdrawal did not automatically leave the way open for Du Rietz, however. The majority of the faculty also considered the chair an ecological one, and were likewise divided along the same lines. One faculty member defined ecology in such a way that Du Rietz was considered to have his »essential qualifications within ecology«; another thought that Stålfelt's production lay »within pure plant physiology«. Sven Ekman, of course, declared the contrary standpoint:

> *»That Stålfelt's main qualifications seems to lie on the border between physiology and ecology is for me a matter of secondary importance, since his most important investigations could evidently be construed as a physiological causal analysis of ecological problems«.*[206]

One of the former pioneer claimants of plant ecology, now professor in geography in Uppsala, John Frödin, intervened too. »Ecology«, he said, is:

> *»the study of the influence of external factors on the life and distribution of the plant, that is, the dependence of the individual wild plant, the species and the community on the environment«.*[207]

Although crediting Du Rietz's ecological investigations, Frödin nevertheless stressed that »the ecological treatment of sociological units« must not be restricted to discussions of possible external factors, but should be subjected to independent analyses. Hence Frödin could not

> *»restrain the thought, that a comparison between...* /Romell's, Stålfelt's and Du Rietz's/ *pure ecological qualifications might change the ranking to the disadvantage of docent Du Rietz«.*[208]

However, although descriptive-comparative and experimental-physiological approaches to ecology were set against each other in the faculty too, it was politically impossible to disavowe the majority verdict of the assessors – Du Rietz was backed by general consent, and eventually appointed professor of plant biology in Uppsala in January 1934.[209]

Aftermath of the Great Polemic

With Du Rietz's appointment in 1934 the Great Polemic was over. As a consequence, the nexus of experimental and laboratory plant ecologists finally dissolved. They either gave up ecology, withdrew from the scene and isolated themselves or failed to attract disciples.

205. Ibid., Nordhagen's assessment.
206. Ibid., minutes of meeting in *matematisk-naturvetenskapliga sektionen* 3/11 1933, Ekman's vote.
207. Ibid., Frödin's vote.
208. Ibid., Frödin's vote.
209. While it is true that the appointment of Du Rietz was in accordance with the social and intellectual environment of Uppsala, his »victory« can hardly be explained with reference to the susceptibility of empiricism and descriptive science in the larger academic circles in Uppsala. Nothing indicates that Du Rietz's »victory« was given in beforehand; it was rather the outcome of a complicated negotiation process between many actors, assessors and faculty members, where each acted according to his individual preferences.

Lundegårdh had already begun to switch to biochemical and physiological problems, and continued to develop a successful department for the study of plant physiological and biochemical problems at *Lantbrukshögskolan*. Turesson, after having applied for four professorships at the universities, was eventually appointed to the new chair in systematical botany and hereditary science at *Lantbrukshögskolan* in 1935. Romell returned from Cornell in 1934, only to resume a subordinate position at *Statens skogsförsöksanstalt*. Stålfelt continued to lecture on plant ecology and plant physiology at *Skogshögskolan*, at *Lantbrukshögskolan* and at the Department of Botany in Stockholm, but did not gather any students around him to do ecological research.

Hence the claim for an experimental and/or laboratory oriented ecology, having its roots in the »new German« botany of the late 19th century, once forwarded by Henrik Hesselman and passed on by the Stockholm school of ecophysiologists, seemed to have come to an end. And with that the emerging ecologization process seemed to have come to an end too. As already indicated above (2-2) the Uppsala school's claim for plant sociology in fact implied a rejection of ecology as an independent scientific specialty. In other words, the outcome of the Great Polemic was in fact a de-ecologization of academia.

In the following Chapter 3 we shall follow up the fate of the Uppsala school of plant sociology institutionalized by Einar Du Rietz,[210] the renewal of claims for synecology, and also a number of new attempts to claim ecology as an experimental and/or laboratory oriented science. Before that, however, we shall take a look at the translation of animal field studies into the emerging social order of animal ecology.

2.5 The animal ecologists

Most claims for ecology during the 1910s and 1920s were made by botanists. But while it is true that most university trained zoologists had specialized in anatomy or systematics, quite a few nevertheless approached problems concerning the relation between animals and environment. Some claimed their studies as ecological as well. In this section we will take a look at the ecologization of animal studies during the 1910s and the 1920s.

With few exceptions the translation of studies of animal-environment relations into animal ecology were made by university zoologists in an academic context. Some zoologists employed at agriculture, forestry or fishery research institutes worked with animal-environment relations as part of their professional task, but none of them translated such »proto-ecological« investigations into the language of ecology; hence they did not contribute to the nascent social order of ecology. The case of fishery research offers a good example. *Svenska hydrografisk-biologiska kommissionen* continued to pursue investigations of the relations between the organisms (bottom fauna, plankton, and fish) and their fluid environment. The work was made in close cooperation with *Lantbruksstyrelsens fiskeribyrå*. After Trybom's death in 1913, another Uppsala zoologist, K.A.

210. A year later the Government decided that plant biology should be an examination subject for the *fil.lic.* degree (see Anon. 1963).

Andersson emerged as the new leading figure in Swedish marine fishery research. Andersson was responsible for the employment of a group of young scientists trained as zoologists in Uppsala: first Arvid Molander (1886-1965), a few years later Orvar Nybelin (1892-1982) and lastly Christian Hessle (1890-1980). Eventually *Havsfiskelaboratoriet* (the Marine Fishery Institute), serving as a central laboratory for marine fishery biological investigations, was established in the small town of Lysekil in 1929, largely by means of private donations.[211)] What these »fishery biologists« and the fishery research policy makers actually did and planned for was certainly »proto-ecological«.[212)] Arvid Molander, for example, in his report on the migration and distribution of flat-fish stated that

> *»In order to be able to explain the causes of the migration of plaice it may probably be necessary to refer to the variations in the nutrient status«.*[213)]

But recalling that ecology is here defined as a social order integrated through a specific ecological rhetoric, these investigations and policy measures were certainly not ecological. Neither Molander nor any of his colleagues claimed their studies as ecology.[214)] Hence, although several scientists trained as zoologists spent their whole effort investigating fish-environment relations, none of them claimed it as a new social order of ecology, but remained within the social order of fishery biology.

The absence of translations of »proto-ecology« into ecology goes for those zoologists working on agricultural and forestry problems as well. Albert Tullgren (1874-1958), director of the Department of Entomology at *Centralanstalten* from 1909,[215)] worked on problems concerning the control of noxious insects, but never translated these studies into ecology. Likewise, the Uppsala zoologist Ivar Trägårdh (1878-1951), who had mainly worked with insect morphology and systematics in Uppsala, became increasingly concerned with environment-organism relations in his professional work after having been appointed to a newly established chair in forest entomology at *Statens skogsförsöksanstalt* in 1915.[216)] In a programmatic statement from 1923 he said:

> *»One of the most important truths, proven by modern biological research, is the intimate relation between all living things. The existence and manifestation of every living being is connected to a multitude of other living beings, and a perpetual exchange takes place in different directions. Therefore every living being may be pictured as a little mesh in the great web of life«.*[217)]

Though this was hardly a novel statement amidst considerable international research efforts on insect pests and their population dynamics and ecology, Trägårdh did not

211. See address from *Svenska hydrografisk-biologiska kommissionen* to the Government 12/12 1927 in *proposition* nr 108, 17/2 1928.
212. Most studies during the period considered here were published in the series *Svenska Hydrografisk-biologiska Kommissionens skrifter, Ny serie. Biologi*, vol 1-2 (1925-1948); see also Otterlind 1955 for an overview over some of the work made by the fishery biologists in Lysekil.
213. Molander 1925,p.8.
214. The only translation of »proto-ecological« fishery investigations into a specific claim for an independent scientific specialty was Naumann's translation to limnology (cf.2-1).
215. The Department of Entomology was a continuation of the independent *Entomologiska anstalten*, so closely associated with the name of Sven Lampa (cf.1-2, note 78). Tullgren succeeded Lampa as director. It is interesting to note that neither Lampa nor Tullgren were professional zoologists (Tullgren only completed his *fil.kand.*)He was appointed professor in 1913, and on the reorganization of agricultural research in 1932, he was made director of *Statens växtskyddsanstalt* (the State Institute for Plant Protection), a position he held until 1939.
216. It was an associate professorship 1915-1921 and was transformed into a full professorship thereafter (Jo 2/12 1921:27). For a general overview of the work at Trägårdh's laboratory, see Butovitsch 1952.
217. Trägårdh 1923; in a collection of popular biological essays (Trägårdh 1925) he expressed an intuitive ecosystem view of the sea, its »economy« and its »metabolism«, without, however, referring to an ecological terminology.

translate his problems concerning forest insects into the problem sphere of ecology. So, even though he started a research programme to investigate forest soil insects and their environmental dependence,[218] he did not contribute to the growing social order of ecology.[219]

The claims for animal ecology made in an academic context fall neatly into two kinds: those made for ecology as a descriptive, almost narrative science (»scientific natural history«) and those suggesting ecology as an experimental and/or laboratory oriented science. Like their plant ecological colleagues the claimants of animal ecology were located in different academic cultures. The claims for ecology as a descriptive field science emanated from the naturalist milieu in Uppsala, while the claim for an experimental animal ecology were made in connection with physiological laboratories.

The scientific naturalist heritage in Uppsala

We recall that late 19th century field zoology, like its botanical counterpart, was an ambiguous mixture of investigations guided by the Darwinian approach to the biology and adaptation of animals and the study of their geographical distribution. This biological/faunistic tradition had been strongest in Uppsala, and it is not surprising to find that Uppsala zoologists took the lead in pursuing »proto-ecological« investigations, as well as translating these into ecology.

Compared to the situation in Stockholm and Lund, where none of the university zoologists conducted animal geographic investigations during the decades considered here,[220] the field activities among Uppsala zoologists were lively and accepted even when the hegemony of comparative anatomy was at its height. However, for a while it looked as if the Uppsalian field zoological tradition would decline too, when Lönnberg and Jägerskiöld, so active around the turn of the century, dispersed around 1905. Lönnberg was appointed to the chair in zoology at *Riksmuséet* in Stockholm in 1904, and Jägerskiöld was simultaneously appointed director to the natural history collections at the museum in Göteborg. Both made important animal geographic and faunistic contributions, but did not recruit Uppsala zoology students – besides they never claimed their

218. See e.g., Trägårdh 1928. Forsslund's and Trägårdh's series of papers from the 1930s culminating in Forsslund's dissertation of 1943 (cf.3-4) are referred to in Butovitsch 1952.

219. To complete the picture the teaching positions created for zoological purposes at the Ultuna agricultural school and at *Skogshögskolan* should be mentioned too. Herman Simmons, lecturer in agricultural botany and zoology from 1914 (professor 1918-1932) had been professionally trained as a botanist in Lund and did not pursue any studies of the kind searched for here; Gösta Grönberg (cf.1-1, note 69), who had been trained as a zoologist in Stockholm and appointed teacher in zoology at *Skogsinstitutet* in 1911 (in general and vertebrate zoology with game- and fishery management at *Skogshögskolan* 1915-1934), mainly devoted himself to writing articles in the journal of the Swedish Kennel Club.

220. Leche was succeeded by Nils Holmgren in 1920 (ED 31/12 1920:2). Holmgren never returned to the animal geographic investigations of his youth (cf.1-3), neither did his students. Hence the negative attitude instigated already in the 1880s by Leche against field studies of animals was continued. Stockholm zoology was dominated by comparative anatomical investigations during the following three decades. It is said that when Carl H Lindroth, one of the leading members of the third generation of ecologists (cf. note 222 and Ch.4-3) began his graduate studies in zoology in Stockholm suggesting insect geography as his dissertation subject, professor Holmgren expressed the opinion that: »the urge to do entomology, to collect beetles, is usually a passing phase«. Therefore Lindroth moved to Uppsala to write his licentiate, and later his doctor's dissertation. In Lund, Wilhelm Björck 's »proto-ecological« Öresund investigations (cf.1-3) had no immediate followers during the period considered here (cf., however, 3-4).

studies as ecological.

The leading claimant of animal ecology in Uppsala was, of course, Sven Ekman (cf.1-3). Ekman had been appointed as biology teacher in Jönköping in 1909, but returned to Uppsala in 1916 to take up a post-doctoral *docent* scholarship. Ekman was the most significant »proto-ecologist« and claimant of ecology in Uppsala during the period considered here. But while working on his treatise on the immigration history of the Scandinavian fauna, he remained isolated and after completing it in 1922 he suddenly turned to quite another kind of problems – for the one and only period in his life he made systematical and anatomical investigations, and rapidly published four voluminous revisions of museum material. Given the comparative anatomical tradition among Uppsala zoologists, the only reasonable explanation for this sudden interest in anatomy is that Ekman wished to qualify for the chair in zoology after Wirén.

The consequence from the point of ecologization was that Ekman only attracted a few students to work on »proto-ecological« or ecological problems during the 1910s and 1920s. Gunnar Alm's lake investigations which were closely connected to his career in the fisheries administration have already been mentioned above (2-1). Likewise Ossian Olofsson wrote on the relation between animal distribution and environmental factors. His dissertation was a direct continuation of Sven Ekman's dissertation of 1904:

> *»Meine Absicht war dabei, die Untersuchung so einzurichten, dass ich gewiss sein konnte, teils so weit möglich alle die Tierformen, die in einer Wasseransammlung vorhanden waren, zu erhalten, teils ihr resp. planktonisches, litorales etc. Vorkommen innerhalb derselben zu bestimmen, teils auch die hydrographischen, physikalischen und Vegetationsverhältnisse der Gewässer kennen zu lernen, um sie in Zusammenhang mit einander zu stellen«.*[221]

Similarly several dissertations contained longer or shorter references to »proto-ecological« investigations. But like Olofsson most of them never translated these studies into ecology. Some, such as Otto Lundblad and Carl H. Lindroth (cf.3-5 and 4-3), mentioned ecology in passing, without making too much fuss about it,[222] and only a few accepted the new terminology. One of those who did, at least partially, was one of the most influential field zoologists in Uppsala besides Ekman. Douglas Melin (1895-1946)[223] was the first academic zoologist to pick up the line from Adlerz in the 1880s (cf.1-3). In 1914 he began

221. Olofsson 1918,p.183.
222. Otto Lundblad, although referring to part of his investigations as »ökologischer und tiergeographischer Beziehung« in his dissertation of 1927, did not translate them to ecology; by ecology he only meant the fact that species are found in different environments; i.e., the ecological section of his dissertation was confined to a list of species found on different localities, an approach akin to what mid19th century field botanists were doing. Likewise Carl H. Lindroth, a former zoology student in Stockholm moving to Uppsala in the late 1920s (cf. note 220), later submitted a faunistic and animal geographic study, including a few ecological notes on »Die ökologische Bedeutung des Klimas« (pp.387-94) as his dissertation (1931). Lindroth is significant in this story, not only because of his family relations (one of his brothers, Arne, was a pioneer ecophysiologist in Uppsala in the 1930s (cf.3-4), another brother, Sten, was the late professor in history of ideas and learning in Uppsala (cf. Preface), but also because he is the only scientist in this story who made a university come-back after a long career as a secondary school teacher. Having had his dissertation down-graded by Sven Ekman and Douglas Melin, he had to relinquish his prospects of an academic career, and for twenty years he taught in secondary schools, playing a leading role in *Biologilärarnas förening* (the Biology Teachers' Association) in the late 1940s (cf.3-1), while continuing an extensive and pain-staking work on insect faunistics, systematics and ecological geography in his spare-time. He was finally appointed professor of entomology in Lund in 1951, and was an important actor behind the ecologization of zoology in Lund during the 1950s and 1960s (cf.4-3).
223. For biographical details on Douglas Melin, see Lundblad 1946 and ED 18/7 1942:2.

his investigations of all aspects of the life habits of a group of insects, including nutrient search, habitat choice, ektoparasites, larval development and movements, geographical distribution, etc., resulting in a very self-governed dissertation.[224] Being an accurate and conscious field observer (»In my studies in the open I have always noted down my observations *immediately*«),[225] he made field trials in order to elucidate the visual acuity of the flies, since he was of the opinion that it was a decisive factor in their ability to take their prey. Unlike Adlerz, who had been an enthusiastic Darwinian, Melin's observations made him suspicious of the Darwinian selection hypothesis, and during a three-year expedition to South America he studied different adaptation phenomena (like myrmekophory and ornitophily, and the relation between flowers and insect pollination), including food-ecology, in order to gather evidence against the selection hypothesis.[226]

Although Melin never made any programmatic claims for ecology as an independent science, he did consider some of his investigations to be ecological[227] and his contemporaries also sometimes considered them as such.[228] His importance for the growing social order of ecology was limited, however. He did not gather many students around him.[229] He was mainly considered an important critic, being in great demand as a dissertation opponent, and his sharpest points had a permanent effect on the discussion climate among Uppsala zoologists.[230]

Adolf Appellöf and the Klubban marine station

Sven Ekman's activities were undoubtedly ecological. But Ekman did not institutionalize any sustained ecological discourse in Uppsala during the period considered here. In fact, not even animal field studies were authorized in Uppsala – the zoologists had nothing akin to the chair in plant biology. However, with the appointment of Adolf Appellöf (1857-1921) to one of the two chairs in zoology at Uppsala in 1910[231] it seemed that the tradition for animal field studies, including work on problems of animal geography, would be revived and that Uppsala zoologists would get a counterpart to Rutger Sernander's seminar and excursions. Appellöf, who had written his dissertation for Tullberg back in 1886, was much older than any of the first generation of ecologists, and had made no studies of this kind while active in Uppsala in the 1880s. But in the 1890s, after being employed as curator, and later professor in zoology, at the Bergen Museum in Norway, he

224. D.Melin 1923.
225. D.Melin 1923,p.1.
226. E.g., he dismissed the idea of mimicry as a Darwinian adaptation; see D.Melin 1935.
227. In the foreword to one of his extensive investigations of the adaptation phenomenon Melin wrote: »In this work I shall chiefly deal with the food-ecology of the flower-birds, a knowledge of which is necessary for discussing the problem of ornitophily /i.e., a kind of adaptation/« (D.Melin 1935,p.5, engl.orig.).
228. Sven Ekman referred to Melin's studies of »ecological phenomena« (ED 18/7 1942:2) and his biographer says that »to him the physiological-ecological questions were the essentials« (Lundblad 1946,p.216).
229. One of his students, Tore Ekblom also pleaded for biological studies, but concentrated on anatomical and morphological problems, arguing that a good insight into biological conditions (i.e., locality choice, nutritional requirements, mating and egg-laying behaviour) necessitated basic morphological and anatomical information (Ekblom 1926). Compare this cautious attitude to biological/ecological problems with the unequivocal ecological attitude taken by Coulianos insect ecology group at the Department of Zoology in Stockholm some 40 years later: working with exactly the same species (Lygaeus), and also in an academic context, they translated their field observations of the insect into pure ecological questions in the 1960s (cf.4-3). Later, around 1940, Melin got a student among the third generation of animal ecologists, Bertil Kullenberg (cf.3-1).
230. Some thought he was »hypercritical« (cf. Gislén's assessment of Melin in ED 18/7 1942:2).
231. For biographical details on Appellöf, see Lönnberg 1920a.

utilized the rich fauna off the coast of Norway to write, not only a series of papers on comparative anatomy, but also to take up animal geographic problems. According to his junior colleague and biographer:

> *»At Bergen's biological marine station zoologists from different countries gathered in large numbers to study the marine fauna under his leadership, to such an extent that this place to some degree became an international center for marine biological research«.*[232]

For example, in a work on the decapod crustaceans of the area he gave rather extensive qualitative discussions of

> *»Faktoren, die für die Verteilung der Fauna des Meeresbodens Bedeutung haben... Druckverhältnisse, Bodenbeschaffenheit... Licht, Sauerstoff, Kohlensäure und andere chemischen Bestandteilen des Wassers etc.«,*[233]

although he did not try to establish the causal relation by experimentation.

When taking up his duties in Uppsala in 1910, he brought his environmental animal geographic thought with him. It was Appellöf who initiated Uppsala university's marine biological station Klubban on the Gullmar fjord[234] financed by a private donation. In 1915 Appellöf gave the first course in marine zoology including »excursions...with the aim to demonstrate different faunistic areas», investigations of «representative samples of the West coast plankton fauna», and observations on «some biological conditions of our more common animals».[235] In addition Appellöf and Sernander had some co-operation. In 1916 the two of them, the young Du Rietz, and another young zoologist made

> *»certain investigations of the fauna and flora of our littoral regions to a common, agreed plan«.*[236]

This was probably the first organized co-operation between environmentally inclined plant and animal geographers in the country.

As a consequence, a parallel to Sernander's seminar could have been created. His biographer notes that:

> *»At Uppsala university Appellöf has infused a lively interest for marine animal geography into those who studied under his leadership«,*[237]

thus bearing witness to Appellöf's enrolment power. However, Appellöf did not enrol students as effectively as Sernander. Nothing comparable to Sernander's vigorous seminar emerged, not to mention any discussions of principles of the kind advocated by «the gang». Actually, although some students touched upon (environmental) animal geographic and biological problems of marine animals, nobody submitted a dissertation

232. Lönnberg 1920a,pp.95-6; Lönnberg attributed Appellöf's animal geographic work to his own initiative. A Norwegian historian of zoology, on the other hand, maintains that Appellöf was engaged by the great Norwegian marine biologist Johan Hjort, who came to Bergen in 1900 as leader of *Fiskeridirektoratet* (Broch 1954); whatever the relation between Hjort and Appellöf, Appellöf's turn to environmental animal geographic problems was evidently connected to his work on fishery research problems; in addition to his academic research he also made »important investigations of the development of life habits of the lobster, which could lay a foundation for a rational regulation of the fisheries« (Lönnberg 1920a,p.96).
233. Appellöf 1906,p.199.
234. The Klubban station, established in 1915, was the second marine station after the Kristineberg station, and also located on the Gullmar fjord (*Kungl.univ. i Uppsala Redogörelse* 1915-1916).
235. Ibid., 1915-1916; the report also tells us that 12 students joined the course, «3 of them female»!
236. Ibid., 1916-1917.
237. Lönnberg 1920a,p.96.

on it during these years.[238] In addition, although later observers have interpreted his work as ecological,[239] Appellöf himself never translated his studies of the relation between animal distribution and environmental factors into the language of ecology.

When Appellöf died a few years later, Nils von Hofsten was summoned to the chair in zoology (comparative anatomy) in 1921.[240] Despite his earlier faunistic and even ecological animal geographic investigations at Spitsbergen (cf.1-3), von Hofsten did not promote any research concerning marine ecological or (ecological) animal geographical problems. However, the Klubban marine station still existed, and in principle provided ample opportunities for anyone who might want to emulate Appellöf's example.

A claim for animal sociology in Uppsala: Torsten Gislén.

Sven Ekman's field approach had parallels with that of his elderly colleague Rutger Sernander's. Both considered ecology a kind of «scientific natural history». In addition, in much the same way as Thore Fries and Einar Du Rietz translated Sernander's scientific natural history into plant sociology, Ekman's field approach was translated into animal sociology by one of his students, a «man of the 1910s». Having been interested in wildlife as a youngster, Torsten Gislén (1893-1954) was introduced to animal geographic and sociological problems at an early age.[241] In fact, Ekman was his biology teacher at the secondary school in Jönköping. Later he acknowledged Ekman's influence on him:

> *«as a young undergraduate, I was allowed to accompany S.Ekman... on his well-known bonitation expeditions on Lake Vätter».*[242]

Probably Appellöf's courses at Klubban stimulated him further. He did not write a dissertation in animal geography though. The anatomical pressure in Uppsala was strong, and Gislén solved a comparative anatomical problem for his dissertation in 1924. But right after finishing it, he went back to faunistics and animal geography. Financing his work with a post-doctoral *docent* scholarship he embarked on a six year long investigation concerning animal sociology problems. To start with he spent the summer of 1925 at the Plymouth Marine Biological Station for studies of echinoderms and the structure of animal communities of the seabed.[243] Back in Uppsala he began to lecture on «marine

238. There might be many reasons for this, ranging from Appellöf's friendly but hardly charismatic personality, to the fact that marine creatures are not very attractive objects for students who want to express their nationalistic feelings. Skogsberg's dissertation (1920) mainly took up systematical/taxonomical problems, although the author maintains that he had studied the »ecology« of the animals as well; likewise Nilsson-Cantell (1921) interspersed his mainly systematical study with occasional biological notes. There was one interesting exception, however. Hessle's (1925) contribution to the knowledge of the biology of a polychaete group was a continuation of his earlier dissertation work on the morphology, systematics and distribution of the group (Hessle 1917), but it reflected above all his experiences as a fishery biologist; in the 1925 paper he discussed their movements, tube building and nutrient uptake, noting, for example, that their »importance for the metabolism of the bottom is increased by their rapid digestion« (Hessle 1925,p.3). Note, however, that he never made any claims for ecology!
239. The Norwegian historian of zoology maintains that: «Appellöf not only confines himself to purely topographical results, but also gets on to ecological factors and circumstances, which build a bridge over to the strongly ecologically accentuated animal geographic research of our age /the 1940s/» (Broch 1954,p.115).
240. ED 29/4 1921:84.
241. For biographical details on Gislén, see ED 16/9 1932:88, Anon. 1954 and Hanström 1969.
242. Gislén 1929,p.1.
243. Gislén was of course very well aware of Petersen's investigations when coming to Plymouth, and he probably did not learn anything methodologically new there (cf. yearly reports from the station, published in *J.Marine Biol.Ass.*).

associations at the south coast of England», and a year later he started a series of divings to the bottoms of the Gullmar fjord in order to map the animal communities on hard bottoms. Of course, this was not a particularly new approach. He did approximately what C.G.J. Petersen in Denmark had done 15 years earlier when trying to quantify the production of seabed communities.[244]

Gislén could have translated his studies of seabed communities into ecological animal geography, like Ekman had done. But he seemingly had other intentions. «The investigations», he said,

> *«were undertaken, in the first place, in order to ascertain the occurrence and distribution of animal and vegetable associations on hard bottoms of the Gullmar fjord... I was interested in an equally high degree in the problem of discerning the quantitative production of every association».*[245]

Thus, he did not approach the bottom communities as a fishery biologist, but he did not claim his studies as ecological either. In fact, he did not even investigate the relation between environmental conditions and the distribution and production of the communities. His two-volume *Epibiosis of the Gullmar fjord* (later called «the greater and smaller pompous trash» by some Swedish zoologists) was a claim for a new specialty – animal sociology. As indicated by its subtitle («a study in marine sociology») he tried to do for animal sociology[246] what Du Rietz had done for plant sociology. His interest in a sociological perspective was (according to his own account) aroused when studying botany in Uppsala during the war, and coming in contact with Sernander's seminar.[247] It was not a very original claim, though. In all but terminological detail Gislén's claim for animal sociology was a copy of his plant sociological prototypes 500 meters down the Villavägen Road in Uppsala. Like Du Rietz he gave a long historical introduction to the litterature on animal communities; he used Du Rietz's minimum quadrat method; he identified himself to the inductive philosophy; and he embraced sociology to the neglect of synecology.[248]

Gislén did not entirely rule out the study of the effect of environmental conditions on the structure of animal communities, however. When making similar investigations of the marine associations off the coast of Japan a few years later he paid great attention to environmental conditions.[249] And later, in the 1930s, he sometimes even discussed the distribution of animals in terms of ecology (see 3-4). From the viewpoint of the late 1920s, however, Gislén's field work, like Du Rietz's analyses of plant communities, could hardly be construed as a claim for ecology. In fact his claim for animal sociology seemed to imply a de-ecologization of animal field studies. On the other hand, this de-ecologization

244. E.g., Petersen 1911, 1913, 1918. Arvid Molander (cf. note 213), a fellow student of Gislén, had pursued rather similar investigations for *Svenska hydrografisk-biologiska kommissionen* in the mid-1920s. Molander made «a close study of the organization of the various communities as to what degree they are dependent on hydrographical conditions and minor variations in the nature of the bottom deposits» (Molander 1928,p.1, engl.orig.). but approached the problem of animal community structure as a fishery biologist, and never claimed these studies as ecological.
245. Gislén 1929,p.62 (engl.orig.).
246. Gislén actually investigated bottom plants as well, but his training as a zoologist, and the fact that the bulk of bottom organisms are animals, legitimizes the notion of animal sociology.
247. «Under his /Sernander's/ leadership the students were taught not only to search for and observe plants separately, but also to consider them in connection with the rest of the vegetation. Thus a keen eye was developed for seeing plant communities» (Gislén 1929,p.1, engl.orig.).
248. «I am of the opinion that the association ought to be physiognomically determined type, prof. H./i.e. Holmgren/ that it shall be 'a combination of species which give the community an ecologically (physiologically) determined type'» (Gislén 1931b).
249. Gislén 1931a.

had little impact. Unlike Du Rietz, Gislén worked alone in a temporary position. He had no Rutger Sernander to help him, and he did not possess Naumann's charisma or flair for self-promotion – thus he had little opportunity to expand his work into an actor network. Actually no students in Uppsala followed him. When applying for the chair of zoology in Lund in 1930, he was ranked far below the top candidate and the assessors were rather sceptical of his work, particularly *Epibioses*. They emphasized his lack of scientific stringency, his vague community concept etc., while on the other hand applauding his energy. His former teacher Ekman, a member of the assessment committee, noted, somewhat sarcastically, that «the merits of the work seems to me to lay mainly in the material collection itself».[250]

Experimental approaches to animal ecology

In Sweden, as well as abroad, claims for (ecological) animal geography and animal sociology, forwarded during the two first decades of the 20th century, were belated mirror-images of their botanical counterparts. But what about experimental and laboratory oriented claims for animal ecology? In fact, during the two decades considered in this chapter, only two minor claims for an experimental approach to ecology were forwarded by men trained in the «new German» zoology in Stockholm and Lund respectively.[251]

The one was made by the emerging group working on embryological and cell physiological problems led by John Runnström (1888-1971)[252] at the Department of Zoology in Stockholm in the late 1920s. Runnström epitomizes the progressive character of the «new German» zoologists. It is interesting to note the remarkable parallels between Runnström's and Lundegårdh's biographies. Runnström was born in the same year as Lundegårdh; both were of petty bourgeois family background (R. was the son of a bakery owner); both went to Stockholm university; both were pioneers with regard to cell research and physiological problems; later in life both fought shoulder to shoulder against scientific traditionalism and for experimental science. Quite early Runnström approached problems concerning the relation between environmental factors and animal form and function.[253] Suspecting that the morphological variation within a species of sea urchins was due to the variation in water salinity, he handed over the investigation of the possible physiological mechanism involved to one of his early students, Per Eric Lindahl (b.1906). The results were published in 1929 in an article entitled «Variation und Ökologie von *Psammechinus miliaris*».[254] A few years later two younger students of Runnström

250. Ekman's assessment in ED 16/9 1932:88.
251. None of the Uppsala laboratory zoologists took up ecological problems during the first three decades of the century. In fact, no Uppsala zoologist worked seriously on ecophysiological or even physiological problems during the 1910s or 1920s. In the 1930s Arne Lindroth was the first Uppsala zoologist to take up experimental studies, and also to translate these into the language of ecology (cf.3-4). Sixten Bock, who used to work on anatomical and systematical problems (cf. note 264) made a pioneer contribution by initiating physiological courses for Uppsala zoology students (see *Kungl.univ. i Uppsala Redogörelse* 1922-23,p.117).
252. One of the first younger members of the group was Sven Hörstadius, later professor of zoology in Uppsala (cf.3-4 and 4-3) and Per Eric Lindahl.
253. See e.g., J.Runnström 1916; we recall that Runnström had published a faunistic undergraduate thesis in 1909 (cf.1-3, note 261).
254. Lindahl and Runnström 1934. Their aim was: «Wir wollten bestimmen, von welchen Faktoren das Auftreten und das Verhalten dieser Tiere in der Natur bestimmt wird» (p.401).

published a similar paper.[255]

With regard to the scope of ecology Runnström and his students parallelled that of the plant ecophysiologists, particularly Lundegårdh:

> *«Vor allem wurde uns der Character der Ökologie als angewandte Physiologie immer klarer. Wo die physiologischen Grundlagen nicht schon vorhanden sind, müssen dieselben erst geschaffen werden».*[256]

However, when asked if the paper was initiated by studies of ecological literature or by contacts with other Swedish ecologists, Lindahl answered:

> *«Not at all... I began with it in 1926 and it was finished in 1928...1929. Early in the spring of 1929 it was completed. Then I had no contacts with anybody at all. I don't know if Runnström had contacts with any ecologists. Were there any ecologists at all in Sweden at that time?»*[257]

He then added: «I first met /the term ecology/ when I came to Naples...in 1929», that is, just before the article appeared in print.

In Lund, however, Gustaf Alsterberg (1892-1970) made a serious and ambitious attempt to establish experimental animal ecology on the academic agenda.[258] In 1919 he was attached to Hans Wallengren who, as will be recalled (1-2), was a pioneer of studying animal physiological problems in Sweden, having studied mussels' filter feeding around 1905 and having introduced physiological experiments in the zoology curriculum. One of Alsterberg's graduate colleagues, Elias Dahr, began studies of the respiration of land snails in 1921 and performed a series of pure laboratory experiments,[259] while Alsterberg himself started a series of investigations of the respiratory physiology of tubificids, a study resulting in a doctoral dissertation only three years later. It had the significant subtitle «eine experimentell-physiologische Untersuchung auf ökologischer Grundlage». Alsterberg's aim was:

> *«to make a simultaneous study of the ecological character of the habitat and the reciprocal adaptations of the organisms examined».*[260]

Alsterberg's approach was close to Lundegårdh's idea of moving the laboratory out into the wilds (it is not unlikely that the two men had contacts when Lundegårdh resided in Lund around 1920). Indirectly criticizing Wallengren and Dahr, he rejected physiological investigations of animals isolated from their natural environment; hence he translated a physiological problem into an ecophysiological one.

After finishing his dissertation, Alsterberg embarked on a somewhat different trail. Being endowed with a post-doctoral *docent* scholarship, he took up the same kind of freshwater ecophysiological investigations which Naumann had pursued in 1917-1920 when working as an assistant to Lundegårdh only to abandon them in favour of lake type

255. Borei and Wernstedt 1935; yet another graduate student, M.Koffman, studied the relation between microfauna and soil, in close co-operation with professor Barthel at *Centralanstalten*. It was not translated into the language of ecology, however.
256. Lindahl and Runnström 1929,p.401.
257. Interview with Per Eric Lindahl 12/8 1982.
258. For biographical details on Alsterberg, see ED 16/9 1932:88 and ED 18/7 1942:2.
259. For an evaluation of Dahr's work, see ED 30/6 1948:4. Somewhat earlier Wallengren urged a student to write a dissertation on »the biology and physiology« of ophiurids (Wintzell 1918). For each species he made qualitative notes on its occurrence, its pattern of movement, and its choice of food (by means of qualitative stomach analysis). However, neither Wintzell, nor Dahr, translated their observations into the language of ecology.
260. Alsterberg 1922,p.170.

classification studies (cf.2-1). For several years Alsterberg studied nutrient circulation and oxygen balance in lakes,[261] besides giving lectures on limnic ecology and physiology. He also published some works on the biology, nutrition and distribution of freshwater snails, including their respiratory adaptation to different environments, a study which might be evaluated as the most advanced autecological work by a Swedish zoologist before the 1950s.[262] If Runnström's and Lindahl's studies parallelled those of Lundegårdh, Alsterberg was in many respects the zoologists' counterpart to Lars-Gunnar Romell.

Alsterberg's work attracted great attention among some of his senior colleagues. Sven Ekman, who had a keen eye for the environmental conditions for the distribution of animals, considered his physiological experiments «an extra-ordinarily prominent physiological-ecological achievement» and referred to the international (German) recognition of «der Alsterbergschen Fundamentalversuch».[263] But apart from being recognized as an ecologist by and then, Alsterberg never made any strong claims for ecology as an independent science, and never succeeded to enroll any students to his programme during the 1920s. He remained the lone wolf of experimental animal ecology.

A small - and indirect - polemic of animal ecology

Within the space of a few years of the late 1920s all the main actors presented above applied for vacant chairs in zoology. Hence their «proto-ecological» achievements were subjected to peer evaluation, and this involved some consideration of the value of animal ecology and animal sociology as well.

The first competition touched upon the value of «proto-ecological» qualifications. As indicated above Sven Ekman had spent a couple of years seeking to qualify for the vacant chair in general zoology in Uppsala. The three assessors were all specialized in comparative anatomy and therefore gave priority to a comparative anatomist;[264] a decision which naturally provoked not only Ekman's, but also Douglas Melin's anger.[265] Usually such appeals did not have any effect – but in this case they did. *Större akademiska konsistoriet* actually disavowed both *matematisk-naturvetenskapliga sektionen* and the assessment committee (comprising the country's leading comparative anatomical troika), and changed the priorities.

In the Great Polemic (cf.2-4) the minority support for Lars-Gunnar Romell had been based on the argument that his ecological orientation was better suited to the idea of the donation chair in plant biology. Similar arguments were mobilized in the present competition; at least one member voted for Ekman on the grounds that his «scientific achievements bear witness to a wider biological view of the subject /i.e. zoology/». Thus, thanks to this intervention from the larger academic community, Ekman was appointed to the chair in 1927, and for the first time since the triumph of the «new German» zoology in the

261. His first main work (Alsterberg 1925) was followed by several papers on thermal and chemical layering in lakes.
262. Alsterberg 1930.
263. Ekman's assessment in ED 16/9 1932:88.
264. Sixten Bock (cf. note 251). For details of the assessments and votes, see ED 20/7 1927:2.
265. Ekman and Melin both wrote protest appeals against Bock whom they considered a mediocre scientist; his only qualification, they said, was that he was a comparative anatomist.

late 19th century a field zoologist, and persistent claimant of ecology was appointed to a university chair in zoology. But otherwise the appointment of Sven Ekman should not be interpreted as a deliberate intervention in favour of ecology. Ekman's ecological orientation was rather seen as part and parcel of his originality, versatility and independence.

The other competition parallelled the Great Polemic, although on a smaller scale. Both zoology chairs in Lund became vacant within two years. The one chair was reserved for a comparative anatomist,[266] while the other was open for all other zoological specialties.[267] By rights John Runnström was given highest priority, but at the last moment he declined in favour of a personal chair in experimental zoology and cell research in Stockholm in 1932. Runnström's withdrawal gave a chance to those who had been ranked below him. Among them were Torsten Gislén and Gustaf Alsterberg – the one having translated animal field studies into animal sociology, the other into ecophysiology. The assessors were deeply divided. While praising Alsterberg's studies of the interaction between tubificids and lake metabolism as «an extremely important ecological description» one assessor nevertheless dismissed him on the ground that he fell outside the delineation of zoology. A majority of the assessors, *matematisk-naturvetenskapliga sektionen* and *större akademiska konsistoriet* followed this line – Alsterberg's studies of the physical-chemical lake environment were not accepted as qualifying him for a chair in zoology. This implies that «proto-ecological» or ecological studies were accepted only insofar as they unequivocally focused on the organism. Sven Ekman, on the other hand, also being among the assessors, gave Alsterberg top priority, emphasizing his originality and profundity.

In contrast, Ekman found many faults with Gislén's output and had Alsterberg not been excluded with reference to the delineation of zoology, Gislén would probably not have been the front-runner. Particularly the assessors disliked his *Epibiosis*-investigation, finding it lacking in cogency. Gislén was even criticized for not paying due regard to the ecological dimensions. As pointed out above, Gislén utilized the inductive principle of the Uppsala school to determine animal communities. Against the criticism that he had failed to discern the communities on ecological and physiological criteria, Gislén responded that ecologically determined communities «only existed in abstract» but were impossible to deal with in practical research.[268]

Alsterberg and Gislén never engaged in any mutual polemic. Their respective approaches to the field study of animals were only contested indirectly, through others. Nevertheless it is not far-fetched to compare the competition between them with the Great Polemic. Gislén's descriptive, inductively based animal community studies and claim for animal sociology were closely modelled on the research program of the Uppsala school. Alsterberg's experimental ecological investigations was much more akin to the program of the Stockholm school of ecophysiologists. The Lund university council's preference for Gislén parallelled that of the Uppsala council's vote for Du Rietz. In that sense, the translation of animal field studies into ecophysiology and hence the claim for an experimental and laboratory oriented ecology was dismissed.

266. Bertil Hanström, who had studied with Leche and Holmgren in Stockholm (ED 19/12 1930:2).
267. See ED 16/9 1932:88.
268. Gislén 1931b. (ED 19/12 1930:2).

With the appointment of Sven Ekman and Torsten Gislén animal «proto-ecological» studies were promoted in Uppsala and Lund, implying a breach in the hegemony of the comparative anatomists over the social order of zoology in Sweden. But did these appointments also imply a step towards the establishment of ecology as an independent social order?

Firstly, it should be remembered that Ekman's and Gislén's appointments were not a result of a deliberate university policy; on the contrary they were something of chance events. Secondly, neither Gislén nor Ekman were appointed primarily on their «proto-ecological» qualifications. Gislén's *Epibiosis*-investigations were a liability to him, and Ekman's field-zoological qualifications were hardly mentioned by the assessors. It is clear from reading the assessment reports, that the leading zoologists mainly evaluated them according to their qualifications in dealing with comparative-anatomical problems. Finally, and most important, the denomination of the chairs were not changed. They remained chairs in zoology. Even though, for the time being, they were filled with men who devoted themselves to animal field studies (and in the case of Ekman also claimed ecology as an independent science) the appointments of Sven Ekman and Torsten Gislén to the zoological chairs at Uppsala and Lund in 1927 and 1932 respectively did not signal the authorization of ecology at the institutions of higher learning.

On the other hand, both Ekman and Gislén came to enrol students during the 1930s and thus acted as points of departure for new claims for animal ecology in the 1930s and 1940s. Some of their students were among the main actors behind the institutionalization of animal ecology in the post-war period. In that sense their appointments were important historical preconditions for the emergence of the social order of (animal) ecology in Sweden.

2.6 The second generation of ecologists : concluding remarks

By the end of the 19th century Swedish «proto-ecology» was a marginal phenomenon at the institutions for research and higher learning. Thirty years later a significant number of scientists trained as botanists and zoologists worked on «proto-ecological» investigations. Within institutionalized forestry and fishery research more than a dozen botanists and zoologists pursued «proto-ecological» investigations, and at the universities in Uppsala, Lund and Stockholm several chairs were filled with men advocating studies of the relation between organisms and their environment. By the end of the 1920s, «proto-ecological» investigations were increasingly being accepted as a regular part of the academic task.

Similarly, compared to the hesitant and passing references to the new term of ecology made at the turn of the century, the new claims for ecology forwarded by a second generation of ecologists during the 1910s and 1920s were more distinct and aggressive. During the late 1910s and 1920s an ecological discourse was established within botanical and zoological circles at the universities. These claims were mainly forwarded in an academic context – «proto-ecologists» working with agricultural, forestry and fishery problems did, as a rule, not translate their work into the language of ecology. The only significant exception was Lars-Gunnar Romell at *Statens skogsförsöksanstalt*, whose claim for ecology was made in a shared practical and academic context.

The 1910s and 1920s also saw the first discussions of the scope of ecology as a scientific specialty. The advent of an ecological discourse signals a first step towards the ecologization of Sweden. Internationally, the works of the Swedish ecologists (and «proto-ecologists») were usually recognized as ecological by other ecologists. For example, in Clements' and Shelford's textbook *Bio-ecology* of 1939, the first comprehensive textbook covering both animal and plant ecology, a few of the men discussed above were referred to, viz., Appellöf, Ekman, Du Rietz, Gislén, Molander, Naumann and Romell.[269] But while British and American ecologists took decisive steps towards the institutionalization of the new discipline during the time period considered in this chapter, that is, from the early 1910s to the early 1930s, the Swedish attempts to institutionalize the emerging academic ecological discourse were largely unsuccessful.

Firstly, the claims for ecology were often not even sustained by the claimants themselves. Henrik Lundegårdh withdrew into biochemical investigations; Göte Turesson into studies of systematics; Elias Melin turned more and more into pure physiological problems; Thore Fries became increasingly critical of synecological analysis before his premature death in 1931; Einar Du Rietz was mainly engaged in plant community nomenclature and the promulgation of the social order of plant sociology; the Runnström group, who had made some flirtations with the ecological rhetoric around 1930, went further into pure physiological and later biochemical problems. And to put the seal on the miseries: Einar Naumann, the Swedish founder of limnology and later source of synecological thinking, committed suicide in the autumn of 1934.

Secondly, those who upheld the claims for ecology were not in a position to institutionalize their claims. Although Lars-Gunnar Romell and Gottfrid Stålfelt continued to claim their studies as ecological, they held subordinate positions: Romell returned from the United States to take up a position as scientific officer at *Skogsförsöksanstalten*, and Stålfelt was appointed associate professor in botany in Stockholm. The only serious experimental animal ecologist during the period considered here, Gustaf Alsterberg, lost the competition for the zoology chair in Lund and left the university to become a secondary school biology teacher.

Finally, the appointment of an ecologist, such as Sven Ekman, to a university chair did not immediately result in the authorization of ecology as a new academic social order. Ekman considered much of his work as ecological, but never attempted to rename the chair.

On the other hand, although the claims for ecology forwarded during the 1910s and 1920s did not result in the authorization of ecology as a new scientific discipline at the universities, several of the men considered here, such as Einar Du Rietz, Sven Ekman and Torsten Gislén, came to enrol large numbers of students to «proto-ecological» and ecological studies during the vital 1930s, and thus came to act as points of departure for a

269. Du Rietz was referred to for a very a-typical work on the life-forms of terrestrial flowering plants (Du Rietz 1931b); in addition works by G.Lundquist, a student of Naumann in Lund (not taken up here), and the chemist and plankton specialist P.T.Cleve (cf.1-3, note 247), were mentioned.

slowly growing social order of ecology in the 1930s and 1940s. These graduate students, in turn, were among the main actors behind the establishment of ecology as an authorized social order in the post-war period. In that sense, the first and second generations of ecologists laid the foundations for the impressive post-war development of ecology in Sweden.

3 Towards a national social order of ecology

Seen from the time-horizon of the early 1930s, the future still seemed rather uncertain for ecology as a new scientific social order in Sweden. As we have seen almost all claims for ecology forwarded in the 1910s and 1920s had been disclaimed or simply not sustained, and hence Swedish ecology remained just one among many botanical and zoological subspecialties. Particularly, no ecologists had succeeded in institutionalizing their claims, not to mention getting it authorized. Yet, during the two decades to follow a growing number of scientists again identified themselves as ecologists. The topic of this chapter is the new wave of claims for ecology appearing during the 1930s and 1940s, and their subsequent institutionalization and authorization, including the foundation of a national ecological society and a journal for ecology.

The institutionalization of Swedish ecology was far behind that of British and American ecology.[1] Ecology was established and recognized as an institutionalized social order in Britain and in the United States by the 1920s. In the early 1930s another two ecological journals appeared. The *Ecological Monographs*, published by the *American Ecological Society* from 1931, was dedicated particularly to more extensive articles on community ecology. The *Journal of Animal Ecology* was launched by the *British Ecological Society* in 1932, with Charles Elton as its prime mover and first editor.

American ecology was still dominated by community studies permeated by holistic thinking. A contemporary observer characterized it using the following key-words: »integration, correlation, coordination, synthesis, holistic, emergent and relationship«.[2] Plant and animal ecology made certain important rapprochements. A conference titled »Plant and Animal Communities« in 1938 was at the time considered to be »the first ambitious attempt to arrange a general public stock-taking of ecology«.[3] Likewise, Clements and Shelford, two of the leading American ecologists, tried to synthesize animal, plant and aquatic investigations in their seminal textbook *Bio-ecology* of 1939. On the other hand, there was also a continual, though not so dominating, concern for autecological and population studies. The first encyclopedic attempt to review animal ecology, Allee *et al*'s *Principles of Animal Ecology* of 1949, written by five established Chicago animal ecologists, contained a bulk of autecological and population ecological material. A corresponding growing concern for autecological studies of plants was summarized in a textbook by Daubenmire in 1947, *Plants and Environment: a Textbook of Plant Autecology*. McIntosh summarizes the result of the institutionalization of

1. The following overview is mainly based upon Allee *et.al.* 1949, McIntosh 1977, and Duff and Lowe 1981.
2. According to Taylor 1936, quoted in McIntosh 1977,p.357.
3. According to Allee 1939, quoted in McIntosh 1977,p.359.

American ecology from the 1920s to the late 1940s:

> *»By mid-century ecology was established widely, if not universally, in the academic world of the United States as a science with its own concepts, techniques, societies, publication outlets, basic specialized literature, influential textbooks, and an embarrassment of terminology for which it was roundly criticized«.*[4]

British animal ecology also gained ground during the 1930s. In 1932 it was institutionalized within the *Bureau of Animal Populations* in Oxford, headed by Charles Elton, who had written several books on the subject, e.g., *The Ecology of Animals* (1927). For plant ecology the years between the wars have been characterized as »a hiatus«;[5] even the grand old man of British plant ecology, Arthur Tansley, discouraged students from specializing in the subject. There were no positions for plant ecologists, though some courses in ecology were introduced into botany programs. With regard to textbooks, British animal and plant ecologists were not as prolific as their American colleagues. But although institutionalization was not as extensive as it was in the United States, the British ecologists nevertheless still surpassed all other countries. An important step towards institutionalization was the foundation of the *Nature Conservancy* in 1949, an outcome of the concern for the countryside and natural areas associated with the general nationalistic feelings during the war.[6] This governmental agency was made responsible not only for the designation and management of nature reserves but also for promoting ecological research.

From a cognitive point of view the inter-war period saw an increasing theoretical maturity of the field, and a number of attempts to unify it as a science. The same year as Clements and Shelford published their textbook on general ecology, August Thienemann in Germany claimed the theoretical foundation for an »allgemeine Ökologie«.[7] Somewhat earlier his compatriot Karl Friederichs had tried to establish the foundation for »Ökologie als Wissenschaft«.[8] Some contemporary observers even saw a commencing specialization within the new discipline:

> *»Ecology as a separate branch of biology arose in the eighteen-nineties, and your speaker this evening has seen its growth from the rather simple 'natural history' of two generations ago to its present expanded state wherein the ecologist, who is an outdoor man, may have comparatively little in common with his biological colleague of the laboratory, and may even speak a different language from that of his brother ecologist who works in some other field than his own«.*[9]

And looking back over the 1930s and 1940s, Dice concluded:

> *»The science of ecology has now approached, if it has not already reached, an early state of maturity. Rarely heard nowadays is the irritating statement that ecology is not a science, but only a point of view«.*[10]

4. McIntosh 1977,p.360.
5. Duff and Lowe 1981,p.143.
6. Duff and Lowe 1981,pp.144-46.
7. Thienemann 1939; according to Thienemann ecology should not restrict itself to be »eine biologische Fachdisziplin«, but should take the task of becoming a »überfachliche, verbindende Naturwissenschaft«, a new queen of the sciences (cf.4-5). Thienemann quoted Friederichs: »Gegenstand der Ökologie ist die Naturwissenschaften überhaupt, aber nicht ihr gestriger, abstrakter, summativer Begriff, sondern die Naturwissenschaft als Gestalt, als Einheit« (Friederichs, quoted in Thienemann 1939,p.279).
8. Friederichs 1934 and 1937.
9. Ramaley 1940,p.4.
10. Dice 1955,p.346.

Thus, on the international scene, the ecologists were now beginning to enact the second stage of the ecological drama – that of a paradigmatic discipline in the sense of the Starnberg school of sociology of science.[11]

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What about Swedish ecologists? Were they trying to enact the same drama as their British and American colleagues?

The third generation considered here consisted of men, most of them born around 1910-1920, who formed their basic intellectual and scientific outlook during the 1930s and early 1940s. Among them were Carl. H. Lindroth (1905-1979), who was elected first chairman of the society *Oikos* in 1948; his brother Arne Lindroth (1910-1985), who much later became the first professor in ecological zoology; Stig Waldheim (1911-1976), who created the first plant synecology group in Lund in the late 1940s; Ivar Agrell (1912-1973) and Helge Backlund (1913-1974), who were the most ardent ecologizers among the Lund zoologists in the 1940s. Wilhelm Rodhe (b.1914), who was an important policy maker for ecology in a post-war university commission; Hugo Sjörs (b.1915), who later succeeded Einar Du Rietz on the chair of plant biology in Uppsala, and Bengt Pettersson (b.1915), who later became the first professor in ecological botany; Carl Olof Tamm (b.1919), who eventually institutionalized the tradition of Hesselman and Romell into forest ecology; and finally the coming »Godfather« of Swedish ecology, that is, Per Brinck (b.1919), the most influential policy maker for ecology in Scandinavia during the 1960s.

Most of the decisive ecologizing achievements of »the men of the 1930s« were made in the 1950s and 1960s. They were hardly more than secondary school students or freshmen when the Great Polemic was settled and the greater part of the previous generation of ecologists lost its enrolment power. Much of the new wave of ecologization in the 1930s and 1940s was still the result of the actions of the two earlier generations. Some of »the men of the 1910s«, like Elias Melin, Lars-Gunnar Romell and Gottfrid Stålfelt, being at the height of their academic careers, continued to be important ecologizers. Likewise the ageing Sven Ekman and Rutger Sernander exerted a considerable influence. But »the men of the 1930s«, though gaining inspiration and help from their predecessors, did not follow passively in their wake. They had to stand on their own feet. In that sense their claims for ecology in the 1930s and 1940s were independent achievements.

To a certain degree this independency applies to their relation to international ecology as well. Academic Sweden suffered from a degree of national complacency, echoed in the following quote from a contemporary university commission:

> *»For the last fifty years our Swedish university organization has been able to display a unique and magnificent development... Nowadays our universities are also fully equal partners in the international community of scientific interest«.*[12]

In fact, this self-congratulatory attitude reflected a certain isolation from the internatio-

11. See Böhme *et.al.* 1973.
12. SOU 1937:36,p.2.

nal scene, which was largely true for the new claimants of ecology in Sweden too. They did not embark upon intensive travels in order to learn from their international peers, like earlier generations of zoologists and botanists had done when going on pilgrimages to the exemplary German anatomical, cytological and physiological laboratories. If they turned their eyes abroad, they saw Germany and German scientific culture, still the dominating ideal for Swedish zoologists and botanists. Not only was the whole Swedish university organization modelled on its German counterpart, but zoologists and botanists, with few exceptions, wrote in German, and had personal contacts with German scientists. Hence »international community of scientific interest« should actually be read »German-speaking scientific community«. Some of them applied the ecological terminology of German ecologists such as Hesse and Friederichs. This germanized national complacency was probably detrimental to the progress of ecology as an academic social order in Sweden. Germany was not a leading nation with regard to ecology.

In contrast the new ecologists had virtually no contacts with Anglo-Saxon ecology. The institutionalized and productive American and British ecology were hardly noticed by the third generation of ecologists. Several of the interviewees active in the 1930s, for example, referred to Hesse's 1924 book on ecological animal geography, while none referred to Elton's 1927 book on animal ecology. None of them took up Anglo-Saxon population ecology. Likewise Clements' extensive writings in plant ecology went largely unnoticed.[13] Hence with regard to their international orientation the third generation of ecologists were very much young Swedish intellectuals of their age.

In the following sections we will analyze the claims for ecology forwarded by these »men of the 1930s«, and their early attempts, together with their predecessors, to institutionalize ecology during the 1940s. First, in Section 3-1, we will focus on the importance of the revival of the naturalist movement both for the recruitment of the third generation, and for the ecologization of the secondary school curricula. Sections 3-2 to 3-4 deal with the extent of »proto-ecological« investigations and claims for ecology, considering plant ecologists and animal ecologists, and »synecologists« and »ecophysiologists« separately. In Section 3-5, finally, we will discuss the institutionalization and commencing authorization of ecology as a result of the interventionist research policy of the post-war state.

3.1 »The men of the 1930s« as intellectuals of a growing naturalist social movement

In earlier chapters we have discussed ecologization as the translation of »proto-ecological« studies to scientific ecology. Having noticed that »proto-ecological« studies oriented towards practical purposes were of limited importance as a prerequisite for ecology, we concluded that the claim for ecology as an independent scientific social order was mainly

13. Bengt Pettersson whose approach more resembles Anglo-Saxon ecology than any of the others at »*Växtbio*« (cf.3-2) does not seem to have received any distinct impulses from abroad. Tamm writes in his assessment of Pettersson that he is relatively unacquainted with literature which does not touch upon specific Gotland problems (ED 16/9 1966:17).

made in an academic context. Furthermore, the ecologization at the universities should largely be seen as a process of translation of naturalist interests into the symbolic social order of ecology. During the time period considered here, that is, the 1930s and 1940s, the number of students with a naturalist interest was growing, as were the extent of »proto-ecological« investigations, and the number of claims for ecology. In this section we will evaluate the growing naturalist social movement as the background for the renewed claims for and institutionalization of ecology.

The amateur naturalist revival

If the coming third generation of ecologists should be described as intellectuals, their leading ideology was that of an accentuated love of nature. Political ideologies were not without importance. Like the previous generation, »the men of the 1930s« could not perhaps see through the large scale cultural movements of their own time, but they were not unaffected by them. Being born around the First World War implies that they were adolescents during the »hesitant and limping« 1920s, and that their intellectual orientations were formed in the 1930s. Some of them participated in the great ideological struggle of the inter-war period between Fascism and democracy. Fascism never grew strong in Sweden, but it existed, and the universities were one of its strongholds. Einar Du Rietz in Uppsala had Nazi sympathies, and so had Sven Thunmark in Lund, only to mention two of the more prominent figures in the story to follow.[14] Such ideological antagonisms were probably not without consequences for the scientific world. One interviewee, a student of Du Rietz, maintains that

> *»we made objections. Yes, I suppose that I became more heated in my opposition to Du Rietz's theories too«.*[15]

Likewise the competition for the limnology chair in Lund in 1945-1947 was deeply polarized by Thunmark's ideological commitments.[16]

However, it was not political ideologies, but attitudes and feelings towards nature which were of decisive importance for the intellectual socialization of this generation. With virtually no exception, they had started as naturalists, most of them during their school years or even earlier. Ivar Agrell's interest in nature, for example, was already fully developed by the age of 12. Bengt Pettersson, who became professor in ecological botany, tells that:

> *»I lay reading popular biology books at night... To be sure, when I was say 15, then I was a biologist already, I was an intense zoologist, I studied ant ethology... I was not more than 13-14 years old«.*[17]

Carl H. Lindroth's biographer remarks:

14. Du Rietz's and Thunmark's Nazi sympathies are beyond doubt, having been confirmed by a number of independent interviewees.
15. Interview with BP 7/2 1982.
16. E.g., Gustaf Alsterberg withdrew his application to the chair with a bitterly formulated letter accusing the assessors and university authorities of having decided the outcome in advance and of being governed by German interests (see »Skrivelse till Konungen« dated 2/1 1946, in ED 17/1 1947:1). Whether Alsterberg was right or not is of less importance – his reaction reflects more general sentiments for and against Nazism at the time. I will not go into any greater detail with the issue, however – the extent of Fascist penetration into Swedish academic life and its effects on scientific work have yet to be studied.
17. Interview with BP 7/2 1982.

»Lindroth was a child of his age: he had a keen interest in animals and plants and their environment. As a schoolboy, and later as a student and a teacher he was always broadening his knowledge and his views, aided by an ever-lasting and indefatigable curiosity of the variety and mechanisms of nature. The versatile work – and broad analysis of biological phenomena – which he carried out during half a century of scientific activity was based on field work«.[18]

Many similar testimonies could be quoted.

Of course, interest in field biological observations at an early age was not new. Popular natural history interest had a long tradition stretching back to Linné's *herbationes* and Sven Nilsson's requests to Swedish hunters to provide him with natural information. Adlerz's biographer has given a picturesque account of how

»the lad could sit for hours lost in observing the small brown ants in his father's garden«.[19]

That was in the 1860s. For a long time, however, interest in nature was unorganized. A pioneer initiative with regard to the organization of naturalists was Einar Lönnberg's foundation of *Göteborgs biologiska förening* (the Biological Association in Göteborg) in 1904,

»an association of those who love nature with its diverse richness of fauna, plants and beauty«.[20]

Göteborgs biologiska förening gathered members of the new upper classes - wholesalers, chief physicians, engineers, etc. – to lectures and excursions. As already indicated (1-3) Lönnberg, and his colleague Jägerskiöld, unfolded a fabulous panorama as faunistic popularizers during the 1910s, 1920s and 1930s from their positions at *Naturhistoriska riksmuséet* and *Naturhistoriska muséet* in Göteborg, respectively. Lönnberg's journal *Fauna och flora*, and a number of faunistic books,[21] helped to bridge the remnants of the 19th century natural history tradition and the naturalist revival in the 1920s and 1930s.

But even though the naturalist tradition had continued unbroken since the days of Linné it is nevertheless justifiable to talk about a revival of interest in nature during the inter-war period. The increasing numbers of, particularly young, naturalists, the extension of naturalist activites to new fields, and the increasing organization of naturalists signifies the emergence of a naturalist social movement during the inter-war years.

The growing numbers of amateur naturalists should be seen in a larger context, as a consequence of the emerging welfare society. The third generation met a Sweden which had completed its transition to modernity,[22] with slowly but steadily increasing prosperity, even reaching the working classes. It was a nation where open class conflict was about to be neutralized by legislation and an historical compromise between trade union leaders and capital owners. The Swedish Model, proposed by socialist intellectuals such as Ernst

18. Brinck 1981,p.5.
19. Anon. 1928.
20. Göteborgs biologiska förening 1929.
21. Lönnberg published, among other things, *Atlas över djurriket* (Atlas to the animal kingdom) 1913, *Sveriges ryggradsdjur* (Swedish vertebrates) in three volumes 1914-15, *Sveriges vatten och dess invånare* (Swedish lakes and streams and their inhabitants) 1923, *Sveriges jaktbara djur* (Swedish game) 1923, the text to Wright and Wright *Svenska Fåglar* (Swedish birds) 1929, and *Svenska fåglars flyttning* (The migration of Swedish birds) 1935; Jägerskiöld's main work was *Nordens fåglar* (The birds of the Nordic countries) 1898, 2nd ed. 1926.
22. Ruth (1984) points to the Stockholm Exhibition in 1930 as the definitive symbol of the modern break-through.

Wigforss and Gunnar Myrdal, gradually gained ground.

Rising wages and shorter working hours permitted middle class people, and even skilled workers, to keep a summer cottage, hence compensating for the alienation from nature associated with urbanization and industrialization. Leisure time even became a planning-issue. In 1937 the Government set up a *Fritidsutredning* (the Leisure Time Commission), to which Lars-Gunnar Romell acted as secretary for two years.[23] Camping had become the fashion of the day, creating such pressures and problems that Romell was moved to advocate, in a leading newspaper, severe restrictions on causal camping and the development of recreation villages as an alternative. »Camping is dirty«, he said.[24]

Camping is not identical with naturalist studies, but Romell's fury and the mere existence of a *Fritidsutredning* indicates the magnitude of outdoor recreation in the inter-war years. This burgeoning interest in the outdoors became the vehicle whereby the naturalist tradition, with its roots in Linné's flower excursions and 19th century popular hunting, turned into a social movement.

The bird-watchers and other amateur naturalist activities

A significantly new element of the naturalist revival around 1930 was bird watching. Until then birds had mainly attracted the attention of collectors and hunters. With the 1930s the rifle was replaced by a pair of binoculars, and bird watching became an aesthetically motivated leisure time activity for middle class men. It was facilitated by the popular and illustrated books published by earlier generations of museum faunists, like Jägerskiöld and Lönnberg. Particularly useful, however, were the handy pocket books by Rudolf Söderberg,[25] designed for immediate outdoor examination, an activity »which is in keeping with the Sunday walking-tours and camping life of our time«, as a reviewer put it.[26] Bird watching turned out to be one of the main routes to ecology, even more so for later generations. Arne Lindroth was one of the pioneers, starting in the hunting and taxidermic tradition, but later turning to the binoculars:

> *»I became interested in nature very early and it was above all birds...I stuffed birds very early during my high school years and was instructed by Henrici at Göteborgs museum. I took part in an excursion to the Hornborga Lake in 1928, and after this excursion I went there myself...and walked around the Hornborga Lake and watched its birds. For instance, I was together with Rudolf Söderberg there«.*[27]

The bird-watchers were eventually organized in *Sveriges ornitologiska förening* in 1945 – a few years earlier the most active had founded a journal, *Vår fågelvärld.*[28]

Popular handbooks on insects similarly stimulated the growing interest in nature.

23. SOU 1940:12.
24. Romell 1938a and 1938b.
25. Söderberg 1932-34.
26. Anon. 1936,p.17.
27. Interview with AL 23/9 1981.
28. Among the prime movers was Nils Dahlbeck, a graduate student of Du Rietz in Uppsala in the 1940s (cf.3-2), and P.-O.Swanberg, a dentist. In 1942 they initiated a Bird Section under *Svenska naturskyddsföreningen*, (cf. below), with Rudolf Söderberg as its first chairman. The most active were »soon disappointed with the low level of activity« (interview with NN 23/11 1982), and in 1945 they founded *Sveriges Ornitologiska Förening (SOF)*, as an independent association.

Entomologiska föreningen in Stockholm had existed since 1879, but the number of amateur students of insects grew rapidly. Of the 90 or so persons who assisted Per Brinck to collect stoneflies for his dissertation two-thirds of them were amateurs.[29]

The publication of new botanical handbooks also reflected a growing popular interest in flower studies. Hesselman has emphasized that Carl Lindman's *Svensk fanerogamflora* from 1918

> *»has mightily contributed to the awakening and spreading of floristic studies in our country«.*[30]

Erik Almquist's dissertation *Upplands vegetation och flora* of 1929 was part and parcel of the same revival,[31] as was the large-scale inventory of the flora and vegetation of the province of Scania initiated by *Lunds botaniska förening* in the 1930s (cf.3-2).

The nature conservationists

Another significant feature of the naturalist revival was the increasing awareness of the need for nature conservation. Concern over the drastic effects of agricultural development, land reclamation, forest clearing, and industrialization on the landscape emerged during the last three decades of the 19th century. An early expression was A.E. Nordenskiölds proposal in 1880 to create a national park, referring to the Yellowstone Park in USA as a model.[32] The spark igniting the conservation movement, however, was a series of lectures given in 1904 by the Prussian State Commissioner for Historic Monuments, H.W. Conwentz. That year a bill was proposed in the *Riksdag*, and a few years later, in 1909, a number of National Parks were reserved, to be managed by *Vetenskapsakademiens naturskyddskommitté* (the Nature Conservation Committee of the Academy of Science). Also in 1909, *Svenska naturskyddsföreningen* (the Swedish Association for the Conservation of Nature, SNF) was founded,[33] publishing a journal, *Sveriges natur*, from 1910. Without doubt the prevailing national romantic ovement[34] facilitated the swift acceptance of Conwentz's ideas.

Neither the Academy Committee nor the *SNF* built up any administrative apparatus worth mentioning. The question of a nature conservancy research organization was not raised until much later. Nature conservancy was largely a voluntary task and for a couple of decades it remained as such, though increasingly backed up by propaganda and legislation. The members of the *SNF* were taught how to take care of nature through their journal and its yearbook (also *Sveriges natur)*,[35] and from local club lectures and excursions. The membership was almost doubled during the two decades considered here – from 3000 to 5000 members. In its early years, its journal and yearbook had usually been filled with notes on erratic blocks, giant's kettles, remarkable oaks and junipers, and

29. Brinck 1949 (cf.3-4).
30. Hesselman 1929.
31. Almquist 1929.
32. For some details concerning the early history of nature conservation in Sweden, see Ödmann *et.al.* 1982, and Schaar 1978.
33. For contributions to the history of *Svenska Naturskyddsföreningen*, see Dahlbeck 1948, and Aminoff (ed.) 1959.
34. See, e.g., S.Björck 1946 and Frängsmyr (ed.) 1984.
35. The yearbook was published as *Svenska naturskyddsföreningens Årsbok* from 1910 to 1938, as *Bygd och Natur* from 1938 to 1942, and as *Sveriges Natur* from 1943.

animals threatened by extinction, etc. Now they began to carry articles on field biological issues, and the effects of industrialization on the landscape. Excursions gradually replaced lectures as the Association's basic activity. But nobody seems to have considered the relations between animals, plants and environment to be objects of conservation, and therefore it is hardly surprising that no claims for ecology were made from nature conservation circles. *KVA:s naturskyddskommitté* continued to work for legal protection of remarkable areas and »natural things«, hence actually continuing in a different guise the old natural history cabinet tradition.

The naturalist revival among teachers and pupils

The naturalist revival found yet another expression in demands for a modernization of school biology. Prospective biology teachers[36] were required to take both botany and zoology in their university degree. As there were no specific educational requirements with regard to the content of the courses in botany and zoology the training of secondary school teachers closely reflected the views of the university botany and zoology elites. Hence, biology teachers were trained almost exclusively in systematics and comparative anatomy.[37] With the exception of a few paragraphs on ecological factors and plant community environment in some botany textbooks, outdoor biology before the 1940s was confined to the obligatory and usually abhorred requirement to collect plants for herbaria. The situation in the 1930s is well described in the following quotation:

> *»A senior biologist among my colleagues used to summarize his opinion of the curricula in our subject roughly in the following way: 'Think, how much systematics, anatomy, morphology and physiology our subjects include. Too bad there is not room for any biology on the schedule'. I dare say that unfortunately one must mainly admit that he is right. The biology of animals and plants, i.e., their conditions of living, with their problems, their adaptation to and reactions towards the environment, their role in 'the economy of nature', all these things are rarely taken into consideration in the higher educational curriculum, and in any case they are hardly included in the biology teacher's training. It is altogether up to the biologist himself to repair this lack by means of private study after completing his academic education. Whoever wants to change his subject into real biology must not neglect that«.*[38]

It is true that *1931 års läroverkssakkunniga* (the Secondary School Commission of 1931)[39] had suggested that the pupils »as far as possible« should have a chance to

> *»make observations and investigations on natural things in nature«.*[40]

In the main, however, these were only recommendations, and besides they were not motivated with reference to the naturalist interests among teachers and pupils, but chiefly for pedagogical, or even public health, reasons.[41] Furthermore the guidelines were

36. Biology teachers were divided into assistant masters (sw. *adjunkt*) and senior masters (sw. *lektor*). To become *lektor* one was required to write a doctoral dissertation (many *lektorer* were scientists actively engaged in research, but typically ranked low in professorial competitions).
37. In the 1910s and 1920s a growing number of prospective biology teachers participated in faunistic summer courses and excursions. In Uppsala and Lund marine zoological summer courses were arranged at Klubban and Barsebäck respectively, and many botany students in Uppsala followed Sernander's excursions.
38. Linnell 1936.
39. SOU 1932:31; the following quotations are from pp.199ff. and p.388.
40. Furthermore, attention should be paid, »as far as possible, to phenomena which can cast light upon the relation between the structure of the organisms and their way of life«.
41. »First hand nature studies« were expected to stimulate the pupils' powers of observation and self-activity; in addition, outdoor biology teaching was sometimes considered a potential means to prevent tuberculosis.

seldom followed in practice: as late as 1946 one teacher complained that excursions »are a rare occurrence«.[42]

Lack of curricular support and shortcoming in teachers' qualifications did not stem the naturalist tide, however. From the 1930s the naturalist revival gradually found its way into the schools. For example, small limnological investigations or vegetation descriptions were sometimes assigned as voluntary tasks to pupils who wanted a good examination rating.[43] Teachers increasingly attended further educational courses: e.g., in the summer of 1937 they could choose between a two-week plant sociological excursion led by Einar Du Rietz in Uppsala, a two-week limnology course led by Sven Thunmark in Lund (cf.3-2), and a marine course led by Lund zoologists (cf.3-4), and these courses seems to have been successful. The late 1930s also saw attempts to found naturalist magazines directed towards a school audience, for example, *Entomologbladet*, intended for all those »who find pleasure in observing living nature«,[44] and *Naturen och vi*.[45]

The most conspicuous expression of the naturalist revival within the educational system came a decade later, when a Stockholm biology teacher addressed his colleagues and pupils, asking for:

»a national association founded by adolescents interested in nature«.[46]

Modelled on *Nederlandse Jeugdbond voor Naturstudie*[47] it was supposed to replace the fading natural scientific and biological pupil associations; it should publish a journal and organize summer camps for nature study. The initiative was met by a response bearing witness to the wide naturalist interest among young people – within just three years *Sveriges fältbiologiska ungdomsförening* (the Swedish Field Biology Youth Association) had 300 members in nine local clubs,[48] and the membership rose to about 2000 during the 1950s.

Hence, in the long run neither teachers nor pupils accepted a restriction of biology to demonstrations of the skull morphology of fishes or cryptogam reproduction patterns, which left outdoor biology to be fostered on a voluntary basis. The growing plea for an outdoor field biology had two objectives: while trying to change secondary school curricula, it was also necessary to change university curricula. These tasks were gradually taken up on the agenda of *Biologilärarnas förening* (the Biology Teachers' Association),

42. Cederholm 1946.
43. This and the following examples are taken from *Medlemsblad för biologilärarnas förening* (Journal of the Biology Teachers' Association).
44. Quoted from advertisement in ibid. 1938. *Entomologbladet* was published by the *Entomologiska föreningen* in Stockholm on the initiative of Carl H. Lindroth (cf.2-5, notes 220 and 222, and 4-3) and Karl-Herman Forsslund (cf.3-4) – on their role, see C.H.Lindroth 1973.
45. *Naturen och vi* (Nature and us) was initiated by two students of Sernander in Uppsala, Nils Dahlbeck and Sten Selander (cf.3-2). Selander was a friend of the publisher Åke Bonnier. Dahlbeck revived the title of the journal in a radio programme a good decade later, one of the great radio favourites of the 1950s and 1960s.
46. Appeal by Cederholm in *Medlemsblad för biologilärarnas förening* nr 1, 1947.
47. Cederholm wrote: »It is really a scandal that there is no counterpart to the Dutch association in LINNÉs country« (Cederholm 1947).
48. L-G.Pettersson 1950; the first chairman, Carl-Cedric Coulianos, became one of the prime movers behind animal ecology at Stockholm in the 1950s (cf.4-3).

founded in 1933 to defend the subject against cuts in the timetable.[49] To restore school biology to its former status became the main goal for the Association over the next 15 years.

The question of outdoor biology was hardly a major issue for the Association,[50] but the mobilization of biology teachers allowed for the naturalists' points of view to be heard. The strength of the naturalist argument was tested for the first time in 1936-39 within the context of *1936 års lärarutbildningssakkunniga* (the Secondary School Teacher Educational Commission of 1936),[51] which was to make a general assessment of the university training for prospective teachers. *Biologilärarnas förening* demanded a reduction in several parts of the university botany and zoology curricula, for example, in cryptogam and invertebrate systematics, arguing that these parts

> *»are without importance for the school and for an allround biological education«*[52].

Instead, studies on living plants and animals, biological aspects, excursions, including obligatory summer excursions, were emphasized, reflecting a growing demand to institutionalize the hitherto voluntary faunistic and floristic activities.[53]

To today's readers these demands for reform were rather modest, but not so from the viewpoint of the contemporary university botany and zoology elites. Arguing that a reduction of the cryptogam courses

> *»would be disastrous for the scientific training of the teachers«,*

the leading botanists refused bluntly to cut the existing subject matter. Professor Skottsberg in Göteborg said:

> *»Systematics...is as modern as any other branch* /of botany/, *unless one believes that modern is identical to what constitutes a current fad«.*

Leading zoologists agreed. They could accept some additions in principle, especially if faunistics and floristics was translated into scientific ecology, but on the other hand

> *»entering even more deeply into the ecological aspects is... not possible due to lack of time«,*

as professor Holmgren in Stockholm said.

As a consequence of the resistance of the university botany and zoology elites, the outcome of these reform attempts from the side of the school naturalists were limited. Arguing that

49. The direct cause was »the staggering blow« against biology from the Minister of Education and Ecclesiastical Affairs, Arthur Engberg, who quite simply cut out a year's courses. The general motive was presumably to strengthen the status of classical languages, and biology was a suitable victim: the subject was generally considered »dull« with reference to its systematical and morphological character, the memorization of Latin names, and the detestable collection of plants. Among the initiators of the Association were Torsten Pehrson, trained as a comparative anatomist from Stockholm, and later professor of zoology there from 1948, and Folke Borg, Gislén's main competitor for the chair in zoology in Lund in 1930-32 (cf.2-5).
50. The main issue was of course to restore the quantitative status of the subject; with regard to qualitative demands, however, racial biology became a major issue. Engberg's cut happened to remove those parts of the curriculum dealing with plant physiology, genetics and evolution, and the politically touchy question of eugenics and racial biology (cf. the National Party's bill nr II:622 to the *Riksdag* of 1935) was coupled to the re-instatement of genetics. The racial biology issue was prominent in the protest address against the Engberg decision (Borg 1934), and the main spokesman in the biology teachers association, Folke Borg, later co-founded a para-nazist association, *Riksförbundet svenska familjevärnet* (the Swedish Family Safeguard National Society) arguing for positive eugenics.
51. SOU 1938:50.
52. This and the following quotations from SOU 1938:50,pp.121-24 and pp.320-23.
53. E.g., the biology teachers mentioned knowledge of plant and animal plankton of lakes and streams, a »necessary demand« which undoubtedly was stimulated by Naumann's and Thunmark's popular summer courses at the Aneboda laboratory in the late 1920s and the 1930s.

»the ecological aspects of the /university/ *education should be noticed as far as possible«,* the Commission recommended a considerable increase of state expenditure on excursions.[54] The plea for a »real biological« biology, including faunistics, floristics, excursions and scattered demands for ecology continued during the war, as witnessed by articles in the journal of *Biologilärarnas förening*, but had no effects, neither on school curricula nor on the university training of prospective biology teachers. Evidently university botany and zoology elites were still too strong, and the naturalist graduate students and commencing ecology groups too weak. The break-through of the naturalist argument and a first step towards ecologization of school biology and university botany and zoology curricula was to come only when other, and stronger, actors became involved. To that we will return in Section 3-5.

A naturalist academic career...

Since the expanding welfare society meant rising opportunities for higher education, the growing tide of popular naturalist interest found its way to the universities too. Matriculation at the universities had been at its lowest during the economic regression in the 1920s, but from the late 1920s the student population began to grow again. Inevitably the numbers of botany and zoology students having a naturalist interest increased too. And in spite of the total dominance of the »new German« botany and zoology in undergraduate courses, »the men of the 1930s« were not enrolled into the traditional botany and zoology to the same extent as earlier generations had been. The old professorial elite kept its positions, but could hardly impute the same enthusiasm for comparative anatomy, cytology, embryology, and systematics as it had shown in the late 19th century. While still providing good opportunities for a secure and respectable career both for a would-be university professor and a prospective secondary school teacher, the traditional museum and laboratory zoology and botany did not capture the minds of the new generation as it had done previous ones.[55]

In fact, just a few of them were fortunate enough to convey their juvenile naturalist enthusiasm into full-fledged naturalist academic careers. An outstanding example is Bertil Kullenberg (b. 1913), who says:

> *»I have always, from early childhood, been interested in plants, starting even before I had begun school, began with plants and birds, and then I had help from books at home, I learned then...had to learn all by myself. Yes of course my parents helped me, so that I could spell my way through the name* /of the plant or bird/«.[56]

At Uppsala he took an all-round undergraduate education to satisfy his naturalist interest,

54. On the other hand the Commission recommended the establishment of courses in hygiene for prospective biology teachers, a consequence of the fact that medical and racial biological issues were much more pressing than the naturalist issue.
55. To do field botany, field zoology or ecology was, of course, not the only alternative to traditional botany and zoology. The former zoological and botanical specialties of physiology and genetics were gradually institutionalized as distinct academic social orders throughout the inter-war years, and hence competed with traditional botany and zoology. Nilsson-Ehle's personal chair in plant genetics (1917) was made permanent after his death. In the inter-war period a chair in livestock breeding and genetics was created at a private institute for animal husbandry in co-operation with *Stockholms högskola*. John Runnström personal chair in experimental zoology and cell research, created at Stockholm in 1932, was also converted to a permanent chair. The first chairs in animal physiology and plant physiology in Uppsala and Lund were created in the late 1940s.
56. Interview with BK 23/11 1981.

i.e., including botany, geography, meteorology and zoology. Kullenberg's future scientific work, which was partly stimulated by Douglas Melin (cf.2-5), developed into a life-long field excursion. Sometimes he focused on flowers, sometimes on insects:

> *»I became fascinated by insect behaviour, their forms, colours«,*

or on birds:

> *»why do the birds look like they do, why do they behave like they do, why are nests of a specific kind* /situated/ *in certain kinds of ramifications and such things«.*

His dissertation, *Studien über die Biologie der Capsiden*, submitted in 1944, was a voluminous monograph of the life habits, food choice, reactions to environmental factors, and reproduction pattern of a group of insects, all based upon very thorough and patient field observations. It evoked acclaim even from traditional zoologists: »admirable with regard to the richness and fineness of the observations«, they said,[57] hence accepting Kullenberg's naturalist research as a proper academic endeavour.[58] Later he was appointed to a new position in entomology (cf.3-5), from which he could continue to pursue his naturalist studies.

... and ecologization

The increasing number of naturalists like Kullenberg at the universities could serve as raw material for later ecologization. To translate an interest in bird-watching and other kinds of naturalist activities into the rhetoric of ecology was an important route for the growth of ecology as a scientific social order. After all, ecology was one of the accepted botanical and zoological subspecialties in academia.

We should not exaggerate the impact of the naturalist revival on the ecologization process, however. Firstly, Kullenberg's naturalist career in Uppsala was still rather special, a naturalist's dream. Floristics and faunistics, like that pursued by 19th century naturalists such as August Holmgren, were little valued in academia, especially in the teaching of undergraduates. A sharp demarcation was usually made between naturalist studies as a leisure time activity, and »real« botany and zoology education. Even though the growth of the social orders of botany and zoology was stagnating, the »new German« botany and zoology was still the predominant problem tradition. The cytological and anatomical revolutions in botany had transformed undergraduate education at all three universities to laboratory courses – and what was not acquired at the microscope was learnt in the herbarium. Most undergraduates never got any training in field botany, and one of the leading laboratory botanists in the 1940s expressed the conventional attitude when saying that it was a waste of time to teach undergraduates about »edible mushrooms, bird song and other things, which amateurs use to learn by themselves«.[59] So when »the man of the 1930s« attended the undergraduate courses in botany, he met with almost no »proto-ecology«, not to mention ecology.

57. See ED 19/11 1948:9.
58. The point is that Kullenberg in retrospect cannot define his subject matter unambiguously: he does not typify himself as an ecologist or an ethologist, nor as an entomologist or an ornithologist. He is all of these and other types at the same time, having no clear disciplinary, or professional identity.
59. Burström 1947.

The same resistance to naturalists was to be found at the graduate level as well. The likelihood of being able to pursue one's naturalist interest varied considerably from department to department. In some places field studies were tolerated but in others very few escaped the tradition for comparative anatomy or cytology. Arne Lindroth as a school-boy intended to become a professional bird-watcher – but it was impossible, he says,

> *»to write a dissertation on it...it was not conceivable then«.*[60)]

Secondly, even though the number of students with a naturalist interest was increasing, their »proto-ecological« activities were not necessarily translated into ecology. One of them says:

> *»/It/ was natural for me... the fact that birds laid eggs, that they sang, that they migrated, that they ate, and things like that... habitat selection....that goes without saying...«,*[61)]

but adds:

> *»I did not call it ecology...«.*

To sum up: while second generation ecologists like Henrik Lundegårdh and Gustaf Alsterberg had no significant naturalist subculture to back them up, the third generation of would-be ecologists were contemporaries with a new wave of naturalist popular handbooks and magazines, a growing number of recreation facilities, binoculars, automobiles, and a whole outdoor subculture. They were the young academic avant-garde of the renewal of naturalist interest in Sweden, the intellectuals of the naturalist social movement. Their careers through the universities, their proposals to pursue naturalist studies in the academic curricula of botany and zoology, and their claims for ecology, should be seen in that perspective. In the following sections we will examine the extent of »proto-ecological« work at the universities and the renewed claims for ecology during the 1930s and 1940s.

3.2 The Uppsala school and the synecologists

The first claims for synecology had been forwarded by scientists attached to the universities, while practically oriented botanists had tended to make claims for ecology from an experimental or laboratory oriented point of view. During the period considered here, that is, the 1930s and 1940s, claims for synecology were still forwarded mainly by scientists at the universities. In fact only a few significant scientists attached to applied research institutes claimed close affinity with synecology. Since they did not enrol students to

60. Interview with AL 23/9 1981.
61. Interview with GS 14/3 1983.

synecology, they will not be considered further here.[62]

Neither did Stockholm or Göteborg botanists pursue any significant synecological investigations during the period considered here.[63] Hence we are left to consider only a few university departments in Uppsala and Lund: Einar Du Rietz's Department of Plant Biology in Uppsala, and the Botanical Museum and the Department of Limnology in Lund. At all three places the Uppsala school of plant sociology and synecology was the obvious point of departure.

»Växtbio« in Uppsala: a cosy place of descriptive vegetation studies

The great majority of botany undergraduates in Uppsala left the university after their *fil.kand.* or *fil.mag.* degree, and only a few continued to write their licentiate or doctoral dissertations. Graduate students had much greater freedom of study than undergraduates but the likelihood that those with a naturalist bent would translate their studies into the language of ecology varied from department to department. A doctoral student with a field botanical disposition who wanted an outlet for his naturalist interest seemed to have an easy choice of supervisors. There was Nils Svedelius, who occupied his chair until 1938; he was not only uninterested in field work,[64] but was also arrogant and pompous – »the only man you spoke ill of«.[65] His successor, John Axel Nannfeldt, was devoted to cytology and systematics, and did not encourage »proto-ecological«, or deliberate ecological, studies. Their colleague Elias Melin, one of the second generation ecologists and professor of botany (plant physiology) through the 1930s and 1940s, was also amiable and soft-spoken – an ideal supervisor for would-be ecologists (cf.3-3). But since he demanded knowledge of chemistry and a preference for experimental work – neither of which were the average naturalist's cup of tea – he did not enrol any students to do

62. *Lantbrukshögskolan*, founded in 1932 by fusion of the earlier *Centralanstalten* and *Entomologiska anstalten*, was the center for scientification and rationalization of Swedish agriculture. With few minor exceptions, such as Hugo Osvald, the former member of the plant sociological »gang« in Uppsala in the 1910s, who had been appointed professor in plant husbandry in 1933 (Jo 21/12 1933), ecology was not part of any curriculum. In his lectures Osvald at least paid lip service to synecology as the scientific basis of plant husbandry (E.Åberg 1959, p.7). In practice, however, Osvald's and his younger colleagues' voluminous research and teaching on the relation between plants and their environment was routinely defined as plant husbandry, and never translated into (syn)ecology (for a history of plant husbandry in Sweden, see Osvald 1959 and E.Åberg 1959). At *Skogshögskolan* men such as Olof Eneroth pursued extensive plant community studies but did not claim them as ecological (cf. below). At *Statens skogsförsöksanstalt* Carl Malmström (cf.1-3) occasionally claimed his plant community studies as ecological. After having finished his long-term studies of the water-logging problem, Malmström turned to forest type and plant community studies in the mid-1930s, and when appointed successor to Hesselman in 1940 (Jo 20/12 1940:19), he continued to work together with Lars-Gunnar Romell on »causal forest plant community research and nutrition ecology« (Malmström 1952). Some of his studies were important contributions to applied plant community analysis, but since his position made a very limited enrolment possible, we will not consider him further here.
63. One might have expected that the single professional botanist in Göteborg, viz., Carl Skottsberg, having been a student of Rutger Sernander, would have pursued »proto-ecological« investigations, or even made claims for ecology. But Skottsberg had been commissioned to plan a botanical garden for the city of Göteborg in 1915; four years later he was appointed its director and given the title of professor. In 1931 he was also summoned to a personal chair in botany at *Göteborgs högskola*, and he came to devote himself to the border area of taxonomy, morphology and floristic plant geography (see Peterson 1964 for biographical details on Skottsberg).
64. Despite his early works in plant geography, including some notes on ecological plant geography (cf.1-3).
65. Interview with ND 20/8 1981.

synecological work.[66)]

The natural gathering point for graduate students wanting an outlet for their naturalist interest in Uppsala was of course Sernander's and Du Rietz's department. My oral witnesses all agree: »*Växtbio*«, as they called it, was cosy! Even a shy, young high school graduate from a provincial town felt that he was welcomed by an open and generous institution, being taken care of by friendly and spontaneous professors, and making many friends during the excursions and the lively seminar. One of »the men of the 1930s« says that »*Växtbio*« was:

> »*the only* /department/ *with an open atmosphere... where it was exciting... where there was some action... where it was open... that was 'Växtbio'*«.[67)]

»*Växtbio*« was not just a substitute for a family, however, and »the men of the 1930s« could not just professionalize their original naturalist pastimes. They were enrolled to Sernander's and Du Rietz's scientific ideas and were trained in the Uppsala school's inductivist approach to plant geographical and sociological problems. We recall that for Du Rietz, the vegetation was the primary object of research, while the site's influence was a subordinate issue, and he never abandoned this basic standpoint: *first* describe the vegetational units, *then* (maybe) try to make ecological correlations. The Uppsala dissertations remained descriptive, and if any problems were tackled they grew out of the descriptive work.

An example of this descriptive stance is Mats Wærn's (b.1912) dissertation work.[68)] Wærn became interested in algae as a school-boy, and he was taken under Du Rietz's wing as a freshman in the early 1930s. For twenty years he worked on his dissertation, and like so many of his colleagues he chose a taxonomic group within a geographically confined area – the algae of the Öregrund archipelago – gathering enormous amounts of material without first having a clear problem in mind. When finally trying to compile his vast material the lack of a unifying theme is said to have dawned upon him. One of his fellows tells:

66. A very independent Uppsala botanist, who made his way to field botany outside both the Uppsala school's and Melin's sphere of influence was Gunnar Lohammar (1902-1975). Towards the end of the 1920s he set himself the task to pursue »möglichst weitgreifender Untersuchungen über die chemischen Eigenschaften der Binnenseewasser, um daraus vielleicht eine klarere Auffassung der Faktoren zu gewinnen, durch welche Vorkommen und Verbreitung der Wasserpflanzen geregelt werden« (Lohammar 1938,p.7). Lohammar took over the problem from Gunnar Samuelsson, who had worked on the ecological plant geography of water plants (cf.2-2) in the early 1920s before being appointed professor in botany at *Riksmuséet*, and »in den ersten Jahren meiner Studienzeit mein Interesse für die Wasserpflanzen angeregte« (ibid.,p.5), Getting help with chemical analyses from The Svedberg's department in Uppsala, Lohammar collected an enormous material of water samples from 156 Swedish lakes. The occurrence of higher vegetation was stated qualitatively and he avoided sociological analysis. His ambition – to demonstrate a relation between the water chemistry and the composition of the vegetation – failed however, and his dissertation remained a detailed catalogue of the chemical composition of lake waters. In the light of the descriptive tradition of the Uppsala school, it was considered a masterpiece of its kind (see assessments in ED 17/1 1947:1). Since Lohammar did not claim ecology as an independent science, or get a university position (he lost the competition over the chair in limnology in Lund in 1947, see ED 17/1 1947:1, and the new professorship of limnology in Uppsala in 1948, see ED 30/12 1948:7) from which to enrol students to do ecological investigations,he will not be considered further here.
67. Interview with ND 20/8 1981.
68. Wærn 1952.

»He had finished the whole manuscript, had translated it, and I even think he had started to print the first part of it – then he came to me: 'What shall I do with it, I have to submit it, everything goes to hell'. But he had done those diving profiles, he had carefully noted what he had found at different depth levels. He was fed up with the material, he could not systematize it, he could not do any sociology out of it. It was like – what shall I do with it? But then came the words that relieved the tension. I remember that I said: 'Write down in the book exactly what you have in your records' /.../ And that was the key. When he did it he was so stimulated, that the dissertation was ready in time«.[69]

Although others have noted Wærn's interest in environmental background factors[70] this anecdote nevertheless illustrates a general trend at »*Växtbio*«. It was the descriptive and inductivist attitude that united the third generation of Uppsala plant ecologists with the Du Rietz'ian plant sociological programme. In the foreword to his extensive study of mires, Sjörs said:

»When starting my investigation of the regional and ecological problems of the Bergslagen mires, I was a young man on a bicycle expedition, and very little aware of really what to investigate. The problems were raised during the course of work, and without doubting they would, I confined myself to travelling about and reconnoitering the area«.[71]

The synecological turn at »Växtbio«

However, although faithful to the descriptive and inductivist attitude, none of »the men of the 1930s« wrote pure plant sociology, the hegemonic tune of the 1920s. Du Rietz himself never abandoned his life's work. He remained »the man with the many terms« and was generally regarded being devoted to »plant sociology jurisprudence«.[72] But he could apparently not enrol his graduate students to do pure sociology.[73] Similarly, while a few »in-between-generation« students recruited during the 1920s took up floristic plant geographical problems none of those recruited in the 1930s wrote pure floristic plant geography.[74]

The naturalist interests of the third generation were instead translated into problems concerning the correlation between the vegetation and the site. Most of »the men of the 1930s« at »*Växtbio*« produced dissertations displaying a renewed concern for the influence of environmental factors upon vegetation, making ecological correlation an aim in itself:

69. Interview with BP 7/2 1982.
70. Sjörs, written comm.
71. Sjörs 1948,p.14; to complete the picture it should be added that Sjörs became more and more problem-oriented as the work progressed: »When I had established a clearer view of the problems, time was ripe for the opposite working method, 'petty' detailed work within a very small, but representative area« (ibid.). However, a majority of the Uppsala dissertations were largely descriptive, often little more than lists of communities and distribution of species.
72. Interview with ND 20/8 1981.
73. Two of the less prominent dissertations, by Almquist 1929 (cf.2-2) and by von Krusenstierna 1945, combined sociological and floristic investigations.
74. Several dissertations on floristic plant geography were submitted in the 1930s and 1940s, but these were all written by older students, recruited during the 1920s or earlier. E.g., the »lichen clergymen« (Sw. »lavprostarna«, Sjörs written comm.) Degelius and Ahlner wrote dissertations on the distribution of lichens; Degelius in addition paid some attention to »der Faktoren welche die Verbreitung dieser Flechten in erster Hand bedingen« (Degelius 1935,p.2) by comparing distribution maps with climatological maps. But such studies did not transcend the ecological plant geography of the 1900s and 1910s.

»I guess one was clear that it was not enough to describe plant societies, but that one had to know something about their environment. The train of thought at that time was, I suppose, actually to try to describe the vegetation and the environment one by one and then coordinate them«. [75]

Among these dissertations we find Tore Arnborg's (b.1912) case study of a Northern Swedish coniferous forest, *Granberget* (of 1943), and Nils Dahlbeck's (b.1911) investigations of Scanian shore meadows, *Strandwiesen am südöstlichen Öresund*, (1945). Arnborg's problem was to

»compare vegetational- and soil types with forest types set up for practical purposes, and accordingly test to what extent the latter could correspond to certain plant sociological units«. [76]

while Dahlbeck wanted

»eine Beschreibung der Pflanzengemeinschaften und ihrer Abhängigkeit von äusseren Faktoren zu liefern. Daher wurden eine Anzahl Analysen des Salzgehaltes und des pH-Wertes des Bodens durchgeführt«. [77]

Other dissertations of a similar kind were Nils Albertson's *Österplana hed*, a very detailed inventory of the limestone vegetation on the Southern Swedish mountain Kinnekulle (1946), and Erik Julin's (b.1906) *Vessers udde*, a study of vegetation and soil composition in a small Southern Swedish deciduous forest (1948). Finally Olof Rune's studies of the flora on serpentine rocks ended with an attempt to elucidate »the causal connection between the vegetation and the chemical composition of serpentine rocks«.[78]

Even Hugo Sjörs,[79] being one of Du Rietz's closest students, who utilized the whole plant sociological apparatus, nevertheless concentrated on the influence of the site in his extensive mapping of the mire vegetation and water regimes in Central Sweden, *Myrvegetation i Bergslagen* (1948). Sjörs' starting point was Du Rietz's well-known but as yet poorly published sociological work on mires of the 1930s. When almost the whole department went on a summer excursion to the province of Småland in 1937, he assumed the task of constructing a vegetation chart of the Annerstad mire, and after finishing his undergraduate education he simply continued with mires, although choosing those in his own home district. This was fundamentally a classical plant geographical problem. Citing Lennart von Post's picturesque account of the difference between the mires of Norrland and the peat-bogs of Southern Sweden, he stated:

»For whoever has grown up in a landscape of intermediary location and character, the question poses itself: what happens in the transition between these two so essentially different types, and how is it mediated?«. [80]

The practical break with Du Rietz is shown by the small word »mediation«, By this, Sjörs meant how the site and its geographical setting influenced the differentiation of plant associations. Indeed he clarified his aim as being to elucidate:

75. Interview with HS 24/9 1981.
76. Arnborg 1943,p.7.
77. Dahlbeck 1945.
78. Rune 1953,p.115ff.; the dissertation was published in 1957. Rune was the youngest member of the generation of *»Växtbio«* graduate students considered here; he was born in 1919, but did not begin his dissertation work until the mid-1940s.
79. For details concerning Sjörs's biography, see ED 28/6 1962:7.
80. Sjörs 1948,p.14.

»the mutual dependence between site conditions and the local and regional differentiation of the vegetation«.

Talking about »mutual dependence« was a big step away from the orthodox plant sociological programme, though, of course, not a novelty in ecological thought. This tendency was most evident in Erik Julin's work. Making an exceptionally all-round investigation of several soil factors, including studies of earth-worms, Julin tried to conceptualize the mutual exchange between vegetation and site. After some search in the literature[81] he borrowed the term »holocoen« from the German zoologist Friederichs.[82] It is interesting to note that the ecosystem concept, suggested by Tansley in 1935,[83] and widely spread in the Anglo-Saxon ecological literature was seemingly unknown to Julin (and his fellows):

> *»I remember rather well when Julin struggled with that, because it was not anything he had made clear for himself from the beginning. But he searched then, when writing his dissertation, for a term that could encompass both the vegetation and the animal life in terms of worms, and soil conditions in a concrete way. After some search in the literature he found Friederichs' ecosystem, holocoen. It was the only term he knew, simply because he had not realized that Tansley had written about ecosystems. Nobody told me* /either/ *that Tansley had written about ecosystems in 1935 or 1936. Einar Du Rietz did not do, and I suspect that /.../ he had simply not read the paper* /by Tansley/«.[84]

Julin interpreted the holocoen as a concrete unit, without grasping the holistic theoretical overtones in Frederichs' concept. Although Julin really could not match his own programmatic claim for »holocoen«-studies, he nevertheless expressed the widespread ambition among his generation at »*Växtbio*« to describe the mutual dependence of site and vegetation.

Why did »the men of the 1930s« turn to the site?

From where did they learn to investigate the mutual relation between vegetation and site? Few men invent a new theme by themselves, and these young men were no exceptions. It is true that Du Rietz himself developed an increasing environmental interest during this period, notably the 1940s. His lectures and field demonstrations is said to have expressed a basic ecological (or at least »proto-ecological«) attitude.[85] For example, the relations between snow-cover duration, calcareous and non-calcareous soil, and the mountain vegetation were clear to him. He also made some investigations of pH and marine water level fluctuations. But his main endeavour remained with the systematization of vegetational units. He was originally and for ever a taxonomist with all his heart. So while not being hostile towards site investigations, or placing obstacles in the way of the third

81. Interview with HS 24/9 1981.
82. Friederichs 1937. Julin writes: »the humus layer of the soil and the ground layer of the vegetation thus constitute the firm basis for the following synthetic account of the biocoenoses and biotops of Vessers Udde, or, in other words, its holocoen... With «holocoen» is meant the summing up of, and mutual exchange between, the organism community and its site« (Julin 1948,p.136).
83. Tansley 1935.
84. Interview with HS 24/9 1981.
85. Sjörs, for example, maintains that: »I must say that Einar Du Rietz was fascinating as an excursion leader... /presenting/ the ecological mode of looking at plants and botany, at shore zonations, and how different factors in nature influence the plants. I believe that Einar Du Rietz is somewhat underestimated as a site ecologist« (interview with HS 24/9 1981).

generation of ecologists, Du Rietz was hardly the main source of the environmental interest among »the men of the 1930s«.

To some extent the turn towards studies of the site might be interpreted as a consequence of contacts with practical concerns. Du Rietz seems to have accepted the ecological approach as a corollary to practical problems.[86] For example, Tore Arnborg oriented himself towards forestry problems (spruce forest regeneration). When attending a course for foresters in 1936 he came in contact with Olof Eneroth, professor of silviculture at *Skogshögskolan*: »it was the cooperation with Eneroth and the forest type concept and the forest type investigations which made me continue«.[87]

A practical problem of great concern for the department was nature conservation. Sernander had been a pioneer with regard to research directed towards nature conservation problems,[88] and both he and Du Rietz were much engaged in nature conservation. To Du Rietz, this was very much an expression of an idealistic and aesthetic attitude towards nature. He wanted to study and to protect the vegetation of archipelagos, high mountain areas and remote mires, i.e., vegetation uninfluenced by human culture.[89] One of the more critical students says that Du Rietz

> *»began more and more to seek out the untouched parts of Swedish flora and vegetation, which he thought were extant on the mires. He wanted the natural pattern in vegetation, that interested him. Everything else was less interesting to him, or he simply rejected it. Meadows or a grazed forest or something else was not interesting... he called it behind-privy-vegetation, 'this damned behind-privy-vegetation' he called it«.*[90]

During the 1930s and 1940s a growing number of »*Växtbio*«-students were engaged in nature conservation investigations initiated by *KVA:s naturskyddskommitté* (cf.3-1). For example, Nils Dahlbeck, an early active member of *Svenska naturskyddsföreningen* and assistant to Sernander in conservation work, is said to have chosen shore meadows as an object of investigation, partly because they were influenced by man.[91] Erik Julin likewise began his investigations as a follow-up of old sample plots laid out by Sernander in 1921 in order to measure the extent of reforestation of a forest meadow earlier used for haymaking.[92]

86. When asking whether Du Rietz accepted Lindquist's (see below) ecological approach to vegetation, one interviewee answered: »Yes... because it was linked to forestry... He could imagine a practical, an applied ecology... for Du Rietz it was natural that a forest scientist cared about the soil« (interview with BP 7/2 1982).
87. Interview with TA 30/9 1981.
88. After having left the chair in plant biology, Sernander was commissioned to investigate the scope for state financed nature conservation (SOU 1935:36); in his report Sernander advocated a »scientistic« approach to the problems of nature conservation. The selection and management of national parks and other areas worth protection should be made on scientific grounds and for scientific reasons, and the conservation measures should be directed by scientists. In contrast to Sernander's elitist view of nature conservation stood Lars-Gunnar Romell, who advocated a more socially responsible conservation policy (cf.2-4). Some observers, for example Sjörs (written comm.), maintain that the unrealistic proposals by Sernander delayed the establishment of a state agency for nature conservation for over 30 years (cf.4-4).
89. There seems to be a close correlation between vegetational studies and aesthetic interests. E.g., Sten Selander, one of Sernander's students, a mountaineer and author of naturalist essays who eventually wrote a dissertation on the floristic plant geography of a region of Northern Lapland in 1950, was a member of *Svenska akademien*. Another, less well known example, was Nils Albertson. Sjörs (1957) emphasizes Albertson's artistic and cultural interests, and point out that his vegetation studies had an aesthetic aim.
90. Interview with BP 7/2 1982.
91. Interview with ND 20/8 1981; Dahlbeck investigated not only the influence of salinity and inundation on the differentiation of salt marsh vegetation, but also sea-weed collecting and the effects of cattle grazing and trampling.
92. Julin 1948.

Bertil Lindquist and his »boys«

Thus, it seems that the ecological inclination of some of the Uppsala dissertations in the 1940s, was, at least in part, the outcome of the confrontation between plant sociological training on the one hand and practical forestry and conservancy interests on the other. We should not overdo the practical interest argument, however.[93] Neither practical interests, nor any concealed synecological inspiration from the side of Du Rietz seems to have been decisive for the third generation's emphasis on site-vegetation correlations. The turn to the site seems to have bee a direct or secondary effect of the enrolment power of an outsider, Bertil Lindquist (1904-1963), who came to »*Växtbio*« around 1930.[94]

At first glance, Lindquist's dissertation, *Den skandinaviska bokskogens biologi*, looks like a typical product of the Du Rietz'ian programme of vegetational analysis, with its hundreds of pages of descriptions of beech forest communities and subcommunities. On the other hand, his approach to vegetation was more ecological than any other Uppsala-dissertation had been. Not for nothing did he subtitle it »The ecology of the Scandinavian beech-woods«. Indeed, the aim of the investigation was to

»inquire into the mutual relations between the vegetation and the site«,[95]

and his site analyses were the then most extensive in Swedish field botany. He investigated the influence of light, nitrification and acidity on the vegetation. He used advanced chemical analysis methods, went to København to learn pH-determinations, and made analyses of nitrate and ammonium-nitrogen content at the Carlsberg laboratory.

Lindquist had learnt the ecological approach in Lund. He had an early interest in nature conservation and landscape history,[96] and delayed his dissertation study to investigate the forest types in the Dalby Söderskog National Park in the province of Scania.[97] He had travelled through the beech forests of southern Sweden together with Sernander in the mid-1920s, but for several years he had also been closely connected to Botanical Museum in Lund. There he probably met with Henrik Lundegårdh, and more important, for a couple of years he was Einar Naumann's assistant. The early investigations was an attempt to get an overview of the beech forest vegetation and site conditions in a way that resembles Naumann's approach to the lake as a totality of chemical and physical environmental factors and communities of organisms.[98]

93. On the other hand there was no substantial exchange of ecological ideas between the »*Växtbio*« students and Hugo Osvald at *Lantbrukshögskolan*. Osvald was a frequent guest at Du Rietz's seminar, and he sometimes joined the departmental excursions. His academically oriented research dealt with plant communities of peat-bogs (a continuation of his dissertation theme). None of »the men of the 1930s« were initiated through plant husbandry problems.
94. For biographical details on Lindquist, see Karlberg 1963.
95. B.Lindquist 1931,p.201.
96. Bengt Pettersson emphasizes this aspect of Lindquist's work: »...Lindquist's connection to the ethnographical, ethnological tradition in Lund, represented by Campbell's investigations of the Scanian landscape. He /Campbell/ was a pioneer in Sweden, much earlier than Selander and /Carl/ Fries etc. Campbell's role as landscape describer is overlooked... Lindquist's connection to Campbell was unmistakable. Campbell came to Uppsala too, and gave lectures about his landscape ecological attachment. He was not a botanist, so he could not get so far, but he was willing and very open to contacts with plant biologists« (interview with BP 7/2 1982).
97. The full result of his Dalby Söderskog investigations were published more than a decade later in a monograph of the vegetational history of the forest (B.Lindquist 1938).
98. Lindquist acknowledged his intellectual debt to Naumann: »My time at the Limnological laboratory at Aneboda and the possibility to study exact methods in modern vegetation research with professor Einar Naumann, has been of great importance for my education« (B.Lindquist 1931,p.182; engl.orig.).

Initially Lindquist seemingly had no ambitions to adopt the programme of the Uppsala school. However, when, for unknown reasons, moving to Uppsala in 1928,[99] he toned down the ecological aspects in favour of plant sociological analysis. Explicitly rejecting his former vegetation analyses,[100] he adopted the Du Rietz'ian programme. Hence Lindquist's dissertation, the most advanced Swedish synecological study so far, was a kind of negotiation product of the second generation ecology in Lund (Lundegårdh, Naumann) and the Uppsala school, and as such it was accepted and applauded by Du Rietz, who defended Lindquist through thick and thin during the following years.[101]

Already before having completed his dissertation Lindquist embarked upon a forestry education. He graduated from *Skogshögskolan* in 1933, became *docent* there and plunged into an entirely new field of research, forest breeding, eventually creating the first Swedish association for practical forest breeding (*Sällskapet för praktisk skogsförädling*), and eventually taking over the chair in silviculture at *Skogshögskolan* in 1947.[102] He did not neglect his old dissertation theme, however. His highly prized monograph on the vegetational history of the Dalby Söderskog National Park in Scania, seems to have functioned as a model for later work on archive-based vegetational history;[103] he wrote a couple of articles on earthworms and snails and their significance for forest soil,[104] and at *Skogshögskolan* he taught silviculture in a way which has been described as »mainly classical and applied ecology«.[105]

But while he did not succeed in enlisting any of his forestry students to do ecology, Lindquist seems to have been a major force behind the turn to the site among the students at »*Växtbio*«, and hence to synecology. His personal knack for enrolling others and implanting synecological ideas in them should not be underestimated. It is easy to imagine why he became an intellectual and personal ideal for several among »the men of the 1930s«, even though his antagonists at *Statens skogsförsöksanstalt* described him as careless and opportunistic.[106] His biographer presents him as an intellectually vivid, enthusiastic and charming person, and an elegant and fascinating lecturer. Whatever the local reasons for Lindquist's success in enrolling others, the fact is that he stimulated a group of men to translate the classical plant geographical and plant sociological problems of the Uppsala school to problems concerning the relation between the site and the vegetation. At least Arnborg, Dahlbeck and Julin were directly, others maybe indirectly, influenced by him. The three of them were sometimes called »Lindquist's boys«. Dahl-

99. He may have moved to Uppsala due to the uncertain situation in Lund, after Naumann had lost the struggle over the chair in botany (cf.2-1).
100. He writes: »A large material of vegetation analyses... have been excluded from publication, since the analytical methods utilized on this material have not been used for the final investigation« (B.Lindquist 1931,p.180; engl.orig.).
101. When Lindquist applied for the chair after Hesselman at *Skogsförsöksanstalten* in 1939-40, Du Rietz supported him against the opposition of Hesselman, Malmström and Romell (Jo 20/12 1940:19).
102. Writing a series of papers, ranging from elm varieties in 1932 to Japanese forest breeding in 1957, he qualified himself as one of the leading forest breeders in the country. After a few years as director of *Sällskapet för praktisk skogsförädling*, he was appointed to the chair in silviculture at *Skogshögskolan* in 1947 (holding the vacancy in 1945-47).
103. One interviwee says: »We read, with certain pleasure, Lindquist's work on Dalby Söderskog (1938)... It opened up new aspects for us by his use of historical sources, and /in/ following a development and emphasizing how an area had been utilized« (interview with HS 24/9 1981).
104. B.Lindquist 1941a, 1941b.
105. Interview with COT 25/9 1981.
106. Unpublished letters between C.Malmström and L.-G.Romell in the Romell family archives; see also Jo 20/12 1940:19.

beck worked as a field assistant for Lindquist in Scania in 1930 and learnt soil analytical techniques from him.[107] Julin also worked as Lindquist's assistant for several years which might explain Julin's somewhat unwarranted investigations of the earthworm fauna at Vessers udde.[108] Arnborg says: »I guess it was Lindquist who initiated /the work/«.[109]

Bengt Pettersson and the dynamics of the culture landscape

A few of the »*Växtbio*« students went their own ways. Magnus Fries (b.1917), yet another member of the Fries family (a nephew of Thore C.E. Fries), connected to Sernander's early studies of vegetation history and wrote his dissertation on the development of quarternary forest vegetation in Western Sweden, adopting von Post's techniques of pollen analysis.[110] Although thinking in site-vegetation terms, his work could not be considered a claim for ecology, however.

The most independent trail taken by any »*Växtbio*« student was that of Bengt Pettersson,[111] who introduced a dynamic perspective on vegetation, a theme cultivated for many decades by Anglo-Saxon vegetation analysts. Like most of his contemporaries, as a young student in the early 1930s he was caught within the framework of the Uppsala school:

> »/I/ *understood nothing of the movements in flora and vegetation. The species grew in their localities, and they stood there as still as in the morphological descriptions of the flora* /handbooks/«.[112]

Being born on the island of Gotland he was expected to do a sociological analysis of Gotland mires. He took his time, however; like Sjörs, Wærn and other »*Växtbio*« students he walked around, observing and collecting plants without an immediate or clear-cut problem for research; although paying considerable attention to the mires he spent a whole decade cycling and wandering around in the peculiar Gotland landscape, thereby acquiring a unique personal knowledge of the flora and vegetation of his native island, as well as its culture. Had he focused on mires he might have been able to apply the Du Rietz'ian schemes, but when taking the whole Gotland landscape into consideration he found it fruitless to keep to the plant sociological doctrines:

> »*Of course it was impossible to apply Du Rietz's theories. It was because the management* /of the land/ *altered.. the landscape changed so extremely rapidly... The altered management gives rise to an immediate reaction in the plant cover, against other plants which spread, other plant groupings, etc. So I found that the Uppsala school was nothing to hold dear*«.[113]

In 1945 he began to photograph test areas in order to document vegetation changes, and after more than a quarter of century of Gotland studies he submitted his dissertation in

107. Interview with ND 20/8 1981.
108. In fact, Julin wrote a small monograph on Swedish earthworms (Julin 1949).
109. Interview with TA 30/9 1981.
110. That work qualified him for an associate professorship in forest botany at *Skogshögskolan* in 1962. A thorough evaluation of his works, made by his contemporaries Hugo Sjörs and Carl Olof Tamm, was done in connection with his application for the chair in plant ecology in Umeå in 1966 (see ED 16/9 1966:17).
111. For biographical details on Pettersson, see ED 16/9 1966:17.
112. B.Pettersson 1958,p.7.
113. Interview with BP 7/2 1982; he considers another source of his dynamic thinking to be a visit to the small Baltic island Gotska Sandön in 1937: »It was a shock... to see how it had changed from the descriptions of Albert Engström and Munthe who had walked there in the 1920s.. and how the whole landscape was so terribly altered«.

1958, being the last in the succession of »the men of the 1930s«. The Du Rietz'ian constancy concept was laid aside, the ideal of untouched vegetation as well. Pettersson presented the dynamics of a cultured landscape. Du Rietz was hesitative:

»Where is the vegetational analysis?«

he is said to have uttered; and he criticized Pettersson's

»repugnance against... using the current concepts and methodology of plant sociology«.[114]

Although sparing of claims for ecology, Pettersson nevertheless advocated an implicit ecological view, which was in clear discordance with the prevalent synecological conception. He made neither pH-measurements nor chemical soil analyses. The whole approach was a break against thinking in terms of correlation between site and vegetation. Instead he introduced a picture of vegetational dynamics where the success and failure of individual species in colonizing the changing cultural environment had a prominent place.

Presumably only Bengt Pettersson among the »*Växtbio*« students had the capacity to carry through this antithesis of the Uppsala school, being considered »a very peculiar kind of scientist«, and »particularly obstinate« by his contemporaries.[115] Otherwise, most of Du Rietz's students followed closely along the lines laid down by »the gang« in the late 1910s.

The offshoots of the Uppsala school in Lund: Sven Thunmark and the limnological group

The Uppsala school, founded by Rutger Sernander, and continued by Einar Du Rietz, constituted the leading group for field botanical and »proto-ecological« investigations in Sweden from the 1910s and for several decades to come. While Stockholm botany remained immune to the enrolment power of the Uppsala school, a number of Lund botanists adopted both the programme of plant sociology and the notion of plant synecology.

After Henrik Lundegårdh's departure, Göte Turesson's conversion to systematics and genetics, and Einar Naumann's failure to win one of the botanical chairs, Lund academic botany once more conformed to the national standards: the cytological and systematical rituals associated with microscopes, test-tubes and herbaria. The delineation of the two chairs made by Areschoug in 1895 was still valid. In 1934 a specialist on cell research, genetics and species formation, Nils Heribert-Nilsson, was appointed to the systematical chair.[116] Heribert-Nilsson taught systematics, anatomy and organography, and although positive to ecology he did not foster any ecological projects. One student with field interests says that »the most boring /subject in Lund in the 1940s/ was botany«, and that he only remembers the organographical diagrams of plants.[117]

114. Du Rietz's evaluation of the dissertation is found in ED 16/9 1966:17.
115. Assessments in ED 16/9 1966:17.
116. The chair had in practice been vacant since Murbeck retired in 1924. Fries was appointedin 1927 (ED 3/6 1927:22), but only held it for a couple of years before his untimely death. The subsequent competition lasted for three years (see ED 23/2 1934:31) until Heribert-Nilsson was appointed.
117. Interview with NN 22/1 1982.

Nor did the holders of the physiological chair display any enthusiasm for field studies, not to mention ecology. Harald Kylin, the encumbant from the early 1920s to 1944, taught cryptogams and embryology, and studied algal pigments largely without noticing that physiology had been revolutionized since the start of his career. Hence, ecologizing in Kylin's department was difficult, and it became even more difficult after he was succeeded in 1944 by Hans Burström, a former research assistant to Lundegårdh,[118] who did not acknowledge field botany and was hostile to synecology.

The Department of Limnology and the Aneboda laboratory proved to be the major outlets for aspiring young lundensian naturalists during the 1930s and 1940s. After Naumann's premature death in 1934, the chair fell vacant and the Department of Limnology was run on a temporary basis, formally under the superintendence of one of the professors in zoology, but in reality under the leadership of Sven Thunmark (1903-1972).

Thunmark had been one of the few recruits to »*Växtbio*« in Uppsala during the 1920s.[119] He is said to have been interested in freshwater organisms already as a secondary school student, and while still an undergraduate he was asked by Naumann to come and work as an assistant in Aneboda. It is said that Thunmark was allocated his first research task on a bike trip: Naumann pointed at a lake as they passed by: »that's Lake Fiolen; write a monograph on it!«. The Fiolen monography was ready in 1931, and turned out to be a counterpart to Lindquist's beech forest investigations. With similar accuracy Thunmark accomplished a number of qualitative floristic-autecological discussions on the causes of plant distribution, as well as a sociological analysis.

Thunmark soon became Naumann's first assistant, and besides working on the Fiolen monograph, was made responsible for the so-called »hydrogeographical investigations«, that is, an inventory of the lakes of the region with respect to shore, sediment and depth conditions, etc. Thunmark recruited large numbers of scouts, also called »dogs«, to assist him »practically free of charge for the state«, as he put it. A first summary of the work came in *Über die regionale Limnologie von Südschweden* in 1937. After Naumann's death, Thunmark's organizing talent found full flight. A dozen secondary school and university students accomplished a lake-iron ore inventory of about 500 South Swedish lakes during the summers of 1935 and 1936:

> *»The students were organized in bicycle patrols, they were in the field all the time, and were closely supervised by Sven Thunmark on his motorbike. Nothing concerning the ways, work, life and conduct of the scouts was hidden from him«.*[120]

Thunmark was an ardent educationalist as well. In the 1930s he initiated courses in limnology for secondary school teachers, seemingly a very popular activity,[121] and courses in microbiology and microscopic techniques, a spin-off from the work on his later dissertation on the microorganism communities of recent iron ochres.

Thunmark remained in all respects true to the Uppsala school's view of nature. His dissertation, the huge collection of micro-organism community analyses and data on

118. ED 1/12 1944:16.
119. For biographical details on Thunmark, see Björk 1976 and ED 17/1 1947:1.
120. Björk 1976.
121. According to announcements in *Medlemsblad från biologilärarnas förening* Thunmark's courses were one of the attractions during the summer holidays.

water chemistry, *Über rezente Eisenockern und ihre Mikroorganismengemeinshaften* of 1942, was in full accord with Du Rietz's plant sociological programme and the Uppsala school's restricted definition of synecology. In most respects, including political views, Thunmark was a true follower of Du Rietz. On the other hand, he was not as successful in enrolling his own graduate students to the programme in turn. After being appointed to the vacant chair in limnology in Lund in 1947,[122] Thunmark built up a rather diversified department. In 1949/50 more than a dozen younger research students were attached to it, working on a variety of subjects: some on freshwater animals or fishery biology, others on plants or microorganisms; some on sediments. One studied the biological effects of industrial water pollution[123] and another[124] took up the effects of lowering the level of lakes. Thunmark himself had turned to water pollution problems by 1943 at the request of a South Swedish municipality, and from the mid-1940s he devoted his main efforts to water pollution.

Thunmark's investigations, especially his inventories, and courses had extensive impact. As indicated above hundreds of students and secondary school teachers passed through the Aneboda laboratory during the 1930s and 1940s. In that respect Thunmark was a most important contributor to the later ecological consciousness. Some of the investigations were also announced as »ecological«, but in general neither his research, nor his courses, were claimed as such. Nor at the time was his work on water pollution translated into the language of ecology. Thunmark and his students continued to consider themselves as limnologists, the delineation of an independent scientific social order instigated by Einar Naumann in the late 1910s. In that respect, the limnological group in Lund did not contribute to the growing social order of ecology.

Stig Waldheim in Lund: from floristic inventories to causal synecology

Although field botanists had little support at the Department of Botany, their craft hibernated among the amateurs in the venerable *Lunds botaniska förening*.[125] In 1938 Henning Weimarck, a leading member of the Association, and *docent* in botany, initiated an inventory of Scanian flora and vegetation. The field work was mainly conducted by undergraduates and amateurs under the auspices of Weimarck and the Association. Some of the undergraduates utilized their field experience for dissertation work, and eventually three theses, employing ecological terminology, were submitted. The two of them restricted themselves to short notes on »habitat ecology«, viz., descriptions of growth sites, and concluding qualitative discussions of the possible causal relation between site factors and geographical distribution. Hence they subsumed ecology to the descriptive floristic geography of the Scanian inventory.[126]

The third was different. Stig Waldheim started like the others, writing short plant geographical articles[127] and a work on the peat mosses of the province of Närke, but soon

122. See ED 17/1 1947:1 (cf.3-5).
123. Sörensen 1948.
124. Lilljeroth 1950.
125. For a history of the Association, see Håkansson 1958.
126. Almborn (1948) studied the distribution of lichens, including some qualitative discussions of »habitat ecology«; O.Andersson (1950, a licentiate thesis) wrote on fungi on sandy soils.
127. For biographical details on Waldheim, see ED 12/11 1948:9.

embarked on a detailed work on soil-inhabiting Scanian mosses, attempting to

> *»der zwischen dem Vorkommen verschiedener Böden unter den Verbreitung der Kleinmoose in Schonen herrschende Zusammenhang nachgewiesen werden«.*[128]

He became a frequent visitor to the Uppsala seminar in the late 1930s and early 1940s, and a few years later he presented a licentiate thesis to Du Rietz, who was »very impressed by Waldheim«.[129]

Waldheim made company with the other »men of the 1930s« in focusing on the vegetation-site relation. But he did not stop at the synecological correlation. He also wanted to elucidate the causal relation between the site and the vegetation by studying the ecological demands of the *individual* plants, hence taking up the largely forgotten thread from Warming half a century earlier. He declared programmatically that:

> *»Die Synökologie, d.h. das Studium des Gesellschaftshaushaltes... ist also eigentlich auf die Autökologie der Einzelpflanzen zurückzuführen«.*[130]

Consequently Waldheim did not confine himself to the qualitative or semi-quantitative method of correlating vegetation and site prevalent in Uppsala, but considered floristic and autecological analysis of the relation of the single species to its environment to be as important as sociological analysis.[131]

Working all by himself in Lund, including developing a small laboratory for soil and water chemical analyses, his attempts towards autecological explanations proved too categorical and were ill received by the physiological botanist Hans Burström who delivered a blistering attack at the oral defence act, accusing Waldheim of circular argument.[132] The faculty had to call in additional referees to assist the evaluation committee, and only after some hesitation was he given the status of *docent*.[133]

Whatever the quality of his work, it is hard to deny that Waldheim went the whole way to ecology. More than most of his contemporaries in Uppsala he was a real ecologizer, and he enrolled students into a small but growing ecology group. A contemporary describes him as »rather inspiring, a very enthusiastic person«.[134] His professor, Heribert-Nilsson, hardly exaggerated when he described as »striking« Waldheim's

> *»capacity to gather younger students around him and arouse their interest for his research tasks«.*[135]

When *1945 års universitetsberedning* (the University Commission of 1945) asked for suggestions for new positions, Heribert-Nilsson proposed a new associate teaching position, apparently intended for Waldheim:

128. Waldheim 1947,p.5.
129. Interview with HS 24/9 1981.
130. Waldheim 1947,p.63.
131. One who knew him well says: »Waldheim was more of an autecologist than a synecologist. /He/ was a skilled systematician, so he saw plants and species« (interview with NN 28/12 1981). On the other hand Du Rietz was an excellent systematician too, although seeing the plant communities as individuals of a higher order.
132. According to interview with NN 28/12 1981.
133. ED 12/11 1948:9.
134. Interview with HS 24/9 1981.
135. ED 12/11 1948:9.

> »*Within the plant-geographical* /sic!/ *part of the subject* /i.e., botany/ *five doctoral and licentiate students presently work with plant geographically ecological investigations. Since these are directed primarily towards the influence of the substrate on the stand formation in nature, a special laboratory for soil and water analysis has had to be set up during the year*«.[136)]

The awkward neologism »plant geographically ecological investigations« signifies the outcome of negotiations between Heribert-Nilsson trying to enrol Waldheim plant geographical problems, and Waldheim's attempt to stake out his own knowledge territory of ecology.

Whatever his professor had in mind, however, Waldheim was on the ecology trail. He was gathering students around him, and building up an ecology group in two small rooms at the old Botanical Museum in Lund. Some of the members of this small ecology group would turn out to be major actors in the ecologization of Sweden in the 1960s and 1970s.

3.3 The experimentalists' claim for plant ecology

During the 1910s and 1920s the predominant claim for ecology had been forwarded by scientists trained in the »new German« laboratory techniques; they were all more or less connected to the Stockholm school. These leading second generation claimants of an experimental and/or physiological plant ecology, viz., Lundegårdh, Romell and Turesson, had all lost the struggle for the leading academic positions during the Great Polemic. As shown in the preceding section, the Uppsala school, headed by Du Rietz, manifested itself throughout the 1930s and 1940s, spreading its influence to Lund as well, and identifying ecology with the Uppsala version of synecology. The claim for an experimental and/or physiological plant ecology was not totally eclipsed, however. In Stockholm an »invisible« college of ecophysiologists continued the tradition, although on a diminished scale, and in Uppsala Elias Melin continued his ecophysiological studies. Out of these local milieus emerged some of the leading actors in the third generation of Swedish ecologists.[137)]

The legacy of the Stockholm school

Although little was left of the once so vibrant Stockholm school of ecophysiology by the 1930s, both Gottfrid Stålfelt and Lars-Gunnar Romell continued to pursue ecophysiological investigations. Indeed, Stålfelt, who had been appointed associate professor in

136. SOU 1946:81,p.129.
137. In Lund neither Harald Kylin nor his successor from 1944 onwards, Hans Burström, took any initiatives to ecophysiological investigations, and none of their students did so either. With respect to Göteborg, *Naturvetenskapliga forskningskommittéen* (the Natural Science Research Commission of 1945, cf.3-5) proposed Tore Levring, who had been trained in cytology and plant physiology by Kylin in Lund, to a personal professorship in marine botany. On the part of the Commission (with John Runnström playing a major role) the deliberate aim was to stimulate studies in marine *ecology* (SOU 1945:48). A private donation eventually made possible the erection of a laboratory of marine botany in Göteborg. Levring, never took up ecological studies, however, but almost exclusively devoted himself to systematical and morphological studies during the 1950s and 1960s.

botany at Stockholm in 1926, turned out to be one of the main claimants of ecology during the period considered here. Some of his most important ecological works were published in the ten years between 1935 and 1944.[138] In addition he taught courses in »physiology, anatomy and ecology« at *Lantbrukshögskolan* during the first half of the 1930s,[139] and gave lectures and courses in »plant ecology« in the Department of Botany in Stockholm. After having been summoned to the chair in »botany, particularly physiology and anatomy« after Rosenberg in 1941(1940),[140] he campaigned to secure a chair in plant ecology and a field station[141] for ecological research:

> *»The task of ecology is to investigate the outer conditions of the plants and the existing movable balances in nature«,*

he wrote in a letter to the Vice-Chancellor, and continued:

> *»Thus, ecology is an extraordinarily important part of botany both for the common man as for the people in general. It is regrettable that it has been neglected till now in the schools, though this can be explained by the fact that no teaching worth speaking of takes place at the state universities«.*[142]

The last sentence should be taken literally. Stålfelt was evidently of the opinion that Einar Du Rietz and the Uppsala school were *not* pursuing ecology.

But in spite of this active claim and politicking for ecology, and in contrast to his successful enrolment of graduates to do physiological research, he did not succeed to secure authorization for ecology. Nor did he manage to enrol any graduate students for ecological investigations. His claim for ecology was an ephemeral one, and although his ecological work was of great indirect importance for the ecologization process of the 1950s and 1960s (cf.4-2,4-3 and 4-4), it did not constitute the basis for a local social order of ecology.

So much for Stålfelt's management of the legacy of the Stockholm school, what about Lars-Gunnar Romell?

Romell had been the leading figure intellectually among the second generation plant ecologists in Sweden during the 1920s, partly resolving the conflict between the descriptive sociological and experimental physiological approaches to nature studies. After having lost the competition for the Uppsala chair in plant biology in early 1934, he returned from Cornell and was appointed scientific officer at *Skogsförsöksanstalten* by Hesselman, where he continued his ecological work in relative obscurity. He launched a series of investigations of forest productivity and processes of decomposition, paying special attention to soil fungi and their role in the nutrient economy of coniferous forests, and to the production of moss, twigs and litter.[143] Both problems can legitimately be

138. E.g., Stålfelt 1937 and 1944.
139. His courses did not have any effect with regard to ecologization at *Lantbrukshögskolan*; besides Stålfelt's few lectures, experimental ecology was not a part of the curriculum in the 1930s. Neither Göte Turesson nor Henrik Lundegårdh seems to have taught ecology. As a consequence, ecology was never established at *Lantbrukshögskolan* during the decades considered here. In fact, no single student attending the agricultural college during the 1930s or 1940s made claims for ecology.
140. Stålfelt was summoned by the university in 1940, but it was not confirmed until late 1941 (see ED 12/12 1941:21).
141. Stålfelt was still keeping the Hallands Väderö station (cf.2-3) in repair, but did not utilize it – it was too far from Stockholm.
142. Quoted from letter to the Vice-Chancellor 8/11 1950 (in »Utredning rörande fortsatt upprustning av Stockholms högskola; betänkande 19/5 1958«; *Riksarkivet*, komm.nr. 1635).
143. Romell 1939a and 1939b.

claimed as cognitive forerunners of the large scale research project *Barrskogsprojektet* (the Coniferous Forest Project) in the 1970s (cf.4-5). When his close friend and colleague Carl Malmström, was appointed successor to Hesselman in 1941,[144] they together continued on a research path defined as »causal forest plant community research and nutrition ecology«.[145] That is, Romell continued his ecological programme launched in the late 1910s.[146] But with one exception he was unsuccessful in enrolling graduate students to do ecology; like all the other extra-academic research institutes, *Skogsförsöksanstalten* had no educational responsibilites.

The single exception was a young botany student in Stockholm, Carl Olof Tamm,[147] who took up the thread from both Stålfelt and Romell during the 1940s – the only third generation ecologist to continue the tradition of the Stockholm school of ecophysiologists. Tamm was practically born into the Stockholm school. Being the son of Olof Tamm (one of Hesselman's former assistants, and later professor in soil science at *Skogshögskolan*) he was early introduced to the factional struggles between Sernander and Du Rietz on the one hand and Hesselman and Romell on the other:

> *»I was inoculated with a certain scepticism against pure descriptive ecology, I suppose«.*[148]

After university studies in Stockholm during the war years, including a course in botany with Stålfelt, where he was trained in basic ecophysiology,[149] he was employed in 1945 as an assistant to Lundegårdh's former student Hans Burström, now professor of plant physiology in Lund. »It was a causal interest«, he says,

> *»I suppose I thought of becoming a physiologist rather than an ecologist«.*

He took up a problem suggested by Malmström and Romell – the so-called nitrate plant problem which he worked on both experimentally in the laboratory, and in the field.

Tamm might well have become a physiologist. But he returned to Stockholm and *Skogsförsöksanstalten* to do field work:

> *»I moved from these water- and nutrient solution experiments to field material«,*

and in 1948 he was employed by Malmström and Romell for a so called Ebermeyer-investigation, i.e., an attempt to quantify the nutrient cycle of the forest trees. Besides these mainly routine investigations, he had ample time to doctoral studies.

In his dissertation finished in 1953[150] Tamm connected several problems, drawing

144. In competition with Romell and Bertil Lindquist (Jo 20/12 1940:19).
145. Malmström 1952.
146. Romell's and Malmström's claims for ecology as an independent science were partly made possible because the majority of the Board of *Skogsförsöksanstalten* favoured basic research as a means for rational forest management: »A fundamental investigation, seemingly having no connection with practice, may be more seriously and more effectively directed towards practical aims, than a superficial orientation with pronounced practical purpose« (Minutes of Board meeting 12-13/11 1940, in Jo 20/12 1940:19). The research policy was dependent, however, on the power balance between arguments favoring basic research and arguments for more practically oriented investigations. The minority of the Board, largely representatives of the forest industry, preferred practical men to pure scientists (See minutes of Board meetings in Jo 20/12 1940:19). An acute case of water-logging or acidification of forest soil might at that time have resulted in a displacement of the power balance in favor of a more practical orientation, and hence obstructed claims for ecology.
147. For biographical details on Tamm, see Jo 29/3 1957:18, Jo 10/8 1973:1, and interview with COT 25/9 1981.
148. Interview with COT 25/9 1981.
149. It was during his botany studies in Stockholm that he started his investigations of the population structure of flowering plants, which have later been regarded a classic in plant population ecology, cf. Harper 1977.
150. Tamm 1953.

upon his early life-table studies,[151] his acquaintance with the problematic of plant community analysis,[152] his physiological training, and his routine work on nutrient cycling. He chose to investigate the growth, nutrition and population structure of a plant community consisting of one single species of moss, and trying to explain the observed ecological relationships and environmental factors determining the structure of the community

> *»I realized that I had an unique chance to study the yearly production in a pure plant society... it was not without a prototype since Romell had made a similar investigation of the production of forest moss in much the same way as I did in larger scale«.*[153]

Combining a detailed analysis of the nutrient uptake and content of the one-species community, and a statistical treatment of the size and growth of single plant individuals within the community, Tamm seemed fully to redeem Stålfelt's programmatic demand that problems in causal plant sociology require

> *»co-operation or even better a grounding in both areas* /experimental physiology and descriptive plant sociology/ *on the part of the single scientist«.*[154]

That is, Tamm, like Romell had done three decades earlier, claimed ecology as a science able to combine a descriptive understanding of the plant community with the experimental understanding provided by work in the physiological laboratory.

Elias Melin's laboratory in Uppsala

The other place where experimental investigations of plants under natural conditions was maintained was Elias Melin's laboratory in Uppsala. We recall that Melin had considered his work on the mykorrhiza-phenomenon during the 1920s as ecological. After having been appointed to the one chair in botany in 1930, Melin still kept in touch with ecology. Although concentrating on physiological mechanisms, particularly methods for the purification of microorganism cultures, he nevertheless always tried to relate his experimental work to natural conditions. Interviewees emphasize the basic »proto-ecological« atmosphere prevailing at Melin's laboratory during the 1930s and 1940s. Hence, although the work done was rarely claimed as ecological, and was in fact institutionalized as plant physiology, a number of later claimants of ecology were trained there.

For example, Erik Björkman wrote his thesis on mykorrhiza, combining physiological laboratory work with a field botanical interest.[155] His work was closely connected to practical forestry problems, and in 1947 he was appointed professor of forest botany at *Skogshögskolan*. In one sense he might be considered a third generation ecologist – he took part in the informal ecophysiological discussions together with Stålfelt, Tamm and others in Stockholm, he taught extensively on plant-environment relations, and, most important, he was one of the co-founders of the *Oikos* journal, and served as chairman of the *Oikos* society from 1955 (cf.3-5). On the other hand he never explicitly claimed his

151. Cf. note 149.
152. Tamm got acquainted with Stig Waldheim during his Lund years, and it is likely that Waldheim's failure to reduce synecological analysis to an analysis of the autecological reactions of individual plants (cf.3-2), was one of the incentives for his own investigation.
153. Interview with COT 25/9 1981.
154. ED 12/11 1948:9.
155. Björkman 1942.

own work as ecological, he never published any substantial ecological work, nor did he use his position to build an ecological research group at *Skogshögskolan*.[156] Another Melin student, Börje Norén (1919-1983), who transformed a »proto-ecological« problem into a laboratory physiological problem in his study of myxobacteria from Swedish soils, later became the first claimant of a microbiological ecology in the 1960s.[157]

A third Melin-student, Börje Åberg (b.1911), studied the influence of water regimes on the morphological development of plants. Åberg's dissertation was a clear-cut example of the core of Melin's ecophysiological research programme. In order to accomplish:

> *»eine nähere Klarlegung der Bedingungen dieser Formbildung«*,

it was necessary to translate it into physiological problems:

> *»Ein ökologisches Problem dieser Art musste ferner notwendig zu physiologischen Fragestellungen führen«*[158]

implying a claim for ecology as an experimental science on a par with laboratory physiology:

> *»Die Ökologie muss...ebenso wie der Laboratoriumsphysiologie versuchen, die Wirkungen einer bestimmten Variablen zu isolieren, wenn auch die Natur in dieser Beziehung viel begrenzter Möglichkeiten bietet als das Laboratorium«*.[159]

This approach was very much akin to Runnström's and Lindahl's ecophysiological studies of environmental influences on the morphology of sea urchins more than a decade earlier (cf.2-5).

Going further into problems of physiology, and never returning to ecological problems, however, Åberg did not contribute to the build-up of a new scientific social order of ecology. But a close collaborator of his did. This was Wilhelm Rodhe, who was also a student of Melin in the late 1930s and early 1940s.[160] Already during his secondary school years in Uppsala, Rodhe had pursued plankton investigations. After having read some of Naumann's works, he was determined to continue with limnology and was able to spend the summers of 1933 and 1934 with Naumann in Aneboda where he did »ökologischen Planktonstudien« (his undergraduate thesis dealt with the horizontal distribution of zooplankton).[161] In Aneboda he met with Åberg, and together they embarked upon a large-scale survey of light conditions, and thermal and chemical layering of a number of south Swedish lakes.[162]

Rodhe was recruited into Thunmark's scouting team, but as many other graduate students, he did not get on well with Thunmark,[163] and consequently returned to be an

156. In spite of the fact that both Erik Björkman and Bertil Lindquist were professors at *Skogshögskolan*, ecology was never established there during the decades considered here. Nor did Bertil Lindquist (cf.3-2) get any ecology students. This was probably due to the strict professional training programmes. The forestry students got a good training in practical subjects as well as in more fundamental sciences, like botany. But, in contrast to the situation at the universities, the curriculum hardly allowed for ecological research initiatives. In fact, no student attending *Skogshögskolan* during the 1930s or 1940s made claims for ecology.
157. Cf.4-2, note 41.
158. B.Åberg 1943.
159. Ibid.
160. Biographical details on Rodhe are taken from ED 30/12 1948:7, and from an interview with WR 8/2 1982.
161. Åberg and Rodhe 1942.
162. Ibid.
163. It is a general opinion among interviewees that Thunmark could not stand graduates who were too independent. Some call him »tyrannical«.

assistant in Melin's laboratory. After having extended his studies of environmental conditions to a number of lakes in the Uppsala region, he took up the problem of lake plankton productivity as a function of the environmental requirements of plankton algae. »These questions«, he said, could be studied in two ways:

> *»by ecological work on the biotopes themselves and by physiological investigations under experimental conditions. Neither of these ways alone, however, leads to the final aim. To reach this a combination of ecological and physiological methods and data is necessary«.* [164)]

He regarded the distinction between the two approaches as follows:

> *»In ecological questions it is often enough to state how an organism reacts to different environmental conditions, since such statements can be used as explanations of the complex relationships in nature. It is a purely physiological task to explain why the organism reacts in just the manner stated«.* [165)]

Thus, through being recruited to Melin's laboratory in Uppsala, Wilhelm Rodhe's original naturalist interest was eventually translated into a professional physiological outlook on nature. In contrast to several other Melin-students and ecophysiologists, however, he did not commit himself fully to laboratory physiology. Instead he devoted his energy to the creation of a field laboratory. Supported by Melin he took responsibility for a piece of land and a farm house donated to Uppsala university at the lake Erken,[166)] where he was to pursue:

> *»on one hand quantitative field studies of the productivity of the lake, with respect to different organisms, and on the other, and in connection with the former, experimental investigations of the conditions for their production«.* [167)]

With Rodhe reviving the ecophysiological programme formulated by the second generation of ecologists of the Stockholm school (in which Melin should be counted a peripheral member), he became the most important third generation claimant of ecology as an experimental science, and later one of the most important sources for »proto-ecological« and ecological work during the 1950s and 1960s. When trying to enrol university and state authorities for his cause, however, he did not do it in the name of ecology. Like Naumann, who had ensured financial support for his new scientific endeavour under the name of limnology, Rodhe also delineated his approach to nature under the label of limnology. The Erken laboratory was early denominated a laboratory for »limnological field research«,[168)] and during his work as secretary to *1945 års universitetsberedning* (cf.3-5), he assured himself a professorship in limnology at Uppsala in 1948.[169)]

164. Rodhe 1948,p.5 (engl.orig.).
165. Ibid.,p.6 (engl.orig.).
166. The property had been donated to the university in the late 1920s; the surplus should partly be used for grants for »scientific studies, also for practical purposes, of the fishes, crustaceans and other conditions of Swedish waters, particularly...lake Erken« (quoted from Rodhe 1946,p.38).
167. Rodhe 1946,pp.41-42.
168. Rodhe 1946.
169. See ED 30/12 1948:7; Rodhe's only competitor was Gunnar Lohammar (cf.3-2, note 66), who withdrew his application.

3.4 The emergence of an animal ecology

As we have seen from the preceding two chapters the scattered claims for ecology in Sweden during the first three decades of the century were mainly forwarded by academic botanists. However, the number of zoologists making claims for ecology increased considerably during the 1930s and 1940s. Although entering the lecture halls and exercise laboratories at any of the three university zoology departments at Uppsala, Stockholm or Lund was a rather tough experience for a newly matriculated naturalist, who had spent his school summers mounting insects or learning bird songs, the opportunities for translating this interest into an ecological research topic were much greater than in the 1910s and 1920s. Particularly in Uppsala and Lund the chances for ecologization had been increased after the appointment of Sven Ekman and Torsten Gislén to the chairs in zoology in 1927 and 1932 respectively.

In this section we will discuss some of these claims, the extent of »proto-ecological« investigation of the relations between animals and their environment, and the extent of ecologization of animal field studies at the three zoology departments during the 1930s and 1940s.

The practitioners' claim for ecology

Before turning to academia the question concerning the ecologization of practically oriented animal studies should be considered. In fact, a number of zoologists working at applied research institutes made extensive »proto-ecological« investigations in the 1930s and 1940s, some of which were occasionally proclaimed as ecological.

The most conspicuous case was Gösta Notini who worked on plant pests in the 1930s and became a pioneer with regard to organized game research in Sweden in the 1940s. After undergraduate studies in zoology Notini (1908-1968)[170] was employed at *Statens Växtskyddsanstalt* in Stockholm.[171] This was the only agricultural research institute outside *Lantbrukshögskolan* dealing with animal-environment relations, and its central objects of research were the relationship between pests and their host plants, investigations of pest life cycles, pest distribution, its dependence on climatic factors, etc. These studies were occasionally defined as ecological, for example, Notini's investigation of weevils on clover, submitted as his licentiate thesis in zoology in Stockholm.[172]

Notini subsequent research was on game. Institutionalized game research was something new in the 1940s. It will be recalled that game was an integrated part of the natura-

170. For biographical details on Notini, see Jo 30/6 1949:68.
171. The institute was a continuation of the Department of Entomology at *Centralanstalten* (earlier *Entomologiska anstalten*, cf.1-3), and had became independent in the great reorganization of Swedish agricultural research in 1932; the institute was headed by Albert Tullgren until 1939, and then by Hernfrid Witte (cf.1-3).
172. Notini 1938; his unpublished licentiate thesis, submitted in 1939, was entitled »Ekologiska undersökningar över spetsvivlar (Apion Herbst)«. In addition E.Johansson published some results of »ecological« investigations (1937). Otherwise, most of these investigations were, of course, »proto-ecological«. For a review of the work of the institute, see Granhall 1960 and publications in the series *Meddelanden frän Statens Växtskyddsanstalt*.

lists' concern; later the zoologists, from Sven Nilsson to Einar Lönnberg and Sven Ekman, considered game and hunting to be important components of their professional concerns. But no claim for an independent game research was made at that time, »mainly because of insufficient economical stimulus«, as a recent observer puts it.[173] In the late 1930s, however, *Svenska Jägareförbundet* initiated a series of game investigations and recruited Notini as scientific officer. Notini pursued several investigations, for example, of hare and of East coast sea bird stocks, but also of animals supposed to be noxious to game, including crows and badgers.[174] He used some of this material to write a doctoral dissertation on the feeding habits of the badger in Sweden,[175] considered to be an independent, pioneering and paradigmatic work:

> *»an exemplary biological monograph... a fundamental work within the field of vertebrate biology«,*[176]

and also considered sufficiently good to secure Notini a chair in 1949 when the old assistant teaching position in forest zoology at *Skogshögskolan* (cf.2-1) was converted into a professorship in »general forest zoology with game protection«.[177]

Thus, a zoologist with extensive experience of applied animal investigations had been appointed to a newly created chair in forest zoology. That was a significant extension of institutionalized »proto-ecological« studies. But from the point of view of ecologization it did not signify much. Firstly, while it is true that the instruction for the chair included the notion of »ecology«,[178] the establishment of game research should rather be seen as a late case of zoologization. Secondly, despite using the word »ecology« now and then, Notini made no serious attempts to claim ecology as an independent science. In fact, both he and his assessors considered his work on the badger a »biological« study, and contrary to several other of his colleagues at various research institutes, he did not participate in the foundation of the national association for ecology. Thirdly, and most important Notini never recruited any students to continue his work.[179] Hence, even though he certainly did make a breakthrough with regard to outdoor animal studies, and was the first zoologist in Stockholm to submit a dissertation on field zoological problems, he never contributed to the social order of ecology in Sweden.

The fate of not being able to recruit students to ecology Notini shared with Karl-Herman Forsslund (1900-1973)[180] at the Entomology department of *Skogsförsöksanstalten*. Being endowed with a naturalist background, Forsslund was engaged as a research assistant to Trägårdh, who had started a series of investigations of insects and their environment in the late 1920s (cf.2-5). But

173. Haglund *et al* 1980,p.268.
174. The results of the early game investigations are reviewed in Haglund *et al* 1980.
175. Notini 1948.
176. Evaluations in Jo 30/6 1949:68.
177. Ibid.
178. According to the Board, the purpose of the chair was delineated as: a) »anatomy, ecology and knowledge of species«, b) »the importance of the fauna for forest management taking the interplay between different components of the forest fauna into consideration«,c) pest control, d) knowledge of game, and e) hunting biology (Jo 30/6 1949:68).
179. On the contrary, Notini's work was re-evaluated in the 1950s and 1960s; he was accused of manipulation of his primary data, and one doctoral dissertation at the Department of Zoology at Stockholm in the 1960s was entirely devoted to a re-examination of Notini's findings (Skoog 1970, cf.4-3).
180. For biographical details on Forsslund, see C.H.Lindroth 1973 and Jo 29/3 1963:13.

»this 'biological period'... by and by passed on to a period of ecological research, which has been more and more characteristic of the scientific production at the department«,

writes a member of the department in retrospect in 1952.[181] Although occasionally being considered ecological these extensive studies were only rarely proclaimed as such, however.[182] A main contribution was Forsslund's decade-long project on the distribution and food ecology of a group of soil arthropods, originally started by Trägårdh in the late 1920s, and eventuating in a dissertation in 1944.[183] It was hardly an original work from the point of contemporary ecology, being mainly an inventory akin to what ecological animal geographers in principle had been doing for many years, but it was generally considered a serious claim for ecology, and a testimony to Forsslund's ecological knowledge and perseverance.[184] It ensured him a position in the national ecological association, *Oikos*, which was founded in the late 1940s (cf.3-5). After having lost the competition over the chair after Trägårdh,[185] Forsslund was allowed to continue in a subordinate position at the department, but did not make any further attempts to ecologize his work, and did not recruit any students.[186]

The cases of Notini and Forsslund show, that in spite of extensive »proto-ecological« investigations and occasional claims for ecology, studies pursued at applied research institutes in a practical context were as a rule not translated into the language of ecology. The same conclusion can be made with respect to marine and freshwater fishery research. A few scientists employed by *Svenska hydrografisk-biologiska kommissionen* at *Havsfiskelaboratoriet* in Lysekil carried out investigations on the relations between fish stocks and environmental factors. This work was defined as »fishery biology«, however, and although one of them had an ambition to do ecology, he eventually translated his research into taxonomy;[187] otherwise none of them identified their work as »ecology«.[188]

At *Sötvattenslaboratoriet* at Drottningholm Gunnar Alm and his associates pursued studies of lake environments, the biology of different fish species, investigations on fish diseases, etc., but these studies were only occasionally claimed as ecological.[189] One who did translate fishery problems into ecology was Lars Brundin (cf. below), who was employed in 1937. Officially working with problems related to freshwater fishery, he took advantage of the situation to deal with limnological, zoogeographical and entomological problems, partly translating these into extensive notes on the ecology of chironomids and other bottom insects – his grand monograph on chironomids from Swedish primary rock lakes contains qualitative discussions of the ecological aspects of chironomid distribu-

181. Butovitsch 1952,p.197.
182. For a review of the investigations, see Butovitsch 1952 and reports in *Meddelanden från Statens Skogsförsöksanstalt*.
183. Forsslund 1944; for an earlier contribution, see Forsslund 1938.
184. This was the opinion of his peers, see, e.g., C.H.Lindroth 1973.
185. During the early war years large forest areas were devastated by insect attacks, and activities of the department were intensely directed towards developing methods for the application of chemical insecticides. When Trägårdh's chair became vacant in 1943-44, an applicant specializing in chemical insect control was preferred by the Board (Jo 10/3 1944:16). The decision triggered heavy protests from the side of zoologists, but after a year's negotiations the question was finally settled in favor of the practical argument.
186. As a late appreciation of his soil ecological research he was given the title of professor in 1963 (Jo 29/3 1963:13).
187. Hans Höglund (cf. below).
188. For an overview of the work at *Havsfiskelaboratoriet* during the period considered here, see *Svenska hydrografisk-biologiska kommissionens skrifter, NS Biologi, 1* (nr 1-7) (1925-35), *2* (nr 1-10) (1937-48).
189. See Alm 1943.

tion.[190] Together with Arne Lindroth and Gunnar Svärdson (cf. below), Brundin joined an informal group at *Sötvattenslaboratoriet* discussing field study problems and organism-environment relations. They never made any joint claims for ecology, however, never established any ecological seminar nor enrolled students to work with ecological problems.

Continued resistance to naturalist studies and ecology in Stockholm

Resistance to naturalists and ecological dissertation work was most apparent at the Department of Zoology in Stockholm. The naturalist revival became apparent among zoology students around 1930, and in 1933 some of them founded a faunistic association, *Svenska faunistiska sällskapet*, with the simple aim of »broadening the knowledge of the Swedish fauna«.[191] They wanted to do this by increasing contacts between professional zoologists and amateurs,[192] by stimulating animal geographical and »ecological« investigations,[193] and finally by creating a kind of provisional summer field stations (that is, asking land owners and farmers to support »a younger biologist or amateur biologist sent out by the association«).[194] They arranged excursions and lectures, and in 1938 they secured economic support to publish a beautiful and lavish magazine, *Populär biologisk revy*.[195]

The zoology students had some support in the faculty. When the zoology chair became vacant in 1944 Gottfrid Stålfelt tried to intervene on behalf of ecology. One of the applicants, the Uppsala zoologist Gösta Jägersten, had often supplemented his comparative anatomical and morphological investigations with studies of the influence of environmental factors on larval development, that is, studies akin to those made by Runnström and Lindahl in the late 1920s (cf.2-5). Probably for science policy reasons Stålfelt translated these studies into the language of ecology, in fact inflating them out of proportion:

> »*He* /Jägersten/ *has furthermore attained qualifications in the ecological part of zoology, which I consider a special merit in this connection. The fact is that animal ecological research in this country has not reached the position it is entitled to, taken its practical significance into consideration. The appointment of Jägersten would result in a much needed strengthening of animal ecological research, and through the ecological element in his competence, the foundation of zoology* .../at the university/ *would be further extended*«.[196]

But Stålfelt's intervention was rebutted by Holmgren. When Jägersten applied for a chair in Uppsala a few months later (where his »ecological qualifications« were not even mentioned), and the Stockholm chair was announced for a second time, the faculty directed (most probably on Holmgren's initiative) that

190. Brundin 1949.
191. Quoted from a leading article in *Populär biologisk revy*, *1*(1),p.4 (1938).
192. They invited a number of leading zoologists in the Stockholm area to act as consultant board members; professor Holmgren was not among those invited, however.
193. But they also wanted to support racial biological investigations!
194. From leading article in *Populär biologisk revy*, op.cit.
195. Under the name *Populär biologisk revy* 1938-39, 1941-42, later as *Svensk faunistisk revy* and finally as *Zoologisk revy*.
196. ED 21/12 1945.

»the future holder of the chair ought to have shown professorial competence within the morphological part of zoology, and to be obliged to teach and supervise scientific work within morphology«.[197]

Consequently a traditional zoologist and specialist on the comparative anatomy of the fish cranium, was appointed.[198]

Neither did Holmgren allow the student naturalist movement to get a foothold. Under his reign through the 1920s, 1930s and 1940s Stockholm zoologists were completely oriented towards comparative-anatomical problems, as witnessed by the long series of morphological dissertations produced.[199] Holmgren flatly refused all naturalist elements, in undergraduate courses as well as in graduate research. A former student who was an amateur bird watcher remembers his first contacts with Holmgren:

»I got a first rate shock, it was terrible. The professor began saying 'As I said the last semester', then it was pickles, animals in glass jars and such things. And the professor said that watching animals outdoors was childish«.[200]

Those having a naturalist interest and continuing with graduate studies wrote their anatomical dissertations, and went bird-watching in their spare-time. The only exception was Notini (cf. above), but he had not passed through the inner lines of the department – in addition he submitted his dissertation after Holmgren's retirement.

»Ekman's boys« in Uppsala: an extension of the causal ecological animal geographic programme

While a graduate student in Stockholm was expected to do comparative anatomical investigations a graduate student disposed towards field studies in Uppsala in the 1930s had better chances of following his inclinations. Although Nils von Hofsten was a comparative anatomist with interests in human genetics,[201] he did not resist either naturalist studies or ecology. Douglas Melin had a positive attitude to animal field studies and attracted at least one student to do graduate work of this kind, viz., Bertil Kullenberg (cf.3-1).

Sven Ekman's role was ambiguous. After all he was a pioneer in animal field studies, not only in Uppsala, but in the whole country. But it is also true that besides giving an environmental touch to his lectures and writings, Ekman did not project himself as a field biologist, even less as an ecologist. In that sense he constituted a striking contrast to his flamboyant and high-spirited colleague Sernander a few hundred meters down the road. Although the two men worked on rather similar problems, they exhibited altogether different personal research styles. Sernander kept court, while Ekman was shy: the »billy-goat« they called him. After his appointment to the chair in general zoology in

197. According to Gislén's assessment, in ED 30/6 1948:4.
198. Torsten Pehrson; see ED 30/6 1948:4.
199. Between 1930 and 1950 all 12 dissertations (except Notini's, see above) dealt with comparative anatomical or cell biological problems.
200. Interview with GS 14/3 1983.
201. Von Hofsten was extra-ordinarily responsible, knowledgeable and popular as undergraduate teacher, but spent most of his time in the 1930s and throughout the war on university administration (he was pro-vice-chancellor 1933-43 and vice-chancellor 1943-47), besides writing on eugenics and population policy problems. As attested by interviewees von Hofsten had a great personal influence on his students, but mainly as an intellectual ideal, not as supervisor.

1927, Ekman largely withdrew into his private office, and the students only saw him at the lectures he delivered which were popularly known as »the herring notes«, that is, the material he was working up for his grand treatise, *Tiergeographie des Meeres* (1935).

Nevertheless Ekman did have a few students, the »Ekman boys«, working on animal geographical problems. Their initiation was an expedition to the Skagerack sea area, arranged by Ekman in the summer of 1933. Out at sea, they trawled, took sediment samples with the Ekman bottom grab, and measured temperature, salinity, etc. On board, Ekman distributed the dissertation subjects. One had to take the polychaetes, others were entrusted with different groups of crustaceans.[202] During the following decade they produced dissertations expressing different degrees of ecological translation of Ekman's research programme: Bror Forsman's dissertation on the life habits of a cumacean species was ready by 1938, Jöran Hult's (1909-1982) on the soft-bottom isopods and Olof Elofsson's study of the ostracods followed in 1941; and Poul Enequist's dissertation on the soft-bottom amphipods, finally, appeared in 1949.

The *leitmotiv* was geographical distribution. By correlating the distribution pattern acquired through the grabs, with the sampled measures of environmental factors, sometimes adding aquarium studies of the conditions of existence of the animals, they were supposed to end up with a causal animal geography. In an introductory remark to a methodological paper on the firmness of marine sediments as an animal geographic factor Ekman gave the clue to the whole programme:

> *»Seit vielen Jahren habe ich mich wiederholt mit versuchen beschäftigt, die Methodik in der tiergeographischen und ökologischen Forschung in der Richtung zu verbessern, dass man mehr oder weniger subjektive Schätzungen durch mathematische und damit objektive Werte ersetzen könne«*;

these objective methods were needed in order to carry through

> *»ein kausalanalytisches Studium sowohl der Zusammensetzung der Associationen als auch der Verbreitung derselben und der Ökologie der einzelnen Tierarten«.*[203]

That is, the ecological analysis should be seen as an explanatory means to the end – a better understanding of animal distribution. That also means that ecological analysis was set on a par with, for example, taxonomic analysis. It is significant that Ekman delimited the dissertation subjects on taxonomical grounds, not with respect to habitat or any kind of ecological question. »Ekman's boys« typically included large taxonomical sections in their dissertations, evidently a consequence of the fact that the species of the area were often so poorly known that it was necessary to expend a lot of time and energy simply making accurate species descriptions. Enequist, for example, distinguished several new amphipod species.

The taxonomical bias was also an expression of the influence of the marine zoological tradition in Uppsala with its injunction: first know your species, then (maybe) investigate its ecology. Hans Höglund (b.1899),[204] for example, writing on the foraminifera of the Gullmar fjord, intended to study their ecology, but ended up with an essentially taxono-

202. We should not forget that Ekman was a specialist on crustaceans, and had been ever since his dissertation in 1904.
203. S.Ekman 1947,p.1; this paper refers to investigations made in the late 1930s.
204. Höglund was another Ekman student, employed at *Havsfiskelaboratoriet*, but outside the circle of the »boys«.

mical dissertation:

> *»It was my intention, from the very beginning, to devote myself to the ecology of the Foraminifera, but I very soon found out that before this could be done, it was necessary to subject the animal group in the proposed investigation area to a thorough taxonomic revision«.* [205)]

And he adds:

> *»The final treatment of the ecological problem must be deferred until the taxonomic position of the entire animal group has been definitely fixed«.*

In a sense, Höglund made a scientifically sound choice. One cannot reasonably study the ecology of a taxon not yet delimited from other taxa. But on the other hand, had the ecological problem really been the primary object, as was claimed, then the choice of the taxonomically ill-defined foraminifera seems problematic. Höglund's argument for »final treatment of the ecological problem« was ecology entirely on taxonomical premises.[206)]

Similarly much of the work of »Ekman's boys« was ecology on the premises of animal geography. The degree of translation into ecology varied among them. While Hult and Elofsson restricted themselves to working up the material collected in 1933, making correlations between measurements of environmental parameters and animal distribution, Enequist, following Ekman's idea of »existence ecology« (cf.1-3) made rather extensive aquarium studies at Kristineberg zoological station. Observing the feeding methods utilized by different species of amphipods, he tried to correlate »food ecological types« with the detritus content in the sediments. Although unable to find such a correlation, he did not hesitate to proclaim that

> *»such a correlation neverheless exists, is in my opinion highly probable, but the investigation method employed is not adequate for a direct establishment of this correlation«.* [207)]

Forsman, finally, paid hardly any attention to the geographical distribution; he concentrated instead on the conditions of existence of his species, on reproduction and ontogenic development, life cycle and life habits in general, to a large extent based on aquarium studies, including some experimental work.[208)]

Although explicitly or implicitly discussing their findings with reference to Ekman's ideas of existence ecology and ecological animal geography, none of Ekman's students tried to claim ecology as an independent science, and none of them contributed to the commencing institutionalization of Swedish ecology which took place in the late 1940s (cf.3-5). Neither they nor Ekman himself tried to establish an ecological group. Having finished their dissertations they dispersed to seek employment as secondary school teachers or in the fisheries administration.

Thus, in retrospect, Ekman and his students' ecological animal geography appears as a temporary interlude in the Uppsala tradition of morphological and anatomical studies of marine animals. Throughout the 1940s Uppsala zoology again became largely identical with the morphological and comparative anatomical programme instigated in the 1880s by Tullberg. When Ekman retired in 1941 he was succeeded by a morphologist and

205. Höglund 1947.
206. On the other hand, in his job at *Havsfiskelaboratoriet*, Höglund pursued biological investigations, for example, of biology of a shrimp species by means of field studies and aquarium experiments (Höglund 1943).
207. Enequist 1949,p.438 (engl.orig.).
208. Forsman 1938.

specialist on embryo development.[209] A few years later Nils von Hofsten, the uncrowned leader of Uppsala zoology between the wars, retired too, and was also succeeded by a morphologist, working on embryo development in marine animals.[210] It was only in the 1950s that one single fourth generation student picked up the line from Ekman (who was then still working as an emeritus professor in the department); and it was not until the 1960s and 1970s that Uppsala zoologists again began to identify their studies as ecological (see further Chapter 4).

An experimental approach to animal ecology in Uppsala

The 1920s had seen a few attempts to study animal-environment relations by experimental methods (cf.2-5). The occasional claims for ecology made by Runnström and Lindahl in Stockholm around 1930 were repeated a few times by their students,[211] but otherwise no Stockholm zoologist made any experimental approach to ecology during the 1930s and 1940s. In Uppsala and Lund, however, entirely new claims for ecology were put forward by experimentalists.

In Uppsala the »new German« zoological tradition had always been identical with comparative anatomical studies, not with experimental physiology. This state of affairs continued during the 1930s and 1940s. Apart from the introduction of undergraduate courses in physiology, nobody trained in the »new German« tradition pursued studies of the physiology of animals. When an associate professorship in zoophysiology was created in Uppsala after the war, only Stockholm zoophysiologists applied for it.

The lack of an experimental physiological tradition in Uppsala did not preclude the existence of experimental claims for ecology, however. In fact, one of Sven Ekman's students, Arne Lindroth, translated animal geographical problems to autecological problems and forwarded an experimental-physiological claim for ecology,[212] the first in Sweden after Runnström/Lindahl's in Stockholm and Alsterberg's in Lund a decade earlier. When attending Ekman's »herring notes« lectures he had noticed a particular animal geographical problem, that is, that certain of the species in the deep trenches of the Gullmar fjord normally only existed at far greater depths in the Skagerack. So, when attending the Klubban marine zoology course in 1931 (»it was such fun and stimulating with those studies, that I was captivated there«),[213] he began to map the animal communities on soft bottoms in order to get a general background picture of the Gullmar depths before embarking on the more specialized task.

But when reading the literature on marine bottom associations, and the plant sociological literature as well, Lindroth became increasingly sceptical to sociological analysis. His licentiate thesis, presenting the results of the sociological investigations, was concluded with an autecological and ecophysiological credo:

209. Sven Hörstadius, who wrote his dissertation with John Runnström in Stockholm (see ED 18/7 1942:2).
210. Gösta Jägersten, who wrote his dissertation in Uppsala; he was appointed successor to Holmgren in Stockholm but shortly after he was appointed to the chair in Uppsala (see ED 23/5 1947:7).
211. Cf.2-5, note 255.
212. For biographical details of Arne Lindroth, see ED 30/12 1965:12.
213. Interview with A.Lindroth 23/9 1981.

»I also mean that such a thing is beyond reach at the moment... An ecological system, which we should strive for, assumes a thorough knowledge of the members of the community. On the autecological base is then built the symphysiology, and only then can we fully understand and treat the community as such. These preconditions are not yet met; it should be the task of physiology and ecology to lay the foundation for sociology«.[214]

While still wrestling with the problems of synecology, Lindroth joined the Skagerack expedition of 1933 – he was supposed to take care of the polychaetes, and use them for an ecological animal geographical treatise, like the other »Ekman boys« did with their crustacean groups. But actually he did not care. He wanted to know the physiological causes of the sociological appearance of his animals, and consequently he put the collected polychaete material aside. After several years of laboratory work, he published, in 1938, an experimental dissertation on the respiration mechanisms of one single polychaete species, paying little attention to geographical/sociological problems. Even the choice of a polychaete was seemingly incidental:

»I really wanted to focus on an autecological investigation, and that turned out to be the respiration autecology of Nereis virens – well one has to begin somewhere. And then, that was it. It could as well have been salinity dependence of deep-living polychaetes or anything else«.[215]

Compare this statement with Höglund's above! Höglund wanted to do ecology (synecology), but found it impossible, and turned to taxonomy. Lindroth, for his part, turned the problem the other way around. For him the ecological problem was the primary one throughout, while the choice of species or even physiological function was of secondary importance. This illustrates the core issue in the process of ecological translation. Lindroth's claim for ecology as an experimental-physiological science was stated in the introductory lines of the foreword:

»Studien der marinen Weichbodenfauna führten mich zu der Auffassung, dass eine Soziologie des marinen Benthos noch nicht aufgebaut werden kann. Die Ökologie der verschiedenen Arten ist ein zum grossen Teil noch nicht durchgeforschtes Gebiet, dessen Erforschung eine notwendige Voraussetzung wäre«.[216]

As for the interest that actually lay behind the translation in this case, that is more difficult to say. Lindroth himself asserts that:

»I should have continued and completed it /the Skagerack material/. *But I was experimentally interested. I should really have been an engineer, constructor, but... Over and over again I made small apparatuses, home made«.*[217]

Lindroth's approach was a consequent further development of Ekman's research program of the conditions of existence of animals. Earlier Ekman had honoured Alsterberg's investigations in the 1920s.[218] But he did not support Lindroth's dissertation, on the contrary he considered it a poor thesis. Consequently Lindroth did not even obtain his

214. A.Lindroth 1934,p.51; the essential part of the unpublished licentiate thesis is published in A.Lindroth 1935.
215. Interview with A.Lindroth 23/9 1981.
216. A.Lindroth 1938,p.369.
217. Interview with A.Lindroth 23/9 1981. However, this observation is a reminiscence half a century after the event, and it should be treated as such, i.e., as indicative of *one* of several possible interests involved.
218. Cf. Ekman's evaluation of Alsterberg in ED 16/9 1932:88 (cf. also 2-5).

docent status, and his university career was temporarily ended.[219] Instead Lindroth was employed in the fishery administration, and from 1943 in a senior scientific position at *Fisketillsynsmyndigheten* (the Fishery Inspectorate).[220] The work was very practical, involving investigations of the reaction of fish to environmental disturbances, and was not translated to ecology for the time being. Nor did he enrol any students to his experimental approach to animal-environment relations. In that sense Lindroth, in spite of being one of the pioneers for an experimental claim for ecology in Sweden, did not contribute to the social order of ecology.[221]

A premature claim for insect synecology in Lund

Around 1930 the »new German« zoology troika in Lund, that is, Wallengren, Carlgren and Bengtsson, retired and were succeeded by a more heterogeneous triplet – Bertil Hanström, Torsten Gislén, and Nils Alarik Kemner, who were in charge of the department during the 1930s and 1940s.[222] All three were »men of the 1910s«, and had been trained as comparative anatomists. Comparative anatomy (and systematics) still took the major share of the undergraduate courses. In that sense little had changed since the late 19th century.

Nevertheless the new troika made life somewhat easier for naturalists who wanted to make an academic career. Hanström, who had been appointed to the comparative-anatomical chair and who devoted his entire professional work to that tradition, was also an ardent amateur naturalist and bird-watcher; for example, he contributed to the growing naturalist revival by taking over from Lönnberg the editorship of *Fauna och flora* and holding this for a quarter of a century.[223] Kemner, who was curator of the entomological collections, which he greatly enlarged, also encouraged insect faunistics.[224] During the 1930s and 1940s his section became a center for entomology in Sweden, attracting numerous young collectors, hoping to professionalize their amateur interest. Although

219. It is difficult to say why he failed to get Ekman's support. Others, among them the Danish Nobel Prize winner August Krogh, wrote a very positive evaluation of it (see Lindroth's application in ED 30/12 1965:12).
220. *Fisketillsynsmyndigheten* was the first state authority for control of water pollution. More than half a century before the modern environmental debate, the pollution of lakes and streams had been a topic for political concern (Ödmann *et al* 1982). *Fisketillsynsmyndigheten* began as a one-man institution in 1937 (Sten Vallin, cf. 2-1, note 62) and was extended in 1942 by the addition of a couple of biologists, among them Arne Lindroth.
221. Two decades later, however, he was appointed to the first chair in ecological zoology (at Umeå in 1965, see 4-4). It is interesting to note that both Lindroth and his assessors retrospectively interpreted his research at *Fisketillsynsmyndigheten* during the 1940s as »ecological« (ED 30/12 1965:12), a striking example of »historical translation« legitimizing the by then rapidly expanding social order of ecology.
222. Kemner was head of the entomological division from 1929 (ED 31/5 1929:103) to his death in 1948; Hanström was professor of zoology from 1931 (ED 19/12 1930:2) to 1957; and Gislén from 1932 (ED 16/9 1932:88) to his death in 1954.
223. Hanström was editor of *Fauna och flora* 1942-1967. He was also responsible for the continued editing of the many volumes of *Djurens värld* (The world of animals; 1st vol. of the 1st ed. in 1939) based on a Danish revision of the classical Brehm's *Tierleben*. Although including small notes on life habits, this work mainly presented a systematical and morphological view of the animal world, however. Hanström did not enrol students to ecology, but among his students was Anders Enemar, who, despite making his professional career as a morphologist, kept his amateur interest in bird studies and later introduced animal ecology to Göteborg in the late 1960s (cf.4-3). For biographical notes on Hanström, see Dahl 1971 and ED 19/12 1930:2.
224. For biographical notes on Kemner, see C.-H.Lindroth 1977.

positively disposed to faunistic studies, Kemner did not automatically accept all kinds of translations to ecology. For example, Lars Brundin's dissertation on the ecology of the beetle fauna of the Torneträsk area in Lapland of 1934, met with an inexplicable lack of appreciation. This was the first explicit claim for synecology among Lund zoologists, indeed the first explicit claim for animal synecology in Sweden. Brundin (b.1907), who was inspired by his father to study insects at an early age, came to the department in 1925:

> *»I chose Lund university, because it was the only university in Sweden where they taught entomology«.*[225]

That did not automatically turn him into an ecologist. Most of his fellows chose to work on anatomical and systematical problems. Brundin was primarily interested in the fauna, and emphasizes that there was no latent ecological interest in Lund in the late 1920s. But already in 1926 the young Brundin was recruited to make an inventory of the insect fauna in the Abisko National Park in Lapland on behalf of *KVA:s naturskyddskommitté*:

> *»I went there because I had an interest in beetles and a certain general orientation in insects. However, there was no fixed plan behind it, you know, only to try to get a representative insect collection«.*

Eight years later Brundin ended up with a synecological discussion of the relation between beetle communities and vegetation, i.e., a variant of the Uppsala school programme. What made him translate his faunistic interest into synecology?

> *»Well, there were the well-known Swedish botanists up there in Abisko... For example, Rutger Sernander came with his so called Excursio lapponica, young students from 'Växtbio'... It was very lively up there in the summers... the discussions with those botanists, some eminent plant sociologists, and studies of the botanical literature available in the small library up there, made me begin to think... and I got the idea of trying to investigate to what degree there was a parallelism between the communities of the plant sociologists and the distribution of the... beetle fauna in the area«.*

Brundin emphasizes Sernander's influence:

> *»I joined Sernander's excursion with the Uppsala students and I was inspired by his extremely vivid and enthusiastic demonstration of soil profiles, what they meant and such things which raised your understanding of the ecological factors«.*

But Brundin was not entirely enrolled into the rhetoric of the Uppsala school. While accepting their delineation of plant communities, he rejected the inductive principle and argued for a deductive animal sociology:

> *»Meine Fragestellung war diese: wie verteilen sich die Arten innerhalb des Torneträskgebietes auf verschiedene Standorte mit Bezug auf Dominanz und Standortstreue, und in welcher Ausdehnung kann die von den Botanikern gemachte Einteilung der Pflanzengesellschaften der Fjelden auf die Coleopterfauna angewandt werden?«.*[226]

In addition Brundin oriented himself to the recent international animal synecological literature. Referring to the co-operation between plant- and animal ecologists in the United States, Brundin's dissertation, published while he was still in his twenties, stands out as a deliberate and very conscious claim for animal synecology.

His pioneering approach to insect field studies should perhaps have won the admiration of his peers, but when returning to Lund

> *»the reception was very half-hearted. Kemner had no understanding of the whole disposition«.*

225. This and the following quotations from an interview with Brundin 29/9 1981. In fact not a single young insect collector professionalized his amateur interest in Stockholm, while a few made their way through the Department of Zoology in Uppsala.
226. Brundin 1934,p.30.

Nor did any of his fellows understand his choice of research topic. The dissertation was downgraded, and Brundin had to leave the department for a position as a fishery investigator at *Sötvattenslaboratoriet*.[227] Thus, the first conscious claim for animal synecology in Sweden was dismissed.

Gislén's faunistic excursions and the turn to ecology in Lund

Brundin never had any students, and seems to have had negligible influence on the intellectual development of Lund zoologists. Despite the troika's indifference to Brundin's claim for animal synecology an increasing number of Lund graduate zoology students translated their naturalist interest into the ecological rhetoric:

> »*There was a fine ecological climate in Lund* /in the 1940s/«,[228]

comments one of them. From the mid-1930s onwards, the huge zoology building in Lund was the home of a lively and growing ecology group:

> »*It turned into ecological chat... it was an awakening interest... we started a kind of discussion club among the younger colleagues, it turned into a lot of ecology*«.[229]

Interviewees have a tendency to see ecological forerunners in many faunistic and animal geographic activities (»historical translation«), but it is nevertheless true that the degree of translation of animal field studies into ecology was more pronounced at the Department of Zoology in Lund than at any other place for animal studies in the country.

Where did this »fine ecological climate« come from? Contemporaries usually assume that it was due to the influence of Torsten Gislén, the third member of the Lund professorial troika. After having been appointed in 1932, Gislén continued to publish faunistic and animal geographic works, primarily articles based on the vast material from his Pacific journey.[230] The pervading theme of his production, to display patterns of distribution of animal species, and explaining them by correlating them to environmental factors, seemed, apart from details, to be a copy of what Ekman and his students in Uppsala were doing. Of great importance for the »fine ecological climate« in Lund were the excursions, an educational innovation instituted in the early 1930s. Gislén was a zealous educationalist (cf.3-1) and kept a sharp eye on his zoology students. In the department's folklore the name of Torsten Gislén is tantamount to the idea of naturalist field excursions. Actually he did for zoology in Lund what Sernander had done for the botanists in Uppsala a quarter of a century earlier:

> »*The excursions were exceedingly popular*«,

and, it is often added, they

227. Brundin started a career in the fishery administration, but by 1937 he was attached to *Sötvattenslaboratoriet* at Drottningholm, where he stayed for almost two decades. His later publications on limnological problems include some ecological reasonings; e.g., the great treatise on chironomids and other lake bottom organisms from Swedish primary rock lakes contains qualitative discussions on the ecological aspects of chironomid distribution (Brundin 1949). But Brundin never made any direct ecological claims, and although attached to the Department of Zoology at Stockholm in the early 1950s, he never enrolled students to ecology. In the 1950s and 1960s he turned more and more to animal geographical and phylogenetical problems, and became the main proponent of the ideas of the German phylogeneticist Willy Hennig.
228. Interview with NN 24/8 1981.
229. Ibid.
230. And a number of smaller articles, mainly on the distribution of single species or groups of species.

»have had an enormous influence on the development of ecology...one learned where they /the species/ *occurred, and Gislén tried to impart some auxiliary knowledge about the environment too«.*[231]

Gislén's excursions certainly routinized and institutionalized outdoor activities, and hence stimulated the general naturalist interest among the students.

On the other hand, faunistic excursions in themselves do not necessarily lead to the formulation of ecological problems, not to mention claims for ecology. In fact, Gislén did not introduce the excursions in order to enhance ecology, but as a pedagogic aid to teach the Swedish fauna. Some of his youngest students are rather reserved about his contributions to ecology: »we discovered him more as a systematician than as an ecologist«.[232] Furthermore although Gislén occasionally talked in terms of ecology,[233] the factual claims for animal ecology made in Lund was rather made by his graduate students, »the men of the 1930s«.

Not all of »the men of the 1930s« in Lund became ecologists, of course. Some worked on purely morphological problems, others worked on systematical and/or animal geographical problems, and others yet struck a balance between these and ecological problems. A typical product of negotiation between different problem translations was Per Brinck's dissertation on Swedish stoneflies. It was mainly within the scope of Gislén's animal geographical programme, but it did not overlook the ecological perspective. Indeed Brinck asserted that he

»embarked on these studies paying special regard to the ecology«,[234]

and elsewhere he writes that:

»Food, food availability and feeding habits are ecological factors of great importance, determining the distribution of many aquatic insect larvae and the frequency of many adults«,[235]

a text-passage indicating the balance between a study of »ecological factors« and a study of »distribution« and »frequency«. In fact, he divided the dissertation into distinct distributional, ecological, and taxonomical parts. Although not considered a particularly original contribution, his contemporaries nevertheless admired his

»considerable capacity to treat one single insect group from taxonomic, morphologic, ecologic and zoogeographic points of view, and the ability to synthesize them all in an excellent way«.[236]

Yet others completely embraced an ecological perspective. The works of Brattström and Dahl, who published their dissertations in 1941 and 1948, respectively, demonstrate the shift from classical animal geographic problems to ecological problems in Lund. Hans Brattström (b.1908), one of the many adolescents who started as a bird-watcher, came to Lund in the late 1920s, and »fell in love with the echinoderms«,[237] when following the

231. Interview with NN and NN 2/9 1981.
232. Interview with NN and NN 8/9 1981; it seems that these different evaluations of Gislén's contribution to ecology coincide with a generation difference. Those who joined him in the 1930s experience him more as an »ecologist«, than those who joined in the 1940s when »the fine ecological climate« was already established. Nothing indicates that Gislén himself changed his attitude towards ecology during his lifetime.
233. E.g., Gislén 1937 and 1943.
234. Brinck 1949,p.vii (engl.orig.).
235. Ibid.,p.154 (engl.orig.).
236. Quoted from Spärck's assessment of Brinck in ED 18/6 1958:7.
237. From interview with HB 31/10 1981; for biographical detail on Brattström, see ED 30/6 1948:4.

marine zoological course at Lund's primitive marine station in Barsebäck. Discovering that »the southern limits reported for echinoderms were wrong«, he wrote a licentiate thesis, and later expanded it to a doctoral dissertation with the aim to determine:

> *»Die Verteilung der einzelnen Arten in verschiedenen Teilen des Sundes, ihre Abhängigkeit von der Tiefe, der Bodenbeschaffenheit, der hydrographischen Faktoren usw«.*[238]

He collected vast amounts of data on echinoderm distribution and site factors, but in the final outcome, the long section on the environment was not connected to the elaborate distributional data. The animal geographical problem was obviously the main object of the research, while the environmental discussion became an appendix – a few pages only were devoted to a discussion of factors responsible for the distributional pattern. Thus, Brattström's dissertation followed in the wake of the main stream of biogeographical work introduced by Bergendal, Wallengren and Björck around the turn of the century (cf.1-3), and as a consequence not even his contemporaries found it particularly exciting.[239]

Erik Dahl's (b.1914)[240] work, on the other hand, marks a shift from animal geographical to ecological problems. Dahl, a close junior colleague to Brattström, started as a faunist and systematician. His dissertation was originally suggested by Gislén, but while taking an animal geographical problem as his point of departure, he ended up focusing on one single ecological factor responsible for the distribution of the animals, in this case a group of marine crustaceans, the amphipods. Dahl writes:

> *»From the beginning it was my main object to study the ecological effect of the salinity factor... It rapidly turned out, however, that before the problems connected with salinity could be seriously attacked those inherent in the detritus factor must first be dealt with«.*[241]

The choice of marine material was a mere coincidence, he maintains: he happened to live at the West Coast.[242] Choosing the amphipod fauna on algae as his material, he could demonstrate that the thin layer of detritus on algal leaves is an ecological factor of ultimate importance for the composition of the fauna.

The Lund insect ecologists: descriptive sociological and experimental physiological approaches

The translation of the naturalist interest into marine animal geography, and later marine ecology, was a side-line in Lund, however, just as field studies of terrestrial animals were a side-line in Uppsala. Conversely, in Lund the Department of Zoology was a stronghold for translating studies of terrestrial animals, especially insects, into animal ecology. Two young Lund zoologists, Ivar Agrell and Helge Backlund, were particularly active contributors to the build-up of the ecological »discussion club« at the department.

Agrell was not only a leading ecologist in Lund in the 1930s and 1940s, but also a

238. Brattström 1941,p.11.
239. When Brattström applied for the chair in zoology in Stockholm in 1948, his dissertation was regarded as a careful and detailed result of hard work; see ED 30/6 1948:4.
240. For biographical details on Dahl, see ED 13/12 1957:78.
241. Dahl 1948,p.9 (engl.orig.).
242. Interview with ED 24/8 1981; another source of inspiration is said to be Karl Lang's *docent* lectures on the aquatic environment in the early 1930s (cf.2-1).

leading intellectual personality in Lund's academic life.[243] His contemporaries describe him as a man with »tremendous intellectual vigour, with broad sympathies, for example, he was a splendid artist«, or, in the words of another, »Who has not met Ivar Agrell can hardly imagine what an intense, enthusiastic and engaging person he was«.[244] He was one of those young naturalists who seems to have learnt beetles and butterflies before learning to read and write properly. He specialized in the collemboles, and when only 18 years old, he was summoned by Trägårdh at *Skogsförsöksanstalten* to determine some critical species. His first published scientific paper, in 1932, was a systematic and animal geographic note on collemboles, ending with some ecological remarks, and while still an undergraduate student he published an article on the collembol fauna on some sand dunes in the vicinity of his parents' summer cottage.[245]

In one respect Agrell's dissertation, *Zur Ökologie der Collembolen: Untersuchungen im Schwedischen Lappland* (1942), approached a classical problem: the causes of animal distribution. But it was not announced as an animal geographic work, not even a study in ecological animal geography. Agrell's dissertation was a programmatic claim for ecology as a independent zoological specialty:

> *»Die Collembolen sind eine Tiergruppe, die in der ökologischen Wissenschaft bisher nur geringem Interesse begegnet ist«,*[246]

he stated as a prelude. If Agrell had a naturalist's interest in collembols, he had a scientist's interest in promoting ecology as a new science. It was

> *»ein möglichst allseitiges Bild von der Ökologie der Collembolen«,*[247]

he wanted to give. To that »allseitiges Bild« he added sociological analysis as well, making a number of methodological contributions to animal sociology. In contrast to Gislén and the Uppsala school of plant sociology, he advocated a deductive (that is, ecological) delineation of animal communities and entered into »a scientific duel« with a Swiss author on this matter.[248]

Backlund was born in Russia and had grown up in Finland (he studied in Helsinki 1931-33) before moving to Uppsala in 1933.[249] While completing his undergraduate education he published some notes on lice, and on quantitative methods for investigating forest soil microfauna. Like several other young zoology graduate students (cf. Brundin and Gislén), Backlund was inspired by Sernander:

> *»Although himself a botanists, the late Professor RUTGER SERNANDER has exercised a great influence in my way of ecological thinking; he was always ready to discuss every problem of interest«.*[250]

In 1940 he moved again, now to Lund, where he took up a study of the insect fauna in beach wrack. Like Agrell, Backlund had a specific ecological aim with his dissertation. In two central chapters he discusses the distribution ecology of wrack fauna, and concludes that the factor determining its geographical distribution is the number of species, the size,

243. For biographical details on Agrell, see Fänge 1974 and ED 21/1 1949:3.
244. From interview with ED 24/8 1981 and from Fänge 1974.
245. Agrell 1934.
246. Agrell 1941,p.1.
247. Ibid.,p.3.
248. For a review, see Renkonen 1949,p.122; see also Agrell 1945 and 1948.
249. For biographical details on Backlund, see Anon. 1974.
250. Backlund 1945,p.8.

and the geographical density of the wracks.[251]

But Agrell and Backlund did not restrict themselves to the descriptive sociological approach to field studies of animals. Their claims for ecology included field observation and laboratory experiments as well.[252] For example, Agrell's aim should be fulfilled

> *»durch Felduntersuchungen in den nordschwedischen Hochgebirgegenden und durch ergänzende Experimente...«.*[253]

By making field measurements of environmental factors, and subjecting his material to a thorough statistical analysis, he had added one important dimension to Brundin's work of a decade earlier. Furthermore, using a gradient gauge, he conducted a series of experiments on the effects of temperature, humidity and light on the collemboles, that is, what he termed preference and resistance tests. Backlund, who also conducted both field and experimental work, even concentrated on the latter. Besides carrying out elaborate field measurements on different environmental factors, such as water content, humidity, temperature, light, organic salts, and the composition of wrack itself, he conducted extensive preference- and resistance laboratory experiments on selected species from the wrack (mainly flies).

Where did they get the experimental approach from? Not from the «new German» zoological tradition, and not from Lindroth in Uppsala. In fact, they were the only Swedish third generation animal ecologists to import ecology from abroad, in this case from Finland. In the preface to his dissertation Backlund points out:

> *»Already when beginning my zoological studies /*in Helsinki*/ I was quite convinced that ecology, particularly synecology, was the most interesting object for research. The papers of HESSE, FRIEDERICHS, and PALMGREN were new, and they opened ways of research which formerly only had been in common use amongst marine biologists and botanists. The treatise of KROGERUS was particularly inspiring by its logical combination of observations in the field, laboratory experiments and sociological analysis«.*[254]

Backlund's dissertation was actually an extension of Krogerus' research programme of the 1920s, published in *Über die Ökologie und Verbreitung der Arthropoden der Triebsandgebiete an den Küsten Finnlands* (1932). Krogerus had investigated the distribution of sand dune arthropods, their communities, the species frequences, constancy and dominance, and he also conducted quantitative experiments in order to ascertain which ecological factors were responsible for the spatial distribution of animals.[255]

251. Hence he was approaching what much later would be known as »island biogeography«, one of the main problem areas in animal ecology in the 1970s.
252. Experimental claims for ecology, of course, was not new in Lund. It will be recalled that Gustaf Alsterberg had made an experimental approach to animal ecology in the 1910s and 1920s. But Alsterberg had no students in Lund to pass on this approach. After having lost the competition with Gislén for the chair in Lund in 1932, Alsterberg left the academic arena and took a position as a secondary school lecturer. He continued his scientific work, but left experimental ecology altogether.He continued histological work, and later applied (unsuccessfully) for chairs in zoology and limnology in Uppsala and Lund respectively (see ED 18/7 1942:2 and ED 17/1 1947:1).
253. Agrell 1941,p.3.
254. Backlund 1945,p.7.
255. Krogerus' work was in turn a consequence of several influences: he worked a lot with Pontus Palmgren, another Helsinki zoologist, and Palmgren in turn was inspired by the Finlandish forest botanists and plant geographers, etc. In many respects Krogerus' work also echoes Richard Hesse's *Tiergeographie auf ökologischer Grundlage* of 1924. But it is not within the scope of this work to follow the geneological chain backwards; being a history of the Swedish ecologists it treats all foreigners as black boxes.

Without trying to analyze the specific interests or mechanisms involved in these particular cases of enrolment,[256] we can only draw attention to the fact that Krogerus' influence reached far beyond that of Agrell or Backlund. The fact that several young naturalists and insect collectors, especially in the 1950s and 1960s, were transformed into ecologists, is usually ascribed to the influence of Krogerus, as summarized by his biographer:

> *»For the younger entomologists of the Nordic countries he stands out as the introducer of a causally-oriented ecological research which in several aspects has been normative«.*[257]

One of the contemporary students catches Krogerus' status among his junior colleagues in Lund in a single phrase:

> *»One travelled to Hangö,* /it was/ *like going to Mecca«.*[258]

These pilgrimages added considerably to the »fine ecological climate« at the Department of Zoology in Lund in the 1940s. In fact, animal ecology in Lund in the 1940s can be seen as a fruitful mixture of field and experimental approaches to ecology.

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To sum up: the ecological group in Lund during the 1940s, was seemingly much more vigorous than the Ekman group in Uppsala. Although still taking animal distribution as their basic point of departure, Erik Dahl, Ivar Agrell and Helge Backlund devoted more attention to »ecological factors« than any Swedish zoology graduate student before them had done. In that sense they were the most outstanding third generation animal ecologists in Sweden. Furthermore, out of a good thirty scientists working at the Department of Zoology in the late 1940s, approximately one-fifth characterized their work as »ecological«.[259] However, by the end of the 1940s, it seemed that the emerging local social order of ecology in Lund was also about to dissolve. Agrell suddenly turned to insect physiological problems in 1944-45, and after a couple of years he was appointed to the first position in zoophysiology in Lund.[260] Backlund went abroad for a position as scientific officer at a locust control center in Northern Rhodesia in 1949, and came back in 1952 to take up a position as a secondary school lecturer in a local town. And Dahl, finally, gave up ecology around 1950, turned to crustacean systematics and morphology, and eventually succeeded Hanström as professor of zoology in 1957. Hence the claims for animal ecology in Lund were not institutionalized. However, as will be shown in Chapter 4, the 1950s involved a great revival of animal ecology in Lund - but these prospects could certainly not have been anticipated in the late 1940s.

256. In the case of Backlund the local circumstances are quite clear: Krogerus was simply Backlund's secondary school teacher in Helsinki. But this is of course no explanation. Dozens of secondary school teachers in Sweden and Finland pursued spare-time zoological research without succeeding in enrolling bright young men for ecology. Thus we cannot establish the mechanisms behind this particular enrolment case, only notice its existence.
257. Brundin 1965.
258. Interview with NN and NN 8/9 1981.
259. See *Lunds universitets årsberättelse* 1940/41-1949/50.
260. ED 21/1 1949:3.

3.5 State intervention and the first authorization of ecology

Facing a new failure?

The number of cases of claims for plant and animal ecology reviewed in the foregoing sections indicate that the 1930s and 1940s was a period of rather extensive local ecologization at the universities. Although many swayed between formulating their field studies as biogeographical, sociological or ecological, the tendency towards a renewal of the claims for ecology as an independent scientific pursuit was nevertheless unequivocal. The third generation was claiming ecology on a much broader front than the second generation had done one or two decades earlier.

The second generation of ecologists had mainly failed to institutionalize their claims. In fact, the third generation seemed to fail as well. Early animal ecologists such as Lars Brundin in Lund and Arne Lindroth in Uppsala were poorly rated by their peers, and had to give up hopes of ecological careers; with one exception the ecologists at *Skogsförsöksanstalten* did not enrol students; Ekman's boys dispersed from Uppsala in the 1940s; the three leading members of the ecology group in Lund gave up their academic ecology careers around 1950; the synecologists at »*Växtbio*« in Uppsala were dispersed; Stig Waldheim and his students were mainly considered plant biologists or plant geographers; Wilhelm Rodhe eventually claimed his ecophysiological studies as limnology, and so forth.

The reason for this inexorable dissipation of the new wave of claims was, of course, the fact that until the late 1940s there were still no established positions or other forms of permanent support reserved for ecologists.[261] That is, ecology was not authorized as a legitimate scientific social order by university or state authorities. The build-up of university natural sciences had stagnated since the turn of the century, except for the small number of personal chairs created to recognize the nation's most outstanding scientists. The traditional botany and zoology elites took a conservative attitude towards changing existing positions. The botanical and zoological chairs had been only marginally redefined during the past fifty years, and, by the very procedure adopted professorial competitions had always favoured the more traditionally inclined applicants. It is true that the chair in plant biology in Uppsala was in principle open to ecologists, but not reserved for them, as witnessed by the 1934-competition (cf.2-4). The fact that some zoology professors were positively disposed to naturalists and ecology is no indication of authorization of ecology either: both Sven Ekman and Torsten Gislén had been appointed primarily on their systematical and comparative anatomical qualifications, and their animal field studies were mainly regarded as incidental sidelines to their »real« work.

261. The following chairs for studies of animals and plants at the universities existed in 1945: botany, particularly physiology and anatomy (Uppsala) - botany, part. physiology and anatomy (Lund) - botany, part. systematics and morphology (Uppsala) - botany, part. systematics, morphology and plant geography (Lund) - botany, part. physiology with anatomy (Stockholm) - botany (Göteborg) - zoology (Uppsala) - zoology, part. comparative anatomy and histology (Uppsala) - zoology (Lund) - zoology (Lund) - zoology (Stockholm) - experimental zoology and cell research (Stockholm) - plant biology (Uppsala) - limnology (Lund) - hereditary research (Lund) - hereditary science with animal breeding (Stockholm), together with the the Institut for Animal Breeding at Wiad). In addition 5 chairs at *Naturhistoriska Riksmuséet* were devoted to animal and plant studies, one each at the botanical, entomological, invertebrate, vertebrate, paleobotanical and paleozoological sections. (SOU 1945:48,p.25-29)

What eventually changed the situation to the advantage of ecology as a new and permanent scientific social order were the demands for an outdoor biology education forwarded by naturalist secondary school biology teachers discussed above (3-1), in combination with the Social Democratic programme for the post-war expansion of research and higher education.

The post-war planning policy for research and higher education

When, during the inter-war years, the botanical and zoological elites rejected all ideas of changing university curricula, they expressed an opinion widely shared among university authorities in principle: everything was fine as it was, as witnessed by *1933 års universitetsberedning* (the University Commission of 1933). Asserting that Swedish universities displayed a unique and magnificent development, it noted that:

> *»the state authorities* /have/ *generously contributed towards the establishment of new chairs and towards the construction and the equipment of new scientific institutions, especially for the medical and natural scientific disciplines«,*[262]

and limited itself to minor adjustments.

A decade and a world war later, opinion had turned right around. Those were the years of intense welfare state planning policy, including research and higher education, reflecting an international tendency towards making science and technology a public policy issue. The war and the nuclear bomb had demonstrated the power of science for national survival.[263] Now state authorities entertained fears that

> *»the academic seats of learning and our country's research institutions have not had the opportunity to follow the enforced development, which has taken place in corresponding areas abroad during the last years.*[264]

The planning programme for the natural sciences began to take form with two commissions, *1945 års universitetsberedning* and *Naturvetenskapliga forskningskommittéen*, outlining the post-war development of the natural sciences in Sweden. Although the prospects of ecology were by no means a main topic on their agenda, both Commissions had consequences for the establishment of the social order of ecology in Sweden.

The large *1945 års universitetsberedning* (the University Commission of 1945), was given the task of reviewing all aspects of university life, including the future of botany and zoology.[265] Although having a huge task to consider, the Commission nevertheless considered the problem of school biology (and hence the university training of prospective biology teachers) to be one of the major ones.[266] Therefore the demands of the naturalists

262. SOU 1937:36,p.2.
263. See e.g., Lakoff 1977.
264. SOU 1946:9,p.8.
265. It published six reports: SOU 1946:9, SOU 1946:81, SOU 1947:75, SOU 1949:48, SOU 1949:54 and SOU 1951:9.
266. Of course, the issue of secondary school teacher training was a general one, noticed both by *Läroverkslärarnas riksförbund* (the National Association of Secondary School Teachers) and the Commission (see e.g. the introductionary remarks to SOU 1949:54). Even so modern languages and biology were the main areas of contention. The leader-writer of the journal of *Biologilärarnas förening* did not exaggerate when emphasizing that biology was the most criticized school subject (*Medl.blad biologilär.fören.* nr 4/1945,p.73).

were to be taken seriously. Although the majority of Commission members had probably never heard of ecology as a botanical and zoological specialty, they nevertheless ended up with a proposal challenging the resistance of the botany and zoology elites, and opening up the way for the authorization of the demands for field biology, and indirectly for ecology, at the universities.

While the general direction for the Commission was set by the Minister of Education, Tage Erlander, and by the Commission's chairman and general secretary, many details concerning the development of the natural sciences, including botany and zoology, were in practice left to one of the young third generation ecologists, viz. Wilhelm Rodhe.[267] Still a graduate research student, Rodhe was appointed a member of the Commission in his capacity as a leading member of *Uppsala naturvetenskapliga studentförening* (the Uppsala Natural Science Student Society). His influence on the Commission was strengthened by the fact that one third of the 23 experts consulted on curricular reforms were experimental and laboratory oriented botanists and zoologists inclined to physiological or ecophysiological studies, or having a naturalist interest, that is, reflecting Rodhe's own disposition.[268]

Rodhe and the expert group represented a reform tendency in Swedish botany and zoology towards experimental studies of biological functions, at the expense of descriptive and classificatory research, i.e., systematics and comparative anatomy as it was actually practiced. In that respect they paralleled the modernizing attempts of *Biologilärarnas förening*, which included demands for field biology and ecology. Under the leadership of another third generation ecologist, viz. Carl H Lindroth,[269] the Association addressed the Commission with a proposal for a two week long summer course in plant ecology («if possible at an ecological station»), excursions, and lectures in faunistics, and demanded that ecology be an obligatory part of university studies in botany and zoology. The justification for these changes was the reform of secondary school biology:

> *»What the prospective teachers need most of all, is a concentration of their studies, on the one hand on the applications of biology, and on the other hand on the ecology of animals and plants, life habits and adaptations. As far as possible these studies should be pursued in nature«.*[270]

This met the broad approval of Rodhe, and with him of the Commission. Consequently they proposed three entirely new university positions with reference to the schools' demands for field biological education, viz., associate professorships in plant biology in Lund, and in entomology and in limnology in Uppsala.[271] Somewhat earlier the other commission (see below) had suggested that the curator of the entomological museum in Lund should be transformed to a full professorship.

267. This can be inferred from the Commission's reports, and has been confirmed by Rodhe (interview 8/2 1982).
268. The biological experts were: Ivar Agrell, Hans Burström, Bertil Hanström, Per Eric Lindahl, Elias Melin, Karl-Georg Nyholm, Torsten Pehrson, John Runnström, and finally Gottfrid Stålfelt, most of which were main actors in the ecologization process.
269. See 2-5, notes 220 and 222, and 4-3.
270. Anon. 1947; the proposal to the Commission was almost identical (cf. SOU 1949:54,pp.176-7).
271. At least in the two later cases they had particular applicants in mind, viz., Bertil Kullenberg and Rodhe himself (interview with WR 8/2 1982).

As a consequence of this concurrence between the naturalist revival and reform of biology, field studies of animals and plants, and to some extent ecology, were authorized both in the secondary school system and at the universities. First of all the demand of the growing naturalist movement had been accepted as a legitimate one. And secondly, the new professorships were filled by men having an outspoken outdoor naturalist interest. In Uppsala Bertil Kullenberg and Wilhelm Rodhe began to build small departments of entomology and limnology respectively, which eventually came to serve as nuclei for further ecologization in the 1950s and 1960s. Likewise Stig Waldheim in Lund had his synecological laboratory established on a permanent footing. Finally, Carl H. Lindroth, one of the grey eminences of the naturalist movement and chairman of *Biologilärarnas förening*, having published not only animal geographic but also ecological papers to an increasing extent during the 1940s, was appointed professor in entomology at Lund in 1951.[272] Hence, some of the avant-garde intellectuals of the naturalist movement were eventually beginning to fill the elite positions of academia.

By installing some of the leading intellectuals of the naturalist movement into offices, this authorization of field studies of animals and plants became an important platform not only for the further enlargement of the naturalist movement in the 1950s and 1960s, but also for the further ecologization process. It should be remembered, however, that the naturalist interest was not primarily translated into the social order of ecology but into the social orders of limnology, plant biology and entomology. The rhetoric of ecology was not without importance in the policy arguments referred to above, but it was not yet a decisive one – as it would come to be in the next wave of authorization of ecology in the 1960s.

A united claim for ecology: founding a national ecology journal

The first step towards the establishment of a nation-wide social order of ecology, viz., the foundation of the first Swedish ecological journal in 1949, was the outcome of a more restricted scientific endeavour, involving both a majority of the leading claimants of ecology and the other post-war science commission, *Naturvetenskapliga forskningskommitten* (the Natural Science Research Commission of 1944). The main aim of this Commission was to suggest means for supporting basic scientific research. It not only investigated the need for a natural science research council, it also suggested new university positions, and proposed state support for a number of new natural science journals.[273]

The last proposal was taken up by the ecologists. A number of ecologically inclined scientists, mainly from the Department of Zoology in Lund and from *Skogsforskningsinstitutet*[274] in Stockholm addressed the Commission, appealing for the foundation of a

272. ED 16/3 1951:5; Lindroth's ecological work is reviewed below, 4-3.
273. Besides an ecological journal, the Commission also discussed journals for cell research, chemistry, paleontology, plant physiology, and a reorganization of *Acta zoologica* (SOU 1946:77).
274. *Statens skogsförsöksanstalt* was renamed *Statens skogsforskningsinstitut* in 1942; in 1962 it was merged with *Skogshögskolan*.

Scandinavian journal of ecology.[275] Three aspects of their argument for *Oikos; Scandinavian Journal of Ecology* are specially worth mentioning.

Firstly, the initiators enrolled the Commission with the argument that ecology was something new and modern, compared to traditional disciplines. They emphasized that the existing botanical and zoological journals reflected a disciplinary division »which has not fully kept pace with the advancement of the natural sciences«.[276] This, of course, was an appropriate argument towards a commission with responsibility for the post-war reform of the natural sciences, and a majority of the Commission were convinced, referring back to »the very lively ecological research in Scandinavia«. Actually, only one of its attached experts, J.A. Nannfeldt, professor in systematical and morphological botany in Uppsala, argued against it:

> *»at least with regard to Swedish botany the need for an ecological journal is not very urgent at the moment«,*[277]

he said.

Secondly, the ecologists did not refer explicitly either to the naturalist movement nor to the demands of secondary school biology. Instead they pleaded for a journal directed towards basic scientific problems. *Oikos* should be an academic journal, not yet another version of *Fauna och flora*. That did not exclude concern for practical problems, however. *Oikos* was intended:

> *»not only for the disinterested research scientists, but also for agricultural scientists, foresters and fishery biologists«,*

because, as they said:

> *»ecology, to be sure, constitutes basic research for their practical tasks«.*

Thus the *Oikos*-initiators tried to translate agricultural, fishery and forestry science problems into the language of ecology. This was a decisive point in the emergence of the social order of ecology in Sweden, since it was actually the first national attempt towards scientification on behalf of ecology akin to that taking place in the 19th century, when botanists and zoologists succeeded in enrolling agriculture, fishery and forestry.

Reconciling the conflict between experimental and descriptive ecology

Thirdly, the *Oikos*-initiative involved a united claim for ecology. They wanted a journal devoted to:

> *»physiological, sociological and geographical ecology, together with methodology«.*

The last point takes us back to the Great Polemic of the 1920s - that is, to the discussion between those advocating descriptive and comparative field studies and those advocating experimental studies of natural phenomena. The *Oikos*-initiative signatories included the whole spectrum of ecological claims from the Great Polemic, ranging from

275. The appeal was signed by: Ivar Agrell, Helge Backlund, Torsten Gislén, Bertil Hanström, Åke Holm, Sven Hörstadius, Bertil Kullenberg, Sven Thunmark, Henning Weimarck, Erik Björkman, Torsten Lagerberg, Karl-Herman Forsslund, Olof Langlet, Carl Malmström, Lars-Gunnar Romell and Carl H. Lindroth, the great majority of whom have been treated in the foregoing.
276. This, and the following, quotations are taken from SOU 1946:77,pp.39-40.
277. From preparatory work to SOU 1945:48 (in *Riksarkivet* komm.nr 1574).

Stålfelt's experimental physiological claim to Du Rietz's and Forsslund's descriptive-comparative.[278] Were all earlier factional feuds dissolved? Had the old contradiction finally been settled by means of scientific argument?

In one sense it had not. The old polemic reappeared on several occasions during the 1930s and 1940s, although not in the same spectacular form. For example, when Hesselman's chair at *Skogsförsöksanstalten* became vacant in 1939, Romell and Lindquist were the main competitors, and they marshalled their respective supporters in a new version of the polemic.[279] Another occasion appeared with Stig Waldheim's dissertation in 1947 (cf.3-2). Although all assessors involved agreed that Waldheim had failed in his ambition to pursue a causal analysis of the relation between the soil chemical factors and the distribution and community structure of mosses,[280] opinions differed on how to evaluate the failure.[281] For example, Einar Du Rietz excused Waldheim's defective soil analyses, by dwelling on the floristic, sociological and geographical details, by pointing to the limited laboratory resources, and finally by declaring that the analytical weaknesses hardly changed the overall ecological conclusions. On the other hand, Hans Burström, Lundegårdh's principal student and now professor in plant physiology in Lund, had totally rejected Waldheim's causal analysis for involving circular reasoning. Hence Du Rietz and Burström reproduced the main either/or standpoints from the Great Polemic. Again an experimental-physiological critique had been countered by a defence of descriptive field work.

But in another sense the conflict really began to dissolve. First, the animal ecologists had never been as severely divided on the issue as had the plant ecologists. For example, Ivar Agrell and Helge Backlund in Lund had combined descriptive-comparative field work with preference and resistance experiments, and therefore had no problems uniting the two claims in the agenda of one national journal. Waldheim's attempt to unite the two claims for ecology found some support too. Gottfrid Stålfelt took up a compromise standpoint. Although also finding the causal part of Waldheim's dissertation »very weak«, and concluding that Waldheim's results simply lacked empirical foundation, he nevertheless praised the floristic, sociological and geographical analyses, and made an official plea for reconciliation of the two main lines of research:

> *»the fact is that it must be considered a weakness of Swedish botanical research, that representatives of the two tendencies still mainly work separately. The fact is that the contemporary research problems require co-operation, or even better factual knowledge of both fields on the part of individual scientists. That is the kind of knowledge licentiate Waldheim has aimed at, but not reached so far. From this point of view I consider his work exemplary after all«.*[282]

So, even though the conflict still remained in principle, the time was seemingly ripe for reconciliation in practice, and *Oikos* was founded on the premise that »physiological, sociological and geographical ecology« were accepted as three different aspects of one

278. But note that Henrik Lundegårdh was not among the initiators.
279. Jo 20/12 1940:19; this case was muddled by the conflict over basic versus applied science, however, and will therefore not be discussed further here.
280. The arguments for and against Waldheim are found in ED 12/11 1948:9.
281. The heated argument is understandable in the light of the fact that a new associate professorship in plant biology was proposed by *1945 års universitetsberedning*, and that Waldheim would be one of the top candidates to it.
282. Stålfelt's statement in ED 12/11 1948:9.

and the same united ecology. The first issue of the new journal was published in 1949, first with Helge Backlund and Carl Malmström, then with Erik Dahl as editors.[283]

Furthermore in September 1948 the same circle of scientists announced the foundation of an ecological society, *Svenska föreningen Oikos* (the Swedish Oikos Association).[284] Its council came to represent all shades of Swedish »proto-ecology« and ecology, including Carl H. Lindroth (chairman), Einar Du Rietz, Wilhelm Rodhe, Gottfrid Stålfelt, Karl-Herman Forsslund, Bertil Kullenberg, Stig Waldheim, Gunnar Gustafsson, Helge Backlund and Carl Malmström.[285]

It is difficult to say why the old conflict was reconciled. Maybe the adversaries recognized that unification was necessary in order to secure state support for the establishment of a national journal. It should also be pointed out that few physiologically trained scientists claimed ecology anymore, as had been the case among »the men of the 1910s«. The new research front in physiology was biochemistry, and experimentalists claiming ecology were probably considered to be out on a side-track.[286]

Whatever the reasons for this united ecological front, the *Oikos* journal and the *Oikos* association were important events in the commencing ecologization of Swedish science and education, being the first manifestation of a united national social order of ecology. A journal is a main requirement for a growing social order. A journal edits, accepts, refuses, in short delineates a given social order against others – in this case lays the norms for what ecology is, and what it is not. By means of the journal the new scientific social order could delimit itself *vis-a-vis* others: Ecology in contrast to botany, ecology in contrast to zoology, ecology in contrast to ethology, and so forth. In fact, right at the start a manuscript on competition among birds was almost rejected for not being »ecology«, but »ethology«.[287]

3.6 The third generation of ecologists: concluding remarks

By the 1940s field studies of animals and plants in their natural surroundings (»proto-ecology«) had become a rather common feature in Sweden. For example, new fields of research were being authorized in relation to various facets of the management of natural resources, such as nature conservation, water pollution control and game conservation, hence further increasing the number of practice-oriented »proto-ecological« research projects. However, problems of agriculture, fishery, forestry, game management and water pollution were seldom translated into ecological problems. A few scientists working at the applied research departments defined their work as ecological, but with few exceptions they did not succeed in enrolling any students. Hence, the new wave of

283. In the mid-1950s the editorship went over to a Danish ecologist, Christian Overgaard-Nielsen.
284. Actually two associations were established: *Nordiska föreningen Oikos*, being the owner of the journal *Oikos*, and *Svenska föreningen Oikos*.
285. According to a duplicated announcement, in my custody.
286. E.g., Agrell, after having turned to physiology is said to have declared that »those who are too stupid to become physiologists become ecologists« (NN 1/9 1981).
287. Interview with NN 14/3 1983.

ecologization in the 1930s and 1940s relied little on the translation of »practical interests«.

Translation of field studies into the new language of ecology was essentially a university phenomenon. After having been marginalized at the universities during the reign of the »new German« botany and zoology, field studies began to be accepted at the universities again. Of utmost importance was the growing demands for field studies of animals and plants made by secondary school teachers. In the late 1940s a number of permanent positions at the universities were created with the aim of strengthening the field training of prospective secondary school teachers.

A new generation of students, the intellectual avant-garde of a renewed naturalist social movement, came to the universities during the 1930s. Several of these »men of the 1930s« constituted a third generation of ecologists in Sweden. Most of them ecologized their work in close contact with second generation ecologists or »proto-ecologists«. Like their predecessors, some worked on their own, but in a few departments a lively ecological discourse was established.

»Växtbio« in Uppsala remained the center for field botany and »proto-ecological« investigations of plants during the period considered here. While the early Uppsala school had stressed vegetational analysis, and had only given cursory attention to the relation between the site and the vegetation, most of »the men of the 1930s« in Uppsala investigated relations between plant communities and environmental factors. However, although using the word ecology now and then, »the men of the 1930s« only rarely declared themselves as ecologists. They were fully aware of the synecological problem, but, except for their mentor Bertil Lindquist, none of them actually claimed ecology (or more precisely synecology) as an independent science in the same way as, for example, Du Rietz had once claimed plant sociology. For that reason the Uppsala school, although remaining the center for »proto-ecological« field investigations in Sweden, was not really ecologized during the 1930s and 1940s either.

In Lund, on the other hand, Stig Waldheim, made a programmatic claim for vegetational and environmental analysis as synecology, and after his appointment to a new associate professorship in plant biology, the ground was prepared for the vivid synecological group at Lund in the 1950s and 1960s.

The claims for plant ecology forwarded by people trained as experimentalists during the 1910s and 1920s (the Stockholm school) was not institutionalized or authorized as such during the period considered here. However, lone second generation plant ecologists, such as Gottfrid Stålfelt, Lars-Gunnar Romell and Elias Melin, continued research along the same lines, and enrolled a few »men of the 1930s« to pursue ecophysiological studies. Among them Wilhelm Rodhe in Uppsala and Carl Olof Tamm at *Skogsforskningsinstitutet* were the most important for the coming explosive growth of ecology as a scientific social order in the post-war period.

In Uppsala Sven Ekman continued his »proto-ecological« studies from the early 1900s, and gathered a number of »men of the 1930s« around him. Although discussing their findings with reference to Ekman's ideas of existence ecology, none of »Ekman's boys« tried to claim ecology as an independent science, and neither them nor Ekman himself tried to establish an ecological group.

Among »the men of the 1930s« in Lund, Erik Dahl, Ivar Agrell and Helge Backlund contributed considerably to the emergence of a vibrant group of animal ecologists. Although still taking animal distribution as their point of departure, these men devoted more attention to ecology than anyone before them had done. In that sense they were the most outstanding third generation animal ecologists in Sweden.

A major theme in the two preceding chapters has been the conflict between experimental and laboratory oriented scientists and scientist working with descriptive field studies, a conflict stemming from the practices instituted by Henrik Hesselman in Stockholm and Rutger Sernander in Uppsala, and climaxing at the end of the Great Polemic in 1934. Although still discernable the distinction between the two traditions was partly transcended by the 1940s. Animal ecologists such as Ivar Agrell and Helge Backlund in Lund tried deliberately to combine descriptive field work with laboratory experiments. Claimants of plant ecology such as Wilhelm Rodhe, Carl Olof Tamm and Gottfrid Stålfelt balanced between an experimental claim for plant ecology and an awareness of the kinds of problems which preoccupied the descriptive field ecologists and sociologists. In that sense the claims for ecology implied the beginnings of a rapprochement between the two opposing claims for ecology. The establishment of the new Scandinavian journal of ecology, *Oikos*, involved a reconciliation of the conflict between experimental and descriptive ecology.

The second generation of ecologists had failed to establish their claims for ecology on a permanent footing. The main event in the period considered here, from the point of view of institutionalization, was the foundation of *Oikos*. Likewise, ecology was authorized as part of the secondary school curricula. But besides the *Oikos* initiative, which was made possible by the combined effects of the demands for a modernization of secondary school biology and the post-war expansion of the natural sciences, the majority of claims for ecology forwarded by the third generation also failed to institutionalize. Many claimants of ecology withdrew from the universities, others turned into other problem areas.

All in all, although the extent of ecologization of Swedish academia was steadily increasing during the 1930s and 1940s, the overall result with regard to the establishment of ecology at the universities was still insignificant. From the time perspective of the late 1940s it was possible to discern plant ecology and animal ecology as scientific specialties within the larger social orders of botany and zoology – but there was little hint of the dramatic expansion of the social order of ecology and the ecologization of Sweden that was to follow only a good decade later. To these events we will now turn.

4 From insignificance to the planning of Sweden

As we have seen, the first outspoken ecologists appeared around the turn of the century, and the international institutionalization of ecology dates back to the foundation of the American and British ecological societies in the 1910s. In that sense ecology is hardly a newcomer in the modern patchwork of scientific disciplines. But its great expansion as a new scientific social order has largely been a post-World War II phenomenon, as witnessed by the recently published comprehensive *Handbook of Contemporary Developments in World Ecology.*[1] Most of the 34 national contributions to the Handbook concentrate on the decades after 1945. This is true not only for Third World nations such as Brazil and Taiwan, but for the leading ecological nations as well. The chairman of the *New Zealand Ecological Society*, founded in 1953, stated in 1976 that

> *»Twenty-five years ago both 'environment' and 'ecology' were words which were hardly ever seen in the popular press and only vaguely understood, if at all, by the man in the street. One had to preface any popular statement about ecology with an explanation of what the word meant«.*[2]

Burgess presents a lot of quantitative data to illustrate »the massive development« of U.S. ecology during the last thirty years.[3] A distinctive trait of both American and British ecology has been their rapid institutionalization in terms of journals, associations, etc. Duff and Lowe maintain that

> *»the most important institutional feature of* /British ecology/ *of the postwar period has been the emergence and growth of an ecological profession«.*[4]

The *Ecological Society of America* grew from 2319 members in 1965 to 5148 in 1973.[5]

Round the world a popular ecological movement began to take form, stressing the need for a new ecological basis for the management of society and the prevention of environmental degradation. The circulation of only one of the popular American ecological-environmental magazines increased from 2000 in 1967 to 30.000 in 1973.[6] »In short«, say Lowe and Worboys,

> *»ecology has become a social movement as well as a branch of biology«.*[7]

It would be presumptuous to try to summarize this grand international ecologization

1. Kormondy and McCormick 1981.
2. Allen 1977,p.8.
3. Burgess 1981.
4. Duff and Lowe 1981.
5. Nelkin 1977,p.82 (Table 1).
6. Ibid.,p.82
7. A plethora of articles, journals and books witnessed the emergence of this vaguely defined »ecological movement«. The British journal *The Ecologist*, founded in 1970, rapidly became a leading spokesman of the movement. For discussions of the phenomenon, see e.g., Bowman 1975, and Lowe and Worboys 1976.

process in just a few paragraphs. Suffice it to note that most historians of contemporary ecology agree that the most conspicuous general trend in the post-war period has been the emergence of ecosystem studies. »Contemporary ecology«, says Kormondy and McCormick, is »a discrete science of the ecosystem«.[8] Consequently, the First International Congress of Ecology, held in 1974, was organized on »Structure, functioning and management of ecosystems«.[9] The language of a holistic, environmental ecology also penetrated deeply into the popular ecology movement of the 1970s;[10] ecology, radical politics and feminism became interlocking elements in a new leftist movement.[11]

Sweden is no exception to this global ecologization pattern. For example, whereas still only a score of scientists worked full-time on ecological problems around 1950, by the mid-1970s approximately 500 scientists were working full-time on problems explicitly defined as ecological. The change in ecological self-consciousness is striking: for example, most authors of articles on ecological issues in the yearbook of *Naturvetenskapliga forskningsrådet* (the Natural Science Research Council) during the 1950s and early 1960s, felt a need to define the word to its readers; from the mid-1960s onwards, in contrast, ecology was usually discussed as a matter of course.[12] The first popular textbooks, intended for a general educated public were published in the mid-1960s,[13] and were followed by a wealth of books and introductions to the new science in the late 1960s and 1970s.

Further, before 1957 no academic positions were defined as ecological; *Naturvetenskapliga forskningsrådet* had nothing akin to an ecology programme during its first decades of existence, and funded little ecological work. By the mid-1970s more than a half-dozen university positions were designated as ecological (cf.4-2,4-3 and 4-4). Likewise, by the mid-1970s the Council not only recognized ecology as an independent scientific discipline, it also gave it top priority: up to 15% of the Council's total financial resources were ear-marked under the heading »ecology« (Figure 4-1).

Finally, while the few ecologists around 1950 were preoccupied with elucidating the correlation between environmental factors and the distribution of animals or plants, studies of ecosystems was a predominating practice by the mid-1970s. The two so called large scale ecosystem projects, *Östersjöprojektet* (the Baltic Ecosystem Project) and *Barrskogsprojektet* (the Coniferous Forest Ecosystem Project) engaged up to 150 scientists in total over a ten year period.

In this chapter we will go into details about the ecologization of Sweden during the postwar period, the proliferation and rapidly advancing institutionalization of ecological claims, the cognitive switch to ecosystem ecology, and the emergence of a national ecology policy. We will try to cast light upon how the insignificant and unstable social order of ecology around 1950 expanded at such a tremendous pace, and how studies of

8. Kormondy and McCormick 1981,p.xxiv.
9. Proceedings 1974.
10. Cf. Odum 1977.
11. See e.g., Enzenberger and Michel (eds) 1973 and d'Eaubonne 1975.
12. This conclusion is based on 77 articles dealing with animal/plante-environment relations, natural resource problems etc., in the yearbook of the Council (*Svensk naturvetenskap*) from 1950/51 to 1974.
13. Dowdeswell 1965, revised by a fourth-generation Lund ecologist (Lennart Cederholm) and Odum 1966, with a foreword by a fourth-generation Stockholm ecologist (Carl-Cedric Coulianos).

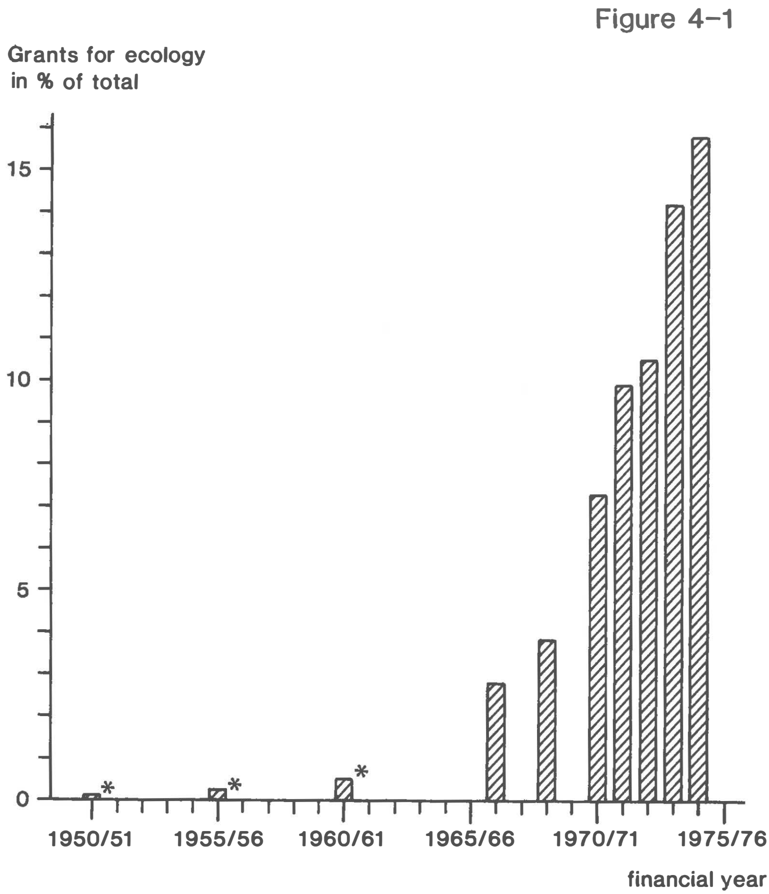

Fig. 4-1: *Naturvetenskapliga forskningsrådets* (the Natural Science Research Council) grants to ecological research in percents of total, financial years 1950/51-1974/75. Source: yearly reports of *NFR*. *: approximate figures only.

ecosystem structure and function became the dominant issue of the institutionalized Swedish ecology. And we will show how ecology even became a major issue in the political life of the nation – how eventually not only academia but the nation as a whole became ecologized.

In earlier chapters we have repeatedly depicted the commencing ecologization in the 1930s and 1940s as a process of translation, whereby a growing number of students with a naturalist interest were enrolled into the rhetoric of ecology. We will continue this line of reasoning in the period considered here. Two parameters are important for this interpretation, namely, the availability of students with a naturalist interests, and the existence of an ecological legacy.

In Section 4-1 we will once again turn our attention to the naturalists, to examine in particular the development of the naturalist social movement into a mass movement, and the large scale recruitment to the universities as a prerequisite for the rapid ecologization process.

The achievements of the second and third generations of ecologists were, of course, also a prerequisite for the expansion of ecology during the post World War II period. The wave of renewed claims for ecology in the 1930s and 1940s had been more successful than the first wave of claims in the 1920s. By enrolling larger and stronger institutional actors, viz., *Biologilärarnas förening*, *Naturvetenskapliga forskningskommittéen and 1945 års universitetsberedning*, the second and third generations of ecologists had begun to overcome the resistance of traditional botany and zoology, and had thus accomplished what the second generation had failed to do in the 1920s. *Oikos* had been founded as a prolegomenon to a national ecology. Ecology had eventually been accepted as part of the secondary school curricula, and was also supposed to be a constituent of the professional competence of zoology and botany graduates. Finally, three new university positions for field studies had been added to the existing one, and had been filled with third generation ecologists. Thus, although the extent of ecologization around 1950 was slight, a few nodal points for future ecologization nevertheless existed. In Sections 4-2 and 4-3 we will follow up the ecological legacy of the late 1940s and the establishment of a number of local social orders of plant ecology and animal ecology.

In earlier chapters we have demonstrated that, even though agricultural, forestry and fishery research to a great extent was »proto-ecological«, and even though problems in agriculture, forestry and fisheries occasionally were translated into ecological problems, the growing social order of ecology was only to a negligible extent the outcome of the identification of practical interests. Similarly, although in Sections 4-2 and 4-3 we will be able to demonstrate a number of cases of ecologists again, during the 1950s and 1960s, identifying agricultural, forestry, fishery and game management interests and translating them into the language of ecology, the identification of these kinds of practical interests were nevertheless of minor importance for the post-war growth and authorization of the social order of ecology.

However, with the 1960s actors within the growing social order of ecology identified a new set of practical problems – those of pollution, landscape deterioration and poisoning of living organisms, including man himself. We will discuss these matters in the two last sections of the chapter. Most of Section 4-4 is devoted to the environmental crisis, the formulation of a national ecology policy and its effects on the institutionalization of ecology; and in Section 4-5 we will try to relate the emergence of ecosystem research projects to the national policy for environmental planning.

Finally we will pay attention to a third important circumstance in the ecologization process, viz., the continuing interest in university reforms. Parts of Sections 4-4 and 4-5 are devoted to a discussion of how the ecologists offered their science as a solution to the reform of university curricula, involving a shift from largely descriptive botany and zoology to a so called »functional biology«.

4.1 A fourth generation of ecologists and mass recruitment of naturalists to the universities

The rapid ecologization of the 1950s and 1960s coincided with the careers of yet another generation of ecologists. For this, the fourth generation, the intellectually formative period was post World War II. They received their undergraduate training in the 1950s, wrote their dissertations during the 1960s, and came to power throughout the 1970s. Among these »men of the 1950s« we find Nils Malmer (b.1928), heir to the successful plant ecology group in Lund founded by Waldheim; Bengt-Owe Jansson (b.1931), prime mover behind the Askö marine ecological laboratory and leader of *Östersjöprojektet* in the 1970s; Folke Andersson (b.1933), leader of *Barrskogsprojektet* in the 1970s; Staffan Ulfstrand (b.1933), one of the intellectual leaders of the Lund animal ecologists in the 1950s and 1960s and later professor in animal ecology in Uppsala; Ingemar Ahlén (b.1936), another Lund animal ecologist appointed to a chair in forest vertebrate ecology in 1966; and Dag Gärdefors (b.1928), prime mover behind one of the first small scale ecosystem projects.

A naturalist mass movement

They themselves were all endowed with a strong field biological interest. More important, their life-careers, and the emergence of the post-war social order of ecology in turn coincided with a new phase in the extension of the naturalist movement. The number of naturalists had grown steadily through the 20th century. What had been a distinct but minor interest in cultural circles around the turn of the century emerged as a social movement with the naturalist revival of the 1930s, and now assumed the magnitude of a mass movement. Its dimensions are reflected in the membership of *Svenska naturskyddsföreningen* (*SNF*), which fluctuated between 2000 and 4000 in the inter-war period, but grew exponentially after the war reaching almost 90.000 by 1983 (Figure 4-2).

However, *SNF* had no direct bearings on the ecologization process, nor did it contribute directly to the influx of naturalist students to the universities. The typical member of the association was middle class, well-educated but middle-aged. The function of funnelling young naturalists into university studies in botany and zoology was instead fulfilled by organizations such as *Sveriges fältbiologiska ungdomsförening* (*SFU*) and *Sveriges ornitologiska förening* (*SOF*) (cf.3-1).

Several interviewees have given evidence about the importance of the »field biologist« background for would-be ecologists in the 1950s and 1960s; a number of cases will be detailed in the following two sections. The membership of *SFU* expanded from a few hundred members in the late 1940s to over 3000 members in 1962, and then to almost 12,000 during the 1970s (Figure 4-3). Bird-watching had emerged as a naturalist pursuit in the 1930s, and the number of its adherents grew slowly but steadily during the 1930s and early 1940s, and then rapidly expanded in the 1950s and 1960s (Figure 4-4). It is said that when one of the first excursions was organized in the province of Scania in the mid-1940s

Figure 4-2

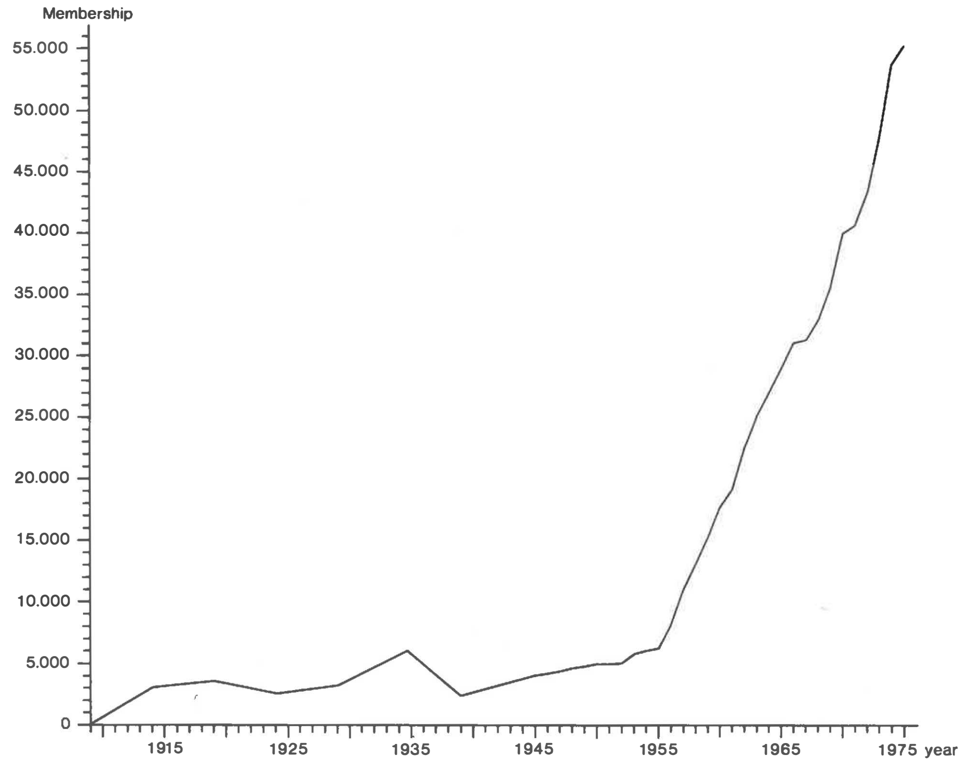

Fig. 4-2: Membership of *Svenska naturskyddsföreningen* (the Swedish Association for the Conservation of Nature) 1909-1978. Source: annual reports of *SNF*, published in *Sveriges Natur* or available in the *SNF* archives.

the organizers were taken by surprise when several hundred people turned up.[14] The quadrupled membership was not without importance for the local ecologization process during the 1950s (cf. below).

The impact of the naturalist mass movement on university ecologization was facilitated by the near exponential increase of university students during the period considered here (a development by no means limited to Sweden). The number of natural science students increased too, from approximately 700-900 in the 1930s to over 5,000 in 1960 and over 15,000 in 1970. During the 1950s the proportion of natural science students rose to 20% from about 11-12% of the total population of university students, then fell back again to the previous percentage towards the end of the 1970s. Thus, all in all, the late 1950s and the 1960s was a period of intense student recruitment to the natural sciences unparalleled in Swedish university history – comparable only to the popularity of natural science studies in the 1880s and 1890s.

14. Interview with NN 23/11 1982.

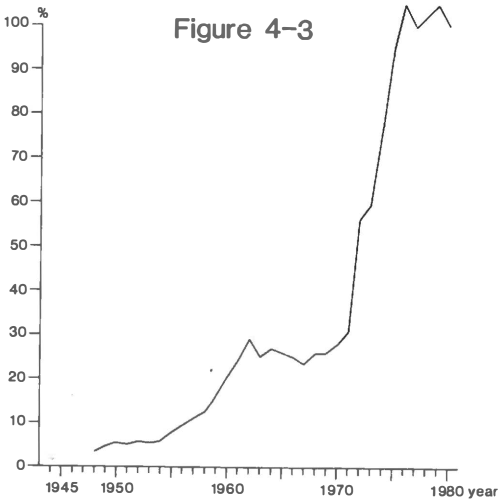

Fig. 4-3: Membership of *Sveriges fältbiologiska ungdomsförening* (the Swedish Field Biology Youth Association) 1948-1980 (in percent of 1980 membership). Source: Källander and Svensson 1981.

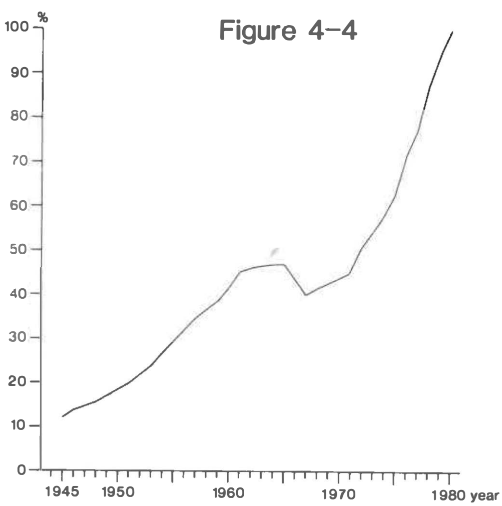

Fig. 4-4: Membership af *Sveriges ornitologiska förening* (the Swedish Ornithological Association) 1945-1980 (in percent of 1980 membership). Source: Källander and Svensson 1981.

Botany and zoology were no exceptions to this general university and natural science expansion. Some figures showing the expansion of the active staff at the Department of Zoology in Stockholm might serve as an illustration (Figure 4-5). The number of postgraduate scientists increased two to three fold towards 1970. The number of technicians showed a similar increase. The greatest expansion, however, is seen in the number of graduate scientists: students working on their licentiate- or doctoral dissertations increased six to ten fold, with a dramatic growth during the period 1955-1965.

The number of published scientific reports and examinations for higher degrees shows a corresponding leap. Figures 4-6 and 4-7 show a dramatic growth in the number of undergraduate theses and doctoral dissertations up to 1970 as a result of the expansion of the number of zoology graduate students during the early 1960s.

The total number of research reports and popular articles shows the same tendency of general expansion (Figure 4-8 and 4-9).

More significantly, figures 4-8 and 4-9 demonstrate that the output of »ecological« papers, both scientific and popular, increased much faster than the output of »other zoology« papers. Figures from other botanical and zoological departments accord with this pattern. Thus, the massive input of students to zoology and botany departments did not result in any corresponding expansion of the social orders of botany or zoology; a rapidly growing share of all students in departments of botany and zoology defined their

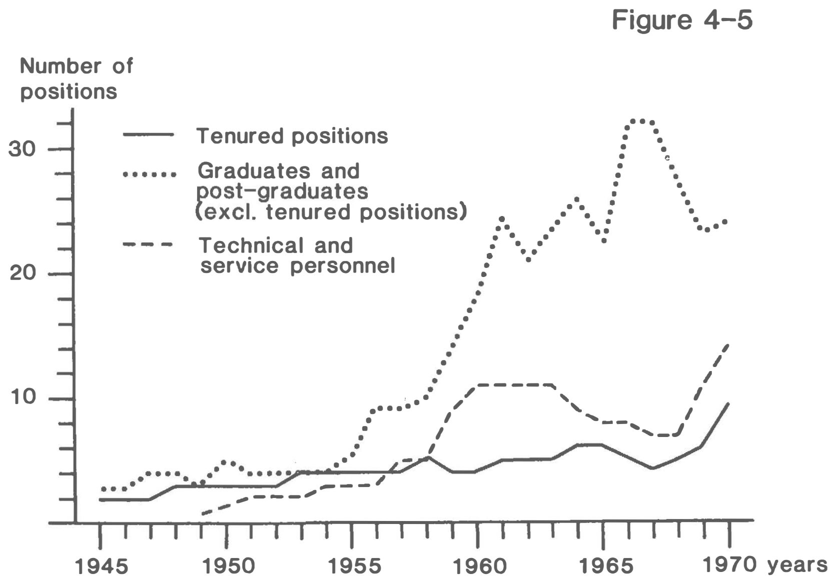

Fig. 4-5: Number of persons associated to the Department of Zoology, University of Stockholm 1945-1970 (except for undergraduate students). Source: Mimeographed yearly list of publications and other materials in the department archives.

education and research as ecological in contradistinction to traditional zoology and botany.

Establishing field stations

The advancement of field studies, and its later translation into the ecological discourse, at the universities once again raised the need for field stations. The essence of ecology is outdoor studies. However, outdoor work by indoor cultured men demands an interface, a materialized indoor-outdoor institution, that is, what Henry Thoreau created at Walden Pond: a hut, a laboratory in the field, a dormitory near the living plants and creatures. The field station is to ecology what the museum was to the systematic botany and zoology that grew out of 19th century natural history, or what the laboratory was to the comparative anatomists and physiologists of the »new German« botany and zoology towards the end of the last century.

From its very beginning the claims for ecology were accompanied by the creation of field stations. The Uppsala plant synecologists (and sociologists) had their refuge in Abisko in northern Lapland; Lundegårdh built the first field station devoted entirely to ecological research at Hallands Väderö; the Uppsala synecologists utilized Klubban; Rodhe could not do without a field station at Lake Erken; Stålfelt had a shed outside Stockholm.

Figure 4–6

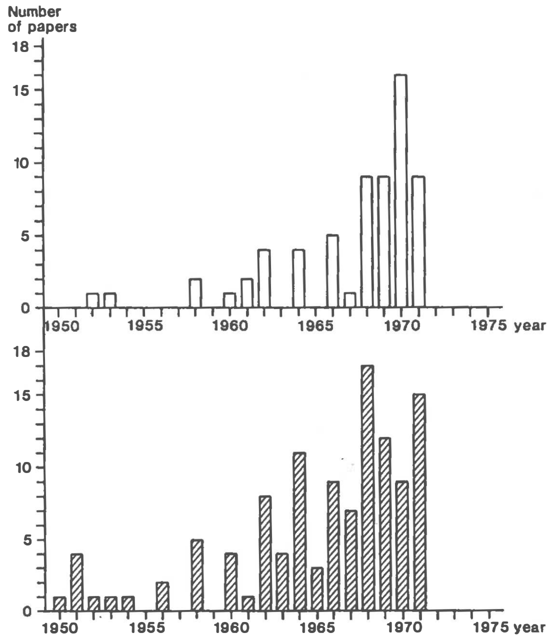

Fig. 4-6: Number of undergraduate theses delivered to the Department of Zoology, University of Stockholm 1950-1971. Below (hatched) ecological theses; above: all other theses. Source: department archives.

Figure 4–7

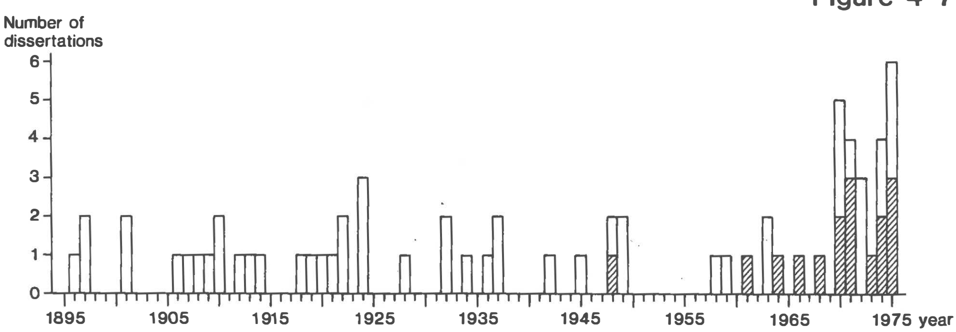

Fig. 4-7: Number of doctoral dissertation defended by scientists attached to the Department of Zoology, University of Stockholm 1896-1975. Hatched areas: biological/ecological dissertations. Source: *Svensk bokkatalog* and department archives.

Figure 4-8

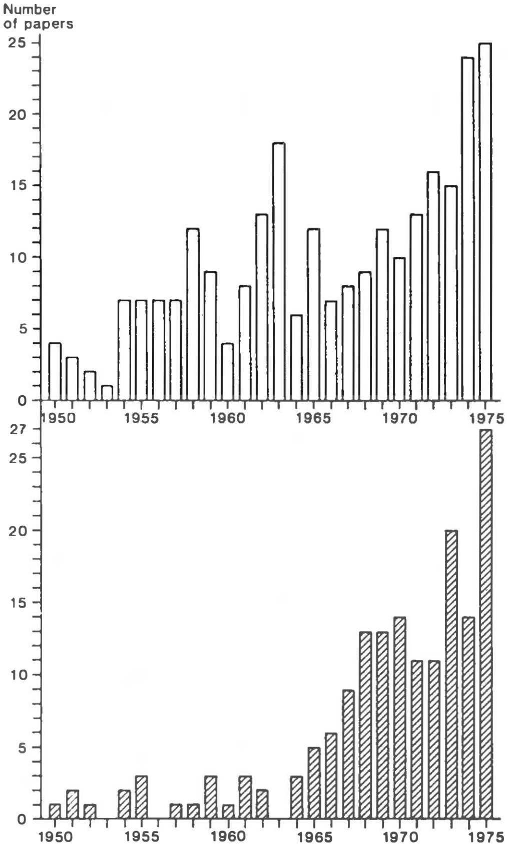

Figure 4-9

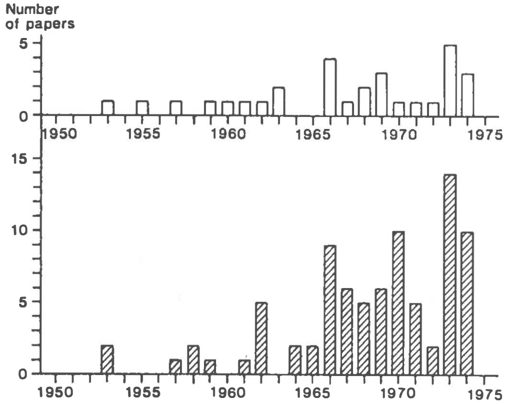

Fig. 4-9:
Number of popular scientific papers published by scientists attached to the Department of Zoology, University of Stockholm 1953-1974. Below (hatched): ecological papers; above: all others. Source: department archives.

Fig. 4-8:
Number of research reports published by scientists attached to the Department of Zoology, University of Stockholom 1950-1975. Below (hatched): ecological papers above: all other papers. Source: department archives.

The proliferation of naturalist studies in the 1950s was likewise accompanied by pleas for field stations. More and more voices were raised emphasizing the need,[15] and after direct proposals from Stålfelt and others, the Government requested a commission to investigate the question. The directives are worth quoting *in extenso* since they summarize the situation for field studies in the 1950s, still fighting to be accepted:

> *»Just three or four decades ago excursions and other education in the field were rare. It is obvious, however, that the study of plants and animals should have the living individuals and their relation to each other and to the environment as its object. Recently field biology has also received more and more place in the academic curricula...«.*[16]

The commission concluded with a strong plea for building more field stations:

15. See the overviews by Fagerlind 1956 and Hörstadius 1957. *Naturvetenskapliga forskningsrådet* appointed a committee for biological field stations in 1957 (annual report of *NFR*).
16. Fältstationsutredningen 1962,pp.4-5.

»There is a strong tendency in modern biological research and education to move activities out into nature to an ever increasing extent«.[17]

Note, however, that during the 1950s the request for field stations was still made with reference to biological research in general, not to ecology. That is, ecology still had not achieved the enrolment power *vis-a-vis* the state authorities it would have only a few years later.

Identifying practical interests

It was this fourth generation of ecologists and their younger colleagues, who turned out to be the main actors of the ecology boom of the 1960s. While the third generation of ecologists came to act as the leaders and figure-heads of the ecology boom, the fourth generation became its main activists. The ecology boom of the 1950s and early 1960s was first and foremost a consequence of the recruitment of a new wave of student-naturalists and the translation of this naturalist interest into the language of ecology.

In earlier chapters we have shown that, although a great number of scientists made »proto-ecological« investigations within the realms of agricultural, fishery and forestry science, the translation of practically oriented »proto-ecological« studies into ecological problems was negligible before the 1950s; furthermore the few that did (significantly the forestry scientists at *Skogsforskningsinstitutet*),[18] did not precipitate new ecology groups, did not partake in a wider ecological discourse, and did not make any significant contributions to the commencing institutionalization of ecology.

However, during the 1960s, it is possible to detect a tendency towards increasing ecologization of agricultural and forestry research, of plant protection research, of freshwater and marine fishery research, and of game research. Scientists working on crop plant protection, game research, forest pest control, etc., tended increasingly to designate their work as ecological. And even though few actually translated their investigations into the language of ecology, many nevertheless adopted more or less to the rapid extension of the ecological rhetoric in the 1950s and 1960s. For example, while most of the work at *Växtskyddsanstalten*[19] aimed at elucidating the life habits of pests, their distribution and their relations to host plants conducted was not translated into ecological problems in the research reports, the ecological character of the research programme was sometimes emphasized in public discourse.[20] An extreme example is a report on »ekologisk forskning vid *Statens veterinärmedicinska anstalt* (the National Institute for Veterinary

17. Fältstationsutredningen 1962,p.10.
18. This limited ecologization continued in the post-war period. E.g., a clear case of ecologization of forestry investigations is Carl Olof Tamm's work at *Skogsforskningsinstitutet* (cf.4-2). Likewise the scientists working at the Department of Forest Entomology at the Institute continued to show an occasional adoption of the language of ecology: »Central to /the department's/ research are individuals or populations and their relation to the environment. Consequently the research is mainly directed towards ecology« (»Översikt över Skogshögskolans institutioner med ekologisk inriktning (helt eller delvis)«; mimeo circulated in connection with the foundation of *Ekologiska forskarkollegiet* in Stockholm 1967/68 (see 4-4).
19. The research departments of the Institute were incorporated with *Lantbrukshögskolan* in 1976.
20. E.g., in connection with the foundation of *Ekologiska forskarkollegiet* in Stockholm in 1967/68 (»Ekologiska frågeställningar och undersökningar vid växtskyddsanstalten«, mimeographed paper distributed at the foundation of the college, cf.4-4). Another example is a discussion paper by Sylvén 1966, presenting plant protection research in terms of the ecosystem concept.

Medicine)« from 1968 translating nine full pages of descriptions of what was earlier considered plain veterinary science into the language of ecology: studies of fish, reindeer and rabbit diseases, of blood-parasites, and even of rat poisoning of domestic animals was suddenly presented as »ecological research«.[21] Translations like these give evidence of the rapidly increasing rhetorical power of ecology during the 1960s.

And vice versa identifying practical interests and securing financial backing from extra-academic sources became an increasingly important element in the build-up and institutionalization of the social order of animal ecology at the universities in the 1960s, as we shall see in the following sections 4-2 and 4-3. Of course, identifying the environmental problem – or rather the environmental crisis – was a decisive turn in the post-war ecologization process. Although the ecologization process in local university departments was by no means a consequence of the environmental crisis or the debate that surrounded it, the environmental concern was nevertheless decisive for the authorization of a national social order of ecology, as will be demonstrated below (4-4 and 4-5).

The distinction between academically oriented research and practically oriented research was not always clear-cut, however, and hence it is sometimes impossible to distinguish between the translation of practical interests and naturalist interests into the language of ecology. In several cases the actors moved between different departments and associated themselves with different extra-academic interests. For example, one scientist emphasizes that his investigation of food segregation between salmonoid fishes

> *»was carried out as a part of a joint effort between several persons and institutions, which is one of the reasons why it could be restricted just to the problem of food segregation«.*[22]

Thus, specific ecological investigations, whether practically oriented or academically oriented, whether identifying with practical interests or naturalist interests, were increasingly interconnected, and gradually coalesced into a national ecological network. Accordingly, some ecologists, although weakly placed institutionally, would play a large role as informal networkers. But these local ecologies and the invisible network formation from below, were overtaken by much more deliberate and extrinsic measures, resulting in the establishment of an authorized national social order of ecology. This national ecology is the topic of the two last sections of this chapter. Before going into that, however, we shall review the development of local social orders of plant and animal ecology at the universities.

4.2 Local plant ecology groups in the post-war period

In this section we will review the fate of the main clusters of plant ecologists at the universities, i.e., the synecologists at »*Växtbio*« in Uppsala, Stig Waldheim's group in Lund, Gottfrid Stålfelt in Stockholm and his younger ecophysiological colleague Carl Olof Tamm at *Skogsforskningsinstitutet*, and finally Wilhelm Rodhe, head of the Department of Limnology in Uppsala. How did they develop the claim for ecology during

21. Hans-Jörgen Hansen, »Ekologisk forskning vid Statens Veterinärmedicinska anstalt«, mimeograph circulated in connection with the foundation of *Ekologiska forskarkollegiet* in Stockholm in 1967/68.
22. N.-A. Nilsson 1965,p.20.

the 1950s and 1960s? Which of them were successful in enrolling students to the ecological discourse, and which contributed to the establishment of the social order of ecology?

Hugo Sjörs and the plant ecologists in Uppsala: continuing the narrative tradition

A university freshman in Uppsala around 1950, having a naturalist interest in plants, actually had even less choice than his predecessors in the 1930s if he wanted to develop his interests. There was not much point in going to Nannfeldt, the professor of systematical botany who mainly trained taxonomists.[23] And although he might have heard that Elias Melin at the physiological laboratory also kept an eye on plants in their natural conditions, he would have been disappointed – now the trend among plant physiologists was definitely towards pure physiological and biochemical problems. As Melin grew older, the ecological approach gradually faded away; during the 1950s and 1960s most of the ecological inclination, so prominent in his work of the 1920s, disappeared. Most of the approximately 20 graduate and post-graduate students which Melin had gathered around him in the 1950s worked on different problems concerning the physiology and biochemistry of fungi; thus, ecophysiological problems gave way to a biochemical approach, even more pronouncedly so when Melin was succeeded by one of his senior students, Nils Fries, in 1956:

> »*They had always stressed biochemistry... /*but Melin/ *was both biochemically and ecologically inclined. After Fries came it was only biochemistry*«.[24]

And Fries himself noted that

> »*as in other fields... the research in plant physiology performed at our Institute is increasingly trying to describe the various physiological processes of plants in terms of organic chemistry, biochemistry, or molecular biology*«.[25]

So if our freshman wanted to fulfil his naturalist inclinations he would be better advised to join »*Växtbio*« at Villavägen Road. It was still a friendly place, and a gold mine for anyone who wanted to learn about Swedish vegetation. It was a large department too – in 1955/56 for example, 80 students were officially registered to follow the courses in plant biology, and 24 were listed to pursue scientific studies for licentiate or doctoral theses.[26]

Einar Du Rietz had been only 38 when appointed Sernander's successor in 1934. By 1950, when he headed the plant geography section of the postponed *7th International Congress of Botany* in Sweden, he was at the height of his reputation and he continued to exercise considerable influence throughout the 1950s. The Uppsala dissertations of the 1950s and the 1960s largely exhibited Du Rietz's general plant sociological and implicit synecological programme. Some of them translated practical problems into synecology,

23. To continue in systematical botany with Nannfeldt did not stimulate the translation of naturalist interests to ecological problems – with one exception: Torbjörn Willén (b.1926) who in 1953 started what »was originally designed to be a comparative taxonomic study of phytoplankton from...different lakes«; from 1955, and in co-operation with Rodhe, he »paid most attention to the quantitative composition of phytoplankton« (Willén 1962,pp. 9-10); Willén also presented some of these studies as ecological.
24. Interview with NN 29/11 1976.
25. N.Fries 1977,p.42 (engl.orig.); one important exception was Curt Forsberg (later professor in limnology in Uppsala from 1980, see U 30/4 1980:12) who, partly associated with Rodhe, partly with Fries, wrote his dissertation on the ecology and physiology of charophytes.
26. *Uppsala universitets årsredogörelse* 1955-56.

hence continuing the tradition from the third generation of synecologists in Uppsala. Evald Uggla, a junior associate to Arnborg, turned the effects of fire on forest vegetation into a question of synecological correlation; Eliel Steen discussed the effects of grazing on plant communities and soil; and Gunnar Wassén later wrote a dissertation on the shore vegetation of a lake before the regulation turned it into a reservoir.[27]

Otherwise most of the graduate students to join Du Rietz continued to be amateur botanists translating their naturalist interest into vegetational studies, plant geography or synecological studies of the correlation between the site and the vegetation. As one of them wrote in the preface to his dissertation:

> *»Seit meiner Jugend habe ich das Glück gehabt die vielgestaltige Natur der schwedischen Westküste kennen zu lernen. Als ich Gelegenheit bekam an der Universität meine naturwissenschaftlichen Interessen weiterzuentwickeln, bedeuteten diese Eindrücke unendlich viel. Für mich war es daher selbstverständlich in diesem Milieu eine Arbeitsaufgabe zu suchen. Ich fand sie am Strande, im Grenzgebiet zwischen Meer und Land, wo eine monographische Bearbeitung der Strandwiesenvegetation und die Untersuchung ihrer Ökologie mein erstes Ziel wurde«.*[28]

They arrived with a passionate interest in the landscape and its flora and vegetation, and they left as plant geographers and (implicit) synecologists.

Du Rietz was tolerant with regard to the choice of research topics, as long as it had to do with vegetational problems, and accepted, although with some irritation, students who preferred Braun-Blanquet's or Raunkiær's systems for vegetational classification.[29] He even tolerated Bengt Pettersson's heresies, that is, to discard plant community analysis altogether and replace it with a floristic (instead of community) analysis of the vegetation dynamics (instead of statics) in a cultivated landscape (instead of a virgin remote mire) (cf.3-2).

But such acceptable heresies illuminate what was beyond questioning. Not even Pettersson broke with the empiricist and inductivist principles. On the contrary Du Rietz's inductivist programme stood strong. Jim Lundqvist's (b.1932) dissertation, *Plant cover and Environment of Steep Hillsides in Pite Lappmark*, initiated in 1956 and published in 1968, is a typical example of this. Lundqvist stated that:

> *»The aims of the present investigations were principally to investigate the environmental factors and the phytocoenoses or special pioneer communities in the way proposed and suggested by Du Rietz. The environmental factors were studied with the aim of giving the background of the later mainly synecological discussion of the book* /Du Rietz 1954/ *and not in the sense of producing causal facts about adaptation or requirement. In Du Rietz's definition of the term synecology was included the study of habitat factors, their ecological amplitudes and the interrelationships between the habitat factors and the plant communities«.*[30]

When retiring in 1962 Du Rietz made his last major statement on ecology. He wanted the chair in Uppsala to comprise plant ecology in a broad sense, not in the narrow sense

27. Uggla 1958; Steen 1958; Wassén 1966.
28. Gillner 1960,p.5.
29. For example, Du Rietz had earlier accepted, though somewhat unwillingly, Waldheim's use of Braun-Blanquet's system, see ED 12/11 1948:9; in fact, one of the »*Växtbio*« students in the 1930s and 1940s, Nils Dahlbeck (cf.3-2) had used Raunkiær's methodology.
30. J.Lundqvist 1968,p.7 (engl.orig.).

used by physiologically oriented scientists such as Stålfelt, whose definition excludes

> *»the study of the organisms and the organism communities themselves and... the explanations to their differentiation and distribution that lie more in age-old circumstances and events than in the present day environment«.*[31]

To interpret ecology as the study of the »relation between the organisms and their environment« was too narrow, he thought. Ecology should be »the science about the plants *in* their natural environment«.[32] The last great manifestation of the Uppsala school was the *Festschrift* dedicated to Du Rietz on his 70th birthday, *The Plant Cover of Sweden*.

However, although Du Rietz had influence over the appointment of a successor from among his former students, he did not vote for the vegetational historian among them, but the synecologist, viz., Hugo Sjörs.[33] For a decade after his dissertation in 1948 Sjörs was away from »*Växtbio*«. He spent three years in Lund in the early 1950s as acting associate professor when Waldheim was ill. Here he learned chemical soil analysis, and worked out an extensive study of the relation between site and vegetation of a park meadow, later regarded as an exemplary synecological investigation.[34] For seven years, while serving as an associate professor at *Skogshögskolan* with Erik Björkman, he worked on a variety of problems, including Canadian mires.

Sjörs was more concerned about synecological problems than perhaps any other member of the Uppsala school, but nevertheless he confined himself to the correlation approach worked out back in the 1930s. This is evident from his textbook, *Nordisk växtgeografi* (Nordic plant geography), published in 1956 as an overview of his Lund lectures, and is even more pronounced in a programmatic article on plant geography of 1961. To analyze ecosystems, he said, one needs

> *»site analysis, analysis of animal communities and vegetational analysis«*

in order to

> *»establish relations – correlations – between the results of these analyses«.*[35]

On the other hand, by emphasizing the »reciprocal« relation between site and vegetation, he implicitly criticized Du Rietz's inductivist methodology. The analytical distinction between site and vegetation which only allowed for comparisons after refined analysis and community classification ought to give way to a more »synthetic« approach,[36] allowing for correlations in both directions.

As a consequence the ecological *relation* between vegetation and site, i.e., synecology, eventually became the central aspect of the late Uppsala school. With Sjörs the Uppsala school became more outspokenly ecological, and at the same time less inductivist. Sjörs also made a lasting contribution to the authorization of ecology in the 1970s by proposing a redefinition of the chair, from plant biology to plant ecology.[37] Nevertheless, the

31. Du Rietz assessment report in ED 28/6 1962:7.
32. Ibid.
33. The other three were Magnus Fries, who had written a dissertation on the development of late quarternary vegetation based on pollen analysis in 1951, Olov Hedberg who had written on systematical and plant geographical problems, and Olof Rune who had written a true synecological study (cf.3-2) – see further the assessments in ED 28/6 1962:7.
34. Cf.4-5; for a peer evaluation of his work, see ED 28/6 1962:7.
35. Sjörs 1961,p.20.
36. Ibid.,p.20.
37. Cf. U 26/6 1980:14.

activities at the department were as diversified as before. Several of the dissertations of the 1970s would not have fitted into the old programme. Some are pronouncedly autecological,[38] and gradually dynamic (i.e., successional) studies have become more common.

In one important respect, however, one core feature of the Uppsala school still prevailed into the 1960s and 1970s. The graduate students it enrolled still almost exclusively had an earlier amateur naturalist interest:

»those who seek us are, I suppose, persons with a very concrete view of nature«.[39]

This fact, together with Sjörs' preference for empirical field studies, led to a literal absence of experimental studies and a general dislike of abstract-analytical thinking. In addition he maintains:

»What I find difficult to tolerate is quantification, quantification for its own sake«,

and adds:

»it is a little like the tale of The Emperor's New Clothes«.[40]

That is, the claim for plant ecology at *»Växtbio«* continued to be a descriptive, even narrative, field approach to plant-environment relations.

This attitude is also reflected in the publication practice. Few members of the Uppsala school have published in mainstream ecological journals, such as *Oikos*, *Ecology*, *Journal of Ecology*, etc., but mainly in the form of monographs, hence allowing for very detailed descriptions of their findings. In this sense, the modern heirs of the Uppsala school still resemble scholars of arts and letters more than natural scientists.

Nils Malmer and the plant ecologists in Lund

Around 1950 a young field botanist in Lund might just as well ignore the plant physiologists' laboratory (but could perhaps do with the museum). Hans Burström had given vent to his antipathy towards field botanists already in the 1940s, and he and his successor never came to touch field problems.[41] Some of the museum botanists under the leadership of Henning Weimarck took up experimental methods in cytotaxonomy. Most of them travelled and collected plants, but did not make other field investigations than floristics. The only remaining possibility for the young field botanist in Lund was Waldheim's group. His small plant biological laboratory was also located at the museum, but led a

38. E.g., L.Karlsson 1973.
39. Interview with HS 24/9 1981.
40. Ibid.
41. During Burström's reign the Department of Physiological Botany expanded tremendously, from a single room, no technical assistance and hardly any doctoral students, to a first-rate modern building with a staff of approx. 50; the expansion was associated with an unequivocal commitment to pure physiological problems and biochemical methodology. Burström's successor from 1972, Lars-Olof Björn, did not even mention the word ecology in his (albeit short) programmatic review of plant physiology (Björn 1973). Formally the Börje Norén research group in »microbiological ecology«, established in the mid-1960s should be counted among the ecophysiological claims for ecology. Norén, who submitted his dissertation in 1955 on »Studies on myxobacteria; with special reference to growth conditions and bacteriolytic activity« written under the auspices of Elias Melin in Uppsala, was attached to a microbiological division at Burström's department in the late 1950s. During the 1960s he enrolled five graduate scientists and they began to designate their research in terms of »microbiological ecology«. In 1972 the group was institutionalized as »the research group in microbiological ecology«. Still funded by *Naturvetenskapliga forskningsrådet* they were invited to join the other ecological groups in *Ekologihuset* (the Ecology House) in Lund (cf.4-3) (mimeographed annual reports from the Division of Microbiological ecology, Lund university).

largely independent existence.

After Waldheim had been appointed to the associate professorship in plant biology in Lund in 1948 he started an extensive project on Scanian calcareous fens, but these investigations as well as his teaching duties were interrupted by long bouts of mental illness.[42] Nevertheless, the small laboratory, in a room at the Botanical museum, expanded slowly during the 1950s. In 1956/57 it was being announced as a separate department,[43] where nine graduate and undergraduate students worked on soil and vegetational analyses. Their approach was a continuation of the example provided by Sjörs and Waldheim, as shown by the two first doctoral dissertations from the laboratory, both published in 1962. One of them, Åke Persson's (b.1925) synecological study of Lapland mires, was very much like an Uppsala correlation study, with an accurate community analysis and a separate, but rather cursory look at the site. It grew out of an early interest in rare mountain plants:

> *»During the summers of 1948-1949 I took part in a floristic investigation of the vascular plants on the northern side of Lake Torneträsk. My interest was then directed to the vegetation in the calcareous fens and springs particularly rich in species«.*[44]

This interest was first translated into a »purely phytosociological« problem, but later

> *»ecologic investigations were started in order to ascertain and elucidate certain ecologic conditions which may underlie the differentiation of the vegetation«.*

Persson's work seemingly inspired several of the younger students recruited in the 1950s – they have subsequently acknowledged his seminal role.[45]

It was not Persson, however, but another of the new generation of students at Waldheim's laboratory who led the development of plant ecology during the post-war period. Nils Malmer had begun as a young assistant to Thunmark in Aneboda back in 1946 and was encouraged to take up a study of the local mires.[46] His readings were concentrated on those works of the Uppsala school which tried to discern the relation between soil chemistry and vegetation, however. When Sjörs arrived at Lund Malmer moved to the plant biology laboratory; together they made a study of the chemical constituents of mire plants and peat in order to contribute to the understanding of the influence of nutrients on composition and production of vegetation.[47] Malmer continued his work of relating both the differentiation of the vegetation and the distribution of single species to a number of very accurately measured chemical and physical site factors, and submitted his dissertation in 1962.[48] Sjörs, of course, valued Malmer's dissertation very highly, especially from the methodological point of view. Although not a pioneer work, he said, it bore witness of

> *»a resolute will to work and a capacity to carry through a really big enterprise«.*[49]

42. It was during one of these sick-leaves that Hugo Sjörs came to Lund for what turned out to be a mutual learning process. When leaving Lund a couple of years later Sjörs had increased his understanding of soil chemistry, while keeping in touch with Waldheim's students. They in turn had been trained in the Uppsala system of vegetational classification.
43. According to *Lunds universitets årsberättelse* 1956/57.
44. Å.Persson 1961,p.5.
45. According to dissertation prefaces.
46. For biographical details on Malmer, see ED 8/11 1963:1.
47. Malmer and Sjörs 1955.
48. Malmer 1962.
49. According to ED 8/11 1963:1.

Malmer was certainly an ardent organizer. When Waldheim was permanently hospitalized in the late 1950s Malmer took over the command. Serving as acting associate professor from 1958 he arranged seminars, excursions and courses for a steadily growing number of research students; the influx of students was especially great after 1955 when undergraduate courses in botany were doubled. Winning the competition for the associate professorship after Waldheim retirement,[50] Malmer eventually took over full responsibility for the laboratory in 1963/64. Two years later the rapidly growing plant ecological group moved into new quarters, acquiring a building of its own, including well equipped laboratories and modern apparatus for chemical analysis.[51] And most important from the point of view of this story – the expansion was carried through in the name of ecology.

Thus, up to the mid-1960s the Lund plant ecologists under the leadership of Nils Malmer had expanded the investigation programme institutionalized by Waldheim in the late 1940s, i.e., translating the students floristic and naturalist interests into synecological correlation studies of vegetation and site, with an emphasis on the chemical composition of the site. A new wave of dissertations by students recruited in the late 1950s and early 1960s were submitted in the late 1960s (cf.4-5), still in the name of ecology. By then the Lund plant ecologists appeared as one of the leading local social orders of ecology in the country.

Gottfrid Stålfelt's final attempt to get plant ecology authorized in Stockholm

The two departments in Uppsala and Lund reviewed so far remained the centers for recruitment of would-be Swedish plant ecologists during the 1950s and 1960s. In Göteborg higher education and research in botany in general was rather insignificant, and except for a single ecological dissertation in 1968, the first small plant ecology group was

50. See ED 8/11 1963:1.
51. One of the interviewees maintains that the earlier lack of space and instruments had severely halted earlier expansion (interview with NN 6/9 1976).

not established until the early 1970s.[52] We will not consider Göteborg plant ecology further here.

In Stockholm the two botanical chairs were designated in the legacy of the long cytological tradition. The one chair, devoted to morphological botany was held by Folke Fagerlind from 1949; with one exception[53] he did not foster any ecologists. The other chair in botany, devoted to physiology and anatomy, however, was held by Gottfrid Stålfelt throughout the 1950s. Stålfelt continued to give courses and lead excursions for plant ecological studies until his retirement in 1959 – in the early 1950s he even founded a small plant ecological field station, Tenö, a few miles north of Stockholm.[54] Together with his younger colleagues Erik Björkman and Carl Olof Tamm (at the nearby *Skogshögskolan* and *Skogsforskningsinstitutet* respectively), he contributed to regular lunch-time discussions on ecophysiological problems in the 1950s.

Hence, a university freshman in Stockholm in the 1950s had an opportunity to translate his floristic and naturalist interest into the language of ecology, although a kind of ecology entirely different to that in Uppsala or Lund. In fact, Stålfelt's lectures and excursions seem to have been a rather important source of inspiration for Stockholm students with a naturalist interest in the 1950s. After all, during this period he was the only university scientist in Stockholm to give regular courses in ecology backed by excursions. One of the pioneers of animal ecology says that Stålfelt's excursions

> *»meant much to me personally... At the excursions he had that unequalled faculty to evoke... what had happened... the interplay... the cycle«.*[55]

and adds that it was Stålfelt that taught him that ecology could be a *science* and not only

52. The single exception was Hans Edsbagge, who was trained by Tore Levring, professor in marine botany in Göteborg from 1950 (cf.3-3, note 137). Edsbagge, who seems to have worked entirely on his own, preferred to do an ecological study: »...wichtig ist in der letzten Zeit /?/ die physiologisch-experimentelle ökologische Forschungsrichtung geworden. Grundlegend für die Ökologie ist aber die Feldarbeid, besonders um erste Auskünfte über eine Organismengruppe zu erhalten. Es muss deshalb betont werden, dass diese Arbeit – die erste umfassendere Arbeit dieser Art überhaupt – hauptsächlich auf nicht-physiologisch-experimentelle Beobachtungen baut« (Edsbagge 1968,p.7). Levring did not pursue ecological studies, but restricted himself to systematics. In the late 1960s a number of students began environmental investigations with money from *Naturvårdsverkets forskningsnämnd* (cf.4-4), and these studies were frequently translated into ecology during the 1970s (mimeographed lists over ongoing research from the Department of Marine Botany, Göteborg university). However, when the chair was refilled in 1979 it was designated as an all-round chair in marine botany: »The research area of the chair shall comprise the composition, distribution and practical utilization of /the plant world of the sea/ with connections to its ecology, physiology and chemistry« (U 19/7 1979:36). Otherwise plant ecology in Göteborg was hardly non-existent. Bertil Lindquist (cf.3-2), who was appointed Skottsberg's successor as professor of botany and director of the botanical garden in 1950/53, never returned to his earlier synecological research, but restricted himself to floristic plant geographical work. After Lindquist's death another specialist in plant geography, Per Wendelbo, was appointed research professor in botany, particularly systematics and plant geography, in 1965. A third chair in botany, devoted to systematical botany, was created in 1964, after a governmental decision to expand the natural sciences in Göteborg. No plant ecological research was pursued in the 1960s, however. Only after Tore Mörnsjö, one of the Lund synecologists in the 1960s (cf.4-5), was appointed lecturer in Göteborg did ecological teaching begin and a small group of students took up ecological studies, but these efforts remained rather modest during the whole 1970s.
53. Måns Ryberg (cf.4-4).
54. See the university catalogue from the early 1950s. »The plant ecological station« at Tenö is first mentioned in the catalogue of 1952; see also letter from Stålfelt to Stockholm university Vice-Chancellor 8/11 1950 (in *Riksarkivet* komm. 1635).
55. Interview with CCC 22/4 1977.

an outdoor hobby.[56]

A significant event was the publication of his textbook *Växtekologi* in 1960, subtitled »the balance between the production and taxation of the plant world«, a summary of his life's teaching and research experiences as an experimental ecophysiologist. Stålfelt's view of ecology was no surprise: plant ecology was the study of the interplay of a series of changing environmental factors and the economy of the plant in relation to these resources.[57] A few years earlier Hugo Sjörs in Uppsala had published *Nordisk växtgeografi*; hence, almost half a century after their appearance, both the Uppsala and Stockholm schools had at last achieved statements of their views on plant ecology in a textbook form.

Stålfelt had repeatedly tried to have plant ecology authorized in Stockholm. We have already referred to his negotiations with the city council of Stockholm for a chair in plant ecology and economic botany (cf.3-3). His attempts were in vain, however. *Växtekologi* became an epitaph to his life's work. In addition he did not enrol any students to pursue graduate research in the name of ecology; on the contrary, he even dissuaded his students from ecological investigations. His graduate students were all »extreme physiologists« as one ecologist put it.[58] After his retirement in 1959 all ecological research activities vanished[59] and the Tenö-station was closed by the university. The last member of the second generation of experimental and laboratory oriented plant ecologists in Sweden had finally failed in his attempts to have his claim for ecology authorized.

Carl Olof Tamm and the forestry plant ecologists

Where Stålfelt failed, Carl Olof Tamm at *Skogsforskningsinstitutet* succeeded. In the 1940s Carl Malmström had defined his and Romell's investigations at the Institute as »ecological«, and Tamm's dissertation in 1953 had been a clear claim for ecology, seemingly fully redeeming Stålfelt's programmatic search for a causal plant ecology including vegetation analyses as well as experimental studies. Tamm continued to express this attitude, and eventually succeeded to have it authorized. His claim for ecology was contested, however, particularly in connection with his appointment to the chair after Malmström in 1957.[60] Malmström recommended him for being a »acknowledged name« in »experimental ecology«, as did his colleague Erik Björkman, but others stood out against ecology. A geneticist in the faculty complained that:

56. Another pioneer animal ecologist in Stockholm says: »I think Stålfelt had enormous importance through his excursions, he knew so much. He was professor in physiological botany, but he probably exerted more influence as an ecologist... how the environment had originated, and how it functioned, soil processes, nutrient cycles« (Interview with BOJ 2/7 1982.)
57. Stålfelt 1960, Ch.1; it was probably this restatement of the old experimental-physiological claim for ecology that induced Du Rietz to re-articulate his warnings against Stålfelt (cf. above, note 31).
58. Interview with NN 5/10 1982; one of these »extreme physiologists« was Torsten Hemberg, who succeeded Stålfelt, the other was Hemming Virgin, who, however, expressed an unusual interest in the development of ecology in the late 1960s when a member of *Naturvetenskapliga forskningsrådet* (cf.4-5).
59. Some undergraduate education in ecology was given later, however, by the plant physiologists, assisted by the ecologists at *Skogshögskolan*.
60. Jo 29/3 1957:18.

»For five decades the ecological aspects have been in the center of the work of the department /of botany and soil science/, *and should remain so for another three decades if Docent Tamm were to be appointed... Would not it be... desirable to have a certain reorientation of the highest command of the department?«.*[61]

Nevertheless, Tamm was appointed, and he continued and enlarged the existing research programme, viz., integrated studies of the forest »ecosystem« drawing on both plant community, environmental and ecophysiological studies. Thus, the ecophysiological aspects of nutrient demand were handed over to one of his junior colleagues at Burström's plant physiological laboratory in Lund, and to continue the vegetational investigations a plant biologist from Uppsala was employed.[62] Tamm himself concentrated on experimental and field studies of nutrient requirements of forest trees.

In fact, Tamm had his claim for ecology authorized; immediately after his appointment a governmental commission changed the name of the chair from »botany and soil science« to »forest ecology«.[63] Thus Tamm's chair was the first in Sweden to be denominated as ecological. Further, Tamm appeared as one of the authoritative actors in academic plant ecology too; he was repeatedly appointed assessor in professorial competitions.[64] In his assessment of other plant ecologists he excluded purely descriptive investigations, for example, floristic plant geography, while accepting a rather broad view of causal ecology, not only experimental work, but also »comparative, where one correlates variations between plant occurrence and environmental conditions«.[65]

Thus, Tamm was successful not only as an ecologist, he also succeeded in getting his claim for ecology authorized and in contributing to the social order of ecology by maintaining a ecological discourse. Obviously, he would have played an even greater role in the build-up of a national ecology in the 1960s, had it not been for his peripheral location at *Skogsforskningsinstitutet*, where his enrolment power was comparatively small[66] compared to the possibilities available at any of the university departments.

The limnologists

We should close this review of post-war development of plant ecology at Swedish universities with a cursory look at the fate of the limnologists. Ever since Naumann claimed limnology as an independent science, most studies of the relation between freshwater

61. Åke Gustafsson's assessment in Jo 29/3 1957:18.
62. Both wrote dissertations. Torsten Ingestad's of 1962 was on macroelement nutrition of forest tree seedlings in nutrient solutions, an investigation that continued Tamm's studies with Burström in Lund in the late 1940s. Hilmar Holmen's of 1964 was on the causes of differences in forest growth after draining of peat lands. Holmen kept strictly to the inductivist principles of the Uppsala school: »On almost no occasion has the intention been to attempt to solve predefined problems«, he wrote, and continued: »Nor have the various factors of the habitat been studied by means of experimental methods. The aim has rather been to record the various phenomena and then to find out how they are related to one another« (Holmen 1964).
63. SOU 1960:17,pp.86-89.
64. To the associate professorship in plant biology in Lund in 1963 (ED 8/11 1963:1) and to the first chair in ecological botany in Umeå (ED 16/9 1966:17).
65. See Tamm's assessment in ED 16/9 1966:17.
66. One of the few graduate students of Tamm in the 1960s was J.G.K. Flower-Ellis, who continued Tamm's dissertation theme in a study of the relationship between age structure and spatial variation in standing crop and production of bilberry stands (Flower-Ellis 1971).

organisms and the lake environment have repeatedly been claimed as limnological.[67] That did not exclude claims for ecology. Thunmark had started his investigations within the realms of the Uppsala school of plant sociology and synecology. Rodhe had been a close student of the kind of experimental plant ecophysiology pursued by Elias Melin, and now and then expressed himself in terms of ecology. Likewise some of their senior students considered their work as partly ecological. For example, Arnold Nauwerck in Uppsala introduced his dissertation on the quantitative relation between zoo- and phytoplankton in Lake Erken by stating that:

> »*Im Fachgebiet der Limnologie, ältester und vielleicht idealster Zweig der quantitativen Ökologie...*«.[68]

Likewise Sven Björk (b.1927), one of the few students recruited by Thunmark during the 1950s and his successor in 1969,[69] titled his dissertation *Ecologic investigations of Phragmites communis; studies in theoretic and applied limnology*.[70] He too considered limnology a part of ecology and lake restoration a topic for »an interdisciplinary ecological science«.[71]

But except for these few statements for ecology, neither Uppsala nor Lund students of fresh-waters and their organisms contributed to the social order of ecology. With respect to the patchwork of scientific social orders in postwar Sweden they largely remained attached to limnological departments[72] and most younger graduates at these departments presented their works unequivocally as limnological.[73] Any detailed account of the development of limnology is beyond the scope of this treatise. Suffice it to say that Rodhe in Uppsala was particularly a most successful institution builder. Throughout the 1950s the Erken laboratory was the center of a large scale educational programme in limnology for Uppsala students (like the Aneboda laboratory had been for Lund students in the 1920s and 1930s), and a new limnological laboratory in Uppsala was created in the early 1960s. Furthermore Rodhe was the prime mover behind *Mälarundersökningen*, a large scale survey of Lake Mälaren launched around 1964.[74] Thunmark's department in Lund declined during the 1950s and 1960s; it was on the brink of closure in the mid-1960s [75] and it was only with the appointment of Sven Björk in 1969 that the department recovered again. In the 1970s Björk's applied limnological research programme, specializing in lake restorations, was very successful, enrolling dozens of graduate students.

The continued institutionalization of the social order of limnology should not conceal the fact, however, that the limnologists, and especially Rodhe, had an important *indirect* effect on the emergence of ecology as a social order in the post-war period. Rodhe was the great mediator of the ecosystem viewpoint in Sweden. Through his lectures and Erken-

67. The institutionalization of claims for limnology as independent departments is not a universal phenomenon. For example, in the United States, research on freshwater organisms and lake metabolism is mainly pursued by scientists attached to zoological, botanical, geological, etc., departments (see Eberly 1965, who considers the limnological laboratory in Uppsala an institutionally rare creation).
68. Nauwerck 1963,p.5.
69. See U 13/6 1969:106.
70. Björk 1967.
71. Björk 1966.
72. When Rodhe retired the chair was continued as a limnological one (U 30/4 1980:12).
73. At witnessed by dissertation titles such as »Limnological studies of...«.
74. For details on *Mälarundersökningen*, see Ahl and Willén 1965 (cf.4-4).
75. *1965 års biologiutredning* (cf.4-4) suggested the closure of the Department of Limnology in Lund; instead a position in »ecological zoology aimed at the aquatic ecology of the cultural landscape« should be created at Brinck's department. The proposal created a stir, and was withdrawn.

courses he spread the gospel of the ecosystem to hundreds of students taking limnology in Uppsala during the crucial years of the 1950s and early 1960s. Many interviewees have given witness to the basic ecological viewpoint permeating Rodhe's teaching in Uppsala:

> *»We read limnology to get a basic ecological view«,*

a zoology student in Uppsala tells;[76]

> *»All who took limnology think of the lake as an ecosystem... those who did not take limnology did not think like that«,*

another says;[77] and so forth.

4.3 Local animal ecology groups in the post-war period

Before 1950 most claims for ecology had been forwarded by scientists trained as botanists. Ecology was largely synonymous with plant ecology. During the post-war period, however, claims for animal ecology became more and more frequent. Animal ecology became a major discourse at all university departments of zoology during the course of the 1960s – the emergence of local scientific social orders of ecology was the most conspicuous event in circles devoted to animal studies. By the 1970s animal ecology was institutionalized on an equal footing with plant ecology. In this section we will survey the rapid spread of claims for animal ecology and the establishment of the new social order in the departments of zoology at Lund, Stockholm, Uppsala and Göteborg, respectively.

An uncertain future for animal ecology at Lund

During the 1940s the Department of Zoology in Lund had been the base of the leading group of animal ecologists in the country. The decision to elect Helge Backlund, and later Erik Dahl, as editors of *Oikos*, was undoubtedly an acknowledgment of the status of Lund animal ecology. The tradition for ecological studies continued nourished by a great influx of naturalist students – the capacity of the undergraduate courses was doubled in 1952. Descriptive faunistic and animal geographic studies of course continued to be the main activity among field oriented zoologists in Lund in the 1950s. Faunistic expeditions became a craze in Lund around 1950. Department staff members participated in the large Chile-expedition in 1948-49, one of the largest projects supported by *Naturvetenskapliga forskningsrådet* until then; Per Brinck and Gustaf Rudebeck went on a South Africa expedition in order to make »animal geographic and ecological studies«; and Torsten Gislén took part in the Danish deep sea Galathea-expedition.[78] Junior colleagues of the third generation of ecologists continued to take up ecological animal geographical problems. Dahl initiated Charlotte Holmquist (b.1917), the first woman to write an ecological dissertation in Sweden, to a study of the problem of marine glacial relicts from an ecological point of view;[79] Gislén inspired Anders Dahm's (b.1923) taxonomical and ecological study of the distribution of turbellarians.[80]

76. Interview with NN 22/12 1976.
77. Interview with NN 29/11 1976.
78. Brattström 1974.
79. Holmquist 1959.
80. Dahm 1958.

But in the early 1950s Lund ecology faced an uncertain future. Several of the leading figures moved out – for example, Ivar Agrell switched to zoophysiology already in the late 1940s, declaring that

»those who are too stupid to become physiologists become ecologists«.[81]

Helge Backlund took a position as a secondary school lecturer in a small province town. Somewhat later Erik Dahl abandoned ecology. Having accompanied the Chile-expedition in 1948-49, he had collected a vast material, but gave it up, apparently out of a lack of method, but also probably with an eye on Hanström's chair. He also gave up the editorship of *Oikos* to a Danish ecologist. After having turned his attention fully to morphological zoology he succeeded Hanström in 1957.[82] Finally, in 1954 Torsten Gislén, the prime mover in Lund faunistics who, since the early 1930s, had given to it its general orientation towards ecological animal geography, met his untimely death. As a consequence, no longer did any of the leading second or third generation claimants of animal ecology in Lund enrol students to do ecological studies.

Carl H Lindroth and the Lund insect ecologists

However, with the »import« and appointment of Carl H. Lindroth, one of the third generation animal ecologists who did not have his roots in Lund, to the newly created chair in entomology in 1951,[83] »proto-ecological« and ecological studies seemed to be revived and institutionalized again. Many had great expectations, hoping that he should build up an ecological institute. Lindroth's translation of faunistics to ecological animal geographic problems dated back to the mid-1930s when, like Agrell and Backlund, he came in close contact with Rolf Krogerus in Helsinki. His vast treatise on the fenno-scandian carabid beetles, including ecological geographical discussions, published between 1945 and 1949,[84] created a great stir among the younger insect faunists in Lund in the late 1940s. In an article published in the mid-1950s he expounded a formal programme for an ecological animal geography. He pointed to the need to search for knowledge of the life conditions of the animals including accurate investigations of environmental factors. Although, like Agrell and Backlund, Lindroth was more of a naturalist than an experimentalist, he too emphasized the importance of developing a causal animal geography, as »a more exact science«.[85] Lindroth came to exert a great influence on students with an interest in insect faunistics, and his partly naturalist, partly experimental programme was realized in a handful of dissertations during the following decades: »They were wild with instruments«, an observer says.[86]

81. Interview with NN 1/9 1981.
82. ED 13/12 1957:78.
83. ED 16/3 1951:5; the chair in entomology had been created in 1949 (cf.3-5). For biographical details on Lindroth, see ED 16/3 1951:5, Anon. 1979, and several articles in *Entomologica Scandinavica Suppl. 15* (1981). Cf. also 2-5, notes 220 and 222, and 3-5, note 269.
84. C.H.Lindroth 1948.
85. C.H.Lindroth 1956; to him, however, the translation of insect field investigations into ecological animal geographic problems was only one possibility. Until his death in 1979 he almost exclusively worked on more classical animal geographic problems, especially the problem concerning the Europe-North America connection. But he encouraged his students to focus on the ecological explanation, and to do pure ecological investigations.
86. Interview with NN 1/9 1981.

This seemingly thriving local discourse in animal ecology combining descriptive and experimental orientations was reflected in Paul Ardö's dissertation on the dipterous fauna of the shore dune ecosystem of 1957.[87] As a school-boy in the 1930s Ardö (b.1921) had become interested in drift-sand areas on the coasts of the southern part of the province of Halland. Through school botany and undergraduate courses in physical geography he had become acquainted with all aspects of the dunes. »This phase«, he says, »must be considered as altogether descriptive«. But later, when

> *»my interest was concentrated on the diptera of the dune district I established that there was a definite relation between the habitat of a great many species and the zones, geological and botanical, physiognomical, to which my general descriptive studies had led. From there it was not far to a more detailed study of the milieu of the diptera and the ecological conditions it might offer. Questions such as where, when, and why insisted upon an answer«.*[88]

Out of these questions – reflecting the induction of a young naive naturalist into the problems raised in the lively ecological discussions at the Department of Zoology in the 1940s – came the first Lund dissertation to be thoroughly and self-consciously ecological. The dissertation in itself was hardly remarkable: Ardö's analysis of the role of plants, the abiotic environment and the experiments performed did not, for example, outshine Backlund's work on the wrack fauna. But Ardö's decision to incorporate a general introductory chapter on »aims and method of work in ecology« in his dissertation bears witness to the general undercurrent among the young animal ecologists in Lund to define and delineate their practice as a new science. Where Lindroth restricted himself to thinking in terms of ecological analysis of animal distribution, Ardö made a claim for animal ecology as an independent science, with its own theoretical aims and its own methodology.

Another dissertation along the same lines worth mentioning is Bengt-Olof Landin's ecological studies on dung-beetles. Landin (b.1925) utilized both very accurate field studies and laboratory preference and resistance experiments to show that the microclimate is of crucial importance for the distribution of insects.[89] Yet another couple of students recruited in the 1960s and submitting their dissertations in the early 1970s followed in the same steps;[90] one of them provides a beautiful testimony of how his youthful interest in wildlife was translated into the problems of ecological animal geography:

> *»As a young man I had ample opportunities to study the rock-pool fauna of the north archipelago of lake Vänern. .... during a number of years I have lived during the summer in the archipelago of Karlshamn, ... my old interest in the rock-pool fauna was revived... In order to find out what species really occurred there, and also to investigate their distribution and ecology, I started in the spring of 1964 a more accurate investigation of the rock-pool fauna... My investigation, started on a broad basis, has in recent years more and more specialized in examinations and experiments on how certain ecologically important factors influence the distribution and dispersal of some interesting species... in the area«.*[91]

The activities of Lindroth and his students were an important element of the continued ecological discourse in Lund during the 1950s and 1960s combining descriptive and

87. Ardö 1957.
88. Ibid.,p.9 (engl.orig).
89. Landin 1961; for a peer review of Landin's work, see U 10/3 1972:26.
90. Bert Persson 1971 and Berggren 1971.
91. Berggren 1971,p.5 (engl.orig.).

experimental orientations.[92] But Lindroth failed to institutionalize his activities. In this respect the would-be fourth generation of ecologists were disappointed - Lindroth was an inspiring discussion partner, but he had no ambitions to build an ecological department, and he did not start ecological courses.[93] Instead, with Lindroth's retirement and death this half-century old programme, first adopted from Hesse's *Tiergeographie auf ökologischer Grundlage* of 1924, later taken up by Krogerus in Finland in the late 1920s, again taken up by Agrell and Backlund, and finally by Lindroth in the 1940s and 1950s, came to an end in Lund. The attraction of the programme was exhausted by the late 1960s. None of Lindroth's students enrolled their students in turn to the programme. Landin, for example, specialized in taxonomy and when he succeeded Lindroth to the chair in 1972 its denotation had been changed to »systematic zoology, especially entomology«.[94]

A Lund students' intervention for ecology

Thus, although Carl H Lindroth contributed to the continuation of »the fine ecological climate« from the 1930s and 1940s, he never contributed to the build-up of an authorized social order of animal ecology in Lund. Around 1960, however, the whole situation with regard to institutionalization of animal ecology in Lund changed with the appointment of Per Brinck in succession to Gislén. Although few men equalled his influence on the emergent social order of ecology in the 1960s, either locally or nationally, his appointment was almost accidental and involved what seems to be the first political intervention by students in favour of ecology.

At first an Uppsala zoologist was appointed only to withdraw, before even setting his foot in Lund, to take up the chair in Stockholm instead.[95] When re-announced, another applicant, Karl Lang, who had once worked on problems of animal ecology, but now worked on systematical problems only,[96] was appointed. Lang's appointment, however, aroused the indignation of the naturalist students in Lund.[97] In a collective appeal to the government they pointed out that most of the graduate students in zoology were busy with research »based on ecological problems«, and they needed a man like

92. On the other hand Edvard Sylvén, a student of Lindroth, but simultaneously attached as a scientific assistant to *Statens Växtskyddsanstalt*, and who made light-trap experiments to study the flight activity of a fruit leaf moth species for his dissertation in 1958, did not translate his work into animal ecology (Sylvén 1958). That does not mean that Sylvén was unaware of ecological problems; on the contrary he introduced another scientific assistant and junior colleague, Valdek Jürisoo (see below, note 135) to »modern ecology« (Jürisoo 1964,p.7).
93. During the 1950s no ecological literature was demanded for undergraduate studies in zoology, while those studying for a licentiate thesis were recommended to read Hesse's *Tiergeographie auf ökologischer Grundlage* of 1924 and Sven Ekman's *Djurvärldens utbredningshistoria* of 1922 (new curricula for zoology, Lund 1954, in the archives of *Universitetskanslersämbetet*, in *Riksarkivet*, vol A:124 and vol EIIb:84).
94. U 10/3 1972:26.
95. Lars Silén, a student of invertebrate systematics, morphology and biology was appointed in 1956, but shortly afterwards took the chair in zoology in Stockholm instead (see below).
96. Recall that Lang had been one of the first Lund zoologists to write a dissertation on ecological animal geography of lakes with Naumann at Aneboda in the late 1920s (cf.2-1) – he had also taught on the ecology of freshwater fauna in Lund throughout the 1930s, but had since switched to systematical studies, now holding the chair in invertebrate zoology at *Riksmuséet*.
97. The faculty and the university senate found Lang more qualified on purely scientific grounds, but nevertheless preferred Brinck for his enterprise (e.g., interviewees point to his successful organization of the Lund university South Africa expedition in 1950-51). The Chancellor of the Universities, however, reacted, pointing out that the university statutes demand that only scientific skill might influence the choice of applicants (see ED 18/6 1958:7 for details).

Brinck, attuned to several zoological disciplines, »not least the ecological«.[98] Their future was at stake, they maintained; the appointment of a museum scientist would be a disaster for their own work and for the whole zoology department as »the central site for ecological research in the Nordic countries«, as they put it.[99]

This was a decade before 1968, and a students' appeal did not change a governmental appointment. Lang was obviously affected by the resistance against him, however; he resigned, and shortly after Per Brinck, Gislén's favourite student and intellectual heir, was appointed to the other zoological chair in Lund. It would soon become apparent that this appointment was crucial for the emerging social order of ecology, not only in Lund, but nationally also. Brinck was not a particularly original scientist, but he possessed a great working capacity and an unusual faculty as an organizer and promoter.

Demarcating ecology in Lund: bird-watching and its translation into population ecology

With Brinck's accession, animal ecology in Lund both expanded and shifted orientation. First, undergraduate and graduate ecological research grew extremely rapidly in the 1960s. Figure 4-10 shows the increase in the number of theses at different levels published by scientists affiliated with the department.

Further, the translation of the naturalist interest into problems of ecological animal geography, that had been the prevailing tendency since the 1930s, rapidly gave way to a translation into problems of population ecology. The last ecological dissertation with the traditional descriptive and geographic orientation was submitted in 1963.[100] In a programmatic article of 1961 Brinck summarized the research programme as studies of

> *»the way of life of an animal group, combined with analyses... of factors determining the confinement of species to different habitats, and their reactions to different environmental factors«.*[101]

This re-formulation of the ecological animal geographic programme to ecology, and specifically population ecology and habitat selection, was rapidly reflected in the way the fourth generation denoted their own research projects. E.g., an investigation called »distribution, dispersal and ecology in terrestrial amphipoda« in 1959/60 was renamed »population dynamics and ecology in terrestrial amphipoda« three years later. Likewise »taxonomy and ecology in Unio, Anodonta and Margaritana« (1959/60) was changed to »freshwater bivalvia population dynamics and ecology« (1962/63).[102] A large majority of the dissertations worked out through the 1960s were translated into this language of population ecology: »habitat selection«, »feeding habits«, »food choice«, »ecological segregation« and »home range« were some of the key-words.[103]

98. Letter from Lund zoology students of 22/5 1958, in ED 18/6 1958:7.
99. Ibid.
100. Dalenius 1963 discussed the effects of environmental factors on the vertical and horizontal distribution of oribatid mites.
101. Brinck 1961.
102. According to mimeographed lists of on-going research in the archives of the Department of Animal Ecology, Lund.
103. See the dissertations by L. Nilsson 1970, Hallander 1970, Hansson 1971, Andreasson 1972, Bengtsson 1972, and Sven Almquist 1973. See also the dissertation on game animals referred to below.

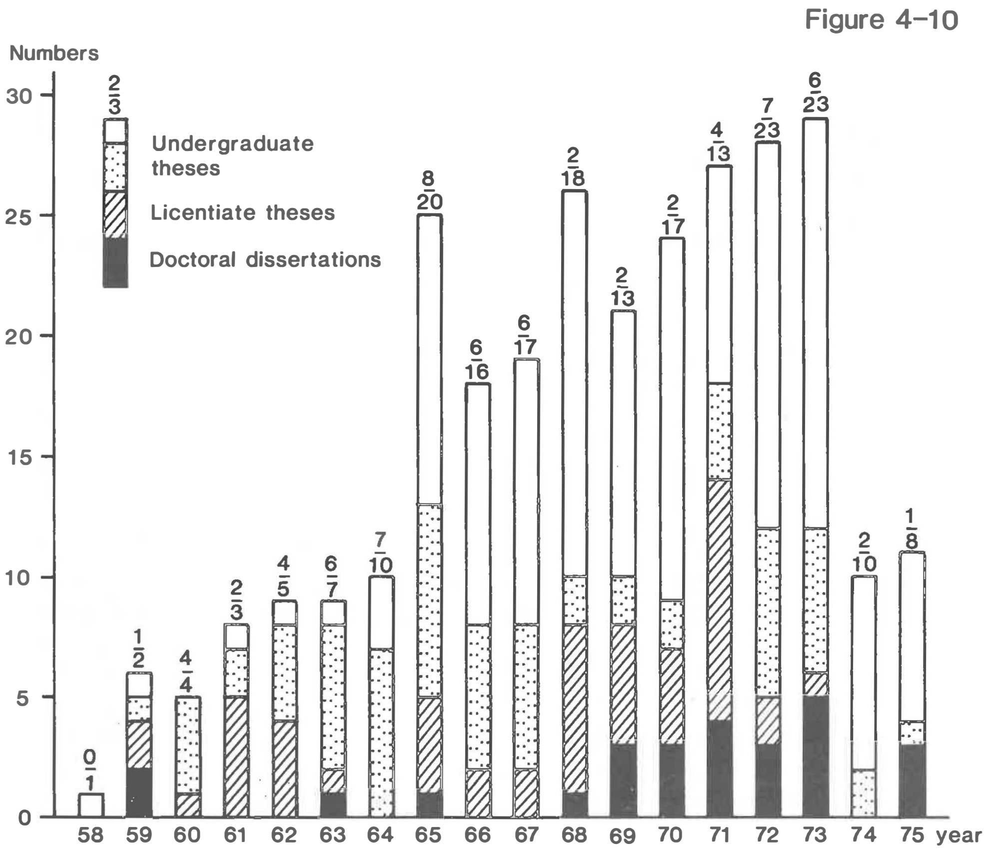

Fig. 4-10: Number of theses submitted by scientists attached to the Department of Animal Ecology, Lund University 1958-1975. Black areas: doctoral dissertations; hatched areas: licentiate theses; white areas: undergraduate theses. Numbers above white areas indicate the proportion of students writing their undergraduate theses, who continued with licentiate or doctoral work. Source: department archives.

The most active translators of naturalist studies into population ecology were the bird-watchers. Besides insect faunistic studies bird-watching was a flourishing and expanding naturalist subculture during the late 1940s and early 1950s. Almost without exception, bird-watching had not been considered a serious academic pursuit before the 1950s (cf.3-1 and 4-1). A pioneer in academic ornithology in Lund, Gustaf Rudebeck (b.1913), had submitted his dissertation in 1950 translating decade-long field observations of bird migration at the Falsterbo peninsula to an animal geograpical problem.[104] A number of studies on birds followed. For example, one investigator started bird-censuses in the provinces of Scania and Lapland; another wrote on tit migration and wintering habits; a third took up the migration and wintering of the grey crow; and a fourth wrote his licentiate thesis on bird migration. The new generation of bird-watchers did not restrict themselves to simple faunistics, however. Partly inspired by David Lack's *The*

104. Rudebeck 1950.

Natural Regulation of Animal Numbers of 1954, they translated bird-watching into a new type of problems, namely population dynamics and its relation to food habits and habitat selection. Birds lend themselves to such studies in a way that invertebrated do not. »In fact«, one of them says,

> *»many were ornithologists when they came to the department, and hence it was natural to interest oneself in population ecological questions«.*[105]

Hence the 1950s was a period of extensive, enthusiastic and sometimes high quality studies of the population ecology of birds; indeed, alongside studies of the ecological geography of insects, bird population ecology came to constitute a central part of the department's total ecological activities during the mid- and late 1950s.

The intellectual leader in this shift of emphasis for the fourth generation was Staffan Ulfstrand, one of the inveterate bird-watchers of the 1950s. While continuing his prolific writings on bird migration and bird population structure, he submitted a dissertation on the production and population movements of animal communities of running waters which marks the translational shift; the dissertation took as its point of departure Brinck's all-round taxonomical, geographical and ecological studies of the 1940s of insects from running waters, but translated these renewed and extended investigations into the problems of modern population ecology.[106]

It should be pointed out that this rapid and extensive ecologization of animal field studies in Lund in the late 1950s and early 1960s was largely a turn-away from the experimental approach instigated by Agrell and Backlund in the late 1930s. The great expansion of animal ecology around 1960 was largely in terms of descriptive field studies. The bird-watchers were essentially naturalists, having no inclination towards experimental work.

Lund ecology in service of society

Ulfstrand's extensive work for his dissertation was occasioned by conservation interests. The running waters chosen for investigation were the upper Vindelälven river which was threatened by exploitation for hydroelectric power:

> *»In 1961 the author* /Ulfstrand/ *was asked to carry out a preliminary faunistical survey in the upper parts of the river system... It soon became evident that a more extensive investigation of the animal communities of the river was desirable«.*[107]

Ulfstrand's case points to a third important aspect of the expansion of animal ecology in Lund around 1960, namely that the focus of investigations was dictated less and less exclusively by problems of pure science (e.g., problems of animal geography), but increasingly by practical concerns. To some extent this change seems to have arisen from the graduate students themselves. For example, Torsten Malmberg's (b.1923) study of the effects of pesticides on the rook population is a striking example of an initial naturalist interest that subsequently acquired focus and significance from practical concerns:

> *»I started, twenty years ago, to investigate the population dynamics of the Rook in Scania«,*

Malmberg says, and adds:

105. Interview with NN 14/3 1977.
106. Ulfstrand 1968.
107. Ibid.,p.7 (engl.orig.).

»The studies were not originally intended to serve any practical purpose, but they have since become relevant to the pesticide situation«.[108]

However, the change in interest identification was mainly effected by Brinck who had early been acquainted with practical animal studies – for a couple of years in the 1940s he had been director of a parasitological laboratory in Malmö. When placed in charge of the Lund department he accentuated the practical implications of animal ecology, and he early began to act as a scientific broker, specializing in establishing contacts between his own graduate students and outside interests, whereby different practical issues became translated into problems of population ecology.

Accordingly the extensive studies of bird populations were gradually superseded by population studies of more economically significant animals. The choice of licentiate- and doctoral theses was not made »according to theoretical interest but according to economic criteria«, one scientist maintains.[109] A large proportion of the dissertations up to the early 1970s were oriented towards freshwater and game management, pesticide problems, parasitology, etc. In total eight scientists worked on parasitological problems, and two of them published their dissertations in 1971 and 1972, without translating their work into ecology. Approximately 20 scientists worked with freshwater organisms, and one of them published his dissertation on the population ecology and environmental relations of crayfish in 1971,[110] with large-scale financial backing from a number of outside interests. Two graduate scientists also received external support for studies of the ecological effects of chlorinated hydrocarbons.[111]

The Lund naturalist students were particularly successful in translating game management problems to ecology. Game research had earlier been a task for *Skogshögskolan* – the chair in forestry zoology at *Skogshögskolan* (cf.3-4) had been created in 1949 after pressure from *Svenska jägareförbundet*. The Association's promotion of game research achieved important break-throughs in the mid 1950s:[112] in 1954, a game research council, *Viltforskningsrådet*, was established with representatives from universities, agriculture, forestry and veterinary interests; in 1955, the Association's former publication series was superseded by the journal *Viltrevy*, usually publishing in English; and in 1956, the Association procured a stationary field laboratory for game research at Boda in the southern part of the region of Norrland.[113] The prime mover of game research at the Boda station for the coming decade was a non-academic investigator, Nils Höglund who published a series of reports on gallinaceous birds in the 1950s. But around 1960 academic zoologists began to make game management studies the subject of academic dissertations, predominantly translating them into the language of ecology. The majority of these came from Lund and Stockholm (those from Stockholm will be covered below).

The Lund dissertations on game animals were supported financially by *Viltforsknings-rådet*. Whereas the first of them – on reproduction of moose in Sweden by Gunnar

108. Malmberg 1971,p.89 (engl.orig.); his dissertation was submitted in 1973.
109. Interview with NN 23/9 1976.
110. Abrahamsson 1971.
111. Berith Persson 1973 and Södergren 1973.
112. Haglund *et.al.* 1980,pp.269-71.
113. After *Statens naturvårdsverk* (cf.4-4) had taken over the tasks of *Viltforskningsrådet* in 1968 yet another game research field station was established at Grimsö in the province of Västmanland.

Markgren[114] – was not translated into ecology at all, subsequent ones gradually assumed the new discourse of population ecology, which was actually introduced by an American game ecologist invited to Lund by Brinck in 1958. Although Ingemar Ahlén's (b.1936) monograph on the red deer reflected the ecological geography programme of the 1950s in translating game management studies into problems concerning »history of distribution«, »taxonomy« and »ecology«,[115] the later dissertations on small predators were fully translated into the new language of population ecology.[116]

One consequence of these developments at Lund was the ecologization of game research at *Skogshögskolan* as well. The ecological element in the description of the chair in forestry zoology (cf.3-4) was strengthened when it was announced as vacant in 1966. Being now designated as a chair in »forestry vertebrate ecology«, the applicants were assessed by a majority of third generation ecologists, including Brinck. In fact, Ingemar Ahlén was appointed professor,[117] thus extending Brinck's programme of practical ecology.

The eventual institutionalization of Lund animal ecology – Ekologihuset

Brinck was not only a leading entrepreneur for identifying practical problems and translating them into the language of ecology, he was also a successful department builder. Shortly after his appointment, he began to demarcate a »systematical-ecological division« of the Department of Zoology, and a few years later his division was known simply as »the department of animal ecology«. In addition ecology was taken up in the zoology courses around 1960 for the first time. Hence, Brinck simply created an ecological department by announcing it as one.

By the late 1960s the Lund animal ecologists under Brinck's leadership were rightly considered to constitute the leading ecological department in Scandinavia. The number of dissertations in animal ecology produced in Lund was significantly higher than in Stockholm, Uppsala and Göteborg, and the local animal ecological discourse in Lund was far more extensive than anywhere else in the country – the population of animal ecologists active at the department between 1965 and 1975 was fairly stable at around 60; in addition numerous foreign ecologists were invited as guest scientists. Without doubt much of this activity was due to Brinck's unusual qualities as a departmental leader. All interviewees depict a man with a considerable working capacity and with an eye to the main chance, a skilled organizer both of his own and others work, and a diligent and able popularizer and salesman for ecology. The financial contacts with supporting agencies were more developed in Lund than elsewhere. Brinck was without doubt the leading entrepreneur for ecology in Sweden in the 1960s.

Although not yet authorized by state authorities (Brinck's chair continued to be a zoological one), the foremost position of Lund animal ecology secured its institutionalization. And to add the finishing touch Brinck moved the whole department to a new

114. Markgren 1969.
115. Ahlén 1965.
116. Erlinge 1969 and Gerell 1971.
117. Jo 6/6 1968:5; Per Brinck and Erik Dahl were among the assessors.

building in 1969/70 – *Ekologihuset* (the Ecology House) he called it. Thus, ecology had its first counterpart to the museum buildings of the mid-19th century and the anatomical laboratories of the late 19th century.

The further recruitment of graduate students took the form of specialization. Ulfstrand gathered two groups of graduate students around him: one group continuing his ecological approach to running waters; the other reviving the populations studies of birds from the 1950s. The latter group rapidly recruited some half-dozen bird-watching graduates students. Erlinge, likewise, built up a group for game population studies, while Brinck took charge of a more heterogeneous collection of graduates, including a successful so-called »ecochemical« group.[118)]

Per Brinck did not restrict his entrepreneurship to the local social order of ecology in Lund, however. Together with leading animal ecologists from Denmark and Norway he persuaded *Nordiska kulturkommissionen* (the Nordic Cultural Commission) to support a nordic college for terrestrial ecology, *Kollegium för terrestrisk ekologi*, which began its activities in 1964,[119)] and later played a significant role in the ecologization process in the late 1960s and 1970s. Up to 1976, when extended and renamed *Nordiskt kollegium för ekologi* (the Nordic College for Ecology), the College had arranged some 60 courses involving altogether more than 700 participants.[120)] Further, in 1965 Brinck took over the editorship of *Oikos* from a Danish ecologist, and turned it into one of the leading international ecological journals during the 1970s, providing also a primary publication outlet for his students. Brinck's personal influence was magnified by the fact that, as one of the few third generation animal ecologists in office, he came to act as assessor in several professorial appointments.[121)]

Finally, a most important event for the future ecologization process was Brinck's election to *Naturvetenskapliga forskningsrådet* in 1961. In fact, Brinck was elected in his capacity as a young and ambitious zoologist, not as an ecologist. Nevertheless this ensured him a prominence in national scientific circles – his career as a local entrepreneur for ecology coincided, throughout the 1960s, with his career as the leader of the build-up to a national ecology policy throughout the 1960s. His local programme for ecology was subsumed within the wider national programme for the expansion of ecology. The emergence of a national ecology in Sweden in the 1960s was largely the story of Per Brinck's activities – not for nothing do ecologists all round the country call Per Brinck »the Godfather« of Swedish ecology.[122)] To these events we will return in the following sections. Before that, however, we will review the build up and institutionalization of local social orders of ecology in Stockholm, Uppsala and Göteborg, respectively.

118. According to mimeographed lists of on-going research in the archives of the Department of Animal Ecology, Lund.
119. Resolution of *Nordiska kulturkommissionen* nr 2/1963; see also the short history by Enckell 1977.
120. Enckell 1977.
121. Including the chair in ecological zoology in Umeå in 1965 (ED 30/12 1965:12) and again in 1976 (U 5/8 1976:10), and the chair in forestry vertebrate ecology at *Skogshögskolan* in 1966-68 (Jo 6/6 1968:5). It should be noted that his colleague Erik Dahl acted assessor in even more appointments – together the two Lund zoology professors were the main assessors in professorial competitions involving animal ecologists during the whole 1960s, sometimes even acting together (Jo 6/6 1968:5).
122. According to several interviewees.

Breaking the hegemony of comparative anatomy: the emergence of a fourth generation of animal ecologists in Stockholm

When founding the Department of Zoology in Stockholm in the 1880s Vilhelm Leche had identified zoology with comparative anatomical studies. Stockholm zoology remained an impregnable stronghold for phylogenetic reconstructions of vertebrate evolutionary history for almost 70 years. In the folklore of the department, Leche's successor Holmgren is best remembered as the professor who ridiculed insect collectors and bird-watchers: »Dicky-bird in the bush«, is said to have had been one of his favourite taunts.[123] Although several graduate zoologists in Stockholm in the 1930s and 1940s had had a naturalist interest they had all been enrolled into the comparative anatomical programme (cf.3-4).

After Torsten Pehrson was appointed professor in 1948[124] things slowly began to change, however. Although having opposed the ecologization of the secondary school curriculum (cf.3-1), Pehrson had nevertheless served as a secondary school lecturer; this may have been one of the reasons why he was a little more tolerant towards students with a naturalist interest than his predecessors had been. In fact, he gave lectures on animal geography, albeit informed by phylogeny. In addition, the post-graduate staff was renewed and expanded during the late 1940s. An Uppsala zoologist, Lars Silén, the first to be »appointed to a higher position in the department without having passed through its ranks«,[125] had mainly worked on morphological and systematical problems, but devoted some energy to the biology of his marine creatures as well. Thus animal biological studies became acceptable. Furthermore, Carl H. Lindroth, by then still a secondary school teacher in Stockholm, and Lars Brundin, both having worked on ecological geographic problems, were appointed to temporary teaching positions in the late 1940s. These staff changes gave students new possibilities to translate their spare-time naturalist interests into academic research problems. Field excursions were introduced as a new item of the curriculum, and a number of undergraduates students, several of whom were active members of *Sveriges fältbiologiska ungdomsförening*, took up a variety of faunistic topics.[126]

As a matter of fact, the situation for field studies changed radically after 1956. The department moved to a new and spacious location, Torsten Pehrson was succeeded by Lars Silén,[127] and the influx of faunistically interested zoology students increased swiftly from about 15 students per year in 1951 to about 100 per year in 1965. Studies of animal distribution, and its relation to environmental factors became more and more common. Many worked on a combination of taxonomical, ecological and geographical problems; a new craze was to establish »investigation areas«, i.e., untouched landscape areas suitable

123. Interview with NN 26/4 1977.
124. ED 30/6 1948:4.
125. Pehrson, n.y.,p.23.
126. Between 1949 and 1954 at least eight undergraduates wrote faunistic papers: one wrote about the dependence of the bird fauna on the composition of forest, another on the feeding habits of tits, a third on home-range behaviour in the garden snail, a fourth on the distribution, periodicity, biotope selection and feeding conditions of small rodents, etc. (archives of the Department of Zoology, Stockholm, and oral communications). In addition there were a few instances of field work in the courses. A zoology student in the early 1950s had a one- or two day brackish-water excursion led by Silén, three short bird trips led by one of the younger bird-watchers, and a couple of »excursions in entomology and limnology« led by Brundin.
127. ED 15/6 1956:20.

for faunistic inventories.[128)]

The accompanying boom for outdoor animal studies was not immediately translated into ecology, however. Several interviewees remember that the occasional excursions and field courses were »more biological than ecological«. For example, the new

> *»brackish water excursion* /was/ *a great departure – but it was hardly ecology... Göran* /Malmberg/ *talked a little about salt percentage«.*[129)]

and another comments that

> *»there was no ecology in the courses in the mid-1950s, it probably did not come until the early 1960s«.*[130)]

The same trend can be seen from the journal *Svensk faunistisk revy* (later *Zoologisk Revy*) (cf. 3-1), edited by Stockholm graduate students. Zoology students continued to publish faunistic and biological investigations, but they were not commonly claimed as ecological until the 1960s. And when talking about ecology they usually referred back to Ekman or Hesse.[131)]

Furthermore during the 1950s the majority of those who continued with graduate research, were still obliged to follow the comparative anatomical trail. It is generally considered that there was

> *»a strong resistance from the leading zoologists to* /let them/ *do things like that* /field studies/«.[132)]

Kjell Engström (b.1929), for example, one of the leading members of *SFU*, and a very active spare-time bird-watcher who published both biological and explicitly ecological studies of birds in the early 1950s, was »pressed to do structural zoology«.[133)] A majority of the graduate students recruited around 1950 wrote anatomical dissertations, some of which were closely modelled on their mentors', whereas others at least implied a progressive development of the programme.[134)]

The few who submitted field studies of animal-environment relations during the 1950s and early 1960s were all closely attached to institutes for applied studies, like Notini had been a decade earlier (cf.3-4). But these studies were not at all, or only occasionally,

128. Johnels 1963.
129. Interview with NN 25/1 1977.
130. Interview with NN 18/4 1977.
131. As witnessed by contributions to the journal *Svensk faunistisk revy* this tradition seemed to be the only one heard of around 1950; in nr 4/51 the signature AJ (Alf Johnels?) reviewed a new English edition of Hesse's classical work *Tiergeographie auf ökologischer Grundlage*, and in nr 2/52, the term »ecology« was for the first time a regular heading in the journal's review section; in 1955 Waldén explained his use of the distinction between »existence-ecology« and »dispersal-ecology« with reference to Ekman's book of 1922 (p.430).
132. Interview with NN 9/9 1976.
133. Interview with NN 18/4 1977.
134. E.g., electron microscopy and associated techniques were introduced.

translated into the language of ecology.[135] and in case they mentioned ecology they still had very little influence on the nascent ecological discourse at the department. The large majority of claims for animal ecology in Stockholm in the late 1950s and throughout the 1960s were forwarded by young zoology students entirely within the academic context.

The rapid turn to ecology in Stockholm

Originally, the translation of this widespread, but diffuse, naturalist and faunistic interest into a scientific ecology was largely carried through by two young fourth-generation ecologists, recruited around 1954, and later strongly supported by Silén. Both took up the combination of experimental studies and field work forming the core of the Krogerus-Lindroth tradition[136] and gathered a number of younger undergraduate and graduate students into two local ecological groups during the 1950s and 1960s.

Carl-Cedric Coulianos (b.1930), the first chairman of *SFU*, was an ardent insect faunist who early learnt the scope and methods of ecological animal geography from Carl H. Lindroth in the late 1940s. Coulianos introduced the combination of field work and preference experiments, so prominent among Lund ecologists in the 1940s and 1950s, to the Stockholm zoologists in order to reveal

> *»real causal relations between animal distribution... and climatic factors in their microhabitats«.*[137]

Learning from Stålfelt that ecology could be a rigorous science, Coulianos became the leading promoter within the Stockholm department for the view that ecology could be an independent scientific endeavour. His claim for ecology attracted students with an interest in insect faunistics. His excursions and courses in entomology and limnology mounted from 1956 onwards were the first elements of organized ecological education at the department. In 1963/64 the ecological pretensions of Coulianos and his younger colleagues were officially recognized by the rest of the department as a »terrestrial-invertebrate-ecological research group«; by then five graduate students worked on problems related to insect distribution and microclimate. In addition, in 1966 Coulianos published a

135. Armin Lindquist, for example, writing his dissertation on the morphology and biology of *Limnocalanus* in the Baltic while attached to *Havsfiskelaboratoriet* in Lysekil in the 1950s, discussed the correlation between hydrographical factors and animal distribution, but did not translate his findings into ecology (A.Lindquist 1961). On the other hand Hubertus Eidmann, who was attached to the Department of Forest Entomology at *Skogshögskolan*, occasionally considered his dissertation studies of a larch tree pest, covering its life habits and the environmental influences on its population changes, as »ökologische und physiologische Studien« (Eidmann 1965). The most advanced claim for ecology made by a practically oriented animal zoologist around 1960 came from Valdek Jürisoo (b. 1926), who not only translated his investigations of the so-called Bollnäs disease into ecology, but actually claimed them as a specific branch of ecology, viz., agro-ecology, with the following programmatic declaration: »Both autecological and synecological studies will further plant protection research. This increases the knowledge not only of life cycles and abundance variations of individual species, but also of their independence upon environment - the chain of the biocoenotic dependence complex which, despite the interference of man, exists in cultivated fields too« (Jürisoo 1964,p.6). Noting that »agro-ecology is a relatively neglected subject when compared with the large numbers of ecological work on natural biocoenoses«, he explained this observation in the following terms: »The reason for this may be that synecological studies on cultivated fields are regarded by many ecologists as pointless and uninteresting since, in their opinion, man prevents the balance of nature from maintaining itself here« (Jürisoo 1964,p.6). Jürisoo never carried through his claim for agro-ecology, however.
136. Krogerus himself contributed with a small paper on the ecology of bog animals in *Svensk faunistisk revy* nr 3/47.
137. Coulianos 1962,p.64.

Swedish edition of Odum's basic textbook in ecology, which was immediately introduced as a standard textbook for the ecology courses.

The other fourth generation enroller for ecology at the department, Bengt-Owe Jansson also came with a strongly developed naturalist interest:

> *»I believe what made me interested in biology was that I lived in Visby, a small town with the big sea outside dominating the town, very green. You had the feeling that the town was so small, and nature so large«.*[138]

Jansson started his scientific career with an investigation of the morphological adaptations of a group of »worms« to the extreme environments between sandgrains on shores:

> *»By studying the bodily structures of systematically different animal groups, one could learn in which respects sand as a substrate leaves its mark on the animals«.*[139]

This was a renewal of the kind of animal studies claimed as »animal biology« 75 years earlier (cf.1-3). But partly inspired by Erik Dahl's studies of sand faunas[140] he turned to ecological problems, and by working closely together with Coulianos for a couple of years, soon he too adopted a variant of the programme for an experimental ecological animal geography:

> *»it soon became clear that laboratory experiments were necessary to confirm the suspected correlations between the distribution of the fauna and a certain environmental factor«.*[141]

Jansson also gathered a group around him which rapidly became very successful. In 1960 they got access to an old archipelago homestead and the following summer a primitive marine laboratory was established on the island of Askö, approximately 60 km south of Stockholm. A handful of undergraduates started a general survey of the fauna and flora and environmental factors. Some of them continued the work as graduate students, each one choosing a habitat for a dissertation (cf. how Sven Ekman distributed dissertation subjects on deck by taxonomic groups in the 1930s). While Jansson continued with the sandy beaches, his wife took the fauna of the Cladophora algal belt, another in the group took the plankton fauna, yet another the rock-pools. New students were gradually recruited, and around 1965 the first publications appeared. Much of the work was within the scope of the programme laid down by Jansson and Coulianos ten years earlier, i.e., field studies of the correlation between environmental factors and animal distribution, supplemented by laboratory preference experiments. Jansson's dissertation of 1968 summarized the programme: a decade's work with experiments and field observations in order to elucidate the relation between the distribution of the interstitial sand fauna and the spatial distribution of a number of environmental factors.[142]

So far, not much had changed since the days of Agrell and Backlund in Lund in the 1930s and 1940s. However, the three dissertations to follow in 1971 were much more

138. Interview with BOJ 2/7 1982.
139. B.-O Jansson 1958,p.67.
140. See e.g., Dahl 1952.
141. B.-O Jansson 1962,p.293.
142. B.-O.Jansson 1968.

varied in their approach,[143] and subsequent developments during the 1970s broke drastically with the Krogerus-Lindroth programme as will be demonstrated in the last section of this chapter (4-5).

The Stockholm department developed its own brand of population ecology too. As in Lund, the translation to population ecology was partly made through identifying with practical interests. In the late 1950s Silén established contacts with animal protection and game interests – a few students were recruited to make investigations of game, and as a result a game ecology group emerged during the first part of the 1960s, translating their investigation into the language of population ecology introduced at the department in the early 1960s. The down-to-earth methodology – viz., that the relation between predator and prey could best be studied by means of analysis of stomach contents – involved a lot of field work, and the group gathered many naturalist students during the 1960s. In contrast to the insect and marine ecologists, no experimental studies were done. Two graduates published their doctoral dissertations in 1970: one on the food ecology of the badger, and the other on the food ecology and population dynamics of the fox.[144]

Compared to the insect ecologists and the marine ecologists the game group was less active in ecologizing their studies. One reason may be the powerful resistance (reinforced by financial patronage) of game management interests towards the absorption of game investigations into the esoteric discourse of ecology. Another reason may be that most scientists working with game mainly have a personal attachment to the animals; if the motive were simply to translate a faunistic or naturalist interest into the language of ecology one would probably not choose such difficult research objects as game species. On the other hand, still from a psychological point of view, studying the climatic dependence of insects, or the food relations between crustaceans and plankton, it is more easy to abstract from the emotional qualities of the animal, and such studies thus lend themselves more easily to translation into ecology.

Thus from around 1960 three small but steadily growing local ecological groups, led by fourth generation ecologists, enrolling students with a naturalist interest, had been established at the Department of Zoology in Stockholm, and with full support from the professor. By 1967 the marine group comprised 18 scientists, while the insect group and the game ecologists comprised about 12 each.[145] Seen from the perspective of the mid-1960s it was not possible to foretell which of them would lead to institutional success and which would fail. Seen from the vantage of the mid-1970s, however, the insect ecologists were not very successful – few dissertations were produced and in the long run

143. These were Björn Ganning's studies on Baltic rock-pool ecosystems of 1971, Hans Ackefors' studies on the ecology of the zooplankton fauna in the Baltic of 1971, and Lars Westin's studies on the biology and ecology of the fourhorn sculpin of 1971. Ganning changed his goal from a experimental study to an ecosystem analysis during the course of the study (cf.4-5); Ackefors, being attached to *Havsfiskelaboratoriet* in Lysekil, made an ecologized, but in content rather classical, study of the correlation between environmental factors and zooplankton horizontal and vertical distribution; Westin's study was a species monograph.
144. Skoog 1970 and Englund 1970.
145. According to mimeographed presentation of »Avdelningen för ekologisk zoologi« distributed to the members of *Ekologiska forskarkollegiet* (cf.4-4) in Stockholm in 1967/68.

the experimental programme in ecological animal geography came to nothing.[146] Likewise the game group disintegrated. Only the marine ecologists under Jansson's leadership, by identifying environmental interests and adopting the ecosystem rhetoric, turned out to be an institutional success, as will be documented in more detail in the last section (4-5).

However, irrespective of the differential institutional success of the separate research groups, ecology as such became an overwhelming institutional success in the department. Whereas academic animal studies had been largely translated into comparative anatomical problems until the mid-1950s, they were largely translated into ecology from the mid-1960s. The earlier short excursions were complemented by an increasing number of field courses during the late 1950s, most of them led by graduate students and spare-time naturalists,[147] and were gathered into two larger course blocks, both denoted as ecological, in the early 1960s. Occasional meetings on ecological issues likewise sprang up at the graduate seminar during the late 1950s, and from 1961/62 the seminar were split into a morphological and an ecological one. Finally, in 1964, the whole department was split into three divisions: one for structural zoology, one for ethology,[148] and one for ecology. The rush to join the ecology seminars was overwhelming. In an internal circular of November 1964 it was stated that

> *»the strongly increased number of participants at the ecology seminar (over 50!) has now necessitated a division of the seminar into two: the invertebrate ecological seminar... and the vertebrate ecological seminar«.*[149]

It is worth noting that this immense increase in the number of ecology students, the corresponding enrolment of graduate students to the three ecology groups, and the commencing institutionalization of ecology, was not a result of science policy or educational policy measures. Neither was it an anticipation of the approaching environmental debate. From undocumented valuations made by interviewees it is possible to estimate the proportion of zoology students having a strong naturalist spare-time interest at 50% or more in the 1960s. Thus, students gathered around the ecology seminars because a few pioneers could offer them an academic outlet for their naturalist interest. They chose to study ecology because it was enjoyable, because they had a spontaneous interest in living creatures in their natural environment which could easily be translated into a new conceptual apparatus.

Throughout the 1960s this local ecology movement remained an undergraduate and graduate affair, however. For the time being there were no higher positions available for these fourth-generation ecologists. The eventual authorization of the ecology wave, initiated among Stockholm zoologists in the 1950s and 1960s, did not take place until the

146. The group did not produce many dissertations until the early 1970s; around 1975 the few remaining insect ecologists took up the new American fashion of evolutionary ecology, which rapidly became a great hit (cf. Wiklund 1975). See also the Epilogue.
147. In 1956 Coulianos started a course in entomology and limnology; in 1957 Kjell Engström initiated a course in ornithology; from 1958 Stockholm zoology students could also take courses in marine biology at the West coast and a course in vertebrate ethology and ecology, and in 1959 they could take a course in game management and nature conservation (archives of the Department of Zoology, Stockholm).
148. Ethological research and education was started at the department by Erik Fabricius, a Finlandish scientist who had worked on fish behavioural investigations at *Sötvattenslaboratoriet* in the 1950s; in 1958 he got a staff position and enrolled a slowly growing number of students, a few doing experimental work, but most doing field studies of the behaviour of game and birds. In 1967 the group comprised five scientists (Fabricius 1962, and archives of the Department of zoology, Stockholm).
149. Mimeo in the archives of the Department of Zoology, Stockholm.

mid-1970s, when the associate chair in zoology was specified as »ecological zoology«.[150)]

From marine laboratory zoology to environmental field investigations in Uppsala

The previous detailed accounts of the development of the local social orders of animal ecology in Lund and Stockholm indicate that the rapid and extensive post-war ecologization at the universities was dependent both upon the influx of large numbers of students with wildlife interests and the strength of the social order of zoology. Where traditional zoology was weak, as in Lund, insect enthusiasts and bird-watchers had an easy way. Likewise in Stockholm, faunistic interest was rapidly translated into ecology as soon as the hegemony of the traditional zoologists was broken. By comparison, the slow emergence of a local social order of animal ecology in Uppsala illuminates more clearly the forces of resistance against ecology.

When Sven Hörstadius and Gösta Jägersten took up their duties as professors of zoology in 1942 and 1947 respectively[151)] they continued resolutely the tradition of the »new German« zoology, i.e., anatomical and morphological studies of marine animals. But they could not enrol many graduate students. Only four doctoral dissertations were submitted between 1950 and 1963. Instead, the growing number of undergraduates (the participation in undergraduate courses in zoology was doubled in 1951/52) was accompanied by a growing concern for field studies and ecology. Already from 1955/56, when the annual reports from the department began to specify ongoing research, 10 out of 25 scientists were reported doing »ecological« investigations, and another 6 to 8 were reported doing »systematical and ecological« investigations.[152)] But only a few of these worked on marine animals, and certainly none of them succeeded in building up a successful ecological group like that of the Askö laboratory in Stockholm.

The hesitant ecologization of Uppsala studies on marine animals was of course closely connected with the resistance to descriptive field studies on the part of the laboratory zoologists. Hörstadius was a spare-time naturalist, but did not encourage field studies for graduate research. Jägersten openly disliked descriptive field studies. For example, Kullenberg's field biological excursions in the 1950s were

> *»not regarded positively by a man like Jägersten who was very strictly tied to he opinion that the most important thing was to work with marine animals since all main types of* /evolutionary/ *development are to be found in the sea, while insects are... one type among all the others«.*[153)]

On the other hand he did not oppose experimental ecology, and actually encouraged a single graduate student in this direction in the 1960s.[154)]

150. The initiative to change the designation of the chair emanated from the department, but was confirmed by the government in 1976 (U 13/5 1976:17); Hans Ackefors (cf. note 143) was appointed as its first holder (U 1/9 1977:10).
151. ED 18/7 1942:2 and ED 23/5 1947:7.
152. Mimeographed lists of ongoing research, archives of Department of Zoology, Uppsala.
153. Interview with BK 23/11 1981.
154. Dybern (1970) focused on experimental verifications of the tolerance of sea-urchins to environmental factors, the influence of light on their biotope choice, and on their life cycle in order to solve distributional problems, hence partly reviving Arne Lindroth's experimental-ecological approach 30 years earlier.

In this respect the two professors in zoology were supported by their colleague in zoophysiology, Per Eric Lindahl, who expressed a critical attitude towards the dominance of faunistics in zoology courses (cf.4-4). Lindahl was upset by naturalist students calling themselves ecologists. What they really did, in his mind, »was animal geography, called ecology«.[155] Thus, descriptive field work on animals was not only discouraged, but even opposed. As late as the early 1960s the first course in ecology had to apply for grants from a private foundation in order to buy utensils for field work.[156]

With the appointment of Karl-Georg Nyholm as successor to Hörstadius on the chair of general zoology in 1964 the situation with respect to field work was improved.[157] Although hardly stimulating ecology or ecological education actively (»You can't teach ecology, can you?«, he is said to have uttered),[158] he had nevertheless expressed an early attention to biological problems in the 1940s, and hence did not discourage field work on marine animals. A number of field investigations on marine animals were designated as ecological, and two graduates began work on problems of marine animal ecology in the late 1960s.[159]

Although field work on marine animals was introduced »under great resistance from the morphologists« descriptive field work on freshwater organims made an easier breakthrough when

> *»the students began to demand ecological research projects, for example those who had taken limnology as their last subject«.*[160]

We have already pointed to the fact that Rodhe's lectures in limnology were a stimulus for ecological thinking among Uppsala zoology students in the 1950s and 1960s; some of them even co-operated with Rodhe. But even more important for the ecologization of the activities of naturalists and field zoologists in Uppsala was the fact that Sven Ekman was still active as an emeritus professor. After his retirement in 1941 and the completion of his great treatise on marine animal geography he focused on freshwater organisms, working continuously throughout the 1950s. Ekman was an old man, and he could not supervise the rapidly growing number of undergraduates, but he was in a sense a living legend for the new generation. His only graduate student, Birger Pejler (b. 1924), passed on the heritage. Pejler's correlations between lake environmental factors and the distribution of planktonic rotatoria was a most accurate and modernized version of Ekman's combined taxonomic and animal geographic programme.[161] He did not, however, take up Ekman's plea for a causal »existence ecology«.

A number of dissertations on freshwater organisms and their environments were submitted in the 1960s and early 1970s; few, though, were actually translated into the language of ecology. One reason for the slow ecologization of the study of freshwater animals in Uppsala was certainly the lack of a charismatic ecologist who could gather

155. Interview with PEL 12/8 1982.
156. Mimeo in the archives of Department of Zoology, Uppsala.
157. ED 29/5 1964:1.
158. As recalled by an Uppsala zoologist.
159. Ingemar Ohlsson's ecological studies of the foraminiferal fauna in Swedish estuaries of 1974, which mainly remained within the scope of the problem of animal geography, however, and Svante Eriksson's ecological studies on zooplankton, which was cast in the language of the ecosystem, of 1972.
160. Interview with NN 5/1 1977.
161. Pejler 1957; his dissertation was in turn replicated by a student, Heikki Amrén, who made a similar study of rotifers from Spitsbergen (Amrén 1964).

together a group to act as the forcing ground for the translation of the naturalist interest into an ecological rhetoric. Although Pejler was the prime mover behind the first course in »ecology and ethology« established in 1962/63[162] and an assiduous propagandist for a basically ecological view, he was hardly an entrepreneur like, for example, Bengt-Owe Jansson in Stockholm. Nor did any of his junior colleagues in turn express the kind of revelational attitude to ecology found, for example, among Stockholm zoology graduates in the 1960s. Instead, most of their work was translated into environmental problems but not specifically into ecology – from Grimås, who, with water authority backing, investigated the effects of impoundments on the bottom fauna of high mountain lakes around 1960,[163] to the dissertations on freshwater organisms in the early 1970s focusing also on environmental problems, such as the effects of impoundments and problems of water pollution.[164]

The most ardent staff naturalist in Uppsala was Bertil Kullenberg (cf.3-1), who had been appointed associate professor in entomology in 1948. He was an outstanding field worker, and very popular among the students. Besides Rodhe at the nearby limnological laboratory and Du Rietz further down the road, Kullenberg was one of the main advocates of organized field excursions in Uppsala in the 1950s. Kullenberg's main achievements were still painstaking descriptive studies, now of insect-flower relations on the island of Öland in southern Sweden. But throughout the 1950s he also took up the experimental approach to field studies. Together with a biochemist[165] he developed an innovative research programme for studying the chemical communication between insects and flowers. After a couple of years they felt the need for a field laboratory and succeeded in enlisting local university people and a private research foundation.[166] In 1963 the field station at Ölands Skogsby was opened and equipped with modern analytical chemical instrumentation.[167] This was the first field laboratory in the country devoted entirely to studies of terrestrial animals, a modern counterpart to Henrik Lundegårdh's field laboratory for plant ecological research at Hallands Väderö established half a century earlier (cf.2-3).

Kullenberg did not claim these studies as ecological other than in passing, however (e.g., the Ölands Skogsby station was officially named the »Uppsala university ecological station«). Nor did he take part in the national ecological discourse or the build-up of a national ecology policy in the late 1960s. Despite being a leading scientific naturalist and a continuous source of naturalist inspiration in Uppsala, he did not consider himself an ecologist. Sometimes he referred to himself as an entomologist[168] and in an article from 1956 he made a claim for »bioclimatology«, which he considered to be a new research

162. Mimeo in the archives of Department of Zoology, Uppsala.
163. Grimås 1965.
164. E.g., Per Aass' studies, of 1973, of the effects of lake impoundments on fish hydroelectric reservoirs, Magnus Fürst's study of the biology of the opossum shrimp in impounded lakes of 1972, Göran Milbrink's investigations of freshwater worms as indicators of water pollution of 1972, Torgny Wiederholm's study of eutrophication in the large lakes of Sweden of 1974, and finally Gunnar Bergh's survey, of 1974, of the fauna and environmental conditions before a planned major pollution.
165. Einar Stenhagen, professor in medical biochemistry at Uppsala 1953-1959.
166. Kullenberg indicates that his choice of research topic was particularly appealing to foundations: »This fact that a flower could act as a female, I guess you can say it in that way, it fascinated people« (interview with BK 23/11 1981).
167. For a history of the station, see Kullenberg and Stenhagen 1973; for a review of the work done at the station, see the collection of articles in the journal *Zoon*, suppl. nr 1, 1973.
168. Kullenberg 1981.

field for the study of the relations between organisms and climatic factors.[169] Furthermore Kullenberg did not act as an ecological broker, as for example Brinck in Lund or Silén in Stockholm. Nor did his students explicitly claim ecology at first. Olof Tenow (b.1929) and Dag Gärdefors, both recruited by Kullenberg in the 1950s, wrote on the relations between temperature, climate and periodic outbreaks of Nordic mountain butterflies, and on experiments of orientation mechanisms in grasshoppers respectively[170] - but these works did not immediately give rise to any significant ecological discourse. It was not until the advent of the national *IBP* initiative in the late 1960s that they began casting their studies in a consistent language of ecology (cf.4-5).

In summary, an institutionalized discourse on animal ecology was not established among the Uppsala zoologists until the 1970s. By then it was a mixture of descriptive field ecology, derived from the activites of naturalist students, and experimental approaches. The final authorization of animal ecology in Uppsala did not take place until 1975-77 when, after Nyholm's retirement, the chair in general zoology was re-designated as a chair in »zoology, especially ecological zoology«.[171] Even then, Uppsala ecology was insignificant compared with Stockholm and Lund ecology. The animal ecologists from Uppsala were not ranked high by their colleagues in Stockholm or Lund, and they did not do well in professorial competitions. For example, when Pejler applied for the new chair in 1977 he was pushed into the background by the Lund fourth generation ecologist Staffan Ulfstrand, and two other applicants from Uppsala were ranked much lower.[172] Thus yet another Lund animal ecologist got the opportunity to reformulate animal ecology in Uppsala – the kind of population ecology which Ulfstrand had pursued in Lund was now transferred to Uppsala.

The identification of naturalist interests: the case of animal ecology in Göteborg

During the 1930s and 1940s animal ecologists showed little concern for practical problems. Doing ecology was an academic translation of a naturalist interest. The naturalists provided the manpower for the post-war development of animal ecology, and the identification of the naturalist interest was also the main factor behind the fast expansion of animal ecology in the 1960s and 1970s. This is beautifully illustrated by the establishment of a local social order of ecology in Göteborg. In connection with the build-up of the natural sciences in Göteborg chairs in zoology and zoophysiology were established in 1962 as tardy acknowledgements to the fin-de-siecle »new German« zoology.[173] The first professor in zoology, Karl-Georg Nyholm (b.1912)(cf. above)[174] initiated work on the systematics, biology and ecology of marine animals; accordingly the new zoology department in Göteborg might well have evolved into a center for marine ecological studies in Sweden. But Nyholm only stayed a year before moving to Uppsala,[175] and with

169. Kullenberg 1956.
170. Tenow 1972 and Gärdefors 1971.
171. U 14/8 1975:25.
172. U 1/9 1977:9; Kullenberg, a Norwegian and a Danish assessor ranked the applicants in the following order: 1) Ulfstrand (Lund), 2) Pejler (Uppsala), 3) Ackefors (Stockholm), 4) Milbrink (Uppsala), 5) Svante Eriksson (Uppsala).
173. ED 1961:I,41; ED 1962:I,33; ED 1963:I,31.
174. Nyholm was appointed in 1963 (ED 18/1 1963:31).
175. ED 29/5 1964:1.

his successor, Anders Enemar, appointed in late 1965, the direction of research changed completely.

Enemar (b.1926) was appointed mainly on his anatomical qualifications[176] - having written his dissertation for Hanström in Lund, he had been one of the pioneers in Sweden in the application of the electron microscope to anatomical studies. But Enemar had also been one of the Lund naturalists who translated bird-watching into population ecology, and when coming to Göteborg he turned his spare-time bird-studies into the dominating professional activity. He certainly did not have to coerce his students. A majority of them apparently considered ecology much more important than any other part of the zoology curriculum, »there was an enormous demand for ecology«[177] – the proportion of graduate students doing ecological research in the late 1960s and early 1970s was between two-thirds and three-quarters of the total.[178]

Hence, after the first Sturm-und-Drang years an ecological discourse was fairly well-established in Göteborg by the early 1970s, and compared to other zoology departments in the country the proportion of graduates working on bird population ecology was high. For the first time the bird-watchers were able to translate their interest into an accepted academic activity – »ornithological population ecology«, as they put it.[179] Among the first dissertations to be submitted in the mid-1970s, one was an advanced study of bird population ecology.[180] Within just a decade, the Göteborg field animal ecologists had come to be looked upon with respect by their contemporaries. The new discourse was not authorized in terms of chairs, however.

4.4 The authorization of the social order of ecology at the national level

Institutionalization of the Swedish ecological discourse had been negligible before the late 1940s. In fact, there had not even existed a Swedish ecology in the strict sense of the word, that is, a national social order of ecology, but only a series of local social orders, that owed their existence to local entrepreneurship and the ability of individual actors to enrol local students and academics at the department and faculty levels. The foundation of the journal *Oikos* and *Svenska föreningen Oikos* in 1948/49 stands out as the first permanent element in the authorization of ecology at the national level.

However, throughout the 1950s *Oikos* remained the solitary national institution of ecology. With the exception of Tamm's chair in forest ecology (cf.4-2), the rapidly

176. ED 17/12 1965:42.
177. Interview with NN 28/9 1976.
178. According to mimeographed lists of ongoing research, Department of Zoology, Göteborg; during the first years they seemingly came mainly in search of an academic outlet for their naturalist enthusiasm, but later another argument for choosing ecology began to appear, namely that doing an ecological thesis might be a qualification for obtaining a job in the environmental sector (cf.4-4).
179. Enemar 1966; a number of Lund and Göteborg zoologists and spare-time bird-watchers went to Ammarnäs in the province of Lapland every summer in order to continue their bird-watching interest.
180. M.Andersson 1975.

expanding local social orders of ecology were not authorized. The journal *Oikos* was an important publication outlet, but neither the journal nor *Svenska föreningen Oikos* played any significant role in the further institutionalization of ecology. Furthermore, none of the national ecology policy actors of the late 1940s contributed significantly to the further post-war institutionalization of ecology. The prime mover behind proposals for ecology made by *1945 års universitetsberedning*, Wilhelm Rodhe, although a participant in the first ecology policy group in the 1960s (see below), largely withdrew to his newly founded limnological laboratory. Similarly Carl H. Lindroth devoted his professional energies entirely to his own research interests after having been appointed to the Lund chair in entomology in Lund in 1951.

Nor did any of the powerful national scientific agencies take initiatives to authorize ecology during the 1950s. *Vetenskapsakademien* did not formulate any ecology policy, nor did any of the state commissions on higher education and research. In principle, *Naturvetenskapliga forskningsrådet* (*NFR*), as the leading national science agency, could have supported the growth of local ecologies and authorized a national social order of ecology. For a decade and a half, from its establishment in 1947, a few ecologists received occasional financial support, not in terms of ecology, however, but in terms of their contribution to botany and zoology. *NFR* acknowledged the existence of ecology only indirectly, as one of the minor specialties of botany and zoology, and no specific ecology policy was formulated. For example, in a nation-wide enquete distributed and collected by Council in 1956, field stations were considered one of the most urgent needs for botany and zoology, but ecology was only mentioned in passing.[181] As late as 1963, the general secretary of the Council made only the briefest mention of ecology in a review of contemporary developments in Swedish natural science.[182]

However, within a few years, ecology had become a permanent and prominent feature of national science and several expanding local social orders of ecology had been authorized. How did this come about?

The accepted wisdom within the short tradition of history of ecology is that the spectacular rise of ecology in the 1960s was a consequence of the external demand for solutions to the environmental crisis.[183] But this thesis does not differentiate between the rise of social orders and their authorization. Given that it usually takes almost a decade to complete a graduate research project (i.e., from the first vague ideas until the completion of dissertation), it is evident that the wave of claims for ecology during the 1960s was by no means a consequence of environmental problems or the environmental debate, which did not emerge until the early 1960s. As pointed out in the preceding sections, the naturalist mass movement and the rise, locally, of enthusiastic fourth-generation ecology actors were the prime conditions for the expansion of local social orders of ecology.

On the other hand, although these naturalists provided the preconditions for the rise of local social orders of ecology, they did not provide the impetus for the authorization of

181. Cf. Fagerlind 1956 and Hörstadius 1957.
182. In his review of Swedish botany, Funcke briefly defined ecology and added: »Undoubtedly ecology is a particularly interesting and important branch of science« valuable not only for basic research but also for applications in agriculture, horticulture and forestry (Funcke 1963).
183. See, e.g., the Introduction to Kormondy and McCormick (eds) 1981.

ecology throughout academia. Just as the first steps towards the authorization of ecology in the late 1940s (i.e., *Oikos*) had been made possible by translating the interests of different state authorities into the language of ecology, so the rapid authorization of ecology and the emergence of a Swedish ecology during the 1960s was also made possible by translating various societal and government concerns into the language of ecology. Without doubt, the wide-spread concern for environmental degradation constituted the main background interest for the national unification and permanent institutionalization of the local social orders of ecology during the 1960s.

In this section we will detail a number of significant attempts during the 1960s to formulate ecological policies and to translate the interests of powerful state agencies into the language of ecology. First we will look at the ecologization of environmental problems, and the emergence of the first permanent national policy group for ecology, and then at the attempts to ecologize research and higher education.

Translating conservation interests into ecology

The rapid spread of societal concern for environmental problems during the 1960s is reflected in the growing literature on the subject (Figure 4-11).

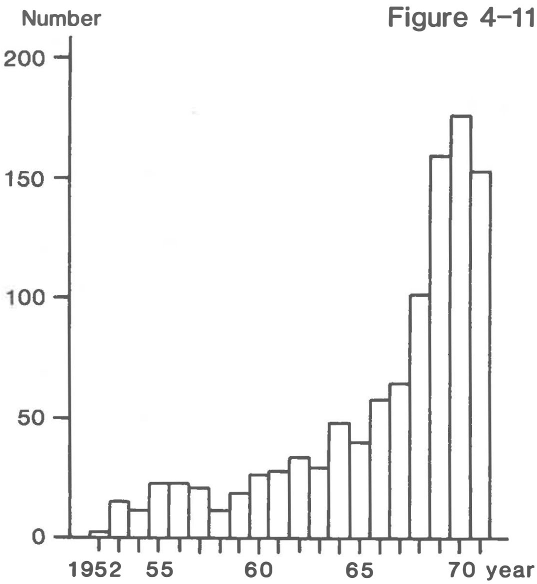

Fig. 4-11: Number of works on environmental problems printed in Sweden 1952-1971 (including reprints and new editions). Adopted from Dahlström-Ekbohm 1975, p.109,fig.1.

The burgeoning concern for environmental problems is also reflected in the number of *Riksdag* bills and their promoters. Some 12 bills on nature conservation issues were introduced by 59 members of the *Riksdag* during the eight years 1953 to 1960. By comparison, during the following four years, 1961 to 1964, as many as 49 bills from 243 members were introduced.[184] That is, the emergence of marked political concern for environmental issues can be dated fairly precisely to a few years around 1960. This environmental concern seems to have been founded in two quite different basic interests: on the one hand, the nature conservation interests, represented by *Svenska naturskyddsföreningen* and *KVA:s naturskyddskommitte;* and on the other hand, people concerned about the nation's productive resources.

Nature conservation had of course been a question for concerned scientists and informed citizens since the turn of the century (cf. 3-1), and the plant biologists in Uppsala and the limnologists in Lund had taken up such questions as the environmental effects of dam construction and water pollution. After the war the impact of lake impoundments became a particularly topical issue. The new wave of industrialization, and the electrification of all aspects of life, was facilitated by harnessing the energy of many of the waterfalls of the northern provinces. Nature conservationists were for a long while ineffectual against the power industry, including the state owned hydroelectric authorities.[185] As balm to the wounds, the water courts adjudged an amount of money to compile inventories of the fauna and flora of the areas to be inundated; the plant biologists in Uppsala had already taken on these tasks in the 1940s, and during the 1950s onwards quite a few scientists were engaged in floristic and faunistic investigations in the northern provinces.[186] A few even worked up their inventories into doctoral dissertations, even though none of them translated this work into the language of ecology.[187]

Another major conservationist topic was, of course, pollution, and more specifically poisoning. Water pollution was an old issue – *Fisketillsynsmyndigheten* of 1937 (permanent from 1942, cf.3-4) embodied a long-standing concern for the effects of industrial discharges on freshwater fishery. A new state authority, *Vatteninspektionen* (the Water Inspectorate) was established in 1958.[188] Scientists working on water pollution problems usually also did not translate their findings into ecology. Hence, nature conservation investigations on the whole did not give rise to claims for ecology as such.

On the other hand, nature conservation problems were translated to ecological problems in a science policy context. Conservationist concern was focussed and formulated by *1960 års naturvårdsutredning* (the Nature Protection Commission of 1960),[189] set up by people engaged in *Svenska naturskyddsföreningen* to review the results of earlier nature conservation legislation. It proposed the creation of a central state authority to coordinate conservation efforts, and as a result *Statens naturvårdsnämnd* (the National

184. L.Lundqvist 1971,tables 3.1 (p.46) and 3.2 (p.59).
185. The conflict between nature conservationists and the power industry was negotiated for the first time in the so called »Sarek Peace« (Sw. »Freden i Sarek«) in the early 1960s; the turning point was a *Riksdag* decision in the early 1970s to protect four great rivers of the Norrland region.
186. A number of these appeared in a series of publications (*Kungl. Svenska vetenskapsakademiens avhandlingar i naturskyddsärenden*) from *KVA's naturskyddskommittée*.
187. See, e.g., Wassén's and Grimås' dissertations (cf.4-2 and 4-3).
188. For details on the emergence of the water administration, see L.Lundqvist 1971,pp.30-38,67-89.
189. SOU 1962:36.

Board for Nature Conservation) was established in 1963 (in 1967, with an extended remit, this was renamed *Statens Naturvårdsverk*, *SNV*, the National Board for Environmental Protection).[190] Thus, nature conservation eventually gained its counterpart to the earlier *Domänverket*, *Lantbruksstyrelsen* and *Fiskeristyrelsen*. Leading members of *Svenska naturskyddsföreningen* were among the prime movers behind the Commission and what ensued, whereas *KVA:s naturskyddskommittee* was left behind.

Ecology was by no means a major issue of the Commission's work, but it was taken up in considerations of the establishment of a research organization for environmental protection. Emphasizing that practical nature conservation should be established upon a »sound scientific basis«, the Commission, using Hugo Sjörs (cf.4-2) as its expert, proposed an autonomous conservation research organization:

> *»it is necessary to create an organization for long-term cooperation between a number of specialists«,*

they said, to tackle conservation problems »in a planned and sustained manner«.[191] Distinguishing between the need for fundamental research and applied research, Sjörs and the commission emphasized that a

> *»strengthened general ecological and biogeographical research«*

should be consistently pursued at the universities, while there was also an urgent need for

> *»a special, of goal-directed nature conservation research«.*[192]

Without going into detail about how this might be organized,[193] the Commission suggested that the question of applied conservation research should be investigated further.

A more specific case of translation of conservation issues to the language of ecology is provided by an issue which rapidly emerged as a controversial topic in the early 1960s, viz., the question of environmental poisoning (the pesticide – or »biocide« – problem). It was partly triggered by the Swedish translation of Rachel Carson's book *Silent Spring* in 1963, and partly by alarming reports from bird-watchers.[194] Between 1962 and 1967 the problem of mercury poisoning was at the center of environmental debate in Sweden.[195] The first to take up research on the issue was Professor Alf Johnels at *Naturhistoriska riksmuseet*. Although trained as a comparative anatomist in Stockholm, Johnels had a strong faunistic interest, and in response to the alarming reports of widespread bird deaths, he initiated a co-operative research program with an inorganic chemist. Over the following years they, and a number of assistants, published a long series of reports.[196] The point is that several of these were translated into the language of ecology; as early as

190. The *Riksdag* decided in 1965 to fuse five earlier authorities, among them *Vatteninspektionen*, into *Statens naturvårdsverk* as a central authority for the whole area of nature conservation. For a discussion of the prehistory of *SNV* and its policy, see L.Lundqvist 1971.
191. SOU 1962:36.
192. Ibid.
193. The Commission pointed in passing to the British and American organizations as models.
194. The Swedish debate which was rapidly taken up in Brohult *et al* 1963, also owed much to the alarming reports from the grand old man of Swedish amateur ornithology, Erik Rosenberg, who had inspired generations of young bird-watchers (see, e.g., E.Rosenberg 1963).
195. See, e.g., the bibliography on environmental poisons in Dahlström-Ekbohm 1975,pp.37-42.
196. They published approx. 75 reports up to 1975 (list of publications published by the Department of Vertebrate Zoology, *Riksmuseum*, 25/1 1976).

1966 Johnels wrote an introduction to »ecological biocide research«.[197]

Translating nature resource interests into an explicit ecological research policy

Sjörs and the »conservationists«, although mentioning ecology as a potential contribution to the solution of environmental question, did not put forward ecology as *the* foundation for a future environmental policy. Somewhat later, however, others proposed ecology to be the major basis for the rational management of the nation's natural resources.

The event that triggered concern for rational resource management was an appeal by Gottfrid Stålfelt (cf.2-3 and 3-3) in 1956. Referring to the accelerating and irreversible exhaustion of natural resources, Stålfelt urged *NFR* to take up the problem, and as a consequence the Council set up a natural resource committee in 1957.[198] It was emphasized that the Committee's concern was not that of nature protection but of »utilizing our natural capital in the best possible way«.[199] As examples

> *»the substitution of coniferous forest for deciduous forest, the exploitation of running waters, and the use of chemical and biological means of plant extermination«*

were mentioned.[200]

Stålfelt tried to enrol the Council to support his own ideas. To begin with, though, his initiative met with little success. Seemingly, *NFR* did not take the question very seriously, and the Committee languished for several years. In fact, the Council paid more attention to the committee , set up at the same time, for field stations (cf.4-1). The reactivation of the Committee, as *Naturresurskommittéen* (the Natural Resource Committee) in 1961 coincided with the appointment of Per Brinck (cf.4-3) as a member of the Council. From the beginning of his academic career, Brinck had kept a keen interest in practical zoology; it will be we recalled, that his research program for the new Department of Animal Ecology in Lund included many practically oriented research projects.

Brinck, though, was by no means the only ecology actor in the renewed nature resource committee. Its leading members[201] had all more or less actively claimed ecology as an independent science. Consequently the Committee came to act as the germ of a national social order for ecology. At one of the first meetings, in November 1961, the Committee's field of action was formulated as:

> *»Biological-ecological relations with man in the center«,*

specifically addressed to questions concerning water pollution (incl. limnology), air

197. Johnels 1966. See also Odsjö and Olsson 1971. Of course, not all concerned about the pesticide issue translated it into the language of ecology. For example, a group of Stockholm scientist invited to a cross-disciplinary seminar on the pesticide problem in 1965 emphasized that »biocide research touches upon many different branches of science« without mentioning any branch specifically. In fact none of them was an ecologist (in »Inbjudan till ett tvärvetenskapligt seminarium om biocidproblem«, mimeo in the archives of the Department of Zoology, Stockholm).
198. Besides Stålfelt, Sven Hörstadius, J.A.Nannfeldt, Frans Wickman and Gösta W.Funcke (general secretary of the Council) constituted the Committee.
199. Minutes of *NFR*-meeting 24/4 1957, item 537 (in the archives of *NFR*).
200. Minutes of *NFR*-meeting 14/5 and 19/5 1958, item 677.
201. In addition to its original members and Brinck, a few new members were appointed, including Wilhelm Rodhe, Carl Malmström and Bengt Lundholm. Lundholm would come to play a major role in the administration of national ecological policy during the whole 1960s and early 1970s.

pollution, forest ecology (extermination of deciduous forests, effects of grazing), agricultural ecology (pesticides) and soil research (soil degradation).[202] Already in 1962 the committee discussed

> *»the extension of an organization for future and more far-reaching ecological investigations«.*[203]

At their first meeting a proposal was outlined for a biological survey of Sweden as a counterpart to *Sveriges geologiska undersökning.*[204] Although this proposal came to nothing *Naturresurskommittéen* evidently had great pretensions, both for the future of ecology and the future of the nation.

Naturresurskommittéen quickly grew up into a mini-council within *NFR*. Of course, its members used the access to financial resources to expand their own local departments. For example, Wilhelm Rodhe from the start pushed the question of a limnological survey, and in 1964 *Mälarundersökningen* (cf.4-2) was instituted – a large scale investigation which rapidly enlisted many younger zoology, botany and limnology students, and quickly grew to become one of the largest research projects supported by *NFR* till then.[205] On the whole, however, the Committee acted as a broker between the Council and local university departments and individual scientists, by surveying the supply of younger scientists who might be willing to pursue ecological environmental research. Hence a number of research projects were negotiated. The funds administered by the committee grew quickly[206]:

1961/62	– – –
1962/63	43.635 SEK
1963/64	219.825 SEK
1964/65	1.121.260 SEK
1965/66	1.370.120 SEK

It is worth noting, that in contrast with Sjörs and *1960 års naturvårdsutredning*, *Naturresurskommittéen* did not make any clear distinction between fundamental and applied research. They considered ecology the scientific basis for the rational management of nature, just as, forty or fifty years earlier, Henrik Hesselman or Einar Naumann had considered botany or limnology the scientific basis for rational forestry or rational fishery management. Hence, the Committee was a pioneer in the cause of ecologizing, not only academia, but the larger society as well. The wise management of Swedish natural resources was to be founded upon the new science of ecology!

The ecological argument for a national board for environmental protection

To proclaim ecology as the scientific basis for the rational management of nature was emphasized even more in 1963 when the leading members of *Naturresurskommittéen* took

202. Minutes of meeting 27/11 1961 (in the archives of *NFR*).
203. Minutes of meeting 13/7 1962.
204. »Utkast till ett förslag om en svensk produktionsbiologisk undersökning« (mimeo by Bengt Lundholm in the archives of *NFR*.
205. For a presentation of *Mälarundersökningen*, see, e.g., Ahl and Willén 1965.
206. Activity report of *Naturresurskommittéen* up to and including 1965/66 (in the archives of *NFR*).

up the question, first raised by 1960 års *naturvårdsutredning* of a future environmental research organization. On behalf of the governmental research advisory board the secretary of the Committee, Bengt Lundholm (b.1916), drew up a memorandum advocating »central planning of natural resource research«.[207] Although such research was thought to be the responsibility of local university departments it was nevertheless proposed that the focus and leadership of an active research secretariat was needed in order to

> *»coordinate and intensify already existing research... initiate research in neglected areas... continually follow up promising research initiatives and be prepared to modify ongoing work«*

and even, if necessary, to directly manage research work.[208]

A year later Lundholm was appointed acting secretary for a new governmental commission, *1964 års naturresursutredning* (the Nature Resource Commission of 1964). As a consequence *Naturresurskommittéen* and the new governmental commission came to act as twin bodies with a common secretariat under Bengt Lundholm, who rapidly became a most influential ecology policy actor, with the backing of Per Brinck and others. Thus Stålfelt's, and later Brinck's, policy for ecologizing natural resource management was eventually to be realized.

1964 års naturresursutredning was a milestone with regard to the ecologization of conservation and natural resource problems. *1946 års naturskyddsutredning* (the Nature Conservation Commission of 1946) had restricted itself to the establishment of national parks, etc., and had not mentioned ecology. *1960 års naturvårdsutredning* had had a much broader scope, and had mentioned the need for background ecological research. Finally, the scope of the 1964 commission was extended to the increasing deterioration of the global environment; taking

> *»the interplay between man and the natural environment as a point of departure for demarcating the research area«,*[209]

it considered ecology to be a fundamental tool for solving these problems. Formulating the expression »miljövårdsforskning« (Sw. »environmental research«), the Commission emphasized that:

> *»environmental research will essentially be founded in... ecologically oriented biology«.*[210]

Elsewhere they stated quite clearly that

> *»ecological research oriented towards natural science... shall constitute the foundation for the nature conservation policy«.*[211]

The Commission's work eventually resulted in a permanent state grant earmarked for »environmental research«, which rose quickly from 7,5 million SEK in 1968/69 to 29 million SEK in 1975. The grant was to be administered by a special research committee, attached to, but somewhat independent from *Statens naturvårdsverk, SNV*. In addition special scientific subcommittees would guarantee the scientific reliability of the research projects supported by the committee. The research would be executed by local university departments, governmental agencies, etc. Hence *SNV's forskningsnämnd* (the Research

207. Naturresursforskning 1963.
208. Ibid.,p.11.
209. SOU 1967:44,p.16.
210. Ibid.,p.17.
211. Ibid.,p.147.

Committee of the National Board for Environmental Protection) came to function as a sectorial research council.[212)]

It is certainly justified to emphasize the central role of »the idea of ecology« in the twined work of *NFR*'s *naturresurskommittée* and *1964 års naturresursutredning*, as has been done by Lundqvist.[213)] The Commission's report was actually replete with concrete examples of ecological research, and subsequently a number of third generation ecologists were appointed to the scientific subcommittees of *SNV's forskningsnämnd*. On a number of occasions *SNV's forskningsnämnd* deliberately backed ecological research; the clearest example is the establishment of Grimsö game research station in 1970/71, as »an investigation area for ecological research«.[214)] In that sense environmental research was ecologized throughout the 1960s.

The establishment of *SNV's forskningsnämnd* also had positive consequences for the promotion of ecology as a social order at the local university level. The large new national funds contrasted with the minimal funds previously available for field studies at the universities. And much of this new money was in practice »ear-marked« for those in a position to translate environmental investigations into scientific problems cast in the language of ecology. From now on, a man proclaiming »I'm an ecologist« had better opportunities when applying for environmental research money, than one proclaiming »I'm a botanist«, or, »I'm a zoologist«. In terms of the theoretical approach taken here, the ecologists translated environmental interests, and hence enrolled environmental authorities, into their rhetorical version of the world – that of ecology.

On the other hand, the extent of ecologization and the impact of »the idea of ecology« on environmental research should not be exaggerated. From the start *SNV's forskningsnämnd* had attempted to demarcate its research area vis-à-vis ecology. For example, at a meeting in April 1969 representatives of *SNV's forskningsnämnd* and the successor to *NFR's naturresurskommittée* (*Ekologikommittéen*, cf. below) discussed »the delimitation« between their respective working areas. *SNV's forskningsnämnd* and its secretariat unambiguously defined its research area as »environmental research«, not as ecology,[215)] and at several occasions it explicitly questioned whether it should support ecological research per se. For example, an internal conference in 1970, noting that many research projects involving game were designed simply to further pure ecology (or ethology), proposed that:

> »/SNV's/ *forskningsnämnd ought to discuss if this direction is an appropriate one to support«.*[216)]

Likewise *SNV's forskningsnämnd's* rapidly expanding financial support to university departments engaged in ecological research was equivocal. Several interviewees have pointed to the fact that many scientists systematically translated their original ecological research problems into the language of »environmental research« in order to secure financial support. Several local departments were split between research directed towards

212. The whole organization was revised in the late 1970s, attaching *SNV's forskningsnämnd* directly under *SNV*.
213. L.Lundqvist 1971,p.104.
214. Minutes of meeting 5/6 1969, item 163 (in the archives of *SNV*); the proposal was made by professor Alf Johnels at *Riksmuseum* (cf. above).
215. See e.g. Miljövårdsforskning under tio år 1977; and numerous memoranda and annual reports from *SNV*.
216. Minutes of 12/11 1970, appendix 2.

basic ecological problems, and investigations formulated by *SNV's forskningsnämnd*.[217] Thus, the authorization of the claim for an ecologically oriented environmental research had contradictory consequences for the growing national social order of ecology.

The establishment of a national ecology research committee

We have already noted that the proliferation of claims for ecology in the 1950s and early 1960s found no appreciable support in *Naturvetenskapliga forskningsrådet, NFR*. During the decade 1965-1975, however, *NFR* adopted a very active ecological policy, and by the mid-1970s up to 15% of its total funds for natural science were allocated under the heading ecology (cf. Figure 4-1).

Ecology was first seriously put on the Council's agenda during the spring of 1967. There were several reasons for this. Firstly, a few members of the council were associated with the rapidly growing local social orders of ecology, viz., Per Brinck, Erik Dahl, and to some extent Hemming Virgin, Stålfelt's former student who had retained an understanding of ecology in spite of his own physiological research orientation. Further, the ecological tide at university departments all around the country could not be ignored in the long run.

The immediate reason for putting ecology on the Council's agenda, however, seems to have been the proposal to set up *SNV's forskningsnämnd*. As its support to environmental research was directed towards the solution of practical problems, *NFR* found it necessary to

> *»bring about a forceful strengthening in this area* /i.e. ecology/, *which is of fundamental importance for understanding the functioning of the biological environment«*.[218]

That is, the Council's support for ecology was thought to be an auxiliary to the environmental research programme. Ecology, like molecular biology and microbiology, was considered a neglected research area, worth supporting because of its potential contribution to the solution of important social problems.[219] Otherwise the Council usually gave support with reference only to the inherent scientific value of research.[220]

The Council decided to create a special committee – *Ekologikommittéen* (the Ecology Committee) with Per Brinck as its chairman. The mere composition of the committee, in all twelve persons hand-picked by Brinck, representative of the diverse range of specialists comprising the environmental movement's scientific elite,[221] reinforced the impression of a goal-directed research policy rather than one for basic ecology. Furthermore, there was considerable continuity with its precursor, *Naturresurskommittéen*. Accordingly, at its first meetings in the late autumn of 1967 a number of socially relevant problems were discussed, including the water-logging problem in forestry, the problem of forest fertili-

217. According to several interviewees.
218. *NFR's* financial request (»petita«) for the fiscal year 1968/69, spring 1967,p.9.
219. Appendix to ibid.,p.3.
220. Simultaneously immunology and ethology were supported with personal chairs and boosted by the research qualifications of the scientists in question.
221. The members were: Per Brinck (chairman), Ingemar Ahlén, Björn Berglund, Erik Eriksson, Holmar Holmen, Bengt Lundholm, Svante Odén, Göran Odham, Hans Palmstierna, Bengt Pettersson, Åke Sundborg, Torbjörn Willén and Hemming Virgin.

zation, counter-measures against acidification, methods for water purification, the dispersal of intestinal parasites, etc., that is,proposals reflecting its members' orientation towards the solution of various environmental problems. One member even suggested that the Committee should

> *»contribute to the establishment of goal-directed research groups at existing departments for free basic research«.*[222]

While Brinck as chairman had great influence upon the general lines of policy, it was its secretary, Bengt Lundholm, who became the prime mover behind its vigorous and very successful organizational activities. A large number of ad hoc working groups, symposia, etc., related to socially relevant problems, particularly environmental problems, were set up[223] involving large numbers of scientists in a variety of university departments, governmental agencies, etc. In this way *Ekologikommittéen* rapidly came to contribute substantially to the commencing ecologization of Sweden (cf.4-5). While *SNV's forskningsnämnd* had begun to define such problems as »environmental research«, *Ekologikommittéen* consistently defined them as – »ecology«.

A local ecology pressure group

The creation of an ecological research body, *Ekologiska forskarkollegiet*, in Stockholm in 1967/68 gives additional support to the thesis that the authorization of the social order of ecology involved the scientists' identification of the environmental problem and its translation into the rhetoric of ecology.

The research body was initiated by Professor Lars Silén, the mentor of the fourth generation of animal ecologists in Stockholm. It represented all departments working on animal field studies in the Stockholm area, and was originally proposed as a governmental body for promoting ecological research in the area. The chairman of the founding meeting probably summarized the general opinion when concluding that

> *»the body has a very important aim in promoting Stockholm ecology which otherwise runs the risk of disappearing«.*[224]

A very concrete aim was, of course, to work for the long desired chair in plant ecology at the university proposed by Stålfelt already in the 1940s. Besides of this, however, the scope of the body was quite similar to the *NFR* nature resource and ecology committees. Both thought in terms of a unified pure and applied ecology, their explicit common background was the environmental problem and the new opportunities for state research funding in its wake.

It is worth noting, however, that *Ekologikommittéen* was not mentioned in the minutes of the founding meeting of the Stockholm ecology research body – from the beginning the Stockholm ecologists turned their attention to *SNV's forskningsnämnd* as a potential source of funds.[225] The reason might be that *Ekologikommittéen* was largely an instrument of Per Brinck's ecological policy ambitions. Brinck's leading position within

222. Minutes of meeting nr 2, item 23 (in the archives of *NFR*).
223. At least twelwe ad-hoc groups and six symposia were organized during the first three years, giving evidence of the vigorous activities of the Committee.
224. Minutes of meeting nr 1, 30/1 1968 (mimeo in the archives of Department of Zoology, Stockholm).
225. Ibid.

the emerging national institutionalized social order of ecology was not always seen positively by Stockholm and Uppsala ecologists, partly because of Brinck's unscrupulous methods as a broker,[226] but also reflecting an ancient rivalry between the three universities. Thus, the creation of the ecology research body in Stockholm might also be interpreted as a reaction towards Lund dominance in national ecological affairs.

After a short period of activity, however, *Ekologiska forskarkollegiet* in Stockholm faded away around 1970. The successful activities of *Ekologikommittéen* and the rapid authorization of several local social orders of ecology in the country by 1970 (cf. below) had obviously made its existence superfluous. No other regional ecology pressure groups were founded. As we shall see below (cf. also 4-5) subsequent policy initiatives with regard to a national ecological policy were almost without exception taken by *Ekologikommittéen*.

Ecology and post-war university reforms

In the preceding pages we have discussed the commencing authorization of the social order of ecology at the national level in terms of the identification of ecology actors with environmental interests and their translation of environmental issues into the language of ecology. In the the following paragraphs we will take up a related theme, viz., ecologization in terms of identification and translation of a reforming interest, specifically the post-war university reforms.

The formidable growth of ecology during the post-war period coincided with the general expansion of the universities, which in turn coincided with the post-war expansion of state expenditure. This, of course, was a universal Western phenomenon. Sweden, however, was doubtless one of the leading nations with regard to the growth of public administration, and the information and service sectors. In the 1960s alone, the number of state officials increased from 14% to 25% of the work force. This trend was paralleled by increased state involvement in social and economic planning.

The series of planning commissions for higher education and research during the post-war period was a strategically important part of this process towards a programmed society. *1945 års universitetsberedning* (cf.3-5), basing its proposals largely on the wishes of the faculties, was modest compared to the great reforms of the 1960s and 1970s. In retrospect the subsequent *1955 års universitetsutredning* (the University Commission of 1955)[227] has been considered as marking »a break-through for planned development«.[228] The 1955 commission did not directly affect the cognitive content of university education. Curricula were still largely determined by the faculties, at least during the 1950s and 1960s. The commission's report had four major administrative consequences, however: first, the creation, in 1959, of an entirely new category of tenured positions, *universitetslektorat* (lecturers), being responsible for the growing undergraduate education but

226. Recall that they also considered him to be »the Godfather« of Swedish ecology; several interviewees remember that they thought some of Brinck's criticism of Uppsala animal and plant ecology to be harsh, even devastating.
227. In several volumes; for a summary, see SOU 1959:45.
228. Löwbeer 1978.

having no research obligations; second, the system whereby the number of teaching assistants increased automatically with increasing numbers of students (Sw. »universitets-automatiken«); third, the curtailment of the traditional power of the professors through the creation of departmental boards including representatives of other staff categories; and fourth the curtail of the autonomy of university faculties through the creation of an entirely new state central board – *Universitetskanslersämbetet* (*UKÄ*, the University Chancellor's Board) – and transferring the appointment of the national university chancellor from the faculties to the government.[229]

All in all, these administrative reforms around 1960 totally changed the power structure at the universities during the 1960s. The professors/faculties rapidly lost much of their local institutional power to the advantage of younger scientists, and perhaps more important in the long run, they lost much of their former control over national science policy to *UKÄ*. As a consequence, they lost much of their enrolment power as well, and cognitive and disciplinary changes became more and more influenced by other actors. Both the new teacher population and the new university state authorities came to act as reforming agents. The reform of the universities, including the natural sciences, became an urgent topic of political debate in the 1960s. While the great post-war expansion policy had focused on the quantitative buildup of the nation's scientific and technological potential, the qualitative questions pressed increasingly to the fore in the 1960s.

The reform efforts had consequences for the emergence of ecology as an institutionalized social order as well. In the preceding sections we have seen how the substitution of ecology for botany and zoology was accomplished by the new fourth generation of naturalists-ecologists at the local, departmental level at the universities. Below we will give a few examples of how the new university authorities, *UKÄ*, came to act as direct or indirect ecologizing agent.

The ecological chairs in Umeå

A decisive event in the authorization of a new scientific specialty, marking its more or less permanent elevation to the status of scientific discipline, is the creation of new chairs, specifically devoted to it. Although much ecological research and education was associated with the chairs in plant biology in Uppsala and Lund, and increasingly also with some of the zoological chairs, no university chair was ecological by name. As late as 1960 a committee responsible for the extension of the natural sciences in Gothenburg was still following in the footsteps of the, by now quite old, »new German« botany and zoology with its proposal for traditional chairs in botany and zoology.[230] The reform interests were weak (the *UKÄ* was in its infancy), and traditional zoology and botany was given free scope. Sven Hörstadius, professor of zoology at Uppsala and traditionally oriented towards embryological problems, headed the commission's expert group. His spare-time contributions to faunistics, and particularly amateur bird-watching,[231] apparently did not influence his university policy decisions (for example, nor did he actively support field

229. Until then, the national University Chancellor had been elected by the local faculties.
230. See Naturvetenskapliga samordningskommitteen i Göteborg (in ED 1961:I 41; 1962:I 33; 1963:I 31).
231. E.g., he was a skilled bird photographer, and was elected chairman of *Sveriges ornitologiska förening* 1947-1960.

work or ecology in his own department in Uppsala).

Botany and zoology were also among the subjects represented when the fifth and latest university in Sweden, in Umeå, was established in the 1960s. The four chairs created for animal and plant studies, however, were not designated as general botanical or zoological, but as chairs in »physiological« and »ecological« botany and zoology respectively. The planning commission responsible for the designation of the chairs in Umeå, argued for ecology as part of a strategy to strengthen modern experimental biology (viz. genetics, physiology, biochemistry and ecology) at the expense of a more traditional descriptive biology. They argued for raising the »educational value of biology« by stressing its »causal analytic side«. »Modern ecology«, they said,

> *»has a character of being a causal analytic and experimental science«.*[232]

In addition they stressed its »immediate importance for practical life«. Introducing ecological zoology, for example,

> *»could to a greater extent stimulate interest in the specific natural conditions of* /the region of/ *Norrland and lead to research activity in direct relation to problems of water management, fishery biology, game management, reindeer husbandry«.*[233]

Although the Commission's view on science and education were closely attuned to prevailing Social Democratic policy, the specific claim for ecology as a contribution to curricular reforms was not put forward by the political representatives on the Commission – in fact, they expressed no views on ecology. Instead, the proposal for ecology chairs was entirely the initiative of one of its scientific representatives – the professor of zoophysiology in Uppsala, Per Eric Lindahl, known as one of the first, albeit weak, claimants of experimental animal ecology in Sweden in the late 1920s (cf.2-5).

Surely, Lindahl was not appointed to the Commission because of his views on ecology but as a person standing for academic renewal and reform. It will be recalled that, when writing his undergraduate thesis with Runnström in the late 1920s, Lindahl already considered himself as »enormously directed to cause and effect« reasoning: he wanted a causal and experimental biology. Hence it is understandable that he suggested two physiologial chairs. But why two ecological chairs? After all Lindahl was extremely dismissive of work done formerly and currently in the name of ecology. But this was not a dismissal of ecology as such, only of its prevalent descriptive orientation. Indeed, he was of the opinion that most so-called ecologists did not work on ecological problems at all. For example, he objected to the fact that Ekman and his students called themselves ecologists. Likewise, his view was that, in Lund.

> *»they did not do any ecology there... it was a kind of animal geography«.*[234]

A true ecology, he believed, demanded a causal-analytic approach. Not surprisingly, he thought that Arne Lindroth's dissertation (cf.3-3) »had been terribly badly judged, unjustly judged«.[235] If the so-called ecologists were not inclined to initiate a causal-analytic ecology, he himself would take the responsibility. Therefore he told the Umeå commission:

232. SOU 1963:76,p.34.
233. Ibid.,p.34.
234. Interview with PEL 12/8 1982.
235. Ibid.

»*we have two things to pay regard to. First, the fact that the ecology that exists at different places in the country is... not stimulated at all, there being no resources, and* /second/ *that ecology, or the animal geography, which is ecology, needs remoulding... Therefore it should be an ecological position*«.[236]

Lindahl's proposal became the Commission's, and although it met some resistance from traditional systematicians and morphologists,[237] the four chairs, including chairs in ecological botany and ecological zoology were established in Umeå in 1966 and 1967 respectively. Although Lindahl had no influence on appointments to chairs,[238] his revival of the second generation of ecologists' claim for ecology as a causal-analytic and practically oriented science partly bore fruit since Arne Lindroth was appointed to the chair in ecological zoology in competition with the more descriptively oriented Birger Pejler (cf.4-3).[239] On the other hand, the chair in ecological botany was given to Bengt Pettersson, who had broken with the sociological and synecological direction of the Uppsala school, but who could hardly be considered either an experimentalist or an applied scientist (cf.3-2).[240]

Thus, ecology was authorized in terms of chairs at the new university in Umeå in 1966-67. Would ecologists have had a chance to get these new chairs, if they had been designated as botany or zoology (as in Göteborg a few years earlier)? Probably not! When Arne Lindroth and Birger Pejler applied for the chair in general zoology in Uppsala in 1964 both were ranked low in comparison with morphologists and experimental biologists.[241] Bengt Pettersson could not hold out hope of ever getting a chair, unless Sjörs or Malmer met an untimely death. It is reasonable to conclude, therefore, that without the political intervention and support of external reform agents, including *UKÄ*, the establishment of the social order of ecology at Umeå would have been a much more protracted affair.

The subsequent development of the two new ecology groups in Umeå did not, however, follow the reform policy. On the contrary, Umeå ecologists were even more narratively oriented than other ecologists in the country. The naturalistic orientation of recruited students was as prominent a feature in Umeå as elsewhere, and both departments were overwhelmed by students from the region of Norrland. For several years Lindroth and Pettersson, overburdened with administrative duties, had to give students a free hand, and as a consequence many continued their naturalist investigations, or translated their work into environmental research problems.[242]

236. Minutes of meetings in the Umeå commission (in *Riksarkivet*).
237. See, e.g., Holm 1964.
238. The applicants were assessed by third generation ecologists: Per Brinck was the Swedish member of the group assessing the applicants to the chair in ecological zoology, while Hugo Sjörs and Carl Olof Tamm were the Swedish assessors for the chair in plant ecology.
239. ED 30/12 1965:12.
240. ED 16/9 1966:17; Pettersson's main competitor among the other eight applicants was Magnus Fries (cf.4-2, note 33). The Finlandish assessor thought Fries more qualified than Pettersson, but both Sjörs and Tamm preferred Pettersson.
241. ED 29/5 1964:1; K.-G.Nyholm, who had been appointed professor of zoology in Göteborg the year before (cf.4-3, note 174) was appointed to this chair. Several others were ranked before Lindroth and Pejler.
242. This short characterization of the two departments is based upon interviews, my own observations and a review of mimeographed publication lists in department archives.

A long-term plan for instituting ecology at the universities

By 1964 *UKÄ* found it necessary to set up a commission, *1965 års biologiutredning* (the Biology Commission of 1965), reviewing the extent of basic biological research in Sweden, in order to achieve a long-term plan for the expansion of the biological sciences, which were now considered crucial for further social developments:

> *»From the point of view of society there now exist strong motives for a forceful concentration on the biological sciences«.*[243]

The starting-point for the Commission's work was the delineation of the biological universe made by *UKÄ*, including

> *»botany (systematical, morphological, ecological, marine botany, physiological botany including cell physiology, paleobotany), and zoology (systematical, morphological, experimental, and ecological zoology) and limnology, genetics, biochemistry, microbiology and radiation biology«.*[244]

The *UKÄ* commission was directed to

> *»put forward suggestions for a necessary differentiation and subspecialization within the above-mentioned branches of science«.*[245]

The commission paid due respect to the existing social orders of botany and zoology. For example, in proposing new ecological positions, it added that »for organizational reasons«, these would »necessarily be connected to their principal subjects, botany, zoology, microbiology«.[246]

But otherwise the emphasis was laid on the »new subjects«, viz., molecular biology (incl. biophysics and biochemistry), genetics, microbiology, and particularly ecology. Ecology had its spokesmen on the Commission: out of the five experts, two were known as important ecology actors, viz., Erik Dahl of Lund and Bengt Lundholm. In addition Börje Norén, the »microbiological ecologist« from Lund,[247] was appointed its secretary. Hence, the argument for ecology was largely given free scope in the Commission's work. The number of pages devoted to the different scientific social orders gives an idea of their salience within the deliberations of the commission:[248]

243. Utbyggnadsplan 1967,p.14.
244. Ibid.,p.1.
245. Ibid.,p.1.
246. Ibid.,p.142.
247. Cf.4-2, note 41.
248. Approx. number of pages devoted to each scientific social order in Utbyggnadsplan 1967, chapters 3 and 4.

scientific social order	*approx. number of pages*
botany	6
zoology	4,5
molecular biology	6
biophysics	6
biochemistry	3
microbiology	6
genetics	4
ecology	11

The disproportionate attention paid to ecology was reflected in the large number of new positions suggested for the subject by the Commission, as follows (ap = associate professor; p = professor):[249]

- »the vegetational dynamics of the cultivated landscape« (ap, Uppsala)
- »experimental ecological botany« (ap, Uppsala)
- »general limnology« (ap, Uppsala)
- »applied limnology« (ap, Uppsala)
- »freshwater microbiology« (ap, Uppsala)
- »water management oriented to agricultural problems« (ap, *Lantbrukshögskolan*)
- »ecological zoology oriented to the terrestrial ecology of the cultivated landscape« (ap, Lund)
- »ecological zoology oriented to the aquatic ecology of the cultivated landscape« (ap, Lund)
- »experimental ecological zoology« (ap, Lund)
- »marine ecological zoology« (p, Göteborg)
- »marine microbiology« (p, Göteborg)
- »biology oriented to ecology« (p, *Tekniska högskolan*)
- »ecological botany« (p, Stockholm)
- »forestry plant pathology« (ap, *Skogshögskolan*)
- »ecological zoology, esp. brackish water ecology« (ap, Stockholm)
- »botanical brackish water ecology« (ap, Stockholm)

In addition it was suggested that some existing positions be re-designated, as follows:

- the zoological chair in Lund should be renamed »ecological zoology«
- the chair in marine botany in Göteborg should be renamed »marine
- ecological botany«
- the chair in zoology in Stockholm should be renamed »ecological zoology«.
- the chair in forestry botany at *Skogshögskolan* should be renamed »ecological microbiology« (while the associate professorship in nordic forest plant geography at *Skogshögskolan* should be transformed to the ordinary chair in forestry botany)

249. Ibid.,pp.91-169.

– the proposed associate professorship in forestry production in Umeå should be renamed »forestry production ecology«.

And finally:

– the associate professorship in ecological botany in Lund should be transformed into an ordinary chair, and
  the associate professorship in entomology in Uppsala should be renamed and transformed into a ordinary chair in »ecological zoology«.

The large number of proposals for new and re-designated professorships indicate that the ecologists had eventually succeeded in identifying themselves with powerful external interests and translating these into the rhetoric of ecology. Even though not all the proposals were implemented, *1965 års biologiutredning* nevertheless stands out as a major landmark in the attempts to authorize the new social order of ecology throughout the entire national system of higher education and research.

Although *1945 års universitetsberedning* had introduced faunistics, floristics and ecology as a part of the examination demands for botany and zoology graduates, these requirements had a weak formal status. It was up to each professor to interpret the examination decrees, and as we have seen above (4-2 and 4-3) local interpretations varied considerable; for example, Stockholm zoology students learnt a lot about ecology, while Uppsala zoology students remained more ignorant. The authorization of ecology in university curricula did not take place until the late 1960s, in connection with the so called *UKAS* commission set up to reform and rationalize undergraduate curricula.[250] Its proposals with regard to botanical and zoological studies, two of the oldest and most tradition-bound subjects within the science faculties, were drastical: it was proposed that both disappear as independent subjects. Instead, the *UKAS* commission proposed an entirely new undergraduate subject, viz., biology, the content of which should be regulated by detailed, centrally formulated curricula.[251] As with *1965 års biologiutredning*, the so-called functional biology, including ecology, was emphasized. Alongside microbiology, genetics and physiology, ecology was now made an obligatory subject in all undergraduate biology courses. Approximately 15% of first year courses in biology should be devoted to ecology, and in addition summer courses in faunistics and floristics were made obligatory. Thus from 1969 ecology was institutionalized in all university undergraduate curricula.[252]

Ecologizing the Academy of Science

The contradiction between *UKÄ* as a reforming agent on the one hand and the traditional

250. Utbildningslinjer 1968.
251. We cannot go into detail about the background to the new subject of biology. Suffice it to say, that *Skolöverstyrelsen* (the Board of Education) wanted a cheaper and shorter training of secondary school teachers, while *UKÄ* wanted a more flexible university biology education adapted to a variety of needs of the academic labour market.
252. In addition, courses in ecology were established in other connections too, e.g., at *Tekniska högskolan*. Of special interest is the environmental programme at Lund, where an integrated ecological education was introduced.

scientific social orders of botany and zoology on the other was again revealed in the discussions around Bergius' donation chair (»professor Bergianus«) assigned to *Vetenskapsakademien* in Stockholm in the 18th century.[253] The question of the designation of the chair was first taken up by the Minister of Education in 1959 – it was thought that it might satisfy the old demand of Stålfelt and the university for a chair in plant ecology.[254] Referring to the donor's will and the current plans for a research professorship, the Academy replied that Bergius' chair should be designated as »systematical botany with horticulture« with a view to »contributing to the development of phylogenetics«, i.e., carrying on in the tradition established by the retiring professor,[255] and the Minister dropped the case.

The victory of the Academy and its botany class was only temporary, however. In the meantime a number of naturalistically inclined Stockholm botany students had oriented themselves towards plant ecological problems. Several of them were active members of *Sveriges fältbiologiska ungdomsförening* and their claims for ecology were part and parcel of their voluntary educational work for the Association – they were in fact the ecological avant-garde of the mass of naturalist members. The doyen of the group got his training from Nils Malmer in Lund and returned home having absorbed »the Lund school recipe book«;[256] hence the further development of the small plant ecology group in Stockholm followed closely in the tracks of the Swedish synecological tradition discussed in previous chapters, viz., the attempt to correlate environmental factors and vegetational structure. The expanding group soon gathered under the auspices of Måns Ryberg (b. 1918), a dissident morphologist who had made some studies of the vegetation of the cultivated landscape.[257] Ryberg was in fact appointed »professor Bergianus« pro tempore 1964.

UHÄ returned to the issue of Stockholm plant ecology on several occasions restating its wish for the chair to be designated as an ecological one. Eventually, in 1969, the Academy's botany class yielded, and the exact designation of the chair was specified as »botany, particularly the ecology of the cultivated landscape and horticulture«.[258] Furthermore the holder was bound to teach at the university. Not unexpectedly, Ryberg was appointed »professor Bergianus«.[259]

Thus, through the intervention of *UKÄ* the resistance of traditional botany, represen-

253. Cf.1-1, note 23; the following information has been taken from the archives of *Vetenskapsakademien*.
254. 1959 års Riksmuseiutredning 1965.
255. The former »professor Bergianus« between 1944 and 1964, Rudolf Florin, had worked on paleobotanical problems.
256. Interview with NN 5/10 1982.
257. See Tamm's assessment in ED 8/11 1963:1; Ryberg is here characterized as a dissident, since his academic training was in morphology and he wrote a morphological dissertation in 1960; but nevertheless he succeeded in turning his floristic interest into academic publications, viz., on vegetational history (Ryberg 1971). Tamm characterized his »main interest... to be developments in nature«, particularly »where man has played a main role« (ED 8/11 1963:1). His ecological production was too small, however, to qualify him for the position in plant biology in Lund.
258. Minutes of meeting of the 6th (botany) class 5/3 1969 (in the archives of the Academy); the botany class yielded by degrees: as a compromise, they suggested the chair be designated as »botany, particularly horticulture and the ecology of the cultural landscape«, thus emphasizing botany and horticulture also.
259. U 11/12 1970:B10; the assessors, the two third generation ecologists Hugo Sjörs and Erik Björkman, without hesitation considered Ryberg more qualified for the chair than the other applicant, Tore Mörnsjö, a Lund fourth generation synecologist (cf.4-5).

ted by the Academy's botany class, was broken down. At last Stålfelt's old wish for a plant ecological chair at Stockholm seemed to be fulfilled. The intervention was of vital importance for the institutionalization of the nascent plant ecological group in Stockholm, although for obvious reasons their activities were rather insignificant compared with the plant ecology centers of Uppsala and Lund. The first dissertation was not submitted until 1973,[260] though another couple followed later in the 1970s.[261]

Ekologikommittéen and the quest for »functional ecology«

As indicated above *Ekologikommittéen* was founded in 1967 as a continuation of *Naturresurskommittéen*. In its first years of activity, as in its former guise, it forwarded ecology mainly by identifying with environmental and resource management problems. Other members of the Council, however, stressed the role of ecology as part of a strategy for reforming biology with arguments similar to those used by physiologically and biochemically oriented scientists who had pleaded for functional biology at the universities at the expense of descriptive biology:

> *»In a possible priority model* /for functional biology/ *ecology and cell research would form the central pillars«.*[262]

The promotion of »functional ecology« as a means of reforming the biological sciences was met with great appreciation in the Council. One interviewee maintains that *NFR* was

> *»scared to death at the thought of a continued descriptive ecology«.*[263]

It was probably this concern that lay behind the announcement of a higher research position in »ecological microbiology« in 1968.[264] A year later, one of the members of the Council oriented towards physiological research, and not attached to the goal-directed ecological policy advocated by Brinck and his adherents, suggested the creation of a »special institute for the promotion of ecological research«[265] – a proposal that met with considerable interest from the rest of the Council (cf.4-5). The two special research recruitment positions announced by the Council in 1969 were designed so as to support »functional« ecological work.

On the other hand the »descriptive« ecologists influenced the actual designation of the positions; the one (on »the relation between ectoparasites and host animals«) lay close to Brinck's area of research, whereas the other (on »consumption in relation to primary production in a Swedish deciduous forest«) was awarded to a fourth generation ecologist supported by Sjörs. Furthermore, for the first time, reference to ecology began to be made in support of investigations in other research areas as well. In preceding chapters, we have shown repeatedly how ecological investigations were justified as an aid in solving other problems, say, of biogeography or taxonomy. However, in 1971, when the Council decided to appoint a committee for zoological taxonomy, such arguments were turned on

260. Wallentinus 1973.
261. In the mid-1970s, however, the group, now led by Lars-Erik Liljelund, took the lead in importing the new evolutionary and theoretical ecology (cf. the Epilogue).
262. Minutes of meeting of *NFR*'s biology delegation 18-19/3 1969, item 9 (in the archives of *NFR*).
263. Interview with NN 3/2 1981.
264. It was filled by Börje Norén (cf. note 247 and 4-2, note 41).
265. Minutes of *NFR*-meeting 7/10 1969, item 1757 (in the archives of *NFR*); the proposal was made by Ivar Sperber, educated as a zoologist in Uppsala in the 1940s, after consultations with, among others, Hemming Virgin and Einar Stenhagen, who were both positively inclined towards experimental ecology (cf.4-2, note 58.

their heads. Support for taxonomy was now advocated with specific reference to »ecology's greatly increased need« for species determinations.[266]

Thus, the circle was complete. A century after the culmination of Linnean museum taxonomy and systematics, marginalizing animal and plant field studies to amateurs or spare-time excursions, taxonomy was now been reduced to the position of an auxiliary to ecology, the new queen of the functional biological sciences.

With these decisions the social order of ecology was eventually authorized at the national research policy level. Only six years after *NFR*'s general secretary had made only passing reference to ecology in a review of national scientific priorities (cf. above), ecology was now being ear-marked as a top priority research area. Later decisions by the Council confirmed the status of the new scientific social order. Of the twelve research recruitment positions suggested by the Council in early 1970 as many as six were considered as ecological.[267] Yet another higher research position, in »ecological biochemistry«, was awarded in 1971.

On the other hand, *NFR's* and *UKÄ's* authorization of ecology was not immediately followed by the universities. As late as 1974 the two Umeå chairs in ecological zoology and ecological botany and the chair in forest ecology at *Skogshögskolan* were the only chairs designated as ecological. And the endowment of *docent* status was, as a rule, still given in terms of botany or zoology, limnology or plant biology – not in terms of ecology.[268] Thus, the authorization of the social order of ecology in the 1960s and the early 1970s, was largely a policy adopted by higher science policy agencies.

4.5 The ecosystem project policy

As we have seen in Sections 4-2 and 4-3 a multitude of local ecology actors and local social orders of ecology emerged in the universities during the post-war period. By the late 1960s ecology was the name given to well-established, and often authorized, scientific activities at a dozen or more local sites. Likewise, as seen in Section 4-4, within a few years, from the late 1960s to the early 1970s, most local social orders of ecology were connected into one national social order of ecology. Simultaneously ecology became the catch-word of the day. A dramatic testimony of the sudden importance of ecology was the radically changed policy of *Naturvetenskapliga forskningsrådet* (*NFR*). In the Council's five-year-

266. *NFR*'s financial request (»petita«) for the fiscal year 1972/73, spring 1971, p.18.
267. The six proposed research recruitment positions in ecology were designated as:
   - »ecological soil microbiology«,
   - »the nutrition- and production ecology of the soil fauna«,
   - »paleoecology, particularly quarternary landscape formation«,
   - »the ecology of primary production«,
   - »secondary production in running waters«, and
   - »the limnic cycling of organic compounds« (Minutes of meeting of *NFR* biology delegation 28/1 1970, item 27 (in the archives of *NFR*).

268. With regard to *docents* only the following were endowed by 1974: at Uppsala none; at Lund one in game ecology and three in ecological botany; at Göteborg none; at Stockholm four in zoology, particularly ecology; at Umeå one in ecological zoology.

plan for the years 1976 through 1980 ecology was elevated to one of the few top-priority areas of national science policy.

Several converging processes in this authorization of the national social order of ecology have been discussed above, viz., the exponential rise of naturalist students and the large number of fourth generation ecologists writing their dissertations on ecological problems; the expansion and reform of university education and research; the identification of the environmental crisis and its translation into ecological problems, etc. In this final section we will again focus on the importance of naming as a means of achieving symbolic integration of a social order, and, more specifically, on the utilization of the concept of the ecosystem as a rhetorical device for the authorization of ecology as a national social order.

Studies of ecosystems are usually conceived as studies of the »wholeness« of organisms and their environment. In that sense ecosystem studies were hardly anything new to Swedish field investigators of animals and plants. The ambition to study the »wholeness« of organisms and their environment has appeared several times in the story told in the preceding chapters. Indeed, Linné had talked in terms of »oeconomia naturae« and »politia naturae«. Thinking in terms of »the balance of nature« was not an uncommon mode of expression from the late 19th century onwards. For example, we recall Ljungqvist's concept of »formation ecological« studies to denote the mutual relation between vegetation and site (1-3), and Hesselman's conception of the forest as an »organic whole« (2-3). The same aspiration can be traced in a number of studies of the 1930s and 1940s, for example in Julin's studies of plant, animal and environment interrelations at Vessers udde conceived in terms of the »holocoen« (3-2), and in Malmström's and Romell's investigations of the mutual relation between vegetation and site at *Skogsforskningsinstitutet* in the 1940s (3-3). The collective inventory of the Muddus national park in 1944-45, carried out by a number scientists attached to *»Växtbio«* and others, comprising studies of plants, soil conditions, insects, etc., foreshadowed cross-disciplinary team-work, although never being intended towards ecosystem studies.[269)]

The most advanced investigation of the »wholeness« of organisms and their environment pursued by a Swedish ecologist before the break-through of the ecosystem concept in the 1960s and 1970s was Hugo Sjörs' all-round study of park meadows in Grangärde finnmark in the province of Dalarna in the years 1948 to 1952. Sjörs had remained faithful to the Uppsala school in his dissertation on mire vegetation of 1948, but the Grangärde study transcended the usual kind of synecological correlation studies. Besides community analysis, he investigated the influence of light distribution and soil water availability on the composition and differentiation of vegetation, and, as something new in the Uppsala tradition, he tried to estimate the primary production and nutrient balance of the meadows. Concluding that

> *»the park meadow is constituted of an elaborate and long regulated interplay, easy to destroy, difficult to restore, between site, plant community and traditional cultivation practices«,*

he stated programmatically that:

269. Arnborg 1963,pp.1-4. For example, Sjörs studied mire vegetation; Sten Rudberg studied geology and geomorphology; Nils Quennerstedt investigated lake vegetation; Erik Björkman surveyed mold fungi; and Karl-Herman Forsslund made entomological collections.

> *»The most important lesson to be drawn from a study of the park meadows is probably the need for a synthetic ecological point of view. It is necessary to consider the unity of relations and interactions instead of trying to isolate different factors from their mutual dependence«,*[270]

evidently a novel outlook in Swedish synecology at the time.[271]

In the post-war period this general view of the »wholeness« of organisms and their environment was gradually conceptualized in terms of the ecosystem. The concept was evidently imported from abroad, from a variety of sources. In fact, the fourth generation Lund animal ecologist Paul Ardö (4-3) seems to have been the first Swede to utilize the term ecosystem in a dissertation heading;[272] somewhat earlier Sjörs had introduced the ecosystem concept to botanical circles in a review article, »Remarks on ecosystems«.[273] Likewise, Wilhelm Rodhe introduced the ecosystem concept in his courses and in a review article on the state of limnology in 1958.[274] These early conceptual introductions had very little effect, however. Most popular scientific articles on ecology in the yearbook of *NFR* ignored the term, as did, for example, Stålfelt in his textbook *Växtekologi.* Nor was the large scale *Mälarundersökningen* initiated in 1964 (cf.4-2) presented in ecosystem terms.[275]

From the mid-1960s, however, the ecosystem concept rapidly gained ground in the consciousness of both botanists and zoologists. For example, the commission reports of *1964 års naturresursutredning* and *1965 års biologiutredning* were written alike with numerous references to the ecosystem. The 1965 commission, which accorded ecology a key role in reforming the biological sciences, registered that

> *»there is no doubt that ecosystem studies are the most topical question in ecology«.*[276]

Pejler's article »Att tänka i ekosystem« (To think in terms of ecosystems) in *Zoologisk revy* 1965 is characteristic of the time.[277] Pejler found it necessary to introduce his readers not only to the term ecosystem, but to the terms ecology, autecology and synecology as well – only a couple of years later it would have been quite superfluous to write introductory articles of this kind to an academic public. From around 1970 pretty well every popular review of ecology was presented in ecosystem terms – to speak in terms of ecosystems became the catchword of the day. For a while most of ecology was legitimated by its significance for understanding the ecosystem. An example that verged on parody came from a group of Lund population ecologists who legitimated their studies of animal dispersal patterns with reference to their

> *»obvious importance for the ecosystem analyses which at the moment are justly set up as an essential goal for further ecological research«.*[278]

270. Sjörs 1954,p.111.
271. In a more restricted sense, Tamm's dissertation of 1953 was a model ecosystem investigation.
272. Ardö 1957. Ardö took the term from the botanical literature; having written his geographical licentiate thesis on sand beach dynamics, he chose to study the insect fauna of the sand beach as a whole for his dissertation (interview with PA 8/9 1981).
273. Sjörs 1955. In *Nordisk växtgeografi* of 1956 he also dealt briefly with the ecosystem concept. Sjörs explicitly refers to »a young zoologist by the name of Paul Ardö who...worked with insects or rather flies in sand dunes. It was a magnificent ecosystem thinking, and he helped me... finding literature on ecosystems. I think that he by and large led me to the idea« (interview with HS 24/9 1981).
274. Rodhe 1958.
275. See, e.g., Ahl and Willén 1965.
276. Utbyggnadsplan 1967,p.70.
277. Pejler 1965.
278. Ulfstrand *et al* 1971,p.189.

The break-through of ecosystem projects in Sweden is usually thought to be a straightforward adoption of the *International Biological Programme* (*IBP*).[279] The *IBP* was aimed at studies of the biological basis of productivity and human welfare, the idea having originated in the wake of discussion of problems of world starvation in the 1950s. The *IBP* initiative got a Swedish counterpart in 1964 through the intermediary of *Vetenskapsakademien* and somewhat later an *IBP* secretariat was created, sponsored by *NFR*.[280] During the period 1967 to 1974 seven larger or smaller ecological or »proto-ecological« research projects were run as *IBP* projects, most of them following the original aim.[281] However, as a consequence of the shift in focus of some of the American projects towards ecosystem analyses,[282] and especially after a meeting for Nordic *IBP*-co-operation in Sandefjord in Norway in December 1966, the question of ecosystem analysis was taken up in the Swedish *IBP* discussions.

The great importance of the *IBP* should not conceal the fact, however, that the first ecosystem projects during the late 1960s were formulated by fourth generation ecologists at local university departments, as an extension of established research practices. In the following we will first review the extension of local initiatives to ecosystem investigations in the 1960s, before concentrating on the formulation of a national ecological policy involving the establishment of an entirely new institutional edifice for ecology, viz., the so-called large scale ecosystem projects instituted by *NFR's ekologikommittée*. Finally we will review the formulation of a national ecological policy extending beyond the realm of research to encompass the social and economic planning of Sweden.

The shift from plant synecology to ecosystem ecology in Lund

Sjörs' acquaintance with the ecosystem concept had no particular consequences for ecological activities at »*Växtbio*« in Uppsala. In Lund, however, thinking in terms of ecosystems revolutionized research practices. The classical pattern of translating naturalist and floristic interests into academic correlation-synecology changed dramatically at the plant biological laboratory in Lund around the mid-1960s.

The change is reflected in the new wave of doctoral dissertations from the laboratory submitted around 1970. Whereas the two first, Tore Mörnsjö (b.1932) and Mats Sonesson

279. See, e.g., Bergman 1975.
280. B.von Hofsten 1967. Note that at that time von Hofsten did not describe the *IBP* as a programme for ecosystem studies; it was only a couple of years later that *Ekologikommittéen* began to understand their ecosystem projects as a direct continuation of the *IBP* initiative.
281. For example, Carl Olof Tamm, Nils Nykvist, and Erik Björkman at *Skogshögskolan* reformulated their already ongoing research activities as *IBP* projects; likewise Per Brinck in Lund reformulated two projects concerning small rodents and ectoparasites as *IBP* projects (Vik (ed.) 1975).
282. The planning of the American ecosystem projects started around 1965; in all, six projects were established, among them the Grassland Biome Project (started in 1968) and the Deciduous Forest Biome Project (started in 1969) (Bergman 1975).

(b.1930), restricted themselves to the classical synecological programme,[283] the following two changed their graduate careers to ecosystem studies in the midst of their work, around 1966-1968. Folke Andersson, who had been introduced to ecology as an undergraduate by Åke Persson (cf.4-2), embarked in 1960 on a study of »the differentiation of the vegetation in relation to the soil-water factor«, i.e., a problem wholly within the classical synecological theme. Andersson's own refinement was to elaborate methods for soil-moisture determinations. In 1966, however, he completely changed the theoretical perspective for his project, turning to »problems concerning the formation and turnover of organic matter«, and submitted the most complete ecosystem investigation published so far by a Swedish ecologist.[284] As he himself writes:

> *»The work between 1966 and 1968 thus became a study of primary production of different ecosystems«.*[285]

Germund Tyler (b.1941), who came to the laboratory in the early 1960s, used his familiarity with the flora of shore meadows to initiate a classical investigation of the relation of vegetation to site combining plant community analysis and accurate chemical and mineral analyses. His aim was

> *»to describe vegetation structure and differentiation of Baltic sea-shore meadows in relation to primary edaphical gradients* /and/ *to analyze the main chemical properties of the soil«.*[286]

But a few years later he changed his work in the same direction as had Andersson, reflecting that

> *»In line with the views of the age and maybe also the demands of the age the aim of ecology has changed fast«.*

Now »functional and dynamic aspects of nature« were in the foreground, and hence his new research goal was

> *»to measure the distribution and turnover of organic matter and mineral elements in a shore meadow ecosystem«.*[287]

This seems to be a pure adoption of the *IBP* programme. And evidently Andersson's and Tyler's specific shift in research practice, from translating naturalist interests into classical synecological problems, to translating them into problems regarding ecosystem function, primary production and turnover of organic matter and mineral constitutents, was triggered by their contacts with the *IBP* projects. For example, Andersson explicitly refers to the 1966 meeting in Sandefjord as his point of departure.[288] On the other hand, Lund plant ecologists had thought in terms of the »wholeness« of vegetation and environment for most of the 1950s, although then still in correlation terms. For example, although not speaking in terms of ecosystems, Stålfelt's textbook *Växtekologi* of 1960, by emphasizing the problems of mineral turnover, is said to have exerted a considerable

283. Mörnsjö, who had started his work in 1958, made a very extensive survey of the plant communities of Scanian peatlands, including stratigraphical analyses of their vegetational history, while paying rather little attention to site conditions (Mörnsjö 1969); compared with other Lund dissertations Mörnsjö's work introduced a dynamic element, discussing vegetational successions and »the dynamics of the bog growth«. Sonesson was initiated by Persson (cf.4-2) in 1960 to an investigation of the correlation between variation in vegetation and variation in habitat conditions of »poor« mires in the Torneträsk area in 1960 (Sonesson 1970).
284. F.Andersson 1970a.
285. F.Andersson 1970b,p.14.
286. Tyler 1971, unpaginated (introduction) (engl.orig.).
287. Tyler 1969,pp.131,135.
288. F.Andersson 1970a,p.14.

influence, more among the Lund ecologists than at »*Växtbio*« in Uppsala.[289] They were thinking in terms of ecosystems already in the early 1960s, and the formal concept is said to have been introduced in undergraduate courses around 1965.[290] One reason is said to have been the search for new and more exciting problems:

> *»One began to tire of plant sociology and to look for something new«.*[291]

Hence the *IBP* programme satisfied a latent but growing need. Both Malmer and his junior colleagues rapidly adopted the whole idea. Within a short time they had formulated a collective research program for the laboratory entitled »Productivity of South Swedish deciduous forest ecosystems and their secondary successional stages«, which was eventually authorized by the international *IBP* and funded by *NFR*. Nine scientists were involved between 1967 and 1972, publishing sixteen articles between 1969 and 1974.[292] One dissertation was started and finished entirely within the scope of the program: Bengt Nihlgård (b.1940), who was recruited in 1966, made comparative studies of beech and spruce forest ecosystems, and investigated microclimate, precipitation and soil influences on biomass and productivity.[293]

Thus within a few years the plant biology laboratory in Lund had been transformed into an entirely new kind of ecology department. The new programme was formulated by Malmer in 1970, seeing causal investigations of the matter- and energy metabolism of ecosystems and their dynamics as a fundamental *must* for modern ecology.[294]

The Andersby backar ecosystem project in Uppsala

A second local initiative towards ecosystem investigations was taken in Uppsala in 1968 by a group combining young students of Kullenberg and of Sjörs, neither of whom was directly involved, however. Kullenberg had always been more interested in species-species interactions. According to one of his students

> *»Kullenberg was not oriented in that direction... Tischler and other* /books used by Kullenberg/... *seemed to lack a clear theoretical foundation«.*[295]

But the new group found their theoretical bearings in the ecosystem concept:

> *»We had been discussing it in coffee-breaks... Odum's textbooks lay behind it... For me personally Rodhe's teaching was decisive... with Lindeman and Odum everything fitted together«.*[296]

Likewise, although Sjörs had been one of the first plant ecologists in Sweden to apply the ecosystem concept he did not stimulate his students to pursue such studies, and in fact most of them continued to translate their naturalist interests into classical plant geography or correlation synecology (4-2).

Nevertheless, a few of them, recruited in the mid-1960s, began thinking in terms of

289. According to interviews with NN 10/11 1976 and NN 6/9 1976.
290. Interview with NN 19/11 1976.
291. Interview with NN 6/9 1976.
292. See Vik (ed) 1975,pp.42-48.
293. Nihlgård 1970.
294. Speech to *NFR*'s ad-hoc group on terrestrial synecology (cf. below), 3/2 1970 (mimeo in archives of *NFR*).
295. Interview with NN 31/8 1982.
296. Ibid.

ecosystem research. But these individual musings would probably have come to nothing without the sudden intervention of Bengt von Hofsten, an Uppsala biochemist and acting secretary of the Swedish *IBP* committee, a man said to be »full of ideas, very enthusiastic«.[297] Von Hofsten initiated a meeting at Sjörs' department, suggesting that the assembled group should start an *IBP*-project. The result was the Andersby backar project.[298]

The initial intention was to integrate studies of primary and secondary production and some of the early investigations involved co-operation between animal and plant ecologists,[299] though later they worked separately. It was also thought to be a very democratic project, a true example of team-work organized from below.[300] The leading figure, Dag Gärdefors, was still a graduate student who otherwise worked on the relation between environment and grasshopper activity, a direct continuation of Kullenberg's research programme:

> *»Dag Gärdefors /was/ like a chief, he held group seminars, he led them... He showed great self-sacrifice in devoting himself to the work«.*[301]

The Andersby backar project was also a theoretically and methodologically advanced project. The group of graduate students had no ambitions to solve any environmental problems; theirs was a basic research project, and consequently they spent much time on methodological issues, e.g., developing methods of sampling for standing crop and energy flow estimates.[302]

The Askö-laboratory and the Baltic as an ecosystem

The third local initiative in ecosystem analysis was taken by the Stockholm zoologists working at the Askö laboratory in the southern archipelago of Stockholm (cf.4-3). Until 1968, the handful of young zoologists led by Bengt-Owe Jansson largely worked according to the programme laid down by Krogerus and Lindroth, i.e., nature was approached as a dispersal of species along ecological factor gradients, a programme epitomized in Jansson's dissertation of 1968.

But within the course of a few years the Askö group suddenly began to comprehend their study area as a functional unity, as an ecosystem. In a first step, between 1965 and

297. Ibid.
298. At least six members of the group participated regularly: Dag Gärdefors, Ulrik Lohm, Tryggve Persson and Olof Tenow from the animal ecological side, and Håkan Hytteborn and, and later Hans Persson, from Sjörs' department. Lohm wrote his dissertation as a result of the project in 1974, Hytteborn and Persson both wrote their dissertations on above-ground and below-ground primary production, respectively, in 1975.
299. E.g., Axelsson *et al* 1972.
300. In the foreword of his dissertation, based on the project, Lohm wrote: »The investigation summarized in the following dissertation was carried out as part of an integrated team-work. All papers resulting from this team-work (more than twelwe) are, or will be, jointly published with the names of the authors in alphabetical order. The present author has taken part in all the various stages of the research work...« (Lohm 1974). This communistic ideal would soon be considered a disadvantage, however; when applying for the chair in ecological zoology in Umeå in 1976 all three were excluded from consideration because the assessors maintained that they could not discern the individual contributions of each applicant (U 5/8 1976:10).
301. Interview with BK 23/11 1981.
302. See the final report in Vik (ed.) 1975,pp.49-54.

1968, some of them turned to studies of food-webs and productivity. Several works from the laboratory bear witness to this sudden metamorphosis. For example, when Ann-Mari Jansson (b.1934) began her work on the fauna of the Cladophora belt in 1963-1965 she tried to elucidate

> *»how the animals react to variations in different abiotic factors... both in the field and by experiments in the laboratory«,*[303]

but by 1965 she turned to problems concerning the food-web and productivity, and yet a few years later she tried to compute primary productivity and the most important food chains of the belt.[304] Others passed through similar changes in their problem formulation. For example, Bengt-Owe Jansson immediately after his dissertation turned his attention to »the dynamics and productivity of Baltic mud bottoms«.[305] This shift from experimental preference studies to productivity studies within the framework of an ecosystem point of view was rapid and extensive. Almost the whole laboratory joined in, as witnessed by a review of 1969 which reveals production measurements and food-web analyses to be the pervading activities.[306] In 1968 the laboratory seminar took up a problem concerning productivity for the first time,[307] and only a year later, in February 1969, the last undergraduate thesis on experimental ecological problems was ventilated.

The existence of the Swedish *IBP* programme may have provided a general background of inspiration for this sudden transition from classical animal ecological problems. The Askö group was never attached to the *IBP* programme, however. The shift to ecosystem studies was partly a logical continuation of the group's investigations. When the Askö group applied for their first major research grant from *NFR* in early 1969 for studies of the productivity of the Askö area they gave the following motivation:

> *»Earlier biological studies at the Askö laboratory have been characterized by rather great heterogeneity. The individual scientists have each...* /studied/ *an animal group or an environment as the leading theme...* /But now/ *the separate research problems begin to overlap. That is, time is ripe for larger team-works, with biological production in the Baltic as the unifying theme...* /This means/ *that the research at the laboratory has converted from its first zoological phase to a general biological one«.*[308]

Further, it should be noted that their turn to studies of productivity and food-webs coincided with the identification of the environmental problems of the Baltic. The increasing pollution of the Baltic had first been taken up by *1964 års naturresursutredning.*[309] By 1968 several state authorities were focusing on the Baltic problem,[310] including *NFR* and the newly founded *SNV's forskningsnämnd* (cf.4-4), which, early in 1969, was given overall responsibility for the issue by the government.

303. A-M.Jansson 1966,p.282.
304. A-M.Jansson 1967.
305. B-O.Jansson 1969a,p.47. Similar transitions can be seen in the papers of several other members of the laboratory.
306. Special issue of *Zoologisk revy*, nr 1-2 1969, containing 24 signed contributions by scientists working at the Askö laboratory.
307. Mimeo in the archives of the Department of Zoology, Stockholm.
308. Grant application to *NFR*, project »Normalmiljöns dynamik«, 31/1 1969, appendix 1 (in the archives of *NFR*).
309. The issue had again been forcefully urged in 1967-68 by Svante Odén, professor of soil science at *Lantbrukshögskolan* (Interviews with BL 9/2 1982 and NN 20/5 1981).
310. See, e.g., the meeting on the Baltic situation in *Miljövårdsberedningen* (the Government's Environmental Working Committee) 16/12 1968 (mimeo in the archives of *Miljövårdsberedningen*).

The Askö group, of course, joined the debate on the Baltic.[311] Already in 1968 they had drafted a Baltic project, and in their annual report of 1968 the group subordinated their scientific activities to the goal of »inducing society to combat the increasing pollution of the Baltic«.[312] Hence when *SNV's forskningsnämnd* began to allocate funds in 1969 the Askö laboratory was among the applicants.[313] Their Baltic investigations expanded quickly; together with yet another pollution project and some smaller projects they secured 4-5 million SEK during the 1970s.

During the late 1960s the Askö laboratory had evolved into a great ecological magnet being exceedingly popular among Stockholm graduate zoology students. Around 1970, approximately 10 scientists were working at the laboratory for their doctoral dissertations under the auspices of Bengt-Owe Jansson; in addition consecutive generations of undergraduates wrote their theses at Askö. Aided by its isolation, they turned this remote island into a modern counterpart of Sernander's cosy seminar in Uppsala 50 years earlier. Bengt-Owe Jansson was a charismatic research leader, with seemingly never-ending enthusiasm, and the members of the group worked closely together. With the new financial support from *NFR* and *SNV's forskningsnämnd* Jansson and his collaborators were at last able to put their project on a firm footing. Thus, within a few years, the Askö laboratory turned out to be one of the most successful enterprises in the short history of the social order of ecology in Sweden (cf. below).

The enrolment power of the Askö project was strengthened still further by the adoption of a common theoretical apparatus and methodology oriented around the ecosystem concept. »Ecosystems appeared as an avant-garde opinion«, one critic maintains.[314] The galvanising event for the new research orientation seems to have been the visit of the American systems ecologist, Howard T. Odum, in May 1970. Odum's energy modelling language suddenly gave the Askö group a unified linguistic tool for ecosystem analysis. Other ecologists at the Department of Zoology were rather indifferent to Odum's message:

> *»the Askö group were ignited by Odum's luminiscent concepts«,*

one of them comments sarcastically.[315]

After a younger member of the team had been sent to Gainesville, Florida, to learn from Odum his modelling and simulation techniques, the group applied for a large grant to study »the energy flow in Baltic ecosystems«[316] in December 1970. The application was rejected, however, and for a while the Askö plans for an integrated ecosystem project

311. See, e.g., B-O.Jansson 1969.
312. Annual report of the Askö laboratory (mimeo in the archives of the laboratory).
313. The power conflicts involved here over science policy have been reported by Jamison 1971.
314. Interview with NN 28/9 1976. Thinking in terms of ecosystems was not anything new to the Askö group. From 1965 E.P.Odum's book on ecosystems was used for undergraduate teaching at the Department of Zoology in Stockholm, and in 1967 the topic of »ecosystem structure and function« was added to individual and population ecology in the ecology courses. But prior to the ecosystem projects, the idea of the ecosystem was usually an heuristical aid only, suitable for organizing an ecology course, but having no bearing on research practice.
315. Interview with NN 28/9 1976. The Askö group's rapid adoption of Odum's ideas soon became part of the national ecological gossip; a Lund ecologist told me in expressive terms about the 16 hour long seminar session at Askö ranging from lunch-time the first day to 4 a.m. the next morning.
316. Grant application to *Riksbankens jubileumsfond* (the Central Bank of Sweden Centennary Foundation), 14/12 1970 (mimeo in the archives of the laboratory).

seemed to have failed.[317] As will be demonstrated below, however, the Baltic ecosystem project was carried through but only after a policy initiative from *Ekologikommittéen.*

NFR's large scale ecosystem project policy.

The establishment of the three ecosystem projects referred to above[318] were only to a small extent the result of a central planning initiative. The projects mainly grew out of local initiatives taken by fourth generation ecologists, although stimulated by the national *IBP* programme. The new national ecology body, *NFR's ekologikommittée* had virtually no influence on their establishment.

Within a few years the situation altered drastically, as *Ekologikommittéen* took over the initiative with regard to the creation of ecosystem studies. While the approach of the Committee during its first three years had been rather tentative, it sharpened up considerably in 1969-1970. The triggering event was the proposal referred to above (4-4) from one of the members of the Council in October 1969 to create a »special institute for the promotion of ecological research«.[319] Shortly after, the Council decided to set up an ad-hoc subcommittee for

> *»elucidating the preconditions for establishing a special agency for terrestrial synecology«*[320].

After a series of local hearings the subcommittee[321] delivered its report to the Council in June 1970, with proposals for a series of long term and large scale integrated ecosystem projects. Through this initiative *Ekologikommittéen* established itself as the leading body of Swedish ecology, and the main agency for the ecologization of Sweden in the early 1970s.

The report of *Ekologikommittéen* on the future build-up of terrestrial synecology – the first concise policy document for Swedish ecology – stands as a decisive documentary event in the authorization of a national social order of ecology. The report concluded that proper management of the nation's natural resources demanded

> *»extensive and effectively organized ecological research of a cross-disciplinary character... /e.g./ on the structure, dynamics and function of the organic world, on production, matter- and energy transformations, and on the regulatory mechanisms of ecosystems«*.[322]

Thus, in its basic view of the use of ecology *Ekologikommittéen* still echoed the philo-

317. The first to report on the use of Odum's approach was Ann-Mari Jansson, whose dissertation, submitted in 1974, was an ambitious attempt to model and simulate several aspects, e.g., nutrient budgets and productivity, of the Cladophora algal belt ecosystem (A-M.Jansson 1974).
318. It is worth noting that the ecologists at the leading ecological department in the country – Brinck's Department of Animal Ecology in Lund – did not formulate any ecosystem project. It is true that one graduate student, who had begun an investigation of food selection in a rodent species, was attached to the *IBP* programme, and accordingly oriented himself towards studies of the relation between secondary consumption and primary production in ecosystem terms; nevertheless he presented his work in terms of population ecology (Hansson 1971).
319. Minutes of *NFR* meeting nr 144, Oct 1969 (cf.4-4, note 265).
320. The restriction to »terrestrial synecology« was made by the inner circle of *Ekologikommittéen* in its initial response to Sperber's proposal.
321. Members were Sperber, Lundholm, Tamm and Brinck, besides Lennart Hannerz (head of *SNV's forskningsnämnd*), Börje Norén (cf.4-2, note 41), and the general secretary of the Council, Gösta Funcke. A fourth generation Lund animal ecologist, Staffan Ulfstrand, was appointed working secretary.
322. This and following quotations from »Åtgärder för förstärkning av den terrestra ekologin«, 8/6 1970 (mimeo in the archives of *NFR*).

sophy of its precursor, *Naturresurskommittéen.*

The emphasis on extensive, cross-disciplinary ecosystem research as a remedy to the deficiencies of Swedish synecology implies that problems of research organization were considered inseparable from scientific problems. Sometimes the report even gives the impression that the solution of organizational problems in Swedish ecology was more pressing than the solution of scientific problems. Out of a number of possible research themes stretching from »winter ecology«, »tropical ecology« and »chemical signal systems« to »population regulatory mechanisms of small mammals and birds« and »terrestrial ecosystem analysis«, the report selected ecosystem studies *in order* to satisfy »a clear wish and ambition for co-operation«, which the Committee detected among younger ecologists. This ambition they contrasted to

> *»the present structure at Swedish universities which is unfavourable to extensive and effective cooperation between different departments«.*

That is, ecosystems should be studied in order to achieve cross-disciplinarity and research co-operation – likewise cross-disciplinarity and co-operation were considered a necessary precondition for ecosystem studies. In other words, »co-operation«, »cross-disciplinarity« and »ecosystem« were inseparable component key-words of the one and same socio-logic.

Thus, in order to remedy the weak position of synecology, the subcommitte not only proposed an increase in funds earmarked for ecology and a focus on ecosystem studies, but also emphasized that ecological research should be concentrated in large scale projects, in which

> *»the research tasks will... be tackled by comprehensive cross-disciplinary teams... collaborating on well planned and followed up programmes«.*

Consequently, the original idea of an ecological institute was rejected.

The principle ideas in the report did not meet with any appreciable resistance in the Council. Instead, the Council quickly appointed one of its members to suggest a number of parallel aquatic ecosystem projects.[323] These suggestions and those of the ad-hoc subcommittee were incorporated into the Council's annual financial request for the fiscal year 1971/72. By thus sanctioning the policy of large scale projects the Council in fact authorized a unified national social order of ecology.[324]

During the summer of 1970 the scope of the new policy was stated more precisely. With regard to aquatic ecosystem studies the choice was easy. Bengt-Owe Jansson and the Askö group had already started an ecosystem project – they had oriented themselves towards the management of the Baltic pollution problem and they were already supported by the Council; hence, a large scale Baltic ecosystem project, *Östersjöprojektet* , was

323. Erik Dahl in Lund submitted three outline proposals for large-scale research projects, all three concerning ecosystem analysis: one on ecosystem research in the region of Norrland (suggested by Arne Lindroth and his associates in Umeå, cf.4-3), another on the energy flow through Baltic ecosystems (suggested by B.-O.Jansson), and a third on environmental adaptation and energy flow in a constant benthic ecosystem (»Förslag om storprojekt inom akvatisk ekologi«, 12/6 1970 (mimeo in the archives of *NFR*).

324. Along with this organizational remedy for Swedish ecology, the ad-hoc subcommittee wanted an active, research planning and executive agency, responsible for the initiation, planning and proposal of new large-scale projects», including the continual management of and economic responsibility for the large-scale projects. This was a revival of the idea of an active research secretariat put forward by Lundholm and *Naturresurskommittéen* in the early 1960s (cf.4-4).

launched in the summer of 1971. Shortly after, Bengt-Owe Jansson was appointed professor in marine ecology at *NFR*. *Östersjöprojektet* rapidly grew to a multi-million research project, and soon established itself as one of the leading international projects in the field of marine ecosystems research. An international evaluation group invited by the *NFR* in 1979 asserted that:

> *»The Askö group had made excellent progress in forming a general conceptual model of the Baltic as an ecosystem... the group has an international reputation for excellence, and is among the leading ones in the field of marine ecosystem analysis«.*[325]

Ecosystem ecology as a remedy for a deteriorating nation

In 1970 *Ekologikommittéen* convened a new ad-hoc subcommittee to propose suitable projects for terrestrial ecosystem analysis. After discussing several possibilities, including the incorporation of existing *IBP*-projects into a large scale umbrella organization, the subcommitte proposed two new large scale ecosystem projects, one concerning the tundra-mire ecosystem, the other a forest ecosystem project. However, while the tundra-mire project could be supported within the financial constraints of the *IBP*-programme,[326] the Council was not prepared to give prompt support to a large scale forest ecosystem project, partly for fear of committing too much money to ecosystem projects, but also apparently out of concern for the scientific quality of the proposed project.

The failure of the forest ecosystem project planning group to win the support of the Council was a severe blow to the ambitions of *Ekologikommittéen*. While *Östersjöprojektet* was largely an autonomous initiative from the Askö group, the proposed forest ecosystem project was the Committee's flagship; its scope and organization expressed the essence not only of the new ecosystem policy, but the whole idea of an ecological basis for natural resource management and long-term physical planning, in other words the ecologization of society. To give it up would be a defeat for the ecologization policy laid down almost a decade earlier by *Naturresurskommittéen* and *1964 års naturresursutredning*. If *Ekologikommittéen* and the planning group still wanted to realize their plans they obviously had to enrol more powerful agents than the Council. For that purpose they turned directly to the national political center – the government, the central administration and the *Riksdag*.

During the spring of 1971, when it was clear that the Council would not give financial support to a coniferous forest ecosystem project, Bengt Lundholm initiated an anthology, *Därför ekologi* (Therefore ecology), which brought together a mixture of different interests temporarily united around a single theme – that a large scale, integrated scientific and political approach was necessary to rescue the nation from the threatening environmental catastrophe. Leading personalities in political and cultural life were enlisted to identify general social and political interests as a target for ecological research. The former prime minister, Tage Erlander, wrote an introductory statement emphasizing the need for close co-operation between science and the state in order to solve the environmental crisis. Lars Gyllensten, a member of *Svenska akademien*, stated that:

325. »Report from the hearing for the evaluation of the project 'Dynamics and energy flow of Baltic ecosystems'«, 22/1 1980,p.1 (mimeo in the archives of *NFR*).
326. See Vik (ed.) 1975.

> *»critical research, like ecology, should link up with political and social interests, in a way that most scientists would find provocative, even disreputable, but which must be done in order to achieve the necessary response«.*[327]

Därför ekologi condensed a whole fog of general scientistic ideas floating around in Swedish ecological circles at the time. First, ecology was not only one among several natural sciences – it was considered a generalizing holistic science, »a very extensive science«, whose quest could be summarized in the single sentence: »How does the whole function?«.[328] In addition, ecology was considered an »integrated natural science«, a science breaking down the borders between classical academic disciplines.[329] As one of the authors expressed it:

> *»Nature is not divided into boxes like the university subjects«.*[330]

Third, ecology was thought to transcend national boundaries and, therefore potentially could match the international dimensions of the global environmental crisis, or, in the clarion words of another author:

> *»ecologists in all countries, unite!«.*[331]

A pervading theme in *Därför ekologi* was the idea of ecology as a necessary instrument for the social and physical planning of a nation rapidly deteriorating due to pollution and other kinds of environmental degradation. Lundholm himself stated that with knowledge of ecological variables it was possible to make meaningful, long-term forecasts and a »total planning« of society.[332] A Lund animal ecologist maintained that:

> *»A forceful and purposeful concentration on ecology... has to comprise total planning and a rapid and effective realization of this planning«,*[333]

and added:

> *»Ecology must guide and steer. It must be a future-shaping science... the active, dynamic participator in determining the prospects for humanity and the earth...* /The most essential goal/ *of ecology is to create a rational and objective foundation for a goal directed future«.*[334]

And finally the leader of the proposed coniferous ecosystem project, the Lund plant ecologist Folke Andersson, exclaimed:

> *»If Sweden is to thrive, an integrated ecological research is needed«.*[335]

It is difficult to evaluate the specific effects of *Därför ekologi*. The combined lobbying activities of *Ekologikommittéen* and the coniferous forest ecosystem planning group yielded results. The opposition parties were united in their support for a coniferous forest project; all parties, from the Communists to the Conservatives, considered it a necessity for the future planning of Sweden.[336] The differences between the parties were marginal. While the Conservative and the combined Liberal-Center bills anticipated that the methods and viewpoints of the project might set a standard for other areas of social planning, the Communist bill depicted the project as

327. Lars Gyllensten, *in* Lundholm (ed.) 1971,p.31.
328. Bengt Lundholm *in* ibid.,p.33.
329. Ibid.,pp.44-45.
330. Erik Skye *in* ibid.,p.147.
331. Thomas Rosswall *in* ibid.,p.61.
332. Bengt Lundholm *in* ibid.,pp.46-47.
333. Sören Svensson *in* ibid.,p.72.
334. Ibid.,p.76.
335. Folke Andersson *in* ibid.,p.144.
336. See *motioner* nr 1972:510 (Conservative); 1972:1088 (Liberal-Center); and 1972:1254 (Communist).

»an ecologically rational foundation... /for/ long term and fundamental plans«
to be developed in order to establish »new political and social goals«.

The ruling Social Democratic Party did not object to the scope of the proposed project, they only councelled for further considerations. For example, the planning of ecosystem projects should include a careful specification of different customers' interests.[337)]

The final outcome was that the project received an extra *Riksdag* grant for a more detailed planning period. After a year's further planning, including major negotiations between *SNV*, *NFR* and *Skogshögskolan*, the project, now named *Barrskogslandskapets ekologi* (*Barrskogsprojektet* in short), was eventually started in the summer of 1972.

With the establishment of *Östersjöprojektet* in 1971 and *Barrskogsprojektet* in 1972, large scale ecosystem studies had become a major part of Swedish science research policy. Within a few years ecosystem research activities had grown from local initiatives by groups of fourth generation ecologists to be the flagship of national ecology policy.

The authorization of the national social order of ecology was largely identical to the establishment of the large scale ecosystem projects. In other words, the ecologization of Swedish natural science was carried through by the launching of ecosystem analysis as the instrument for the salvation of the Swedish environment. This stance was widespread among leading Swedish ecologists at the time. For example, several assessors for the new donation chair in »ecological environmental management« at *Lantbrukshögskolan* created in 1973 understood it as a chair in ecosystem ecology:

»The achievements should... be investigated... chiefly at the level of the ecosystem«,
one of them said, and another emphasized that

»the object of research is the landscape as environment, it is practically ecology at the ecosystem level«.[338)]

In fact, on the part of *NFR*, ecology was equated with ecosystem analysis for most of the 1970s. The third generation ecologists who were members of the Council withdrew between 1972 and 1974, and were replaced by two fourth generation ecologists, Nils Malmer and Bengt-Owe Jansson, both closely associated with the former local ecosystem projects. They were the first of the fourth generation ecologists to make their way from naturalist obscurity to leading positions in Swedish natural science, and within the Council they maintained to equate ecology and ecosystem analysis with solutions to major physical planning problems until the late 1970s.[339)] Thus, research funding under the heading »Ecosystem structure and function« continued to be one of the three top-priority research areas in the five-year plan for the biological sciences for 1976-1981, besides »Structure and function in mammal cell genes« and »Mechanisms of cell differentiation«.

Thus, with the large scale ecosystem research policy the ecologization of Swedish natural science which had started with the first claims for ecology as an independent botanical speciality around the turn of the century 1900, had reached its climax.

337. *Utbildningsutskottet* (the *Riksdag* Educational Committee) 1972:22,p.6.
338. Jo 10/8 1973:1; assessments made by S.L.Jansson and Arne Lindroth.
339. The ecosystem policy climaxed around 1977-78 (see the annual report of *NFR* 1977/78,p.11).

»A basic ecological view« of Sweden

Ekologikommittéen's successful rapprochement with politicians was greatly facilitated by the fact that the contemporary political consciousness was receptive to the notion of a »basic ecological view« (Sw. »ekologisk grundsyn«) of political and physical planning.[340] In essence, it was an old theme, of course, going all the way back to the ideas of »rational agriculture«, »rational forestry«, »rational fisheries« , etc, so prevalent in late 19th century discussions referred to in Chapter 1. In its modern form – substituting »ecological« for »rational« – it had been advocated several times through the 1960s, especially by people in *NFR's naturresurskommittée* and *1964 års naturresursutredning* (cf.4-4). But the great political breakthrough of the notion, and with it the breakthrough of ecological rhetoric into larger political circles, came with the report of a governmental commission on natural resource management and physical planning in 1971.[341]

The task of physical planning, said the Commission, is to balance conservation and exploitation demands, a balance which has to be founded on

> *»actions which appear rational from an ecological point of view and a long-term perspective«.*[342]

In a special section on »ecology and the physical planning of society«, the Commission considered ecology to be identical to ecosystem ecology. »It is pressing«, the Commission said,

> *»that future physical planning, and in addition all social planning, be founded on a more coherent ecological basis«.*[343]

Where did the Commission get its notion of a »basic ecological view« of physical and social planning? One of its central experts describes the existence of a loose network of ecologists around the Commission, helping it to translate the issue of central planning of physical resources into ecological terms:

> »/We/ *had talks with Folke Andersson,* /we/ *were in oral contact with Bengt Lundholm,* /we/ *talked with* /Alf/ *Johnels and Sören Svensson, Mats Ohlson, Kjell Engström, Bengt-Owe Jansson,* /Nils/ *Malmer,* /Tryggve/ *Troedsson.* /Carl Olof/ *Tamm,* /Malin/ *Falkenmark,* /Svante/ *Odén, Bengt Pettersson... The essence of these discussions was included in the report, there were so many contacts back and forth, you could not isolate any unequivocal routes of influence...* /for/ *the basic ecological view«.*[344]

The Commission's report provided the foundation for a governmental bill, sanctioned by the *Riksdag* in 1972:

> *»An application of an ecological viewpoint means...that national physical planning shall contribute to a development of society within the framework set by natural resources and the natural environment, and that the diversity of natural ecosystems is retained«.*[345]

With this *Riksdag* decision, the rhetoric of ecology had even captivated the central political authorities.

340. An excellent discussion of the notion is found in Emmelin 1982; a more critical approach is taken by Herrmann 1979.
341. SOU 1971:75.
342. Ibid.,p.51.
343. Ibid.
344. Interview with NN 30/3 1981.
345. *Proposition* 1972:111.

Furthermore, physical planning at the local and county levels was increasingly defined in terms of ecology. A number of local projects were started to see how a basic ecological view could be applied to a municipality; a number of municipalities even appointed »municipal ecologists« (Sw. »kommunekologer«).[346] The notion of a »basic ecological view« also became widely used in the 1970s by the environmental and nature conservation movements, e.g., *Svenska naturskyddsföreningen*. A detailed analysis of the emergence of the ecological mass movement during the 1970s is beyond the scope of this treatise, however. Suffice it to say that the leading organization of the naturalist movement's young avant-garde, *Sveriges fältbiologiska ungdomsförening*, adopted an ideological platform within the frame of a »basic ecological view« in 1976-1978.[347]

Despite their adoption of ecological rhetoric in the discussion of the proposed coniferous forest ecosystem project in 1972, the political parties generally took a more agnostic attitude towards ecology, generally being rather sceptical of placing »ecological principles« above the political process. That applies especially to the Social Democratic Party. On the other hand, their scepticism did not prevent them from taking up the ecological challenge now and then; for example, the Social Democratic Minister of Housing, Ingvar Carlsson, addressed *SNF*'s annual conference in 1975 with a speech on »Ecology and social planning«.[348]

But otherwise it was not the Social Democratic Party but the Center Party (formerly the Farmers' Party) that stood out as the major »ecological« political party during the 1970s; their journal *Politisk tidskrift* was full of allusions to a »basic ecological view« and »ecological planning«. Having joined the new right-wing coalition government in 1976, a new natural resource- and environmental committee was appointed in 1978 in order to carry through the intentions of the governmental bill of 1972 referred to above:

> *»For the future development of society an application of an ecological viewpoint is necessary to a greater extent than hitherto«.*[349]

A journalist who specialized on environmental problems summarized the whole issue in 1975:

> *»Environmental protection is getting a new content. Nowadays it shall be called ecology. The demand that a basic ecological view should characterize politics is growing stronger and stronger. Some people mean that political power should be subordinated to 'ecological principles'. That is absurd, the social planners say. But the environmental movement is getting politicized«*[350].

The ecologization of social and physical planning was not just a prestige issue for political parties, movement organizations and commissions, however. Scientists too took their own ideas of ecological planning (as expressed in *Därför ekologi*) seriously. A so-called Gotland project,

346. E.g., the »municipal ecologist« in Växjö was supposed to »administer environmental questions... and other natural and landscape planning issues in connection with physical planning« (newspaper advertisement, Jan. 1984).
347. Anon. 1976.
348. Mimeo, 14/11 1975 (in the archives of *SNF*).
349. Instruction to *Naturresurs- och miljökommittéen*, quoted by Emmelin 1982,p.1.
350. Berglund 1975.

> *»a regional systems study of energy, economic and ecological aspects of the island of Gotland, Sweden«,*

being a direct application of the Askö laboratory version of Odum's regional ecosystem energy flow studies in Florida, was launched by Askö laboratory collaborators in 1976.[351)]

The efforts to ecologize Sweden found many expressions. For example, Per Brinck and Bengt Lundholm worked for the setting up of a secretariat for international ecology under the joint auspices of *NFR* and *SIDA* (the Swedish International Development Authority), directing its activities »toward tropical and subtropical areas«.[352)] And in 1973, as already mentioned, a donation chair in »ecological environmental management« was created at *Lantbrukshögskolan*.[353)]

Thus the 1970s were a decade of thorough-going ecologization, not only of academia, but of the whole of Swedish society as well. The planning of Sweden was supposed to be an application of ecological principles.[354)]

In fact, however, these thought elements has quite old roots. In the 1930s, both Friedrichs and Thienemann in Germany considered ecology to be the science above all sciences. To Thienemann ecology was not simply one out of many biological disciplines, but a truly synthetic science:

> *»Umschiesst die allgemeine Ökologie alle Naturwissenschaften, so treten auch die Methoden aller Naturwissenschaften in die Dienst allgemeinökologischer Forschung. Ihr eigen aber ist ganzheitliches Denken«,*[355)]

which offered a possibility for

> *»die grosse historische Stunde ganzheitlicher Zusammenschau«.*[356)]

And accordingly ecology ought to play a central role in contemporary culture. Consequently Thienemann, by no means unaffected by Nazi ideology, thought it necessary

> *»die Gesamtheit des Volkes mit den Grundlehren der allgeimeinen Ökologie immer mehr vertraut zu machen«.*[357)]

In other words, the ecologization of society, in Thienemann's *Weltanschauung*, was the means to a truly rational planning of the whole of social and natural life; that is, his view of ecology seems to have combined a populistic tinge with elitism, to constitute the natural ideology of bureaucratic planners and centralizers (cf. Epilogue). The view of ecology forwarded by the core group behind the establishment of the national social order of ecology in Sweden accords on all essential points with Thienemann's views.

351. »Gotlandsstudien; en presentation« and other mimeographed material from the project (in the custody of A.-M.Jansson).
352. Leaflet from the secretariat, printed in 1972 (in the custody of Bengt Lundholm).
353. Jo 10/8 1973:1; its first holder was Eliel Steen, an earlier student at »*Växtbio*« in Uppsala (cf.4-2).
354. We have no measure of the extent of belief in an ecological basis for planning. With respect to England, however, Elkington and Roberts (1977,p.212) suggest that 89% of planners consider ecology to have developed to a level making it directly applicable in the planning process.
355. Thienemann 1939,p.280.
356. Ibid.,p.281.
357. Ibid.,p.283.

4.6 Swedish post-war ecology: concluding remarks

During the 1950s and 1960s the previously scattered individual claimants of ecology, and the insignificant, small and local social orders of ecology in Sweden grew, in number as well as in enrolment power. By the late 1960s several hundred scientists at the five universities pursued scientific activities under the label of ecology. Undergraduate courses, research reports and seminars were announced as ecological. Ecology provided the banner for the mass enrolment of naturalist students to scientific studies and research. And gradually these activities were authorized as well – beginning with the chair in forest ecology at *Skogshögskolan* in the late 1950s, followed by the two chairs in animal ecology and plant ecology at the new university in Umeå in the mid-1960s, most of these local social orders of plant ecology and animal ecology were awarded specifically designated chairs by the end of the 1970s.

Simultaneously, a national social order of ecology emerged during the 1960s. Natural resource management research was to an increasing extent sponsored by central authorities in terms of ecology. The great, centrally controlled, university reforms of the late 1960s included new ecological chairs and new undergraduate ecological curricula. The natural science research council secured the establishment of a national knowledge potential in the field of ecology, and from around 1970 a new strategy for the build-up of ecology was laid – the large scale ecosystem research policy. Throughout the 1970s ecosystem projects were a dominating feature of Swedish ecology. Up to 15% of the total funds of the natural science research council was alotted to ecological research, including ecosystem research, in the mid-1970s.

Finally, not only was the social order of ecology authorized by universities and national science policy authorities. Wider sectors of Swedish society became pervaded by ecological perspectives as well. »A basic ecological view« of social, economic and physical planning was widely spread during the 1970s, not only in scientific circles, but among social and physical planners, and in the central administration, environmental organizations and many political parties as well.

This rapid and extensive ecologization of post-war Sweden, had, of course, its parallels abroad. For example, the picture of post-war ecology depicted above bears certain similarities to the picture of post-war British ecology given by Lowe and Worboys.[358] The *Nature Conservancy*, like its Swedish counterpart *Naturresurskommittéen*, was a skilled agent for ecology. Originally the Conservancy had control over quite small funds, but thanks to effective lobbying and dexterous work in popularizing ecology (cf. *Därför ekologi*) as a remedy for the environmental crisis, the Conservancy succeeded in strengthening national ecology. In addition, the authorization of ecology was followed by an ecological ideology stressing social usefulness and a unified ecological world picture – this ideology was, according to Lowe and Worboys, of essential value for establishing coherence and a feeling of collective identity among its practitioners.

The post-war boom in ecology begs the question of the mechanisms behind it. In one

358. Lowe and Worboys 1976.

sense, the common assertion that the rapid rise of ecology was a consequence of the environmental crisis is not supported here. As we have seen from Sections 4-2 and 4-3 above, environmental problems only rarely motivated local claims for ecology. Some ecologists were inducted into ecology while working on environmental problems; but the spectacular increase of ecological claims at local university departments in the late 1950s and early 1960s obviously had no specific connection to the environmental crisis or environmental debate. After all, the environmental crisis of the 1960s was nothing new. Environmental deterioration has been an integral part of human history – forest devastations in ancient China and in the Mediterranean area, and »The Great Stink« in the London area in the 19th century are but a few examples of the permanent environmental crisis of mankind.[359] As indicated in preceding chapters, environmental deterioration had been a recurrent topic of concern for Swedish scientists, foresters, fishery biologists etc., since the 19th century. None of these events gave rise to a scientific ecology.

Therefore, the historical circumstances rather seem to justify the general analytic perspective adopted here: that is, the ecologization process of the 1950s and 1960s is interpreted as an enrolment process, whereby third generation ecology actors enrolled a fourth generation of ecologists, by translating naturalist interests into the language and problem sphere of scientific ecology. The growth of local social orders of ecology is seen as the consequence of this identification and translation of the naturalist interest of a rapidly increasing number of students. Thus the rapid development of post-war ecology was first and foremost a consequence of a naturalist mass movement in the universities.

On the other hand, the rapid emergence of an authorized national social order of ecology during the 1960s seems to have involved the identification of other, and more powerful, interests. The appearance of a sufficient number of articulate ecologists on the national science policy scene made it possible to translate the concern for the environment and the deterioration of the nation's natural resources into the language of ecology. The expanding social order of ecology promised a specific scientific solution to the ghost of general pollution and poisoning threatening the basis of society; the ecologists succeeded in defining the environmental crisis as an ecological problem. Hence, the authorization of a national social order of ecology in the 1960s first and foremost involved the identification and translation of agencies basing their existence on environmental interests. State officials, parliamentary politicians, and concerned citizens, etc., had no *a priori* reason for being interested in ecology. But the ecologists found a reason. Thus, ecologization of the larger society, the attention to ecology shown by well-informed citizens, journalists and followers of the environmental movement should be seen as a result of the ecologists' efforts in enrolling and enlisting them.

The identification and translation of the environmental interest and the creation of a national social order of ecology was largely an outcome of the initiatives of a handful of third generation ecologists. As we have seen, the main actors were a core group of ecologists working within the confines of a small number of policy bodies, including *1964 års naturresursutredning*, and *1965 års biologiutredning*, and two committees of the natural science research council (*NFR*), viz., *Naturresurskommittéen* and *Ekologikommittéen*. Within a few years this core group appeared as the major agent in the authori-

359. See, e.g., Bilsky 1980.

zation of ecology at the national level. The two leading figures were the Lund animal ecologist Per Brinck and the science administrator Bengt Lundholm – in the ecologists' gossip they are repeatedly referred to as »the Godfather« of Swedish ecology, and »His Master's Voice«, respectively. Together with the rest of the core group, these two men largely determined the course of the establishment of the national social order of ecology in Sweden, and, although to a lesser extent, even the course of establishment of local social orders of ecology.

A similar argument concerning process of interest identification and translation vis-á-vis agencies involved in the post-war reform of the universities could be made. Thus, the concern among third and fourth generation ecologists for solving larger social and environmental problems should be seen against the background of Social Democratic policy for the reform of higher education, orienting it towards societal concerns. In 1945, Gunnar Myrdal, by then one of the leading European Social Democratic intellectuals, published a pamphlet advocating the break-down of the »old academic class... /and/ aristocracy«.[360] Myrdal depicted a conflict between young scientists interested in working on problems relevant to society and the entrenched professorial core, only interested in protecting their privileges. By supporting directly the new and youthful scientific forces, the state might by-pass the conservative professorial elite. Several details of the story presented above give evidence to the assertion that the emergence of post-war ecology involved a similar by-passing of the traditional botanical and zoological professorial elites (cf. also 3-5). Some of the major actors in this story, for example Per Brinck and Bengt Lundholm, seem to have acted in a similar manner. Brinck's intimate contacts with leading Social Democratic politicians is testified by interviwees. Lundholm's ideas of an active research secretariat epitomizes the most radical Social Democratic views of the possibilities of steering science. Their vision of ecology was that of a science devoted to the solution of the major problems of mankind.[361] The publication of *Därför ekologi* and the establishment of the ad-hoc subcommittee on terrestrial ecology set up by *Ekologikommittéen* in 1970, also bear witness to the alliance between, on the one hand, national environmental and university reform agencies, and, the young avant-garde of the social order of ecology on the other.

The identification of the environmental issue and its translation into the language of ecology, was largely, but not exclusively, the work of third and fourth generation ecologists adhering to a descriptive field approach. Ecology, to them, was largely the application of standardized methods of observation and correlation to field studies of animals and plants. Those third and fourth generation ecologists who leant towards experimental studies were not in the forefront in identifying environmental problems. In one respect, however, the experimental ecologists, so active in the inter-war period, contributed to the establishment of the post-war national social order of ecology. The reform of higher education and research involved not only a reorientation of botany and zoology towards practical problems, but also a reorientation towards modern, experimental, so-called »functional« biology, in contrast to what was characterized as an old-fashioned descriptive botany and zoology. Some of the actors discussed above, with

360. Eyerman 1985.
361. Brinck always kept to his view. E.g., in 1976, as a chief editor of *Oikos*, he invited a series of papers on the ecology of human development, particularly in the Third World. The introductory commentaries to the papers expressed a Fabian view of the possibilities of ecology to solve the miseries of human affairs.

connections to the tradition of experimental ecology, were among the initial proponents of ecology as a means of reforming the largely descriptive botany and zoology into »functional« ecology.

Although the old debate between descriptive field ecologists and more experimentally oriented ecologists did not assume the same proportions as in the inter-war period, it nevertheless remained a latent source of disagreement. With the establishment of large-scale ecosystem research projects around 1970 the conflict seems to have been finally superseded, however. We have already indicated that several members of *NFR* were »scared to death« at the thought of a continued descriptive ecology. The large-scale ecosystem projects in fact promised a possibility for combining naturalist field work with a new kind of scientific conduct. Modelling and simulating an ecosystem, in particular, seemed to introduce an experimental touch to descriptive field work, a new rigour and a new causal sensibility.[362] Thus, the ecosystem projects stood out, in the minds of the leading science reformers, as the first serious attempt to organize a true scientific ecology. Ecosystem theory seemed to give to ecology a set of concepts and ideas, which was its own and not borrowed from other disciplines. This helped to establish the status of ecology as an independent scientific discipline rather than a specialty, whether of botany or zoology. The theory of the ecosystem thus accentuated the theoretical and scientific character of the new scientific social order.

362. Matematisk modellering 1976. See further Wallén 1980.

Epilogue

As indicated in the Prologue, the aim of this treatise has been historical, not analytical, albeit informed by a rather specific analytical scheme. The point of departure has been to view the rise of ecology as one aspect of the scientification of society. The ecologization of Sweden has been interpreted as the rise of a discursively conscious ecology, in contrast to »proto-ecological« practices. I have focused particularly on scientists who specifically claimed as »ecological« studies of animals and plants in their natural surroundings, and on the processes whereby individual claims for ecology grew into a larger social order of ecology, locally as well as nationally. The establishment of a social order of ecology has been accounted for in terms of enrolment, that is, the process by which ecology actors identify certain issues and interests, and translate them into the language and rhetoric of ecology. This includes the interests of prospective members of the social order, and the interests of larger social agents authorizing it.

In the preceding four chapters we have seen how the little word ecology, uttered in passing by a few field botanists around the turn of the century, spread through 20th century Sweden, and eventually turned out to be a rhetorical device for reconstructing the nation. An enormous gulf separates the claim for ecology as the science which

> *»must guide and steer.. a future forming science.. the active, dynamic participator in the game of the future of humanity and the earth«,*

and whose most essential goal is to

> *»create a rational and objective foundation for a goal directed future«,*[1]

from the modest claim for ecology as an aspect of the scientific study of plant distribution forwarded by Eugenius Warming and his Swedish followers barely 70 years earlier. The bridging of this gulf is what constitutes the story of the ecologization of Sweden.

The comprehensibility or in-comprehensibility of historical events is the ultimate test of the analytical scheme used here. Therefore I will abstain from trying to provide a conclusion to this story. However, one pervading characteristic feature of Swedish ecology warrants a few comments, viz., the distinction and conflict between, and later reconciliation of, two different approaches to field studies of animals and plants.

Before the late 19th century field studies of animals and plants were an integral part of the activities of natural historians. During the 19th century, however, this all-round pursuit was gradually forced out of academia and replaced – first by a specialized systematical museum botany and zoology, later by the »new German« laboratory botany and zoology. As a consequence, field studies were marginalized to amateurs and to practical applications, such as forestry and fishery research.

1. Sören Svensson *in* Lundholm (ed) 1971,p.76. (See 4-5).

In the late 19th century, field studies were revitalized at the universities under the influence of Darwinian ideas of adaptation and evolution by descent. A renewed approach was taken by men who were trained in descriptive and systematical museum botany and zoology – the study of animal and plant geography, animal and plant community structure and immigration history went hand in hand. Although more accepted in academia than the old natural historical approach, the new plant geography was nevertheless rated as a secondary pursuit compared to the modern »new German« botany and zoology.

The discrepancy between descriptive field workers and laboratory botanists and zoologists is not an unknown theme in the history of biology. In fact, Ernst Mayr postulates the existence of a fundamental »experimentalist-naturalist dichotomy« in 19th century biology.[2] Likewise, Garland Allen maintains that

> *»...experimentalists and naturalists looked at the world in very different, and at the time in what appeared to be mutually exclusive, ways«.*[3]

In Allen's view the dichotomy often took the form of direct conflict. The present study indicates that conflict between the two approaches among Swedish botanists and zoologists was a predominant feature of Swedish academic life. It will be recalled that Rutger Sernander and other field botanists were ranked low by the »new German« laboratory botanists in a professorial competition around the turn of the century, and that the leading comparative anatomist, Vilhelm Leche in Stockholm, abhorred simple, descriptive field studies of animals.

Drawing on his studies of Thomas H. Morgan and early genetics, Allen asserts that »probably nowhere were the battle lines drawn more sharply than in the study of evolution and heredity«. As demonstrated in the present study, however, the principal standpoints of the fundamental »experimentalist-naturalist« dichotomywere transferred to the area of field studies itself. And, in fact, conflicts over different approaches to field studies were at any rate as fierce as those discussed by Allen.

The paradigmatic example of this conflict between two approaches to field studies is the »Great Polemic« between the Uppsala and Stockholm schools during the 1920s and early 1930s. This conflict concerned field studies of plants and plant communities (discussions of a similar kind, although not so fierce, were carried over different approaches to field studies of animals and animal communities). At Sernander's plant biology seminar in Uppsala, Thore Fries and Einar Du Rietz, both gifted systematicians but ignorant experimentalists, claimed plant sociology as a descriptive and inductive approach to field studies of plant distribution. The main spokesman of the Stockholm school, Henrik Lundegårdh, a gifted laboratory scientist but careless with regard to systematical problems, advocated experimental and physiological studies of plant distribution. The Great Polemic climaxed in the early 1930s, when the main representatives of the different approaches applied for the chair in plant biology in Uppsala.

This takes us to the issue of ecology. Different views of the scope and content of (plant) ecology was an integrated element in the Great Polemic. Second generation

2. Mayr and Provine 1980.
3. Allen 1979,p.179.

members of the Uppsala school had claimed studies of the correlation between environmental factors and the distribution of plant communities as synecology. In contrast, the Stockholm school claimed causal, experimental and physiological studies of plant adaptation as ecology.

These opposing claims for ecology was not the main issue in the Great Polemic, however. The two approaches to field studies of animals and plants were not based on ecological arguments. In fact, the Uppsala school tended to disregard ecological questions in favour of plant sociological or even systematical problems. Likewise, the experimentalists tended to turn to physiological and even biochemical problems.

What then was ecology's place in this version of the »experimentalist-naturalist dichotomy«?

In order to answer this question I will accede to a second point made by Allen. Taking the Danish geneticist Vilhelm Johannsen as a paradigm example, he points to the historical fact that the dichotomy between the two traditions in studies of heredity and evolution were gradually transcended. Similarly, we may disclose a similar tendency to transcend the two conflicting approaches to field studies of animals and plants. This transcending, or reconciling tendency was largely identical with the unequivocal claim for ecology as an independent scientific social order.

Even the pioneer claimants of ecology, although not yet establishing an ecological discourse, exhibited such a tendency towards reconciliation of the contending approaches to field studies. To men such as Sven Ekman and Rutger Sernander, the geographical distribution of animals and plants was the basic problem. They refined descriptive studies of organism distribution and organism communities. But in adding the adjective »ecological« to their approach (ecological animal geography and ecological plant geography) they adopted the Darwinian idea of morphological adaptation, itself an offshoot of the »new German« botany and zoology. On the other hand, men such as Henrik Hesselman, who was trained in the »new German« botany, chose to take up field studies – to him ecology implied adopting a physiological approach to deepen the understanding of the mechanisms of plant adaptations. Likewise, Nils Holmgren used the term ecology to denote the mixture of morphological and field studies.

The first consistent claim for ecology as a reconciliation of the two conflicting approaches to field studies is evident in the works of Lars-Gunnar Romell in the 1920s and 1930s. It will be recalled that Romell, being one of the main figures in the Great Polemic, opposed both Du Rietz's descriptive, inductive anti-experimentalism and Lundegårdh's blind faith in new apparatus and physiological measurements. While Du Rietz wanted to observe the results of »nature's own experiment«, and Lundegårdh advocated moving »the laboratory out in the wilds«, Romell proposed ecology as an »experiment with nature«.

Romell's claim epitomizes the essence of the emerging social order of ecology – a reconciliation of the two contending approaches to field studies of animals and plants. That is, ecology as the combination of the naturalist's sense of the uniqueness of the single organism and its reactions to the environment, and the experimentalist's sense of natural science as an activity of hypothesis making and experimental testing. In Romell's vision of nature, ecology was a combined narrative and experimental enterprise.

During the 1930s and 1940s the number of individual claims for ecology grew steadily, and a number of local social orders of animal and plant ecology were established. Likewise, an increasing number of scientists claimed ecology as a combination of descriptive field studies with experimental methods and physiological thinking. Gottfrid Stålfelt made an official plea for joining the two lines of research. Among third generation plant ecologists, Carl Olof Tamm at *Skogsforskningsinstitutet* took up Romell's heritage: trained in plant physiology by one of Lundegårdh's students, he returned to field studies under the auspices of one of Sernander's students. At Uppsala, Arne Lindroth tried to apply advanced physiological methods to solve problems of animal geography, and at the Department of Zoology in Lund, Ivar Agrell and Helge Backlund, main figures of the local social order of animal ecology, combined animal community studies with preference and tolerance experiments. Several other examples could be mentioned.

The first step towards the institutionalization of the reconciliation between the two claims was taken with the creation of *Oikos; Scandinavian journal of ecology* in 1949. The signatories of the *Oikos* initiative included the whole spectrum of ecological claims from the Great Polemic, ranging from Stålfelt's claim for ecology as an experimental ecophysiology, to Du Rietz's and Karl-Herman Forsslund's descriptive and comparative approach.

During the post-war period the establishment of a naturalist mass movement led to the massive recruitment of naturalist students to the universities, resulting in a rapidly growing number of individual claimants of ecology, a lively ecological discourse, and the establishment of local social orders of ecology at all the universities. But the influx of student-naturalists was probably also the main reason why, by the mid-1960s, descriptive approaches again seemed to be the dominant ones. The experimentalists' claim for plant ecology stood rather weak by the mid-1960s.[4] In local departments plant ecology was largely identified with a naturalist pursuit. Likewise, a majority of the claims for animal ecology forwarded during the 1950s and 1960s concerned descriptive field studies.

This renewed predominance of the naturalist claim for ecology was again to be challenged. We have already indicated that several members of *Naturvetenskapliga forskningsrådet* were »scared to death« at the thought of a continued descriptive ecology. Several attempts were made to inoculate naturalist ecology with an experimental bias. For example, in the early 1960s Per Eric Lindahl tried to mix experimental and field approaches when planning the new independent ecological chairs. But the final attempt to reconcile the two contending approaches was made by *Ekologikommittéen* around 1970, proposing a large-scale ecosystem project policy.

The large-scale ecosystem project policy promised combining naturalist field work with a new kind of experimental approach. Field botanists, field zoologists and physio-

4. A symptomatic example is the single case of plant ecological graduate research made in Göteborg in the 1950s and 1960s – Hans Edsbagge, a student of Levring – who, although trained in an ecophysiological tradition, nevertheless preferred to do a field study (cf.4-2, note 52).

logists were thought to work side by side. Modelling and simulation techniques in particular seemed to introduce an experimental rigour to descriptive field work. Simultaneously, the ecosystem projects in their methodology and the degree of formalization and mathematization involved, stood out in the minds of the leading science reformers as the first serious attempt to organize a truly scientific ecology. The symbolic value of the large-scale ecosystem projects was increased by the feeling that ecosystem research was guided by due regard to theory and experiment. While many ecologists had considered ecology to be more of a viewpoint than a science, ecosystem theory seemed to give to ecology a distinct and central set of concepts and ideas, which were peculiarly its own and not borrowed from another discipline (such as physiology or biogeography). This helped to establish the status of ecology as an independent scientific discipline. The theory of the ecosystem, and hence ecosystem research, thus accentuated the theoretical and scientific character of ecology. Hence the establishment of large-scale ecosystem research projects around 1970, implied both the final authorization of ecology as an independent science and the final reconciliation of this Swedish version of the »experimentalist-naturalist dichotomy«.[5]

It is tempting to compare this thesis of ecology as a social order that reconciles two different approaches to field studies with Alvin Gouldner's thesis of ecology as an ideology bridging two different factions of a new intelligentsia class.[6] As already indicated in the Prologue, a class is a social order at a high level of aggregation. In *The Future of Intellectuals and the Rise of the New Class* of 1979, Gouldner suggests that »environmentalism - ecology« and »general systems theory« are two newer forms of the ideology of an historically new class. The multi-science character of the new ecological ideology, says Gouldner,

> *»provides an ideological framework that can unite various types of technical intelligentsia. At the same time its rejection of the idea of domination of nature, its intimation of a husbanding and indeed of a return to 'nature' is also attractive to many humanistic intellectuals«.*[7]

Like the new ecology, systems theory embodies a new vision of unity, Gouldner continues. But

> *«if ecology is grounded in organismic metaphor and has romantic antecedents, systems theory resonates a mechanical metaphor more continuous with the technocratic consciousness and, unlike ecology, embodies a humanistic imperialism centered on the impulse to manage (dominate) the environment. If ecology has a strong populistic tinge, systems theory is imbued with a stronger elitism, being 'the »natural« ideology of bureaucratic planners and centralizers... '»*[8].

Both ideologies, Gouldner says, may be understood as different efforts to bridge the various competing and divergent factions of the new class. While

> *«systems theory's elitism... narrows the social solidarity that it can foster, limiting it to the technical intelligentsia at best... ecology's capacity for fostering unity, while also grounded in a multi-science view, is, at least in some of its versions, open to a larger constituency and is potentially productive of a broader solidarity inclusive of humanistic no less than technical intelligentsia».*[9]

5. In another view, Lowe and Worboys (1976) assert that ecology of the 1960s restored the authority of the natural sciences in a society increasingly hostile to science. In their opinion, ecology and systems theory might give rise to a new »unified science«, restoring the leading position of science in society.
6. The notion of the intelligentsia as a new class has been discussed at length in the literature and will not be taken up here; cf., e.g., Eyerman *et al* in press.
7. Gouldner 1979,p.42.
8. Ibid.,pp.42-43; the quotation within the quotation is from Lilienfeld 1978,p.263.
9. Ibid.,p.43.

Indeed, Rutger Sernander stands out as a paradigmatic example of a man who combines ecology as an elitistic «scientific natural history», manifested in his views on nature conservation, and a populistic love of rural districts. In a similar way, Lars-Gunnar Romell was able to combine a sophisticated mathematical approach to nature with a caring attitude to the cultivated landscape. The authors of *Därför ekologi* of the early 1970s turned their attention both to systems theory and to the larger constituency of the environmental movement. Numerous similar examples have been given in the text.

Indeed, it would be possible to interpret the entire process of ecologization of Sweden, from Sernander's, Hesselman's and Ekman's work at the turn of the century, to the establishment of the large-scale ecosystem projects as instruments for the planning of Sweden, as part and parcel of a more general process of reconciliation of humanistic intellectuals with the technical intelligentsia. However, the aim of this essay has been to make a preliminary survey of the ecologization of Sweden, not to reconstruct it in accordance with Gouldner's new class thesis. Nevertheless, I think it would be worthwhile in future research, to implement the main core of the new class thesis into a reconstruction of the ecologization process.[10]

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This story ends with my first personal encounter with the ecologists – i.e., the establishment of the large scale ecosystem projects in the early 1970s and the ecologization of society at large. While the pioneers at the turn of the century had only caught the interest of a few professional botanists, their fourth generation successors spread the word ecology to the whole of society, translating the widely diverse interests of thousands of fellow scientists, state bureaucrats, and environmental movement radicals. At the turn of the century the man in the street knew nothing of ecology – three quarters of a century later he was enlisted to the daily gospel of ecology through the mass media.

What happened thereafter is not part of the story. Suffice it to say that ecosystem ecology, having been a rhetorical tool in the authorization of the national social order of ecology, has given way to a new ecology – the so-called «theoretical ecology», or «evolutionary ecology». It has been taken up by a fifth generation of young ecologists born in the 1940s and 1950s[11]. «The men (and women!) of the 1970s» have been very successful. During the last ten years the new ecology has invaded almost all ecological departments in the country. Practically nobody pursues ecosystem studies anymore. Instead, animals and plants are discussed in terms of evolutionary «life strategies», an approach astonishingly similar to Gottfrid Adlerz's and Axel Lundström's a century ago. The whirl seems to have made a full turn.

The switch to an «evolutionary» language in ecology seems to be accompanied (no

10. A provisional discussion of systems ecology from the standpoint of the new class thesis is offered by Emmeche *et al* 1981 and Söderqvist 1981b.
11. An early appeal for evolutionary ecology in Sweden was made by Christer Wiklund, a fifth-generation Stockholm animal ecologist (Wiklund 1975).

causality is intended here) by a stagnation of enrolment into the social order of ecology. Actually, the number of students «going ecology» declined already during the early 1970s. Likewise, enrolment into ecological graduate work has decreased. Ecology no longer appears to be the exciting scientific (or political?) frontier. Lost are the days, only a decade ago, when the young fourth generation teaching assistants and their curious students explored the brand new textbooks and went out into the wilds together. Ecology has been routinized. The revolutionary movement of fourth generation ecologists has already created its new administrators. This is, of course, the inevitable outcome of the authorization of any (scientific) social order.

The new evolutionary ecology seems to have many characteristics which makes the pursuits of this fifth generation of ecologists qualitatively different from their immediate predecessors. They seem to do ecology for fun only, indifferent to practical problems, including the salvation of the nation. They are mathematically and theoretically sophisticated, sitting indoors calculating on computers, rather than travelling out in the wilds. They are individualists, abhorring the idea of large-scale ecosystems projects. Indeed, the transition from ecosystem ecology to evolutionary ecology seems to reflect the generational transition from the politically conscious generation of the 1960s to the «yuppie» generation of the 1980s.

In fact, this generational shift has recently caught the attention of new class theorists.[12] Hence, the new evolutionary ecology poses fascinating problems, not only for a theory and history of ecology, but also for a further development of the new class thesis. I will refrain from trying to say anything substantial about these events, however. Perhaps this preliminary essay on the ecologization of Sweden up to the 1970s will stimulate some of the practitioners of the new evolutionary ecology to reconsider and reinterpret the history of Swedish ecology from the horizon of the «yuppie» generation.

12. Bill Martin, Department of Sociology, University of Wisconsin, Madison, is presently confronting the new class thesis with the «yuppie»-phenomenon.

Literature

In addition to works quoted in the text, a few works of general interest (bibliographies, general reviews and biographies) have been included. Non-printed sources (mainly *konseljakter*, Cabinet Meeting Acts) are not listed here, but in text footnotes (see Prologue, note 76). For biographical articles only surnames are listed. Journals are abbreviated as follows:

AfB	Arkiv för botanik
AfZ	Arkiv för zoologi
APS	Acta phytogeographica suecica
BihKVAH	Bihang till kungl. vetenskapsakademiens handlingar
BN	Botaniska notiser
ET	Entomologisk tidskrift
GFF	Geologiska föreningens förhandlingar
GKVVH	Göteborgs kungl. vetenskaps- och vitterhetssamhälles handlingar
KFSLF	Kungl. fysiografiska sällskapets i Lund förhandlingar
KFSÅ	Kungl. fysiografiska sällskapets i Lund årsbok
KLHT	Kungl. landtbruks-akademiens handlingar och tidsskrift/ Kungl. lantbruksakademiens tidskrift
KVAH	Kungl. vetenskapsakademiens handlingar
KVAÅ	Kungl. vetenskapsakademiens årsbok
LUÅ	Lunds universitets årsskrift
MBLF	Medlemsblad för biologilärarnas förening
MKL	Meddelanden från kungl. lantbruksstyrelsen
MSS	Meddelanden från statens skogsförsöksanstalt /Meddelanden från statens skogsforskningsinstitut/
MSUFS	Meddelanden från statens undersöknings- och försöksanstalt för sötvattensfisket
MSV	Meddelanden från statens växtskyddsanstalt
OE	Opuscula entomologica
OES	Opuscula entomologica supplement
RIFRD	Reports from the institute for freshwater research, Drottningholm
SBL	Svenskt biografiskt lexikon
SBT	Svensk botanisk tidskrift
SBU	Symbolae botanicae Upsaliensis
SFT	Svensk fiskeritidskrift
SGU	Sveriges geologiska undersökning
SHBKS	Svenska hydrografisk-biologiska kommissionens skrifter
SMT	Svenska mosskulturföreningens tidskrift
SMVKT	Svenska moss- och vallkulturföreningens tidskrift
SN	Sveriges natur
SNÅ	Sveriges natur årsbok
SST	Svenska skogsvårdsföreningens tidskrift
SvN	Svensk naturvetenskap (Naturvetenskapliga forskningsrådets årsbok)
TfS	Tidskrift för skogshushållning
UUÅ	Uppsala universitets årsskrift
ZBU	Zoologiska bidrag från Uppsala
ZR	Zoologisk revy

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List of institutions

(English translations may differ from those officially used).

Abisko naturvetenskapliga station	Abisko Natural Science Field Station
Barrskogsprojektet	The Coniferous Forest Ecosystem Project
Biologilärarnas förening	The Biology Teachers' Association
Biologiska muséet	The Biological Museum
Botaniska sektionen (av naturvetenskapliga studentsällskapet i Uppsala)	The Botanical Section (of the Natural Science Student Society in Uppsala)
Botaniska sällskapet	The Botanical Society
Centralanstalten för försöksväsendet på jordbruksområdet	The Central Institute for Agricultural Research
Civildepartementet	The Ministry for Civil Service Affairs
Collegium Medicum	Collegium Medicum
Domänstyrelsen	The National Board of Crown Forests and Lands
Ekologihuset	The Ecology House (in Lund)
Ekologikommittéen	The Ecology Committee
Entomologiska anstalten	The Entomological Institute
Entomologiska föreningen	The Entomological Society
Experimentalfältet	The Experimental Field Trials
(Lantbruksstyrelsens) fiskeribyrå	The Fishery Division (of the Board of Agriculture)
Fiskeristyrelsen	The Fishery Board
Fisketillsynsmyndigheten	The Fishery Inspectorate
Forstliga försöksanstalten	The State Forest Research Institute (cf. below)
Fritidsutredningen	The Leisure Time Commission
frökontrollanstalt	(county) Seed Control Institute
Göteborgs biologiska förening	The Biological Association in Göteborg
Göteborgs högskola	The University College in Göteborg
Göteborgs kungl. vetenskaps-och vitterhetssamhälle	The Göteborg Academy of Science and Letters
Handelshögskolan	The College of Commerce
Havsfiskelaboratoriet	The Marine Fishery Institute
hushållningsråd	(county) Agricultural Economic Association
Hydrografiska kommissionen	The Hydrographical Commission

Karolinska institutet	The Medical College
Kristinebergs zoologiska station	The Zoological Station at Kristineberg
KVA:s naturskyddskommittée	The Nature Conservation Committee of the Royal Academy of Science
Lantbruksakademien	The Academy of Agriculture
Lantbrukshögskolan	The College of Agriculture
Lantbruksstyrelsen	The Board of Agriculture
Lunds botaniska förening	The Botanical Association in Lund
Manufakturfonden	The Manufacture Foundation
Matematisk-naturvetenskapliga sektionen	The Faculty Section of Mathematics and Natural Science
Miljövårdsberedningen	The Government Environmental Working Committee
Mälarundersökningen	The Lake Mälaren Survey
Naturresurskommittéen	The Natural Resource Committee
Naturvetenskapliga forskningskommitteen	The Natural Science Research Commission of 1945
(Naturhistoriska) Riksmuséet	The National Museum (of Natural History)
Naturvetenskapliga forskningsrådet (NFR)	The Natural Science Research Council
Nordiska kulturkommissionen	The Nordic Cultural Commission
Nordiska muséet	The Nordic Museum
Nordiskt kollegium för ekologi	The Nordic College for Ecology
Nordiskt kollegium för terrestrisk ekologi	The Nordic College for Terrestrial Ecology
Riksarkivet	The National Archives
Riksdagen	The Swedish Parliament
Skogshögskolan	The College of Forestry
Skogsinstitutet	The Forestry Institute
Skolöverstyrelsen	The Board of Education
SNV:s forskningsnämnd	The Research Committee of the National Board for Environmental Protection
Statens fiskerinspektion	The State Fishery Inspectorate
Statens naturvårdsnämnd	The National Board of Nature Conservation
Statens naturvårdsverk (SNV)	The National Board for Environmental Protection
Statens skogsförsöksanstalt	The State Forest Research Institute
Statens skogsforskningsinstitut	The State Forest Research Institute
Statens veterinärmedicinska anstalt	The National Institute for Veterinary Medicine
Statens växtskyddsanstalt	The State Institute for Plant Protection
Stockholms arbetarinstitut	The Stockholm Workers Institute
Stockholms högskola	The University College in Stockholm
Större akademiska konsistoriet	The University Council
Svenska akademien	The Swedish Academy

Svenska botaniska föreningen	The Swedish Botanical Association
Svenska hydrografisk-biologiska kommissionen	The Swedish Hydrographical-Biological Commission
Svenska jägareförbundet	The Swedish Hunters' Association
Svenska mosskulturföreningen	The Swedish Peat Culture Association
Svenska naturskyddsföreningen (SNF)	The Swedish Association for the Conservation of Nature
Svenska turistföreningen	The Swedish Tourist Association
Svenska utsädesföreningen	The Swedish Seed Association
Svenska skogsvårdsföreningen	The Swedish Association for Forest Management
Sveriges fältbiologiska ungdomsförening (Fältbiologerna)	The Swedish Field Biology Youth Association
Sveriges geologiska undersökning	The Swedish Geological Survey
Sveriges ornitologiska förening	The Swedish Ornithological Association
Sällskapet för skogsförädling	The Association for Forest Breeding
Södra Sveriges fiskeriförening	The Fishery Association of Southern Sweden
Tekniska högskolan	The College of Technology
Ultuna lantbruksinstitut	The Agricultural Institute at Ultuna
Undersöknings- och försöksanstalten för sötvattensfisket (Sötvattenslaboratoriet)	The Investigation Institute for Freshwater Fishery (The Freshwater Laboratory)
Universitets kanslersämbetet (UKÄ)	The University Chancellor's Board
Uppsala naturvetenskapliga studentförening	The Natural Science Student Society in Uppsala
Vatteninspektionen	The Water Inspectorate
(Kungl.) Vetenskapsakademien	The (Royal) Academy of Science
Viltforskningsrådet	The Game Research Council
»Växtbio« (Växtbiologiska institutionen)	The Department of Plant Biology (in Uppsala)
Växtsociologiska sällskapet	The Plant Sociological Society
Östersjöprojektet	The Baltic Ecosystem Project
1931 års läroverkssakkunniga	The Secondary School Commission of 1931
1933 års universitetsberedning	The University Commission of 1933
1936 års lärarutbildningssakkunniga	The Secondary School Teacher Educational Commission of 1936
1945 års universitetsberedning	The University Commission of 1945
1946 års naturskyddsutredning	The Nature Protection Commission of 1946
1955 års universitetsutredning	The University Commission of 1955
1960 års naturvårdsutredning	The Nature Protection Commission of 1960
1964 års naturresursutredning	The Natural Resource Commission of 1964
1965 års biologiutredning	The Biology Commission of 1965

Name index

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TO SAVIOURS OF THE NATION
A Sociologically Informed Narrative Survey of the Ecologization of Sweden 1895 - 1975.

av

Thomas Söderqvist

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ABSTRACT
This work describes the emergence of ecology in Sweden throughout the 2oth century. To this end a specific analytical scheme has been used. Ecology is seen as an aspect of the scientification of society. The ecologization of Sweden is interpreted as the rise of a discursively conscious ecology, in contrast to "proto-ecological practices. The work focuses on scientists who specifically claimed as "ecological" studies of animals and plants in their natural surroundings and on the processes whereby individual claims for ecology grow into a larger social order of ecology, locally as well as nationally. The esta lishment of a social order of ecology is accounted for in terms of enrolment, i.e., the processes by which ecology actors identify certain issues and interests and translate these into the language and rhetoric of ecology. This includes the interests of prospective members of the social order (students) and the interests of larger societal agents authorizing it. In a concluding remark the growing social order of ecology is seen as a reconciliation between two contending approaches - experimentalist versus naturalist - to field studies. The final step towards reconciliation was taken with the launching of the large scale ecosystem projects in the early 1970s.

KEYWORDS
ecology, history of - 20th century Sweden - science, institutionalization of - rhetorics - enrolment - conflicts in science