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— 8 —

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SCIENCE AND NATURE

essays in the history of the environmental sciences

edited by

Michael Shortland

BRITISH SOCIETY FOR THE HISTORY OF SCIENCE

© *British Society for the History of Science 1993*

ISBN 0-906450-08-X

Designed and typeset in Stempel Garamond by Jeffrey Dean,
The Stingray Office, 16 Charles Street, Oxford OX4 3AS

Printed in Great Britain by The Alden Press, Oxford

Contents

	Foreword	MICHAEL SHORTLAND	*vii*
1.	Science and the environment: New agendas for the history of science?	PETER J. BOWLER	1
2.	The ancient climate in the eighteenth and early nineteenth century	THEODORE S. FELDMAN	23
3.	Pollution and the protection of inland fisheries in inter-war Britain	JOHN SHEAIL	41
4.	*The smoke menace*: Cinema, sponsorship and the social relations of science in 1937	TIMOTHY BOON	57
5.	Conserving nature and building a science: British ecologists and the origins of the Nature Conservancy	STEPHEN BOCKING	89
6.	Of desert and watershed: The rise of ecological consciousness in Victoria, Australia	LIBBY ROBIN	115
7.	An elusive science: Ecological enterprise in the Southwestern United States	SHARON E. KINGSLAND	151
8.	On 'environmentalism': The origins of debates over policy for pest-control research in America, 1960–1975	PAOLO PALLADINO	181
9.	Radiation ecology, systems ecology and the management of the environment	CHUNGLIN KWA	213
10.	The failed promise of human ecology	EUGENE CITTADINO	251
	List of contributors		285
	Index of names		287

Foreword

MICHAEL SHORTLAND

Readers of this collection of original essays are sure to know that the history of the environmental sciences has developed over the past decade or so into one of the liveliest and most challenging areas of the history of science. Few would be able to prophesy how deeply environmentalism and ecological philosophies are likely to cut into our understanding of the development of science and technology, but already they have brought about some major shifts. There are signs that the impact of a revisionist study of the Scientific Revolution such as Carolyn Merchant's *The death of nature* (1980) will be repeated as other once-familiar episodes are subjected to 'ecological' scrutiny.

Several of the authors in this volume have produced significant wide-ranging work in the field, but there are presently available only a small number of reliable historical overviews of the environmental sciences. This being so, the essays published here should serve the function of alerting readers to some of the latest scholarship and of plotting some paths for future research. As Peter Bowler's introductory remarks suggest, such terms as 'environment' and 'ecology' are dense and presently controversial: charting their history and the history of activities pursued under their names, one becomes immediately immersed in fractious disputes and polemics which reveal a great deal about science and ideologies of science. Each of the contributors to this collection therefore offers insight into the dynamics of scientific controversy and the contexts in which value-laden terms such as 'nature' and 'ecology' are appropriated. This gives the volume as a whole a wide potential audience. Not only historians and philosophers of the environmental sciences will find here material of interest, but also sociologists of science, science teachers, social and cultural historians, and — why not — scientists themselves.

At the end of 1990 a call for papers was made in the *British journal for the history of science* and it was hoped that work of sufficient quality would be submitted to warrant the publication of a special issue on the history of the environmental sciences. The response was far greater than anticipated and it was therefore decided to collect those papers accepted for publication into a BSHS Monograph. The Monograph Series offers an excellent forum for a wide-ranging collection of this kind, the first Monograph having appeared under the title *Images of the earth* in 1979. In closing their introduction to

this book, its editors L. J. Jordanova and R. S. Porter expressed disappointment that historians had expended so little apparent effort to understand 'the myriad meanings for the human race of its own planet'. There is still much work to be done, but readers will find in this collection many stimulating contributions towards that task.

It is, finally, a pleasure to acknowledge with thanks the support and encouragement I have received from John Brooke, editor of the *BJHS* and Paul Weindling, editor of the Monograph Series. In addition to the contributors themselves, who have worked hard to make their scholarship accessible to a non-specialist audience, the following have provided advice and assistance in the preparation of this volume: Sharon Beder, Janet Browne, James R. Fleming, David N. Livingstone, John Mackenzie, Gregg Mitman, Roy Porter, Harriet Ritvo, Sara F. Tjossem, and Douglas Weiner.

— 1 —

Science and the environment: New agendas for the history of science?

Peter J. Bowler

The appearance of a British Society for the History of Sciences monograph devoted to the environmental sciences prompts me to offer the following reflections on the emergence of this topic as a potential theme in the discipline's future development. These reflections arise from a significant change in my own interests, initiated (I must confess) by a commission to write an introductory-level survey of current thinking on the development of the environmental sciences.[1] Coming from a background in one of the long-established fields that has traditionally been regarded as the key area in the history of science — the rise of evolution theory and the so-called 'Darwinian revolution' — I have been forced to widen the scope of my reading extensively in order to have any hope of providing an overview of what is only just emerging as a topic of central importance. My efforts to expand into this new area have made me acutely conscious of the extent to which our work is determined by preconceived ideas about what should count as 'mainstream' history of science.

The decision to recognize a distinct category of sciences dealing with the environment has been prompted in recent decades by the general public's growing awareness of the threat posed by modern technology to the earth and its inhabitants. Scientists such as Jim Lovelock have responded to the resulting social pressure by seeking ways to break down professional barriers and create a new kind of science which will study the complex interactions between the different facets of the physical and organic environment.[2] Many scientists themselves find it difficult to adjust to the new categories that environmentalists wish to impose on our study of nature. Historians will face similar problems of adjustment: the disciplinary boundaries that we have inherited will no longer apply in a future world in which people will ask different kinds of questions about the past. I suspect that there are many historians of science who will welcome the examination of our discipline's priorities that will result from the attempt to reconcile ourselves to the new situation.

[1] This has appeared in the UK as *The Fontana history of the environmental sciences* (London, 1992) and will be published in America as *The Norton history of the environmental sciences*.

[2] See James Lovelock, *Gaia: A new look at life on earth*, Oxford, 1987.

But why should an outsider — an environmentalist, for instance — concern him or herself with the internal problems of a small group of specialized historians? The very fact that science itself is now changing its priorities alerts us to the fact that the existing state of science is an historical artefact. We think about science in a certain way because we have been taught to do so by the past. Anyone concerned with the present state of science needs to know something about its past development, and historians believe that they have skills which may prevent the kind of oversimplifications that so often creep in when scientists, or people with a particular concern about science, create a view of the past to serve their own interests. If outsiders wish to make use of our skills, it may be worth their while to consider for a moment the process by which historians — who are constantly rethinking their existing models of the past — tackle this latest problem.

When I first began to study the history of science in the mid 1960s it would have been very difficult to work explicitly on what would now be called the 'environmental sciences'. Students were directed into a group of topics pre-selected by the very fact that they were being used to put the discipline onto the academic map. This group included the 'Darwinian revolution', but it did *not* include the environmental sciences, even though many of the issues we were encouraged to study represented alternative cross-sections through areas that would now be included in that category.

The suggestion that we should now think more carefully about these sciences dealing with the environment goes far beyond the mere addition of 'the rise of ecology' to the original list of favoured topics in the history of science curriculum. That list was constructed because the various modern sciences wished to highlight particular episodes as crucial to the development of the scientific world view. These episodes were endowed with even greater significance if, like the Darwinian revolution, they could be related to important changes in Western culture as a whole. But evolutionism may also be viewed as a theme linking many different branches of the environmental sciences, and from this perspective we may wish to ask a very different set of questions about its role. Changes in modern perceptions may thus help us to reassess our priorities in defining areas of historical research, offering fruitful new approaches to traditional topics in addition to suggesting new areas in which to work.

Such changing perceptions do not, however, arise purely in response to external pressures. Developments have taken place within the history of science which have undermined our reliance on the traditional sequence of conceptual revolutions. Historians have become more aware of the way in which the development of science has been shaped by professional constraints and opportunities, and these factors are well illustrated in the emergence of the various branches of science dealing with the environment. This awareness deflects attention away from some of the traditional concerns

with great theoretical initiatives, or at least forces us to reconsider the impact of those initiatives. At the same time, historians concerned with the constraints imposed by scientific disciplines are becoming more sympathetic to the kind of history done by working scientists. Although approaching the issues from opposite directions, historians and scientists may find common ground in studying the interaction of empirical and social factors in the development of many specialized areas that would once have been left to 'internalist' history.

The recent popularity of environmentalism marks a dramatic shift in popular attitudes towards nature, although similarly holistic views have a long history among intellectuals. Western culture — and hence Western science — has been influenced by a wide variety of different philosophies of nature. The history of the environmental sciences is not co-extensive with the history of attitudes and behaviour towards the environment, but the cultural background cannot be ignored if we are to make sense of those areas of science that deal with the environment. Historians of science must take note of the fact that their studies will be read by scholars and activists who have a much more general interest in the human race's interaction with the natural world. We must be prepared to deal with the demands of those who expect the history of science to play a role in the modern debates.

History enlightened by an environmentalist awareness will reveal the extent to which science was shaped by the demands of a society determined to control nature. But historians have become increasingly suspicious of efforts to portray the past in terms of heroes and villains. The majority of them are unlikely to condone the setting up of an oversimplified image in which most scientists are dismissed as rabid materialists bent on the destruction of nature. We shall see that studies of the history of ecology, for instance, have found little evidence to suggest that this science was founded as an environmentalist alternative to mainstream science. After surveying some actual and potential changes within the history of science itself, I shall offer some reflections on the role that history may play in the wider debate on how our civilization came to dominate, and ultimately to threaten , the environment.

What are the environmental sciences ?

Lovelock's approach requires the breakdown of barriers that have hitherto separated a number of scientific disciplines. His holistic study of the earth and its biosphere stresses the interactions between a wide range of processes operating within and between the earth's physical environment and the living things which make use of the planet's physical resources and the sun's energy. If we accept this range of processes as the subject matter of the 'environmental sciences', we must unify several fields of study traditionally included within the earth and the life sciences. A sceptic might well ask why

historians should feel the need to respond to such an initiative. Even assuming that it makes sense to unify these sciences in our modern situation, would it not be anachronistic to impose such a unity artificially upon past eras when more rigid disciplinary boundaries were taken for granted? Historians must be careful not to be sucked into a search for the 'forerunners' of modern ecology based on the assumption that any earlier study of natural interactions must have been based on current values.

Ecology is clearly a central theme of the environmental sciences, yet articles on the history of ecology appear routinely in the *Journal of the history of biology* without anyone complaining that we ought to create a *Journal of the history of the environmental sciences* to receive them. From such a traditionalist perspective, ecology is merely a branch of biology with clear links to other areas such as taxonomy and evolution theory. Similarly, the fact that evolution theory seeks to explain the diversity of life on earth in terms of how populations adapt to different environments does not justify separating it from the life sciences and including it within the environmental category.

The same point could be made with respect to the earth sciences. Historians have been studying the development of geology for years now, and have even treated the emergence of a specialized professional group of geologists as a major theme. Environmentalists would destroy that unity by demanding that certain areas of geology which deal with the forces shaping the earth's surface should be transferred to their own area, leaving other branches such as stratigraphy behind. To impose such a division on the past would make the historians' job impossible because it would require the erection of artificial barriers between subjects which we know were closely linked at the time.

It would certainly be inadvisable for historians to create an artificial category for the environmental sciences in their study of the past. But are we quite certain that the pre-existing disciplinary boundaries are a good model for the whole history of science? Ecology may be a branch of biology, but the concept of a distinct branch of science known as 'biology' has turned out to be highly problematic in historical terms.[3] The old study of natural history from which biological taxonomy emerged reflected a desire to comprehend the diversity of the natural world which was only loosely related to the 'medical' side of what we should now call the life sciences. Evolution theory has to mediate between the internal biology of the organism (genetics and individual development) and external factors (the interaction between populations and their environment). If biology (or the life sciences) represents an artificial category, perhaps it makes just as much sense to treat ecology and evolution theory as branches of the environmental sciences. Recognizing other possible categorizations does not necessarily mean that

[3] See Joseph A. Caron, '"Biology" in the life sciences: A historiographical contribution,' *History of science*, (1988) **26**, pp. 223–68.

we must throw away the old ones, only that we must be prepared to admit a multiplicity of ways in which the past may be understood.

The same is true for the earth sciences. There are certain aspects of geology which seem to cry out for inclusion in the environmental sciences since they deal with the processes which have shaped, and are still shaping, the earth's surface. Historians of the environmental sciences will simply have to accept that these aspects of geology cannot be understood without reference to other areas, such as the construction of the stratigraphical column, which do not seem quite so relevant to their immediate interests. The great advantage of admitting an alternative categorization of the earth sciences is that it facilitates a trend that is already underway towards recognizing that geology has never existed in isolation. The earth sciences include oceanography and meteorology too, disciplines neglected by historians until very recently. As I shall suggest below, study of earlier periods will reveal levels of interaction between these branches of the earth sciences that allow them to fit comfortably under the environmental umbrella. Again, we need a multiplicity of categories, not a simple replacement of the old by the new.

Changing fashions in the history of science

The extent to which the history of science has been dominated by 'industries' focusing on particular episodes — often the work of individuals who have been identified as the 'heroes of discovery' — is now increasingly recognized. We have tended to specialize in the great theoretical revolutions, especially those which challenged the traditional Christian world view and opened up new areas to rational investigation. This emphasis on conceptual innovation has tended to marginalize other aspects of science and has deflected attention away from those areas which did not seem to be undergoing any revolutionary development. In the last decade or so this framework has begun to break down as a result of internal tensions which have convinced many historians of the need to look more closely at the social and operational context within which scientific knowledge is generated. These changes have altered our perspective in a way that facilitates the move toward recognizing the environmental sciences as a legitimate category. At the same time, however, external pressures identical to those affecting the scientists themselves have created a demand for the history of the environmental sciences to be recognized as a legitimate area of study.

Over a decade ago the British Society for the History of Science published a monograph edited by Ludmilla Jordanova and Roy Porter entitled *Images of the earth: essays in the history of the environmental sciences*.[4] The title indicated a desire to promote the environmental sciences as an object of

[4] L. Jordanova and R. S. Porter, eds., *Images of the earth: Essays in the history of the environmental sciences*, Chalfont St Giles, 1979.

study, but the lead does not seem to have been followed up. Indeed, most of the articles in this book could have been published quite easily in a more conventional format. There was little effort by the authors to identify the environmental sciences as a distinctive area of study based on hitherto unstudied relationships between the sciences. Perhaps the time is now ripe for a more concerted effort to move the history of science in the direction indicated by the title of this volume.

The traditional models of history favoured until recently have shaped our imagination in many different ways. The concentration on the Darwinian revolution is a case in point. The advent of evolutionism was identified as a key area of study because it symbolized a major conceptual development within modern science and culture. There was a strong tendency to emphasize those discoveries (especially Darwin's discovery of natural selection) which have come to be seen as the foundation stones of modern evolutionism. Historians also tended to focus on those aspects of the Darwinian debate which highlighted the supposed conflict between science and religion, or the possible ideological dimensions of Darwinism. Even within the history of biology, the emphasis on Darwinism had a host of consequences. It dictated, for instance, a switch of emphasis around 1900 from the old areas of 'natural history' to the new science of genetics as the next phase in the main line of development. Palaeontology and biogeography, widely seen as crucial in the era of Darwin himself, were suddenly left aside as though they had nothing else left to offer once the fact of evolution had been established.

Such centres of attention had their satellites too: it is difficult to escape the feeling that the concentration on the 'uniformitarian-catastrophist debate' in the history of geology was prompted by the feeling that this debate was a prelude to the Darwinian revolution in biology. In both cases, the presumed significance of the episode was based on the assumption that the uniformitarian and Darwinian theories had played key roles in the emancipation of science from old-fashioned religious values. The old idea of a 'war' between science and religion predisposed historians to concentrate on certain episodes which could be presented as major campaigns in the war, while scientists actively encouraged the study of individuals who could be highlighted as the founders of important modern theories.

We now tend to dismiss these accounts as 'Whig history' in which the past has been manipulated to fit modern values. Detailed studies of how new theories were introduced have forced us to question the old technique in which certain forward-looking individuals were singled out for praise, while all who opposed them are identified as the enemies of rational thought. More important, we have begun to realize the extent to which the various sciences were developing independently of the great theoretical debates picked out by hindsight. The study of how science was actually practised, and of the social groupings that emerged to facilitate and legitimize the acquisition of

scientific knowledge, has forced us to reappraise the whole framework within which the history of science was studied. I want to argue that in many ways, these changes make it easier for us to appreciate the potential value of using the 'environmental sciences' as an alternative cross-section through the earth and the life sciences.

The earth sciences

The old interpretation of the uniformitarian-catastrophist debate was undermined by Reijer Hooykaas, Martin Rudwick and others who showed that the catastrophists should not be regarded as villains obstructing the development of geological theory.[5] But recent work is transcending such myths in an even more fundamental manner, breaking down the old categories and opening up the possibility of creating entirely new models for the development of science. Rudwick and a number of other scholars have revealed the extent to which the image of the uniformitarian-catastrophist debate has imposed artificial constraints on our interpretations of nineteenth-century geology. These historians are now exploring developments which took place independently of the debate over the rate of geological change.[6] Their studies of the interaction of fieldwork, theory and professional interests has provided a new framework for understanding the history of geology — a framework which may help future historians to treat geology as a branch of the environmental sciences rather than a battleground in the war between science and religion.

Significantly, many late-nineteenth-century developments in the earth sciences have received little attention. Mott T. Greene's book has surveyed a range of ongoing debates that have been largely ignored because they were not associated with wider cultural issues, although they were of fundamental concern to the geologists of the time.[7] Precisely because there were such extensive internal debates, these topics may offer fertile pastures for historians interested in the functioning of geology as a discipline. Here the so-called 'internal' history of the scientist interested in the origins of his or her

[5] R. Hooykaas, *Natural law and divine miracle: The principle of uniformity in geology, biology and history*, Leiden, 1959; M. J. S. Rudwick, 'Uniformity and progression: Reflections on the structure of geological theory in the age of Lyell', in Duane H. D. Roller, ed., *Perspectives in the history of science and technology*, Norman, Oklahoma, 1971, pp. 209–27.

See also Nicolaas A. Rupke, *The great chain of history: William Buckland and the English school of geology*, Oxford, 1983.

[6] Rudwick, *The great Devonian controversy: The shaping of scientific knowledge among gentlemanly specialists*, Chicago, 1985; James Secord, *Controversy in Victorian geology: The Cambrian-Silurian debate*, Princeton, 1986; David Oldroyd, *The Highlands controversy: Constructing geological knowledge through fieldwork in nineteenth-century Britain*, Chicago 1990.

[7] Mott T. Greene, *Geology in the nineteenth century: Changing views of a changing world*, Ithaca, NY, 1982.

own discipline may fruitfully interact with the historian's greater awareness of the social pressures acting within the scientific community. The success of the journal *Earth sciences history* suggests that this kind of interaction is beginning to bear fruit.

The only episode in the later history of the earth sciences that has attracted wide attention is the emergence of the theories of continental drift and plate tectonics.[8] Here the incentive has been the opportunity to analyse a major conceptual revolution, and several of the most recent studies explicitly use this episode to test rival interpretations of how new theories are produced and disseminated. But this application of hindsight has produced a very distorted image of the early-twentieth-century earth sciences. The debate over Alfred Wegener's theory of continental drift has been analysed in considerable detail because the theory seemed to anticipate a key element of modern mobilism. Other areas of debate within the earth sciences — which attracted far more attention at the time, and which may be of considerable interest to modern scientists — have been ignored by historians because to study them would smack too strongly of a narrow, internalistic approach. There was, for instance, a controversy over the formation of granite, which has so far received no attention from historians.

One factor that emerges in all discussions of the twentieth-century revolution in the sciences is the increasing role played by new disciplines such as geophysics and oceanography. As historians become more aware of the changing social structure of the scientific community in the area of earth sciences, they may perhaps become more willing to take note of the widespread interest expressed by the scientists themselves in the historical background to these disciplines. There have been regular congresses on the history of both geophysics and oceanography organized by the scientific communities, and there are now signs that professional historians are beginning to get more deeply involved in this kind of activity. In a forthcoming commentary, Eric L. Mills outlines the difficulties that have attended the creation of a forum for discussing the history of oceanography, but points out the opportunities that may be offered by such an interdisciplinary area of science.[9] Precisely because they are interested in how social and profes-

[8] Anthony Hallam, *A revolution in the earth sciences: From continental drift to plate tectonics*, Oxford, 1973; William Glen, *The road to Jaramillo: Critical years of the revolution in earth sciences*, Stanford, 1982; Robert Muir Wood, *The dark side of the earth*, London, 1985; H. E. Le Grand, *Drifting continents and shifting theories: The modern revolution in geology and scientific change*, Cambridge, 1988; and John A. Stewart, *Drifting continents and colliding paradigms: Perspectives on the geoscience revolution*, Bloomington, Indiana, 1990.

[9] Eric L. Mills, 'The historian of science and oceanography after twenty years', forthcoming in a special issue of *Earth sciences history* devoted to oceanography. A Newsletter on the history of oceanography is now available from Mills (Department of Oceanography, Dalhousie University, Halifax, NS, Canada). The Fifth International Congress on the History of Oceanography is now in preparation. There are two older histories of oceanography: Clarence P.

sional factors influence the study of nature, historians of science can now make common cause with scientists interested in the internal functioning of their own disciplines in a way that was not possible when attention was focused on the big revolutions in scientific thinking.

Mills's comments on the difficulties which have had to be overcome to create a historical discipline focused on oceanography force us to confront the way in which the original structure of the history of science was shaped by preconceptions about which areas of science constituted appropriate topics for study. There are a number of sciences which, because of their interdisciplinary nature, or because they were perceived as being on the fringes of 'real' science, were at one time avoided by professional historians. Oceanography and meteorology are two such sciences — and it is interesting to note that the latter area has now also begun to attract some attention.[10] Perhaps the most obvious example of a topic that has been excluded from most surveys of the history of science is geography. David Livingstone had made a plea for the closer integration of the history of geography — an active discipline in its own right — and the history of science.[11] By focusing on the environmental sciences as a topic worthy of study, we may be able to bring about the integration of a whole range of important, yet hitherto ignored, topics into mainstream history of science.

The life sciences

From the environmentalist perspective, of course, there is no sharp dividing line between the earth and the life sciences. Oceanography includes the biology of the oceans, just as geology includes the study of processes which shape the evolution and migration of terrestrial organisms. My use of the phrases 'earth sciences' and 'life sciences' as section headings indicates only the different directions from which historians will begin to study the environmental sciences, not a rigid distinction that must be preserved within the new field.

Idyll, ed., *Exploring the ocean world: A history of oceanography*, New York, 1969; and Margaret B. Dean, *Scientists and the sea, 1550–1900: A history of marine science*, New York, 1971. Biological oceanography has received up-to-date treatment from Eric L. Mills, *Biological oceanography: An early history, 1870–1960*, Ithaca, NY, 1989. The American Geophysics Union is issuing series of volumes collecting papers on historical matters, see C. Stewart Gilmor, ed., *History of geophysics*, Washington, 1984, 1986.

[10] Robert Marc Friedman, *Appropriating the weather: Vilhelm Bjerknes and the construction of modern meteorology*, Ithaca, NY, 1989; also James Fleming, *Meteorology in America, 1800–1870*, Baltimore, 1990.

[11] David Livingstone, 'Science, magic and religion: A contextual reassessment of geography in the sixteenth and seventeenth centuries', *History of science*, (1988) 26, pp. 269–94. On later developments see Margareta Bowen, *Empiricism and geographical thought: From Francis Bacon to Alexander von Humboldt*, Cambridge, 1981.

Even within the conventional framework of the history of the life sciences, however, we have begun to see changes that may facilitate the rearrangement of priorities. Here again we are confronted by a tension between two levels of historical interest which is now beginning to dissipate. There have always been people interested in the history of natural history, and in the origins and development of the various specialized sciences which emerged to describe and classify the branches of the animal and vegetable kingdoms.[12] Professional historians of science have concentrated instead on major theoretical revolutions such as the advent of evolutionism. We are all aware of Darwin's work on biogeography, his interest in South American fossils, and his pioneering study of the barnacles. Yet the number of good surveys of the general history of biogeography and palaeontology is relatively small,[13] while studies of detailed debates in taxonomy are virtually non-existent. Significantly, those studies that we do possess tend to break off once the triumph of Darwinism seems assured, as though later developments in areas such as palaeontology would be of interest only to specialists. In the late nineteenth century, historians of biology have traditionally switched their attention to the laboratory-based sciences which would eventually solve the problem of heredity and complete the Darwinian revolution. Those disciplines which belong more to the category of the environmental sciences fall into the background because it is assumed that they no longer have anything to contribute to the theoretical debate.

I have long urged the need to gain a broader perspective on the 'Darwinian revolution' by searching for inputs from non-Darwinian evolutionary theories.[14] Many of those inputs come from precisely those areas of the life sciences ignored by the traditional historiography of post-Darwinian evolutionism. I now suspect that instead of looking for theories to categorize as Darwinian or anti-Darwinian, we would be better employed trying to put together an overview of the various debates that took place among scientists attempting to reconstruct the history of life on earth. Evolution *was* an important theme in the late nineteenth and early twentieth century life sciences, but a number of disciplines tried to make use of it, and we need to be aware of the tensions and interactions between scientists with different pro-

[12] The Society for the History of Natural History serves as a forum for historians in this field; see their journal *Archives of natural history*.

[13] Janet Browne, *The secular ark: Studies in the history of biogeography*, New Haven, 1983; Martin Rudwick, *The meaning of fossils: Episodes in the history of palaeontology*, 2nd edn, New York, 1976; see also Adrian Desmond, *Archetypes and ancestors: Palaeontology in Victorian London, 1850–1875*, London 1982. A survey which does cover the late nineteenth and early twentieth centuries is Eric Buffetaut, *A short history of vertebrate palaeontology*, Beckenham, 1986.

[14] Peter J. Bowler, *The eclipse of Darwinism: Anti-Darwinian evolution theories in the decades around 1900*, Baltimore, 1983, and *The non-Darwinian revolution: Reinterpreting a historical myth*, Baltimore, 1988.

fessional axes to grind. Museum and field workers tried to reconstruct the history of life on earth through palaeontology and biogeography. Ronald Rainger's work on American palaeontology in the early twentieth century shows the potential gains to be made from a detailed study of how scientists functioned within a particular disciplinary framework provided by the great museums of the time.[15] By means of such studies we should be able to link the broadest areas of theoretical debate over the origin and development of life with the kind of specialized research done by those historians whose primary concern is the functioning of a single discipline.

Ecology was to some extent a product of the tension between laboratory and field studies at the turn of the century. Given our modern fascination with this science and its implications, it is hardly surprising that historians have recently begun to take an interest in its origins. There was already a body of historical material written by the ecologists themselves.[16] This technical approach culminated in the appearance of Robert McIntosh's *The background of ecology* in 1985.[17] In the 1980s the field began to attract an increasing number of historians anxious to apply the historiographical techniques developed in established areas. Donald Worster's *Nature's economy* of 1977 (reissued 1985) was a pioneering synthesis which has helped to put the area on the map.[18] Since then the work of Ronald Tobey, Eugene Cittadino and others has explored the widely differing circumstances under which a scientific ecology emerged within the scientific communities of Europe and America.[19] The demand for a more rigorous type of field study was a crucial factor in the creation of several early communities of ecologists, while the potential importance of the science for agriculture allowed both government and private funding to be exploited.

[15] Ronald Rainger, 'Just before Simpson: William Diller Matthew's understanding of evolution,' *Proceedings of the American Philosophical Society* (1986) 130, pp. 453–74, and 'Vertebrate palaeontology as biology: Henry Fairfield Osborn and the American Museum of Natural History,' in R. Rainger, K. Benson and J. Maienschein, eds., *The American development of biology*, Philadelphia, 1988, pp. 219–56.

[16] For surveys of the history of ecology to the early eighties see Frank N. Egerton, 'The history of ecology: Achievements and opportunities.' *Journal of the history of biology* (1983) **16**, pp. 259–310, and (1985) **18**, pp. 103–43.

[17] Robert P. McIntosh, *The background of ecology: Concept and theory*, Cambridge, 1985.

[18] Donald Worster, *Nature's economy: A history of ecological ideas*, Cambridge, 1985.

[19] Ronald C. Tobey, *Saving the prairies: The life cycle of the founding school of American plant ecology*, Berkeley, 1981; Eugene Cittadino, 'Ecology and the professionalization of botany in America, 1890–1905', *Studies in the history of biology* (1980) 4, pp. 171–98, and *Nature as the laboratory: Darwinian plant ecology in the German Empire, 1880–1900*, Cambridge, 1991.

Changing views of nature

So far we have been concerned with the internal development of science, although even here attitudes have been swayed by preconceptions about the role played by science in public debates. The recent growth of interest in the history of ecology suggests that our priorities as historians can be influenced by changing values in society as a whole. As the environmental sciences come to be of greater public concern, so we may expect attention to be deflected onto the origin and development of these sciences. There can be little doubt that in many cases, further historical studies will reinforce the popular belief that science has all too often served as the handmaiden of a materialistic and exploitative value system. The opportunities which led to the creation of a wide range of disciplines concerned with the study of the environment were, to a large extent, the product of Western culture's growing willingness to invest in science as a means of understanding and exploiting the world we live in.

There is already a substantial literature on Western society's varied and ever-changing attitudes towards the natural world. Yi-Fu Tuan has stressed the ways in which beliefs about the structure of the world are culture-dependent, while a number of authors have addressed more directly the question of how Western attitudes to nature have changed.[20] A matter of immediate concern to writers in this area is the source of the harsh, exploitative attitude which seems increasingly to have dominated our interaction with the natural world. At the same time, the study of 'environmental history' has sought to document the interactions themselves, showing how human exploitation has modified the natural environment.[21] As concerns for the state of the modern environment grow, more historical effort will presumably be expended on these efforts to trace the roots of our present predicament.

The history of science clearly operates within a more restricted range. We are not directly concerned with attitudes expressed outside the scientific community, nor with technological or industrial developments pushed for-

[20] Yi-Fu Tuan, *Space and place: The perception of experience*, London, 1977, and *Topophilia: A study of environmental perception, attitudes, and values*, Englewood Cliffs, NJ, 1974. The best account of early attitudes towards nature is Clarence J. Glacken, *Traces on the Rhodian shore: Nature and culture in Western thought from ancient times to the end of the eighteenth century*, Berkeley, 1967. See also Keith Thomas, *Man and the natural world: Changing attitudes in England 1500–1800*, London, 1983; Harriet Ritvo, *The animal estate: The English and other creatures in the Victorian age*, Cambridge, Mass., 1987; and John M. Mackenzie, *The empire of nature: Hunting, conservation and British imperialism*, Manchester, 1988. On American attitudes see especially Roderick Nash, *Wilderness and the American mind*, New Haven, 1957.

[21] See for instance Donald Worster, ed., *The ends of the earth: Perspectives on modern environmental history*, Cambridge, 1988.

ward without scientific input. But it would be foolish to pretend that science was not influenced by broader attitudes towards nature or by the opportunity to participate in the exploitation of natural resources. To this extent, the history of the environmental sciences must treat itself as one component of a more general historical programme aimed at understanding how Western society achieved its domination of nature. Since many now think that the attitudes underlying that domination must be undermined if we are not to destroy the world in which we live, the history of science will become part of the environmental debate. For many activists, the point is not to study science but to change it, and if history can help to expose science's involvement in exploitation, it will be called into play for this purpose.

Historians of science are no strangers to this kind of situation. Much of our earlier fascination with Darwinism was generated by its involvement in religious and ideological debates. For writers such as Robert Young, the purpose of history is to expose the involvement of science in the process by which groups of humans exploit one another.[22] To apply the same logic to the exploitation of the environment is but an extension of the existing pattern of argument. The claim that the actual generation of scientific knowledge is influenced by social values remains highly controversial, but there are probably few historians who would now deny that biological theories, for instance, have been used to justify racism and other forms of exploitation, and that in various indirect ways the professional development of certain disciplines was influenced by these applications. In a similar manner it is relatively easy to show that many areas of the environmental sciences have been influenced, if not actually brought into being, by the desire to control and exploit the natural world. Far from shying away from such debates, I believe that historians of science should participate in them, offering their expertise in the hope of encouraging a more informed level of argument. Historians who have strong commitments should — as in the life sciences — be free to use history as a means of making their points. Even those who object to the radical programme adopted by left-wing historians of biology would probably accept that their intervention has stimulated debate and encouraged the development of more sophisticated levels of sociological analysis. The same may occur when committed environmentalists write about the history of the environmental sciences.

Academic historians must, however, try to ensure that the resulting debates are based on a serious study of the evidence. There is a danger that writers with strong political agendas may seek to use history as a means of bolstering their positions, creating a background for the modern debates based on an oversimplified model of past heroes and villains. In the life sciences we have progressed beyond the stage in which a sociological

[22] R. M. Young, *Darwin's metaphor: Nature's place in Victorian culture*, Cambridge, 1985.

analysis was designed primarily to show the extent to which biology was exploited by the supporters of social Darwinism and genetic determinism. The latest work in this area — of which Adrian Desmond's analysis of the pre-Darwinian debates offers a prime example[23] — Shows that the relationship between science and ideology is far more complex. Many different interest groups within a society, both radical and conservative, may seek to use science for their own ends. The historians' job is not to conceal these involvements in the name of scientific objectivity, but to expose and comment on the various levels of social influence. Science is not value-free, but nor is it the prerogative of a particular set of conservative values. Conservatives may often be in a better position to exploit science, but the nature of 'conservative' values changes through time, as does the kind of science most easily adapted to those values.

The same level of sophistication must be encouraged in any future debate on the history of the environmental sciences. Although environmental groups are becoming steadily reconciled to the use of science, they have inherited a legacy of suspicion based on past convictions. Science has often been portrayed as an agent of the military-industrial complex, its materialistic theories as a classic illustration of Western culture's contempt for the natural world. From such a perspective it would be easy to construct a 'heroes and villains' approach to the history of the environmental sciences in which a few valiant exponents of holism battled against the entrenched forces of materialism until vindicated in recent decades.

Historians have no interest in trying to defend science from the charge that it has frequently been involved in exploitation of the environment. On the contrary, much work on the development of the environmental sciences will necessarily explore the opportunities offered by Western society's growing demand for material resources. At a deeper level we can search for the philosophical foundations of the exploitative attitude reflected by many scientists. At the same time, however, historians will be in a position to show that science has never been a monolithic institution. Widely different value systems have influenced scientists from time to time, often in ways that would be unpredictable to someone not versed in the complexities of the issues. In these circumstances, our goal must be to encourage all participants in the environmental debates to make informed use of history, preventing if we can the erection of stereotypes that may be difficult to eradicate.

There were, of course, episodes in which scientists were motivated by alternatives to the philosophy of reductionist materialism, and we may well find that the history of these episodes will appeal to environmentalists seeking to provide their movements with emotional and intellectual roots. In itself this may be quite legitimate: environmentalism certainly does have its

[23] Adrian Desmond, *The politics of evolution: Morphology, medicine, and reform in radical London*, Chicago, 1989.

origins in the thinking of some late-nineteenth-century opponents of materialism. But the fact that the public at large has been encouraged to identify the term 'ecology' not with a branch of science, but with the environmentalist philosophy, warns us of potential dangers. One can foresee a situation in which efforts are made to link ecology with other anti-materialist trends in science, including vitalism in physiology and Lamarckism in evolutionary biology, as the supposed foundations for an alternative, more humane, world view. Needless to say, such an interpretation is hardly born out by serious historical studies of the various branches of the life sciences involved. The history of the environmental sciences should allow us to warn that the relationship between science and the philosophies of exploitation and conservation is a good deal more complex than outsiders may think. Perhaps historians will be able to convince those involved in the current debates that, rather than imposing modern labels on the past, we should be trying to understand the different ways in which the attempt to understand the world has been linked to the effort to exploit or preserve it.

Science and the domination of nature

Historians seeking to uncover the origins of Western culture's attitudes towards nature have offered various suggestions as to the roots of our exploitative value system.[24] Science has been seen as a characteristic product of this system, again with various suggestions as to the detailed cultural influences at work. It has been argued that the mechanical philosophy of the seventeenth century helped to establish the link between science and exploitation, since it portrayed nature as a system which could be broken up into isolated units, each of which could be understood and controlled without effect on the whole. Feminists have argued that science represents the use of masculine imagery to displace a more holistic philosophy in which nature was essentially female in its attributes.[25] Analysis of these suggestions is beyond the scope of the present article; they are listed here merely to indicate the complexity of the issues that remain once it is accepted that science does indeed have links to the philosophy of exploitation. I suspect that many historians will be more interested in exploring the practical links between science and the domination of nature. The following paragraphs are offered merely as an illustration of how a single issue in this analysis might

[24] See for instance Lynn White, Jr, 'The historical roots of our ecological crisis,' *Science* (1967) 155, pp. 1203–7. This is reprinted along with other relevant articles in David and Eileen Spring, eds., *Ecology and religion in history*, New York, 1974. See also John Passmore, *Man's responsibility for nature: Ecological problems and Western traditions*, London, 1974.

[25] See for instance Bowen, *Empiricism and geographical thought* (note 11), and for the feminist perspective, Carolyn Merchant, *The death of nature: Women, ecology, and the scientific revolution*, London, 1980.

be explored in a way that would benefit both historians of the environmental sciences and commentators interested in the link between science and economic power.

As European power spread around the world in the eighteenth and nineteenth centuries, science was increasingly called in to survey different aspects of the earth's physical and biological environment, often with direct practical applications in mind. In the last phase of the age of exploration, scientists were increasingly involved in the opening up of new territories.[26] Both at home and in colonized territories abroad, governments began to finance surveys and other mechanisms for assessing the potential of the environment for exploitation. As the history of science becomes more aware of the role played by institutions and other professional structures, it is forced to take greater account of the role played by the demand for practical information about the world.

Geology acquired its identity as a science in the early nineteenth century, and in Britain it did so in the age of what Martin Rudwick has called the 'gentlemanly specialist'. But geology had always had a strong practical foundation, and in the course of the nineteenth century, geologists were increasingly able to exploit the potential for practical applications to gain access to state funding for surveying. In America and in many of the newly-developing countries of the time, geological and other surveys played a major role in creating a climate within which science could begin to flourish. The chain of magnetic observatories set up around the British Empire under the direction of Edward Sabine in 1838 also helped to put science on the map in new countries such as Canada and Australia. Historians now located in countries whose scientific development was boosted by nineteenth-century surveys naturally look to these developments with an interest that is not always matched in Europe, where the surveys did not play such a critical role.[27]

[26] There is an immense literature on the exploratory voyages of Captain James Cook and other major figures; for surveys see for instance Derek Howse, ed., *Background to discovery: Pacific exploration from Dampier to Cook*, Berkeley, 1990; Roy Macleod and Phillip F. Rehbock, eds., *Nature in its greatest extent: Western science in the Pacific*, Honolulu, 1988; David MacKay, *In the wake of Cook: Exploration, science and empire, 1780–1801*, London, 1985. More generally see John M. Mackenzie, ed., *Imperialism and the natural world*, London, 1990.

[27] The role of geological and other surveys in the development of American science is explored by A. Hunter Dupree, *Science in the Federal government: A history of policies and activities to 1940*, reprinted New York, 1967 and in more detail by Thomas G. Manning, *Government in science: The United States Geological Survey, 1867–1894*, Lexington, 1967 and Manning, *U.S. Coast Survey vs. Naval Hydrographic Office: A 19th-century rivalry in science and politics*, Tuscaloosa, 1988. On the role of the Geological Survey of Canada and the Magnetic Survey in the creation on Canadian Science see Suzanne Zeller, *Inventing Canada: Early Victorian science and the idea of a transcontinental nation*, Toronto, 1987. See also Ann Savours and Anita McConnell, 'The history of the Rossbank Observatory, Tasmania', *Annals of science* (1982) **39**, pp. 527–64. The role of the Geological Survey in Britain is part of the story told by

The European powers dominated many parts of the world that did not subsequently enjoy the level of development characteristic of North America. The link between science and exploration in the age of imperialism is now emerging as an important theme. The geologist Sir Roderick Murchison promoted exploration through his presidency of the Royal Geographical Society and made free use of imperialist metaphors in describing the 'conquest' of nature.[28] The European powers set up natural history museums and botanical gardens both at home and in the colonized territories to explore the potential value of their empires. Such institutions not only facilitated the exploitation of economic animals and plants, but also provided a valuable source of jobs for the expanding core of trained European scientists.[29] Although historians are now beginning to explore this dimension of nineteenth-century science, much remains to be done (the Trigonometrical Survey and Geological Survey of India are obvious candidates for study).

Darwin's theory of evolution was a classic product of the age of imperialism: its origins lie in the biogeographical insights gained during a surveying voyage in a Royal Navy ship, HMS *Beagle*, while the theory itself exploits the metaphor of the invasion and conquest of new territories by more highly evolved forms as one of its central themes. The late-nineteenth and early twentieth-century debates over how the theory of evolution should be applied in areas such as palaeontology and biogeography were conducted by scientists many of whom were employed by the geological surveys, museums and botanical gardens created in the middle decades of the century. Even those biologists who rejected the Darwinian theory of natural selection were anxious to portray the history of life on earth as a series of distinct episodes in which newly evolved groups had expanded around the world, exterminating or marginalizing their predecessors.[30] Studying the use

Rudwick, *The great Devonian controversy* and Secord, *Controversy in Victorian geology* (note 6); see also Harold E. Wilson, *Down to earth: One hundred and fifty years of the British Geological Survey*, Edinburgh, 1985.

[28] See Robert A. Stafford, *Scientist of the empire: Sir Roderick Murchison, scientific exploration and Victorian imperialism*, Cambridge, 1989 and James Secord, 'King of Siluria: Roderick Murchison and the imperial theme in nineteenth-century British geology', *Victorian studies* (1982) 25, pp. 413–42.

[29] See Susan Sheets-Pyenson, *Cathedrals of science: The development of colonial natural history museums during the late nineteenth century*, Montreal, 1989. See also Lucile H. Brockway, *Science and colonial expansion: The role of the British Royal Botanic Gardens at Kew*, New York, 1979. There is little on the botanic gardens established in the colonies, although Cittadino's *Nature as the laboratory* (note 19) discusses the role of tropical botanical gardens in encouraging the development of German ecology.

[30] The extent to which late nineteenth-century evolutionary theory exploited the concept of cycles of expansion and contraction in the history of particular groups needs further study, although I have commented on the existence of this alternative to the better-known image of continuous development in my *The invention of progress: The Victorians and the past*, Oxford, 1989.

of such metaphors in the age of imperialism will help us to understand just how deeply the environmental sciences were influenced by the prevailing ideology of the time.

Science and holistic philosophy

The role of Imperialism is just one strand in the complex web of interactions between science and the West's domination of nature that historians are beginning to explore. Environmentalists may wish to see this as yet another illustration of the dangerous effects of a scientific world view based on materialism. As in other areas, though, we must be ready to warn against the setting-up of oversimplified black-and-white images of the past. Environmental historians are already aware of the complexity of the issues in their own area. The most radical calls for the defence of nature against mankind were associated with a holistic philosophy that emerged in the late nineteenth century in opposition to a perceived union between industrialization and materialism. Yet many of those who believed that mankind had the right to exploit nature were concerned to ensure that science was used to conserve the resources needed for future generations. Nor have the advocates of holism been free from moral criticism, as the possibility of a link between environmentalism and certain right-wing ideologies has revealed.[31]

Historians of science should try to demythologize the past. The conceptual development of science has not been driven solely by materialism, nor has opposition to materialism been the sole source of a more interactive view of nature. Some ecologists have constructed complex models of natural relationships inspired by the feedback loops of cybernetics. But given the modern belief that 'ecology' represents a moral reaction against materialism, there is a danger that the science of ecology might be perceived as the product of an almost vitalistic form of holism. Eugene Cittadino records that he was first attracted to the history of ecology by the possibility that it might represent an alternative, more holistic world view.[32] His subsequent work does not bear out this possibility, but there are signs that the distinction between ecology and environmentalism may be overlooked by less careful writers anxious to show that a new kind of science emerged in response to the reaction against materialism. Ecology emerged as a recognizable discipline in the 1890s, at approximately the same time as the foundation of the Sierra Club and other early stirring of the environmentalist movement. This might easily be presented as something more than a coincidence. Worster's *Nature's economy* has been criticized on just these grounds, while a more

[31] See Anna Bramwell, *Ecology in the twentieth century: A history*, New Haven, 1989.
[32] See Cittadino, *Nature as the laboratory* (op. cit. 19), preface.

recent book links 'ecology' to a vitalistic counter-current within late-nineteenth and early-twentieth century culture.[33]

In this latter case it is fairly easy to see that 'ecology' refers to the environmentalist movement, not the science — but the potential for confusion along these lines should not be underestimated. If modern environmentalists are naive enough to think that scientific ecology was a by-product of their own movement's origins, historians of science are in a position to show them just how complex the situation really was. An obvious point that strikes anyone approaching the literature on the history of ecology is the wide range of different motivations that influenced the founders of the different schools of ecological theory. Ecology was not a product of a growing desire to seek an overarching philosophy of nature; it arose from a desire to 'modernize' field studies as an alternative to the domination of the laboratory sciences. In an age of rapidly expanding opportunities for the creation of new disciplines and research schools, there was a proliferation of different approaches which ensured that ecology was never a unified science. This fragmentation tells us that ecology arose not from a new philosophy seeking to unify nature, but from a diverse collection of professional motivations that seems to typify the 'divide and conquer' philosophy of science that modern environmentalists find so repugnant. Even when early ecologists followed a holistic philosophy, they nevertheless functioned within professional niches. The broad campaign for the application of a holistic philosophy to ecological questions is very much a product of the last couple of decades.

By emphasizing this fact, historians can circumvent any effort to claim that scientific ecology was a product of the emergence of modern environmentalist attitudes. But there is a danger that in so doing, the history of the environmental sciences may help to reinforce the suspicions of those critics who claim that science is, by its very nature, a manifestation of the materialistic world view. If the history of science can play a useful role in this situation, it will be through its ability to suggest that some of the moral or ideological labels which have been attached to science or to particular branches of science are often the product of and oversimplified vision of the process by which we came to be in our modern predicament. Because historians are trained to beware the influence of hindsight, we shall be in a position to warn against the assumption that modern attitudes towards science reflect eternal verities which will necessarily impose themselves upon scientists' future activities.

One way of emphasizing this point will be to show that there have been episodes in the past when scientists really did seek to reverse the trend towards the fragmentation of nature by emphasizing the complex interactions between natural processes. If historians can show that scientists have

[33] Bramwell, *Ecology in the twentieth century* (op. cit. 31).

been able to make use of different conceptual strategies in different times and circumstances, we can help to prevent the modern debate being oversimplified to the level of name-calling, as though aggressive materialism and woolly-minded vitalism were the only alternatives available to us. In effect, we can show that the interactive, holistic kind of science proposed, for example, in Lovelock's Gaia hypothesis has antecedents in earlier episodes when scientists were influenced by a similar way of thinking.[34] The purpose of such studies is not to hunt for the precursors of ecology, as though there has been a single strand of interest in interactive processes running through the development of science as an alternative to reductionism. Indeed, one of the most pressing questions that historians need to address is the nature of the links that might exist between successive manifestations of the holistic approach. But the fact that there has been a series of such manifestations is itself of relevance to any assessment of the nature of the environmental sciences.

Even in the age of imperialism, there were cultural pressures that helped to create a less mechanistic view of nature. The great surveys of the mid-nineteenth century were a product of what has sometimes been called 'Humboldtian science'.[35] Alexander von Humboldt's call for the collection of more information about the environment was linked to a philosophical programme which encouraged a whole generation of scientists to look for exactly the kind of interactions between natural processes that modern environmentalism wishes to see highlighted. Nor was Humboldtian science a retreat into natural theology or vitalism — on the contrary it inspired some of the most innovative developments of the period. Darwin may have used the opportunities and metaphors of imperialism, but he was also inspired by Humboldt to search for interactions between phenomena at various levels. In this case, the sheer impact of Darwin's theory has forced historians to take it seriously, but there are many other examples of nineteenth-century scientists who transcended the newly emerging professional barriers between disciplines. Their work has often been neglected precisely because it does not fit into the categories we seek to impose on the past. James Croll's theory of the ice ages linked astronomy with geology, oceanography and meteorology to produce a complex interactive explanation of this phenomenon.[36] Physicists also became involved, through attempts to explain the forces driving ocean currents and other environmental processes.

[34] The background to Lovelock's philosophy is explored directly in Lawrence E. Joseph, *Gaia: The growth of an idea*, London, 1990.

[35] On Humboldtian science see for instance Susan F. Cannon, *Science in culture: The early Victorian period*, New York, 1978, chapter 3. On Humboldt's view of science see Malcolm Nicolson, 'Alexander von Humboldt and the geography of vegetation', In Andrew Cunningham and Nicholas Jardine, eds., *Romanticism and the sciences*, Cambridge, 1990, pp. 169–85.

[36] Croll's theory is outlined in his *Climate and time*, London, 1875; on the impact of his theory on geology see Christopher Hamlin, 'James Geikie, James Croll and the eventful ice age', *Annals of science* (1982) **39**, pp. 565–83.

If ecology was a product of the age of specialization, it originated from the collapse of the Humboldtian tradition that had sustained science through into the age of Darwinism. The result was the kind of fragmentation that modern environmentalists see as the most dangerous aspect of science. A properly-developed historiography for the environmental sciences may help to show that this fragmentation was not the inevitable product of a commercial attitude to nature lying at the heart of science; it was a consequence of science's rapid development under circumstances which permitted and even encouraged the creation of new disciplines and research schools. The transition from Humboldtian science to the proliferation of rival specializations was a product of the changing social and cultural environment within which science operated. Neither phase can be seen as the expression of some built-in character expressing the very nature of science itself. If we can use history to show modern environmentalists that science has routinely adapted itself to new circumstances, we may help to avoid a situation in which the role of science in the modern debates is prejudiced on the basis of stereotypes derived from oversimplified images of the past.

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The ancient climate in the eighteenth and early nineteenth century

THEODORE S. FELDMAN

This paper has two parts. The second and principal part offers an illustration of the rise of historical thinking about climate. The Enlightenment and early nineteenth-century literature on climates is considerable. For my illustration I have chosen from this literature the debate over the ancient Biblical and Roman climate and over climatic change since ancient times. (I will use 'ancient' in this article for the Biblical and Roman period and 'very ancient' or 'geological' for the much earlier periods involved in palaeobotanical and geological speculations.) By way of introduction, the first part of the paper will clarify what is intended by 'the rise of historical thinking' and will sketch the several contexts and meanings of 'climate' in the Enlightenment.

1. 'HISTORY' AND 'CLIMATE' IN THE ENLIGHTENMENT

Let me begin with 'the rise of historical thinking'. It is well known that a properly historical mode of thought, or 'historicism' as it is also called, appeared in the late eighteenth century not only in the discipline of history itself but in many areas of the sciences, including geology, biology, and even physics.[1] I have borrowed a characterisation of historical thinking, especially

[1] I will note here only a few of the numerous relevant studies. For history see Friedrich Meinecke, *The rise of historism* (tr. J. E. Anderson), New York, 1972; and Peter H. Reill, *The German Enlightenment and the rise of historicism*, Berkeley, Los Angeles and London, 1975; for geology Francis C. Haber, *The age of the world: Moses to Darwin*, Baltimore, 1959; D. R. Oldroyd, 'Historicism and the rise of historical geology', *History of science*, (1979) 17, pp. 191–213, 227–257; Rachel Laudan, *From mineralogy to geology: The foundations of a science, 1650–1830*, Chicago and London, 1987; for biology, Philip R. Sloan, 'Buffon, German biology, and the historical interpretation of biological species', *British journal for the history of science*, (1979) 12, pp. 109–153; J. Lyon and Sloan, *From natural history to the history of nature: Readings from Buffon and his critics*, Notre Dame and London, 1981; Sloan, 'Natural history, 1670–1802', in R. C. Olby, G. N. Cantor, J. R. R. Christie, and M. J. S. Hodge, eds., *Companion to the history of modern science*, London, 1990, pp. 295–313; Reill, 'History and the life sciences at the beginning of the nineteenth century: Wilhelm von Humboldt and Leopold von Ranke', in George Iggers and James M. Powell, eds. *Leopold von Ranke and the shaping of the historical discipline*, Syracuse, 1990, pp. 21–35; for physics, Norton Wise, with the collaboration of

as it applies to climate, from the literature on this subject. Historical thinking, as it appeared in the late Enlightenment, exhibits four key characteristics: a sense of the flux of time; an appreciation of the uniqueness and individuality of historical situations, and of their embeddedness in a particular time and place or historical context; reliance on a considerable body of concrete evidence; and a critical attitude towards that evidence. This definition is not intended to be rigorous or exhaustive but to serve as a plausible characterization of historical thinking or a historical approach. I will use 'history' and 'historical' in this sense.

History, then, is more than an account of the past; less obviously, perhaps, historical thinking requires more than an appreciation of change. Acknowledging change over time satisfies only the first of the above criteria, a sense of the flux of time. This holds for historical thinking in science as well as for history proper, as D. R. Oldroyd makes clear with regard to what he calls 'genetic' theories of the earth. In these theories the earth (or the solar system, or plant and animal species, etc.) changes according to universal laws. Buffon's celebrated theory of the cooling earth is a typical of this kind of thinking. Buffon proposed that the earth has been cooling from a very hot original state according to the laws of physics; once the earth reached a temperature low enough to admit life, the continually cooling climate supported progressively smaller and less energetic life forms. Kant's nebular hypothesis and Linnaeus' theory of the spread of plants with the gradual reduction of the seas[2] are likewise genetic. Because these theories base change on general, universal, and constant laws, they miss the unique, the individual in history. They are abstract and *a priori* and do not grow out of the accumulation of concrete historical evidence.[3] As for the study of the human past (i.e., history loosely speaking), Meinecke pointed out that natural law theory of the seventeenth century, rational and mechanical, denied the individuality of the historical situation in a similar way.[4]

It is important to emphasize these points with regard to climate. A historical approach to climate requires more than an interest in past climates, and more than an awareness of climatic change. Speculation on climatic change dates at least as far back as Theophrastus;[5] if we look only for perception of

Crosbie Smith, 'Work and waste: political economy and natural philosophy in 19th-century Britain', *History of science*, (1989) **27**, pp. 263–301, 391–449 and (1990) **28**, pp. 221–61; cf. J. Huizinga, 'Naturbild und Geschichtsbild im achtzehnten Jahrhundert', in Huizinga, *Parerga*, Basle, 1945, pp. 147–174.

[2] For Linnaeus see James Larson, 'Not without a plan: geography and natural history in the late eighteenth century', *Journal of the history of biology*, (1986) **19**, pp. 447–488.

[3] Oldroyd, op. cit. (1), 193, 228.

[4] Meinecke, op. cit. (1), 3–4.

[5] Clarence Glacken, *Traces on the Rhodian shore: Nature and culture in Western thought from ancient times to the end of the eighteenth century*, Berkeley and Los Angeles, 1967, 130. See also Kenneth Thompson, 'Forests and climate change in America: some early views', *Climatic change*, (1980) **3**, pp. 47–64, on 47.

climatic change, we shall find nothing new in the eighteenth century. In fact, this study will begin with authors who argued, in an ahistorical fashion, in favour of climatic change, and it will end with authors who, arguing more historically, denied it. In examining their work one must keep in mind the entire complex of factors contributing to a historical approach.

Let me turn now to the shifting meanings of 'climate' in the Enlightenment. At the beginning of the nineteenth century August Wilhelm Lampadius, Professor of Chemistry and Mining at the Freiburg Mining Academy, included in his textbook of meteorology a chapter on 'Climatology'. He was, to my knowledge, the first to use the word. Lampadius might be said to have christened the discipline of climatology, which was then being established by Alexander von Humboldt, Leopold von Buch, Joachim von Schouw and others. (Von Buch and von Schouw will figure later in this paper.) Lampadius' definition of 'climate', if not precise, sounds modern enough: 'Climate in more recent times is almost always understood as the weather of a place, that is, the character of its atmosphere'. We may take Lampadius at his word: the definition of climate was more or less established in its modern sense by the early nineteenth century.

If 'climate in more recent times' was 'almost always understood' in this way, it had not been so in the previous century. As with many concepts of Enlightenment natural philosophy, climate's definition was broad, imprecise, and changeable.[6] As our guide to its meanings let us take Diderot and d'Alembert's *Encyclopédie*, which offers three definitions. According to the lead definition, climate is a part or surface of the Earth, bounded by two circles parallel to the Equator, and of such a breadth, as that the longest day in the parallel closer to the Pole, exceeds the longest day in that next the Equator by some certain space, *viz.* half an hour.[7]

Climates, then, were zones of latitude. This definition dominated encyclopaedias and the extensive meteorological literature of the period.[8] It be-

[6] For the broad and changeable definition of physics see John L. Heibron, 'Experimental natural philosophy', in G. S. Rousseau and Roy Porter, eds., *The ferment of knowledge: Studies in the historiography of eighteenth-century science*, Cambridge, London and New York, 1980, 357–87, on 361 ff., and Heilbron, E*lectricity in the 17th and 18th centuries: A study of early modern physics*, Berkeley, 1979, Ch. 1.

[7] Denis Diderot and Jean le Rond d'Alembert, eds., *Encyclopédie ou dictionnaire raisonée des sciences, des arts et des métiers*, 35 vols., Stuttgart–Bad Cannstatt, 1960; facsimile reprint of 1751–80 edn, s.v. 'Climat' (III, Paris, 1753), pp. 532–536, on 532. This definition is translated from Ephraim Chambers' C*yclopedia; or, an universal dictionary of arts and science*, 2 vols., London, 1728, whose language I have used here.

[8] It is either the only or the lead definition in John Harris's widely regarded *Lexicon technicum*, 2 vols., London, 1704–10; Ephraim Chambers' *Cyclopedia; or, an universal dictionary of arts and science*, 2 vols., London, 1728; Samuel Johnson's *Dictionary of the English language*, 2 vols., London, 1755; and the *Encyclopaedia britannica; or, a dictionary of arts and sciences*, 1st edn, Edinburgh, 1771 through 4th edn, Edinburgh, 1810. For Enlightenment meteorology see Theodore S. Feldman, 'Late Enlightenment meteorology', in Tore Frängsmyr,

longed to the ancient tradition of mathematical geography; the earth's climates had been calculated by Ptolemy.

The *Encyclopédie*'s second definition ran as follows:

> Vulgarly, the term 'climate' is bestowed on any country or region differing from another, either in respect of the seasons, the quality of the soil, or even the manners of the inhabitants; without any regard to the length of the longest day.[9]

The best-known representative of this context was (and remains) 'the illustrious author of the *Spirit of the laws*', i.e. Montesquieu, whose theories the article briefly summarized.[10] But climate so understood also belonged to an ancient tradition:

> The most ancient physicians had observed the influence [of climate on the passions, predilections and customs of a people]; and considerations of this type are such familiar topics to physicians, that if the author of *The spirit of the laws* had taken for granted that their doctrine on this matter was quite widespread, he would have been able to content himself with establishing that the laws, customs, and types of government of each people are necessarily related to their passions, predilections, and customs, without taking the trouble to determine the relation of those passions, predilections and customs to . . . climate.[11]

The tradition alluded to grew out of the Hippocratic treatise *Airs, waters and places*, and ascribed the physical and mental character of a community to its environment, including winds ('airs'), water sources ('waters'), soil, vegetation, the orientation and location of habitations ('places'), sanitation, diet, occupations, and so on. A neo-Hippocratic revival, inaugurated by the physician Thomas Sydenham in the mid 1600s, continued strong throughout the Enlightenment and generated considerable interest in climate and its influence on culture.[12]

J. L. Heibron and R. E. Rider, eds., *The quantifying spirit in the eighteenth century*, Berkeley, 1990, pp. 143–78.

[9] Diderot and D'Alembert, *op. cit.* (7), III, 532. The language again from Chambers.

[10] Ibid., 534.

[11] Ibid. Clarence Glacken agreed that Montesquieu was serving up 'dishes . . . from old and well-tested recipes'. Glacken, op. cit. (5), 576.

[12] See Genevieve Miller, '"Airs, waters, and places" in history', *Journal of the history of medicine*, (1962) 17, pp. 129–40; L. J. Jordanova, 'Earth science and environmental medicine: the synthesis of the late Enlightenment', in Jordanova and Roy Porter, eds., *Images of the earth*, Chalfont St Giles, 1979, pp. 119–146; Frederick Sargent II, *Hippocratic heritage: a history of ideas about weather and human health*, New York, 1982; James C. Riley, 'The medicine of the environment in eighteenth-century Germany', *Clio medica*, (1983) 18, pp. 167–178; Riley, *The eighteenth-century campaign to avoid disease*, Houndmills and London, 1987; Riley, *Sickness, recovery and death: A history and forecast of ill health*, Iowa City, 1989.

The *Encyclopédie*'s third definition emphasized another aspect of the Hippocratic tradition, the relation of climate and disease:

> Physicians consider *climates* simply according to the temperature or degree of heat proper to them: *climate*, in this sense, is exactly synonymous with *temperature*.[13]

The definition continued with a discussion of climate's influence on health, disease, and appropriate medical treatment.[14] This aspect of the Hippocratic tradition constituted what has been called an 'environmental medicine' in the eighteenth century[15] and greatly stimulated the pursuit of meteorology and medical topography (medical topography studied the relations of geography and disease).

The encyclopaedias do not capture a fourth and last context of 'climate', found in speculations on climates of the very ancient or geological past. Buffon's theory has already been mentioned. In the early nineteenth century, geologists and botanists like Cuvier, Buckland, the Brongniarts, the younger de Candolle, and Lyell used evidence of fossil plants and animals to support speculations on the climates in which they lived.[16] A related line of thought contemplated climatic effects of alterations in the earth's orbit or the sun's luminosity, or of heat loss from the earth into surrounding space.

These meanings and contexts helped shape the debate over the ancient climate. During the eighteenth century several literary figures and historians developed an increasingly historical approach to the ancient climate; their work grew out of the Hippocratic tradition relating climate and culture. One might expect natural philosophers and meteorologists to respond to their pronouncements, but prior to 1800 they did not. In the 1770s meteorologists began entirely separate investigations of past climates from a fundamentally ahistorical point of view. Beginning around 1800 climatologists combined earlier approaches, drawing on documentary, botanical, and meteorological evidence and applying techniques of source criticism and philology borrowed from the emerging discipline of history. Not only can we say that they thought historically about climates: in their work scientific and historical techniques coalesce at times to such an extent as to obliterate the distinction between science and history. By the 1830s a satisfactory consensus had been reached on the question of the ancient climate.

One final point must be made before I follow these developments in some detail. While the authors examined in this paper comprise coherent and representative traditions, they by no means exhaust historical thinking about climate. Besides speculations on geological climates, I have had to leave aside

[13] Diderot and D'Alembert, op. cit. (7), 534.

[14] Ibid., 534–6.

[15] See works cited in ref. 12, esp. Jordanova; Glacken, op. cit. (5), Chs. 2, 9.

[16] Janet Browne, *The secular ark: Studies in the history of biogeography*, New Haven and London, 1983, pp. 86–94; Laudan, op. cit. (1), pp. 171–8, 210–16.

an important debate over climatic effects of deforestation. I hope to present a more complete analysis of this material in the future.

2. THE DEBATE OVER THE ANCIENT CLIMATE

Eighteenth-century views

The Abbé Dubos' *Réflexions critiques sur la poësie et sur la peinture*[17] won for him election to the Académie Française, where he became perpetual secretary, and a reputation as the founder of modern aesthetics.[18] In it he took up a question from the controversy of the ancients *versus* the moderns: What are 'the illustrious centuries', 'the ages in which men of genius attain the merit of which they are capable'; and why are they so?[19] He discovered four ages of genius: 'that which commenced ten years before the reign of Philip, father of Alexander the Great, that of Julius Caesar and Augustus, and that of Julius the Second and Leo the Tenth, finally that of Louis the Fourteenth'.[20]

Dubos found 'moral' causes — peace and prosperity, the patronage of ruler and people, a strong artistic tradition — insufficient to explain the clustering of genius and concluded that physical causes such as climate must be responsible. Just as certain years are more favourable for the growth of trees and plants, he argued, so they may be for the education of men. The Earth's emanations, continually changing, alter the qualities of the air; the air we breathe affects our blood, which nourishes our organs during growth and development. And so 'the human machine is not less dependent on the qualities of the air, and on the variations of those qualities, than a country'. 'Thus', he concluded, 'certain generations in France are more spiritual than other generations'.[21]

This argument has obvious affinities to the Hippocratic tradition relating climate and culture. But as Clarence Glacken pointed out, theories of environmental influence on culture are typically unable to account for cultural change, since the environment is more or less static.[22] Earlier authors did not hesitate to bring this up, objecting that if climate is to be held responsible for the flowering of the arts, why for example do present-day Greece and Italy fall short of their ancient stature? Dubos had recourse to an altered environment: 'the air of Rome . . . has greatly changed'. 'Pestilential exhalations' from neglected sewers and swamps and from recently excavated mines have

[17] 7th edn, 3 vols., Paris, 1770; reprinted Geneva, 1967. The first edition appeared in 1719.

[18] Armin Hajman Koller, *The Abbé du Bos — his advocacy of the theory of climate: A precursor of Johann Gottfried Herder*, Champaign, IL, 1937, 2–3.

[19] Dubos, op. cit. (17), I, 134, 115.

[20] Quoted in Koller, op. cit. (18), 26.

[21] Dubos, op. cit. (17), I, 250–255.

[22] Glacken, op. cit. (5), 109.

warmed the Roman climate. Dubos found evidence for climatic change in ancient literary sources. The *Annals of Rome* report a violently cold winter 480 years after the foundation of the city that froze the Tiber and kept snow on the ground for 40 days. Juvenal and Horace wrote of snow, ice, and a frozen Tiber as common occurrences. Nowadays the river doesn't freeze any more than the Nile at Cairo, and it is a hard Roman winter when the snow remains on the ground for two days. 'We would be better informed, if the ancients had possessed thermometers; but their authors . . . tell us enough to convince us that the Roman winters then were more rigorous than they are today'.[23]

On the face of it this discussion is historical. The question Dubos addresses — the clustering of genius — seems a historical question. He uses ancient sources, argues for climatic change over time, and ties change to historical causes like the breakdown of Roman sanitation. On the other hand what the modern ear perceives as the quaintness of Dubos' argument arises largely from its ahistorical character. His theory is mechanical: the Earth's emanations affect (in an unspecified way) 'the human machine'. As mechanical, the theory is general and universal and cannot comprehend the individual and unique in history. The modern sensibility also detects a discontinuity in Dubos' theory, which jumps from the physical (emanations) to the physiological (blood and organs) and the psychological (genius). One senses the absence of that continuous and ramified net of effects and causes that characterizes modern historical explanation and that, in this case, would spread from climate to agriculture, economy, government, and perhaps finally to culture, art, and genius. More simply put, Dubos does not embed climate in its historical context.

Dubos' remarks were taken up by several literary figures over the course of the century. David Hume and Edward Gibbon quoted him approvingly. Hume did so in his well-known essay 'On the populousness of ancient nations', which also belonged to the debate of the ancients and moderns — for those favouring the ancients argued that the older civilization supported a greater population than modern nations.[24] Hume attacked one of their arguments in particular. The Earth, they believed,

> must . . . have its infancy, youth, manhood, and old age. . . . In the flourishing age of the world, it may be expected, that the human species should possess greater vigour both of mind and body, more prosperous health, higher spirits, longer life, and a stronger inclination and power of generation.[25]

[23] Dubos, op. cit. (17), 297–9.

[24] David Hume, 'Of the populousness of ancient nations', Essay 11 of Hume, *Essays moral, political, and literary* (ed. Eugene F. Miller), Indianapolis, 1985, *Part II* (first published 1752), 377–464. Cf. Glacken. op. cit. (5), 629–30.

[25] Hume, op. cit. (24). The notion of the ageing and decline of the Earth was almost a century

From the greater 'power of generation' derived the supposedly greater populations of ancient nations.

This widely-adopted theory (perhaps 'metaphor' would be better) has strong genetic elements: it admits development and progression but conceives them as universal law. In arguing against this position Hume adopted a more historical approach. He rejected what he called 'general physical causes', such as the ageing of the Earth, in favour of 'particular physical causes', among which he included disease and climatic change.[26] Now 'general' in eighteenth-century natural philosophy denoted 'universal', 'lawlike', 'mathematical', while 'particular' referred to the individual, the concrete, whatever was accessible only through empirical research.[27] 'Particular physical causes' were therefore closer to what we would recognize as historical contingencies than 'general physical causes'. In favouring them Hume was thinking more historically about the past.

Hume agreed with Dubos that the climate had warmed since ancient times. 'Who could discover the mild climate of [modern] France in Diodorus Siculus's description of that of Gaul?' Or the Black Sea climate from Ovid, who in exile there complained of conditions 'scarcely experienced at present in [St] Petersburgh or Stockholm?' Petronius uses 'colder than a Gallic winter' as a proverbial expression; Aristotle reports that 'Gaul is so cold a climate that an ass could not live in it'.[28] Now Dubos used historical sources, but not many; he appears to rely on anecdotal material rather than on the 'painstaking accumulation' of evidence, as Oldroyd put it.[29] On the other hand, Hume's method of estimating ancient populations, which a modern scholar has called 'epoch-making', involved exhaustive accumulation and careful analysis of ancient sources.[30] Applying this method to climate, Hume accumulated a considerable body of source material. His inference from climate to population seems less stretched to us than Dubos' from climate to genius. His preference for the particular over the general, his multiplication of sources, and the connection between climate and population, while not worked out in detail, begin to weave that net that embeds climate in its historical context.

Edward Gibbon gave the ancient climate more summary consideration than Dubos or Hume. He warned that

old by this time and had been set forth by Burnet in his *Sacred theory of the earth* (1680). See Paolo Rossi, *The dark abyss of time: The history of the earth and the history of nations from Hooke to Vico* (tr. Lydia G. Cochrane), Chicago, 1984, 36–7.

[26] Hume, op. cit. (24), 378–379.

[27] Cf. Diderot and d'Alembert, op. cit. (7), 1 (1751), 'Système figuré des connaissances humaines'; for 'general' and 'particular' as applied to physics see Heilbron, *Electricity* (6), 9.

[28] Hume, op. cit. (24), 449–51.

[29] Oldroyd, op. cit. (1), 207.

[30] P. A. Brunt, *Italian manpower*, Oxford, 1971, 11–12, quoted in Hume, op. cit. (24), 378–9 n. 2. According to Brunt, Hume's method remains today the only one allowing a reasonable judgement of Italy's ancient population.

> the general complaints [in ancient times] of intense frost and eternal winter are perhaps little to be regarded, since we have no method of reducing to the accurate standard of the thermometer the feelings of the expressions of an orator born in the happier regions of Greece or Asia.

In support of Hume and Dubos, however, he added evidence 'of a less equivocal nature', citing half a dozen ancient authors on the frequent freezing of the Danube and Rhine rivers. The barbarians, who favoured winter campaigns, transported across the rivers

> their numerous armies, their cavalry, and their heavy wagons, over a vast and solid bridge of ice. Modern ages have not presented an instance of a like phenomenon.

And he mentioned Caesar's report of the reindeer thriving in Germany and Poland, while today, according to Buffon, 'he cannot subsist . . . in any country to the south of the Baltic'.[31] There are just hints here of a critical attitude towards the ancient sources and of biogeographical considerations, which would become important components of the nineteenth-century approach to the ancient climate.

The remarks of Dubos, Hume, and Gibbon constitute what might be considered a literary context for eighteenth-century discussions of the ancient climate. I want now to turn to a meteorological context. Recall that meteorologists used 'climate' in the mathematical way as a synonym for latitude or latitude zone. Under this definition climates, as fixed zones of the Earth, could have no history. It is not surprising, then, that few meteorologists investigated climatic change. In fact, for most of the century they did not investigate climate at all. The chief goal of Enlightenment meteorology was to discover weather patterns on which to base predictions of local weather or, in the case of environmental medicine, to discern correlations between the weather and particular diseases.[32] These goals lay far from the description of climates. But if climate and climatic change were not objects of their intense scrutiny, meteorologists nevertheless began in the last third of the century to investigate past weather.

As in our own time, the late eighteenth century combined climatological and political disasters. The Seven Years' War, with its legacy of destruction, had no sooner ended than a decade and more of cold, wet summers and bad harvests struck Europe.[33] The winter of 1776 was the coldest in the century;

[31] Edward Gibbon, *The history of the decline and fall of the Roman Empire*, 6 vols., New York, 1902, I, Ch. 9, 488–9.

[32] Cf. Feldman, op. cit. (8). 'Meteorologist' may seem obscure and anachronistic. But in fact, meteorology was a recognized subject that attracted considerable interest in the Enlightenment, and those who engaged in it called themselves 'meteorologists'.

[33] Emmanuel le Roy Ladurie, *Times of feast, times of famine: A history of climate since the year 1000*, (tr. Barbara Bray), New York, 1971, 60.

a severe winter in 1788 followed a July hailstorm that destroyed half the grain crop of France.[34] These disasters helped bring on the French revolution the following year.

Amidst the general malaise natural philosophers turned to the past to find whether times were really growing worse. During the winter of 1776 the Académie Royale des Sciences at Paris established a commission to compare the winter with the coldest recorded winter to date, that of 1709. Charles Messier, a member of the Academy more famous for his catalogue of nebulae, studied incidents of the freezing of the Seine and reports of cold winters dating back to 1392.[35] They both carried out exacting comparisons of old and new thermometers, and the commission came out with a recommendation for thermometer reform, which the Academy later undertook. Other meteorological disasters of the century's end stimulated similar efforts.[36]

The comparisons of thermometers and the thermometer reform belonged to an important movement of contemporary natural philosophy, which may be called 'exact experimental physics'.[37] Its practitioners designed precise instruments, took extensive precautions to insure that they rendered accurate readings, and employed them in systematic and exhaustive measurement. These techniques were regarded as revolutionary.

They were also closely related to history. Comparisons of instruments, instrument reform, and precautions taken to insure accuracy amount to criticism of the sources for natural philosophy, which parallels source criticism in history. In fact exact experimental physics adopted a highly critical attitude in general towards instruments and the data they provided. Its emphasis on exhaustive and systematic collection of data likewise has a clear parallel in the discipline of history. Its application to the past heightens the analogy.

This promising analogy, on the other hand, does not diminish the fundamentally ahistorical character of late Enlightenment meteorology. This can best be seen in another meteorological inquiry, the quest for a lunar influ-

[34] Robert D. Harris, *Necker and the revolution of 1789*, Lanham, MD, New York and London, 1986, 273; Michelle Vovelle, *The fall of the French monarchy, 1787–1792* (tr. Susan Burke), Cambridge, 1984, 86.

[35] Etienne Bézout, Antoine Lavoisier, and Charles Vandermonde, 'Expériences faites par l'ordre de l'Académie, sur le froid de 1776', Académie Royale des Sciences, Paris, *Mémoires* 1777, 505–26; Charles Messier, 'Mémoire sur le froid extraordinaire', ibid. 1776, 1–555.

[36] For example, Messier, 'Observations sur les grandes chaleurs, la sécheresse et la diminution des eaux de la Seine, à Paris, pendant les mois de juillet et août 1793, comparées aux chaleurs observées les années précédentes, à compter de 1753', *Mémoires de l'Institut*, An XI 4, pp. 501–564; Jacques-Dominique Cassini, 'Recherches sur les plus grands chaleurs qui ont eu lieu à Paris, depuis 1682 jusqu'en 1794', ibid., pp. 338–362.

[37] Theodore S. Feldman, 'Applied mathematics and the quantification of experimental physics: the example of barometric hypsometry', *Historical studies in the physical sciences*, (1985) 15, pp. 127–97, esp. 149–77; Feldman, 'Late Enlightenment meteorology' (8), 156–8; Heilbron, *Electricity* (6), 73–83.

ence on the weather. Natural philosophers proposed to test this ancient doctrine by correlating weather observations against the moon's motion. Giuseppe Toaldo, Professor of Astronomy and Meteorology at the University of Padua, was the foremost proponent of this line of research. He discovered correlations between recurring weather patterns and numerous lunar cycles, among which the most important was the 19-year synodic period. (The synodic period is the interval after which the moon returns to the same position of its orbit on the same day of the year; it is the least common multiple, so to speak, of the solar and lunar years.) Toaldo compiled chronologies of bad weather spells dating back to ancient times. In every case he confirmed that weather disasters returned after 19-year or other cycles.[38]

This work illustrates the tenacity of meteorologists' interest in weather patterns and correlations. Unlike the investigations of Dubos, Hume, and Gibbon, it lacks even the sense of the flux of time. Searching the past for correlations, meteorologists found sameness rather than change: similar weather from earlier periods which they compared with their own. Time for them did not generate change; it was not progressive or directional. Nor was it continuous: rather, it was punctuated by similar or identical weather events occurring at regular intervals. This holds not only for Toaldo's theories but for the Paris academicians' and other investigations of past climates as well.

Nineteenth-century views

The eighteenth-century literary and meteorological traditions had no contact with each other and were pursued by individuals in entirely different professions. While the literary group adopted an increasingly historical approach to the ancient climate, the meteorologists examined past weather in an ahistorical fashion. In the nineteenth century these two traditions combined with each other and with other interests, particularly paleobotany and plant geography. This cross-fertilization of traditions is reflected in the careers of the individuals involved, especially in the first of them, Noah Webster.

Though best known as a lexicographer, Webster's literary activity ranged across journalism, politics, and historical editing. His edition of John Winthrop's *Journal* is considered a pioneering example of scholarly historical publication, while his lexicography involved him in philology, a subject closely connected with the emergence of history. Webster also pursued interests in meteorology and environmental medicine. *Experiments*

[38] Giuseppe Toaldo, 'Discorso sopra l'anno 1776', *Giornale astro-meteorologici* 1, 131–72; 'Sopra la lunga siccità dell'inverno 1779', ibid., 2, 3–45; 'Della caretia del 1782, e 83', ibid., 131–53.

respecting dew (1809) took up a popular meteorological subject.[39] His *Brief history of epidemic and pestilential diseases*, which became a standard in its field (and which is anything but brief), adopted a historical approach to the Hippocratic theory of disease. In it Webster searched ancient sources exhaustively for support for environmental rather than contagious causes of endemic and epidemic disease.[40] Webster's career, then, combined literary, historical, meteorological and medical interests.

This union of interests and approaches also appears in his paper on ancient climate, 'On the supposed change in the temperature of winter' (1806).[41] 'It is a popular opinion', he wrote,

> that the temperature of the winter season, in northern latitudes, has suffered a material change, and become warmer in modern, than it was in ancient times . . . indeed I know not whether any person, in this age, has ever questioned the fact.[42]

Among those who had not questioned the fact Webster cited Dubos, Hume, Gibbon, and several American writers. His citation of the first three helps to establish the coherence of the literary tradition on the ancient climate; the Americans speculated on the response of climate to deforestation.

Webster cast a critical eye on the sources for the ancient climate. Reports of severe winters, he pointed out, represent exceptional rather than normal weather; 'any man . . . who attends to the descriptions of them in the original authors' will be convinced of this.[43] And the Latin sources of these reports, like Virgil's *Georgics*, describe mountainous regions. Furthermore, modern thermometers located in cities like Rome give artificially warm readings. As for Gibbon's remark that modern armies never cross frozen rivers, this is 'contrary to all historical evidence, and even to facts which took place during that author's life'. Webster listed half a dozen recent winters in which European rivers had frozen solid.[44]

Turning to the biblical climate, Webster addressed references to snow, ice, and other wintry meteors, found especially in the *Book of Job*. To these he applied philological considerations, borrowing from authors like Sir William Jones, who were using textual analysis to understand historical cultures. Jones' research on the language of *Job* suggested Arabian authorship and a setting in Uz, near Chaldea in Arabia Deserta. Taking into account the precession of the equinox, Webster found that the timing and character of

[39] I have not been able to examine this work.

[40] Noah Webster, *A Brief history of epidemic and pestilential diseases*, Hartford, CT, 1799; *DAB*, s.v. 'Webster, Noah'.

[41] Noah Webster, 'Dissertation on the supposed change of temperature in modern winters', [1799; revised, 1806] in Webster, *A collection of papers on political, literary and moral subjects*, New York and Boston, 1843, pp. 119–162.

[42] Ibid., 119.

[43] Ibid., 126.

[44] Ibid., 131–2, 135.

the spring rains mentioned in the book confirmed that location. One cannot therefore draw conclusions from the book, as had been done, about the ancient climate of Egypt and Palestine. Finally, Webster questioned the intent of *Job* — whether it 'describes a series of facts or is a species of dramatic composition, intended to represent the vicissitudes of life, and the human passions'.[45]

'But the most positive evidence' against climatic change Webster found in plant geography. Hume and others had used ancient reports that the grape did not mature north of the Cevennes mountains in southern France as evidence for climatic warming. Webster criticised their sources: ancient authors could not specify the variety of grape planted, which may have differed from today's, or the Romans may have tried to propagate an Italian variety in France. The olive and fig also grew in ancient Italy; pomegranates, olives and figs grew in ancient Palestine. 'We know precisely the degree of heat necessary to bring [these fruits] to perfection; that is a climate as mild as South Carolina and Georgia'.[46] Finally, the olive-growing regions listed by Strabo and by Arthur Young were identical. 'I am willing to rest the whole argument on this fact. . . . I do not find, in history, any evidence that a change of climate generally has carried any of the delicate fruits into latitudes where they did not thrive in the earliest ages'.[47]

There are a number of important elements in this essay: source criticism and philology, plant geography, calculations of the precession of the equinox, and considerations of thermometer exposure. Taken together, they represent a union of methods of natural philosophy and the emerging discipline of history. And in his attention to the setting of ancient literature and considerations of ancient plants, spring rains, harvest times and more, Webster continued the work begun by Hume of filling in the context of the ancient climate.

Webster had complained that after a few mild eighteenth-century winters 'all the world cried out, what a change of climate.'[48] Leopold von Buch found them crying even in the Norwegian hinterland. Von Buch is best known today as a geologist. But in the early decades of his career he combined a wide range of interests.[49] With his colleague and friend Alexander von Humboldt he helped found what Humboldt called 'physique générale'.[50] *Physique générale* was an 'alliance of the physical sciences' that 'permits us now to treat together parts of physical geography that were

[45] Webster, op. cit. (41), 120–1.
[46] Ibid., 123.
[47] Ibid., 133–4.
[48] Ibid., 136, 138.
[49] J. Ewald, 'Leopold von Buch's Leben und Wirken bis zum Jahre 1806', in Leopold von Buch, *Gesammelte Schriften* (ed. J. Ewald, J. Roth, and H. Eck), 4 vols., Berlin, 1867–1885, 1, pp. v–xlviii, esp. p. x.
[50] Ibid.; Joachim von Schouw, *Tableau du climat et de la végétation de l'Italie*, Copenhagen, 1839, vii; Ludwig Kämtz, *Lehrbuch der Meteorologie*, 3 vols., Halle, 1831–1836, pp. ix–x.

separated in earlier times';[51] it encompassed meteorology and climatology, geology, and biogeography and was another offspring of the cross-fertilization of disciplines. Von Buch's contributions to the field reflect this combination of approaches. 'Bemerkungen über das Klima der canarischen Inseln' investigated trade winds and their influence on plant geography and temperature distribution; 'Über die Grenze des ewigen Schnees im Norden' applied plant geography, especially with relation to height, to the study of the snow-line; and 'Über die subtropische Zone' developed a climatological and plant-geographical definition of the subtropical region.[52]

Von Buch was also interested in history. In his *Reise durch Norwegen*[53] he kept a constant focus on the relations among history, economy, plant geography and, sometimes, climate. For example, he ascribed the lack of forest cover and the desolation of the Arctic Norwegian coasts to their long winter, dreary summer and low mean annual and summer temperatures. As a result the coasts and countless islands can support no agriculture or forestry; the only industry there is the herring fishery. He described the economic history of the region and reported that the herring have recently left the area.[54] Linking in this way climate, forest cover, and economy von Buch embedded climate in its historical setting.

Von Buch found Norwegians unanimous in their belief that the climate had recently altered. Norwegian summers have grown cooler, they reported, the winters not as cold but longer. Earlier, there was no spring in the North, only winter and summer; now spring arrives, but at the time that one used to expect summer. The sowing time, older people told him, is 8 to 14 days later than in their youth. Fruit used to be grown at Drontheim but is no longer. In the mountains small glaciers have appeared and are growing, and mountain peaks formerly free of snow are covered year round. Many feared this trend would culminate in the disappearance of summer altogether, so that the North would experience only one season, similar to March in Germany, and quite unfavourable to agriculture.[55]

Von Buch, however, rejected the possibility of a permanent change of climate. Climate is variable, he argued, and the present alteration will likely soon reverse itself. He searched the Norwegian *Heimskringla*, a thirteenth-century collection of sagas, for evidence. There were years in the Middle Ages when the snow covered the countryside well into summer, but the good years eventually returned. And so they will now. Several decades ago

[51] Von Schouw, op. cit. (50), vii.

[52] Leopold von Buch, 'Einige Bemerkungen über das Klima der canarischen Inseln', Preussische Akademie der Wissenschaften, Berlin, *Abhandlungen, physische Klasse*, 1820–1821, pp. 105–120; 'Über die Grenze des ewigen Schnees im Norden', *Gesammelte Werke* (49), II, pp. 716–45; 'Über die subtropische Zone' (1829), ibid., IV, pp. 1003–8.

[53] Leopold von Buch, *Reise durch Norwegen*, Berlin, 1810.

[54] Ibid., 314–28.

[55] Ibid., 363–4.

Peter Kalm reported his conversation with an old man who told of a much longer growing season than in his youth; now the growing time had shortened again. The changes were cyclical; the Earth's temperature was not gradually cooling, as some claimed. Where, von Buch asked, did oaks and beeches once grow but now grew no longer ? Or firs and spruce ? There was no evidence that the mean temperature anywhere on Earth had fallen even half a degree.[56]

Von Buch combined evidence from medieval sources and from the wide range of subjects encompassed in *physique générale*: seasons and sowing times, fruit culture, plant distribution, glaciers and the snow line. He set climate in its historical and economic context. And though he denied that change had occurred (and we have seen that this is not essential to a historical view), there is a clear sense of progression in his remarks on the appearance and growth of glaciers and on the possibility of gradual climatic change culminating in an altered state of the seasons.[57]

Like von Buch, Joachim von Schouw (1789–1852), Professor of Botany at the University of Copenhagen and Director of the Royal Danish Botanical Garden, incorporated history in *physique générale*. He consciously modelled his method after that of Humboldt, von Buch, and other founders of the discipline, among whom he should be numbered. 'Equally distinguished', according to François Arago, in botany and meteorology,[58] he helped establish plant geography and climatology. His *Grundzüge einer allgemeinen Pflanzengeographie*,[59] composed shortly after Humboldt's famous paper on isotherms, was the first systematic treatise in both sciences, whose interdependence he repeatedly emphasized.

Von Schouw published several pieces on the ancient climate and planned but did not complete two books on climate history.[60] He was sceptical towards claims of climatic change. 'Fewer scientific problems are so often discussed yet so rarely decided by proofs, as whether climatic relations have

[56] Ibid., 365–70. As this discussion suggests, the debate over climatic change was complex, and there was no agreement on the direction of a supposed change. Norwegians believed that the climate was growing cooler, an alteration both more recent and opposite in direction to that favoured by Dubos, Hume, and Gibbon. On the other hand, both groups believed that winters were warming. Although von Buch does not directly engage in the debate over the ancient climate, his discussion is related to it via the issue of winter warming.

[57] On might expect von Buch, as geologist, to take up the question of geological climates. I have not found that he did.

[58] François Arago, 'Sur l'état thermométrique du globe terrestre', France, Bureau des Longitudes, *Annuaire du Bureau des Longitudes* 1834, pp. 171–240, on 204.

[59] Joachim von Schouw, *Grundzüge einer allgemeinen Pflanzengeographie,* (tr. by the author), Berlin, 1823.

[60] These were to be enlargements of von Schouw, op. cit. (50) and a memoir originally in Danish and published in English as 'On the supposed change in the meteorological condition of the different parts of the earth during the historical period', *Edinburgh Journal of Science*, (1828) **8**, pp. 311–26.

changed over time'. He criticized participants in the debate for speculating on causes (i.e., general laws) rather than collecting facts and examining them critically. When they did discuss facts, he complained, they failed to distinguish among regional climates or periods of history.[61] In rectifying these faults von Schouw cast a finer historical net over the climate of the past.

Von Schouw distinguished two historical periods: a 'meteorological period', for which thermometric observations were available, and a 'historical period', for which one had to rely on general historical accounts. To the records of Denmark's 'meteorological period' von Schouw applied the full range of exact experimental criticism, including discussions of instrument comparability and exposure, tables of corrections for variable times of observation, and a survey of methods for calculating mean temperatures. He found no evidence for 'the seemingly universally accepted theory that the winters have grown milder, the summers cooler'.[62]

For the Danish historical period von Schouw insisted on 'a complete, exact and critical study' of the sources. The 11th-century chronicler Adam of Bremen had called north Jutland a wasteland, leading many to conclude that Adam's climate was colder than theirs. But Adam excepted rivers and lake shores, and elsewhere he called Jutland and Sweden fruitful. The waste therefore resulted not from low temperatures but from lack of cultivation. Other chroniclers from the 14th to the 18th century reported crossings of the frozen Baltic from the German coast to Jutland and Sweden. But these authors were not independent; they often copied each other or reported hearsay. And of course the 'winter the likes of which no one had ever lived through' was rare; such winters do not make a climate. Yet after all the Baltic did seem to have frozen more frequently in the past, and von Schouw was forced to admit a harsher winter climate during what we now recognize as the Little Ice Age.[63]

Turning to the ancient climate, von Schouw perhaps echoed Dubos: although no 'artificial thermometer and hygrometer did exist, yet a number of relations and phenomena are known, which, like a kind of natural thermometer and hygrometer, lead more or less to ascertain the climateric relations'. These phenomena included plant and animal distribution; agricultural practices; snow fall, snow line, and the freezing of lakes, rivers, and seas; and aspects of culture like clothing and home heating that vary with climatic warmth.[64]

As a botanist von Schouw was especially interested in plant distribution. He emphasized the importance of the polar and equatorial limits of species

[61] Von Schouw, 'Über die vermeintliche Veränderung der climatischen Verhältnisse Dänemarks und der benachbarten Länder und über die Periodicität dieser Verhältnisse', *Hertha*, (1827) 10, pp. 307–53 (Excerpted from his *Skildring af Veirligets Tilstand i Danmark*, Copenhagen, 1826), on 307–8. [62] Ibid., 308–319, 328. [63] Ibid., 319–26.

[64] Von Schouw, 'Supposed changes in the meteorological constitution' (60), 311–12.

for resolving the question of the ancient climate. If a plant grew in a region in ancient times and now has its polar limit there (i.e., it grows no farther north, or no cooler), then the climate has not become warmer; if it has its equatorial limit there (i.e., it grows no closer to the equator, or no warmer), then the climate has not grown cooler. If, of two ancient plants, one now has its polar, the other its equatorial limit there, the climate has remained constant; 'knowing the mean temperature for this limit, we should be able to ascertain almost with certainty the mean temperature which that region had about 2000 years ago'.[65]

For evidence of ancient plant distribution von Schouw exploited numismatic and archaeological as well as literary materials: drawings on coins and gems, paintings in the Roman baths, in the caves of Elytheia, and at Herculaneum and Pompeii, and sculptures at the Vatican, at Pompeii, and in Egypt. As before he insisted on 'the most rigorous criticism'. For example, the ancients were not careful in describing plants; smaller parts, now considered essential in determining species, were 'utterly unknown to them'. Species may have disappeared because of changing patterns of cultivation rather than climatic change; the same applies to varying harvest times. And when collecting literary evidence of hard weather in ancient times, 'great allowance must be made for the weakness of human memory'.[66]

Following these methods von Schouw built a compelling argument against climatic change. From Biblical, numismatic and literary evidence he determined that the date palm was common in ancient Palestine. Since the date's polar limit lies at 21 °C, the ancient climate could not have been colder than that. On the other hand the equatorial limit of the grape is 21° or 22 °C. Therefore the mean temperature of Jerusalem in antiquity was 21 °C to within a degree. Its modern temperature was the same. The cultivation in ancient Palestine of wheat, which cannot grow in climates hotter than 24°–25°, the coincidence of ancient and modern harvest times, and the need for artificial heat argued for a constant climate over the centuries.[67] Von Schouw obtained similar results for the climates of Egypt, Arabia, and India. Later he concluded:

> The same Nature which surrounds us, also surrounded our Pagan forefathers thousands of years ago; . . . that Nature (material Nature) has remained unchanged, or been but little altered, while the human race, both as a whole and also the individual races, have undergone such great change.[68]

This passage, with its juxtaposition of nature and human history, is typical of his thinking about climate.

[65] Ibid., 313–14. [66] Ibid., 312–14. [67] Ibid., 316–17.

[68] Von Schouw, *The earth, plants and man: Popular pictures of nature* (ed. and tr. Arthur Henfrey), London, 1859, 15–16.

Von Schouw's investigation employed the full range of scientific and historical techniques surveyed in this paper. In his work science and history were very closely allied, as when for Denmark's 'meteorological' period he borrowed from science the name for a historical period. In a similar way, the distinction between philology and plant geography blurred when he probed the meanings of Theophrastus' *phegos* and Pliny's *fagus*. The two ancient naturalists reported these plants growing on the plains of Greece and Italy. Von Schouw decided that they were neither the same tree, as had been supposed, nor the beech, which modern Greeks called *oxua*. He concluded that the beech, which favours cooler temperatures, did not grow on the ancient Greek and Italian plains, and, once again, the ancient climate had not been cooler than his own.[69]

By the middle of the 19th century most climatologists agreed on the constancy of climate since ancient times. Their work was only part of the rise of historical thinking about climate. The whole story must take into account theories of climate's relation to geological history, to the origin and distribution of species, and to the history of the human races. These topics remain objects for future investigation.

[69] Ibid., 320–22.

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Pollution and the protection of inland fisheries in inter-war Britain

JOHN SHEAIL

Introduction

The publication of a World Conservation Strategy and the report of the World Commission on Environment and Development (the Brundtland report) of 1987 represent the most recent, global stage in a long history of debate as to the best ways of safeguarding the natural environment.[1] In Britain at least, the inter-war years can be regarded as an important period in the perception, understanding and management of what would be today called environmental issues.

By taking the thin 'time-slice' of the 1920s, the paper will provide insights into how Ministers and their expert advisers came to perceive and tackle the problems of river pollution within the opportunities and constraints of the necessarily bureaucratic organization of Government. It will look in turn at the methods adopted to secure improvements to the watercourses, and thereby fisheries, through a combination of legislation and appeals for the collaboration of industry. It highlights the importance given to survey, monitoring and research. The paper concludes by focusing on the debate as to whether further regulations could be imposed, say through a central rivers authority, before research had identified more practical ways of mitigating, if not eliminating, the impact of pollution.

Background

Out of the proliferation of new government activities in tackling social evils in the nineteenth century, MacDonagh put forward a model, made up of five stages. The first occurred when an 'intolerable' evil was exposed. The instinctive reaction was invariably to legislate, but close account had to be taken of the political influence of those who had a vested interest in such 'evils'. Various forces of inertia also had to be overcome. The result was a compromise in the form of permissive and enabling legislation. As it became

[1] International Union for the Conservation of Nature, *World conservation strategy*, Geneva, 1981; World Commission on Environment and Development, *Our common future*, Oxford, 1987.

clear that this was not enough, a second stage was reached, in which a body of professionally qualified persons was appointed to act as experts or inspectors to execute the will of parliament. They in turn pressed for further legislation, and for a 'superintending' body to oversee it. As it became clear that this third stage still did not go far enough, MacDonagh discerned a fourth stage, in which the search for instant solutions gave way to a more dynamic concept. It was recognized that there would always be a need for a body of expertise to remedy and tackle the ever-changing manifestations of the problem. Initiatives might have to be taken in areas of government not previously contemplated. Finally, a fifth stage might be reached, in which the executive officers secured even wider powers of administrative discretion, so as to enable the findings of survey and research to be used to prevent, rather than merely cure, the evils they confronted.[2]

How relevant is the model in comprehending the action taken to protect inland fisheries from pollution in the 1920s? For the decision-makers of that period, it was clear that the freshwater fisheries of England and Wales were wasting resources. Whilst potentially the most productive in Europe, even the most highly prized game-fish rivers had undergone a decline since at least the late nineteenth century. The most productive years for the river Tyne was the period 1878–82, when an average of 550,000 lb. weight of salmon was obtained per annum. Since these were the first years for which records were kept, it was impossible to tell how far they represented the capacity of the river. The decline was thereafter relatively regular to an average level of 80,000 lb. weight in the years 1918–23.[3]

The first three stages of MacDonagh's model failed to prevent fish stocks being depleted at a faster rate than could be made good by Nature. Sustained agitation in the mid-nineteenth century had led to the appointment of a Royal Commission. Its recommendations, based on the principles of a closed season, the free ascent of salmon, and prevention of pollution, were implemented in the Salmon Act of 1861, which also included provisions for the appointment of inspectors. The latter played a key role in stimulating moves to introduce a further Act of 1865, which, in turn, set up locally elected boards of conservators over fishery districts that comprised complete watersheds. The inspectorate was incorporated successively in the Home Office, Board of Trade and, from 1903, in the newly formed Fisheries Department of the Board of Agriculture (which became the Ministry of Agriculture and Fisheries in 1919). Historians have characterized the Board's approach as one of caution and empiricism, imbued with the accumulated wisdom of 'secretarial common sense'.[4]

[2] O. MacDonagh, 'The nineteenth-century revolution in government: A reappraisal', *Historical journal* (1958) 1, pp. 52–67.

[3] Public Record Office (PRO), MAF 41, pp. 30 and 303.

[4] R. M. Macleod, 'Government and resource conservation: The Salmon Acts Administra-

The Salmon and Freshwater Fisheries Act, 1923

How far did the post-war years offer the chance of a new beginning? The rampant pollution of the war years, and stimulus of post-war reconstruction, led to renewed calls for the restoration of streams and rivers 'as suitable habitats for flourishing aquatic communities'. The Fisheries Minister, Lord Ancaster, spoke of how the Government did not accept polluted rivers as an inevitable consequence of a great industrial nation. His most senior official was the Fisheries Secretary, Henry G. Maurice, who wrote in November 1924, of how the epithet 'moral' might be applied in a double sense. The conversion of rivers into sewers had a directly demoralising influence on those who lived on their banks. Secondly, the recreations of angling, boating and rowing, which could be found on clear rivers, were among the healthiest obtainable, and, in Maurice's words, 'calculated to act as an antidote to a good deal of the brooding discontent which makes for industrial unrest'. Among the keenest supporters of the Ministry's anti-pollution work were members of Working Men's Anglers' Associations, numbering some 600,000 at least.[5]

The question was not whether, but how, pollution should be curbed, if not eliminated. The difficulties of securing a conviction for gross damage to fisheries were legendary. Under the Salmon Act of 1861, it was an offence to discharge liquid or solid matter into a river to such an extent as to kill fish. This provision had, however, become more or less a dead letter owing to the fact that proof had to be given that the substance discharged had actually killed the fish. Furthermore, no penalty could be imposed if the defendant could prove a right to use the river for the discharge of waste, or had adopted 'the best practicable means within a reasonable cost to render the substances harmless'. The position was further complicated by the right of the defendant to have the question of 'the best practicable means' referred to the High Court and tried by a jury. The law and procedures were so complex and costly as to deter Boards of Conservators from commencing proceedings.[6]

In February 1923, the Cabinet approved a Salmon and Freshwater Fisheries

tion, 1860–1896', *Journal of British studies* (1967) **7**, pp. 114–50; P. Bartrip, 'Food for the body and food for the mind: The regulation of freshwater fisheries in the 1870s', *Victorian Studies* (1985) **28**, pp. 285–304. Analogies may be drawn with the approach of Ministers, officials and 'experts' in the Local Government Board, which, also in 1919, became the Ministry of Health.

[5] PRO, MAF 41, 299. Lord Ancaster was member of parliament for the Horncastle division of Lincolnshire, before succeeding to the earldom in 1910. He became chairman of the Rutland County Council in 1922. He gave his recreations as hunting and shooting. Maurice was called to the bar in 1904, and joined the Fisheries Department in 1912. His pastimes were identified as fishing and natural history. His friendly manner, and considerable abilities as a speaker and chairman played a crucial part in developing 'a community of expertise' in the Department (G. S. Cansdale, 'Obituary: Henry Gascoyen Maurice', *Oryx* (1950) 1, pp. 50–52).

[6] PRO, MAF 41, 30.

Bill, intended to codify 19 previous measures and introduce new powers to tackle *inter alia* pollution. Clause 8 made it an offence to discharge into any water such matter as would 'make the water poisonous or injurious to fish or the spawning grounds, spawn or food of fish'. An offence was committed even if the fish survived. As a concession to industrial interests, it would be a defence for those continuing a manufacturing process at the passing of the Bill to plead that they had used 'the best practicable means within a reasonable cost to prevent such matter doing injury to fish, or the spawning ground, spawn or food of fish'. Even in these instances, however, a major advance would have been made. The defendant would have to prove that there were 'no possible means of disposing of it otherwise than by discharge into the river'. As an explanatory memorandum remarked, the Bill had struck 'a fair middle course' between the interests involved. Almost all fishermen recognized that manufacturing interests were of paramount importance.[7]

The voluntary approach

If the third stage of MacDonagh's model had been attained, in terms of legislation, the position was far less clear in terms of the status and scope of 'the superintending body'. As the Ministry emphasized on many occasions, its powers remained severely circumscribed. In his contribution to a volume on the work of the Ministry of Agriculture and Fisheries, published in 1927, Maurice described how the functions of the Fisheries Department might be summed up in two words — intelligence and advocacy. Its role was to educate and, more particularly, to conserve existing stocks of fish by strengthening the hand of local interests with help and advice. It could not prosecute for offences. That was the responsibility of the Boards of Conservators and private vested interests. The Ministry could only help the former by providing 'scientific information and skilled advice'.[8]

Following a conference convened by the Salmon and Trout Association, a deputation met the Minister of Agriculture. The conference, attended by a wide variety of interests, had passed unanimously a resolution calling for the appointment of an *executive* committee to tackle 'the deplorable position of the fisheries in relation to river pollution'. It should consist of representatives of the Salmon, Trout and Coarse Fishing Interests and Federation of

[7] PRO, MAF 41, 40 and 299. Salmon and Freshwater Fisheries Act, 1923, 13 & 14 George V, c. 16. The requirement to adopt only 'the best practicable means' was a feature of all legislation to control air pollution (E. Ashby and M. Anderson, *The Politics of clean air*, Oxford, 1981).

[8] H. G. Maurice, 'The Fisheries Department', in F. L. C. Floud, ed., *The Ministry of Agriculture and Fisheries*, London, 1927, pp. 273–99. Ministry of Agriculture and Fisheries Standing Committee on Rivers Pollution (MAF SCORP), *River pollution and fisheries*, London, 1924; MAF SCORP, *River pollution and fisheries*, London, 1930.

British Industries (FBI). The Minister of Agriculture suggested to the deputation that a small *advisory* committee might be set up. As the Association's representative, Reginald Beddington, recalled later, there was considerable concern on the part of the deputation that this might be a ploy to side-track the issue. There was, however, no practicable alternative but to give 'the proposed expedient a trial'. A Standing Committee on Rivers Pollution (SCORP) was appointed, under the chairmanship of Lord Ancaster (Maurice took the chair after Ancaster left office in 1924).[9]

On the premise that parliament would not agree to any strengthening of its statutory powers, the Fisheries Department was forced to make a virtue out of political necessity. Lord Ancaster assured a meeting of local authorities in the Trent watershed in April 1923 that the stirring of public opinion was of much greater importance than statutory controls. The Ministry believed more could be achieved through the goodwill and assistance of industry and a sense of local responsibility.[10]

However great their scepticism as to the motives of the Ministry, the Salmon and Trout Association could hardly resist the appointment of an advisory committee that developed the concept of collaboration with the FBI and support for local Boards of Conservators. Furthermore, as an editorial in the Association's *Salmon and Trout Magazine* pointed out, there was need for more information before any sound executive policy could be implemented. The earlier Royal Commissions had focused largely on the chemical aspects of pollution and its regulation. There was urgent need for the biologist to supplement the work of the chemist. The engineer would have to apply

> the results in the practical form of appliances workable upon a sound economic basis before the full benefits of the knowledge obtained can be available.

The biological aspect was particularly important — fish were unfortunately more sensitive than chemical tests. It would be the business of the SCORP to devise the biological methods required to test the soundness of the inferences made by the chemist as to the effects of polluting substances on the health of watercourses.[11]

From the Ministry's perspective, the SCORP would enable the Fisheries Department to provide a vastly improved 'intelligence' service. It would be composed of leading figures in the relevant voluntary and professional bodies. As Maurice noted, the Committee would gather 'such a volume of direct evidence of actual and injurious pollution', through local interests and agents, as would greatly 'strengthen the position of those who seek to have

[9] *Salmon and trout magazine* (1921) **26**, pp. 167–70 and 177–91, and (1922) **27**, pp. 255–8.
[10] PRO, MAF 41, 302.
[11] *Salmon and trout magazine* (1921) **26**, 167–70.

the pollution question seriously tackled, by legislation or otherwise.[12] As early as October 1922, a press notice recounted how the two conflicting interests could be reconciled 'by pointing out to manufacturers suitable means of disposing of their waste products without unduly hampering trade'. Wyken Slough, a lake near Coventry, had been 'denuded of all forms of life' in the summer of 1921. The loss was attributed to drainage from the adjacent chemical dump of a Courtauld's factory. Whilst disclaiming all responsibility, the company had installed special treatment tanks, following representations made through the FBI.[13]

It was much harder to know how to react to cases of blatant obstruction. Despite written assurances from the South Durham Steel Works Company that spent acid effluent was collected and sold, enquiries in the spring of 1925 showed that it was discharged by a recently constructed drain to a field, from whence it found its way into the Tees. As Lord Gainford (the chairman of a Tees Joint Committee of manufacturing and fisheries interests) pointed out, the greatest difficulty was that even those manufacturers, genuinely anxious to help limit pollution, resented any publicity or formal communications that appeared critical of industry's practices. A fine sense of judgement was required as to when and how the SCORP might intervene. As Lord Gainford discovered, later the same year, a more conciliatory spirit was becoming evident on the Tyne. He believed manufacturers had at last come to recognize that their effluents had practically destroyed the salmon of the river Tyne. All the descending smolts had been killed, and spawning fish were unable to enter the river. Next to nothing had been caught.[14]

It was not long before signs appeared of the fisheries question entering MacDonagh's fourth stage, namely of the SCORP widening its investigations to cover not only industrial, but sewage, pollution. Whilst co-operation from industry would eliminate the most harmful effluents (and there were some signs of this happening), there would be little benefit to fisheries if sewage continued to pour into the river. The principal effect would be 'to save the fish from being poisoned so as to exterminate them by suffocation'. The experience of the Tyne estuary was instructive. The impact of sewage on the river increased as the population of Tyneside grew, and outlying districts were empowered to discharge their sewage into the tidal waters. In the course of travelling over long distances in enclosed pipes, the sewage had already undergone some putrefaction, and would therefore absorb dissolved oxygen much more quickly than fresh sewage at the same temperature, when released into the river.

[12] PRO, MAF 41, 299. [13] PRO, MAF 41, 302. [14] PRO, MAF 41, 303.

Survey, monitoring and research

Whatever the positive merits of a voluntary approach, there was a very practical reason why resort to the law was eschewed. As was pointed out when a local Fishery Board pressed for the prosecution of the Luckett Mines in Cornwall, the defendants would have no case to answer until the requisite scientific and topographical data were collected. As Macleod has remarked, in a review of Government and the deployment of expertise at the turn of the century, legislation and administration could only proceed as fast as the knowledge on which action was based. The closer an issue was to the limits of accepted knowledge, the greater might be the divergence of professional opinion.[15]

Maurice hoped the experience gained by the SCORP in monitoring the state of a selection of rivers could be used to set up subcommittees for every watercourse, so that over time each could be classified, according to its actual or potential value for fishing, and ways in which its condition might be maintained or improved.[16] The local response varied. The surveys carried out on the river Wharfe by Leeds University were a good example of well-directed local effort. There was no obvious body to 'fight the battles' of the Bedfordshire Ouse. Local authorities might fail to respond to even the most personal overtures made by Ministers. At a meeting of authorities in the Trent watershed, addressed by Lord Ancaster in April 1923, many councils asserted that they were quite capable of looking after their respective part of the river without the intervention of a joint committee. The most that could be achieved was a one-day survey to identify the worst forms of pollution, provided it cost nothing to the authorities. Officials in the Fisheries Department took consolation from the fact that the mere prospect of such a survey caused some sanitary authorities to take steps to improve the quality of their discharges.[17]

As Maurice reaffirmed in May 1928, the object of the surveys and monitoring was to collect such a volume of evidence of pollution as would persuade or coerce polluters to take remedial steps. By the end of 1926, the first hundred surveys had been completed by the chemist on the Department's staff, Dr E. C. Jee, who had been seconded to the SCORP as its Technical Adviser. He was usually assisted by local personnel. Two types of watercourse were surveyed, namely those where pollution was suspected, and those previously surveyed so as to assess whether there had been any improvement.[18]

[15] PRO, MAF 41, 302; R. Macleod, ed., *Government and expertise: Specialists, administrators and professionals, 1860–1919*, Cambridge, 1988, pp. 1–24.

[16] MAF, SCORP, *River pollution and fisheries*, London, 1925; MAF SCORP, *River pollution and fisheries*, London, 1926.

[17] PRO, MAF 41, 302.

[18] PRO, MAF 41, 313.

Valuable experience of the rewards and constraints of these types of survey was gained on the rivers of south Wales and Monmouth, carried out at the instigation of the South Wales and Monmouth Federation of Angling Sports. Using as a baseline the data collected for the Royal Commission on Sewage Disposal in August 1904 on the river Usk, sampling at two points in August 1922 indicated an improvement in terms of levels of dissolved oxygen. Over the next six years, four lines of investigation were pursued in collaboration with the Medical Officer of Health, and with a grant from the Corporation of Newport. Intensive sampling of the subsurface and midstream water was carried out from Newport Bridge at hourly intervals over a tidal cycle in March 1923. Weekly observations were made about the same time each day of the week (except Sunday). Owing to the possibility of the results being affected by small bodies of polluted water, the results were presented as monthly means. Five such series were collected — March, July and October 1923, July 1924 and July 1928. The results suggested that, whilst parts of the estuary might occasionally relapse into an unsatisfactory state, as measured by the extent of de-oxygenation of the water and/or its putrescibility, such a condition did not appear to persist long enough to constitute a definite menace to the fisheries.[19]

As the Fisheries Department discovered, it was one thing to appoint an advisory committee, but another to find the resources to collect the necessary field data, especially biological information. As well as Jee, two further officers, a biologist and further chemist, were seconded to the SCORP. They were based at the Fisheries Experimental Station at Alresford in Hampshire. The station, the first of its kind in Britain, was acquired by the Department in April 1922, initially to carry out practical studies on the impact of the washings from newly tarred roads on stream life. It soon began to undertake research on other forms of pollution. A backlog of surveys and reports quickly built up.[20]

The Director of Fisheries Research, Dr E. C. Russell, refused to allocate more staff at the expense of other branches. Whilst the earlier undertaking given to the Treasury that the Standing Committee would cost nothing remained substantially true, the Department was in fact 'robbing the Peter of the sea fisheries to pay Paul of the inland fisheries'. At Russell's instigation, a Scientific Advisory Committee was appointed to identify the most cost-effective way of drawing up an inventory of all the larger and more polluted rivers. At its second meeting, the Committee approved a two-fold approach. The rapid chemical surveys of the most obviously polluted rivers should continue. They were important in assessing the extent of pollution, and had a great propaganda value. Turning to the more general studies, there was

[19] PRO, MAF 209, 923.

[20] PRO, MAF 41, 300; J. Sheail, 'Road surfacing and the threat to inland fisheries', *Journal of transport history* (1991) **12**, pp. 135–47.

need for ecological research, focused on the balance of all the different forms of life under particular environmental conditions. Both observational and experimental studies were required. The Treasury approved in 1925 the recommendation that a further two assistant naturalists should be appointed, as well as an additional post in the Laboratory of the Government Chemist to carry out detailed chemical analyses.[21]

It proved unexpectedly difficult to find a river affected by a 'simple' source of sewage pollution, suitable for developing methods for biological survey. Whilst the river Lark in Suffolk was eventually chosen, in the vicinity of the sewage outlet from Bury St Edmunds, it was soon discovered that conditions were greatly affected by the beet-sugar factory, four miles upstream. The Scientific Advisory Committee nevertheless regarded the report of R. W. Butcher and F. T. K. Pentelow, of the Alresford Station, as a fine example of broadly-based investigations. As well as describing the consequences of the reduction in dissolved oxygen, the authors had drawn attention to the losses of different forms of plant and animal life. There was need for ecological research on the mechanism by which the river life might be restored, if pollution stopped.[22]

Whilst the Alresford station proved to be a 'blind alley' in the sense that it was poorly located and under-resourced, it gave further prominence to the need for such stations, that combined laboratory and field facilities for research in both freshwater and terrestrial ecology. It was a model for the establishment of the Freshwater Biological Association at Windermere, established by a consortium of learned and professional bodies for research and teaching purposes in 1929.[23]

The concept of a central authority

As each river was surveyed, or resurveyed, members of the SCORP posed the question, with increasing stridency, 'What are you going to do?' In June 1926, the South-Western Rivers Subcommittee wrote to the Standing Committee, protesting that, despite the valuable diagnostic work done, there was still no prospect of effective action being taken. The Medical Officer of Health, who had played a crucial part in monitoring the Usk, threatened to withdraw his help unless there was some evidence of remedial action being taken.[24]

[21] PRO, MAF 41, 312.

[22] R. W. Butcher, F. T. K. Pentelow and J. W. A. Woodley, 'An investigation of the river Lark and the effect of beet sugar pollution', *Ministry of Agriculture and Fisheries, Fishery investigations series,* I, Vol. III (1930).

[23] J. Sheail, *Seventy-five years in ecology: the British Ecological Society*, Oxford, 1987, p. 105. For the microbiological aspects, see K. Vernon, 'Pus, sewage, beer, and milk; Microbiology in Britain, 1870–1940'. *History of science* (1990) 28, pp. 289–325.

[24] PRO, MAF 41, 303 & 209, 923.

Maurice could hold out little promise of immediate action to secure what was, in effect, that other component of MacDonagh's third stage, a superintending central body. Speaking from the chair at a meeting of the SCORP, his reply fell into two parts. Neither the Ministry, nor indeed the Ministry of Health, could prevent or cure pollution single-handed. It called for a much broader Government initiative. Secondly, whilst major advances had been made under the aegis of the SCORP in identifying the incidence and severity of different forms of pollution in watercourses, any permanent remedy had to be founded on a knowledge of how that pollution might be prevented in the first place. This would require experiments at the laboratory and works scale. As Maurice observed, manufacturers should be carrying out the necessary research. They were, however, too heavily handicapped by the depression. Since successive Governments had encouraged the growth of what amounted to vested interests in pollution, he personally believed the Government should be prepared to shoulder the burden of finding a cure.[25]

At Maurice's instigation, the Minister of Agriculture and Fisheries took the chair at the next meeting of the Standing Committee, in July 1926, from which he stressed how the first important step was to show polluters the way to remedy the damage caused to fisheries without incurring excessive expense. No one could advocate a campaign of river-cleansing if it meant throwing workers out of employment and closing down works. Whilst none disagreed, several members of the Committee doubted whether there would be the necessary determination and resources for research if it was approached from the fisheries standpoint alone. The initiative had to be broadened to take account, for example, of the health and amenity aspects of rivers. Some kind of authority was needed, that would have 'supreme control over all inland waters'. The Minister was told of how the Salmon and Trout Association, and the British Waterworks Association, hoped shortly to make a joint approach to the Government, setting out in detail the need for such an authority. The Minister used this as a pretext to close the discussion.

The joint memorandum was sent to the Prime Minister, Stanley Baldwin, in November 1926. It used the device of highlighting current inconsistencies to justify advice given to the Government over many years. Attention was drawn to the chaotic distribution of responsibility for river management both between and within Government departments. Whilst the agricultural side of the Ministry of Agriculture and Fisheries encouraged the establishment of beet-sugar factories, the fisheries side became ever more alarmed as to the effects of the resulting effluent. It was difficult to see how the Ministry of Health could reconcile schemes for turning inadequately treated sewage

[25] PRO, MAF 41, 303.

into rivers identified as sources from which further water might be abstracted for drinking purposes. Every major inquiry since the Royal Commission of 1866–69 had supported the concept of some kind of central authority. The joint memorandum pressed for the establishment of a national, co-ordinating authority, with powers to set up River or Watershed Boards for all rivers. It would consider every river problem from a national perspective.[26]

The Prime Minister's Private Office suggested that Lord Balfour might meet a deputation on Baldwin's behalf. Among his ministerial duties as Lord President of the Council was a responsibility for science. Balfour's lifelong interest in science had been put to good effect in his promotion of what became the Department of Scientific and Industrial Research and the Medical Research Council. Impressed by the need for greater co-ordination in the Government's promotion of civil science, he had instituted a Committee of Civil Research, to advise on broad questions of national policy, involving scientific and economic research, and to co-ordinate work involving the relevant Government departments.[27]

For the purposes of the deputation, Baldwin's Office suggested that Lord Balfour might be joined by the Ministers of Health, and Agriculture and Fisheries. As a briefing to Lord Balfour made clear, the Ministry of Health saw no need for a national, co-ordinating body 'to give orders to local authorities'. With the exception of fisheries, the entire question already fell within the purview of the Ministry. It would be enough to appoint an Advisory Body, composed of representatives of the two Departments, and such outside bodies as the British Waterworks Association and FBI.

For his part, Lord Balfour told the deputation how the Cabinet 'listened with a most sympathetic ear, and with a complete understanding of the feelings' represented by the deputation. He doubted, however, whether all those who used the magic phrase, 'a central authority' agreed with one another as to the precise powers which might be given to such a body, or how it might relate to central and local government. Too much emphasis had been placed by the deputation on co-ordination and the giving of advice.

In Lord Balfour's view, the fundamental thing was to acquire new knowledge by using 'the best scientific brains in the country'. That was not to say the path would be a short one. It would be absurd to suggest that by merely setting up the most perfect scientific organization, a solution would be found straightaway. In Lord Balfour's words, research was a long and doubtful business. The best equipped laboratories, the most instructed men of science, could not anticipate discoveries. Nevertheless, the Government had been convinced for some time that such research was required. The necessary funds would be provided.

[26] PRO, DSIR 13, p. 4.

[27] K. Young, *Arthur James Balfour*, London, 1963, pp. 446–449.

The leader of the deputation, Lord Gainford, protested that it had 'not got anything very definite out of the Government'. At a meeting of the SCORP the next day, Lord Mildmay accused the Minister of not being in touch with the subject. Reginald Beddington, the representative of the Salmon and Trout Association, gave formal notice of a resolution, calling for the dissolution of the SCORP. In his judgement, a brick wall had been reached. The Committee had no executive powers of its own, and the Ministry, whilst supportive, had only a small staff and no powers to deal with offenders. He was sure the representatives of industry would be much happier dealing with a central authority representative of all water interests.[28]

Maurice succeeded in persuading the Committee to withdraw the motion of dissolution, at least temporarily. As chairman of the Committee, he certainly felt they had reached a dead end. It was, however, to the credit of the fishery interests that they had brought to a head an issue that involved so many other interests. As Fisheries Secretary, he was aware of earnest conversations taking place between Government departments.

A Water Pollution Research Board

The response of members of the SCORP to the apparent rebuff highlighted the difficulties of such advisory bodies in reading the Government mind. Did Lord Balfour's attitude reflect a lack of concern on the part of Ministers, or an inability reveal what positive moves were already underway behind 'closed-doors'? Could Balfour have really been so ignorant as to what was required? He had received a two-hour briefing from Maurice and the Secretary of the Department of Scientific and Industrial Research (DSIR) prior to the deputation. As Henry T. Tizard, who was the Principal Assistant Secretary in the DSIR, later recalled, Balfour not only co-ordinated and backed scientific projects, but he often understood and discussed with officials the science involved. There were occasions when he asked for schemes to be reconsidered, and he was invariably right.[29]

Rather than a stalling mechanism, was it possible that Lord Balfour's refusal to entertain the idea of a central water authority arose from a genuine conviction that the immediate priority was to secure further advances in research? To answer the question, it is necessary to recount a series of initiatives taken over several years.

In October 1923, it is possible to discern an important feature of MacDonagh's fifth stage, namely an initiative to prevent, rather than merely mitigate, the 'evils' of pollution. Maurice wrote to the DSIR, suggesting that it might undertake research on ways of eliminating the need to discharge

[28] PRO, DSIR 13, 4, and MAF 41, 303; Parliamentary Debates, Lords, 68, pp. 601–24.

[29] Young, *op. cit.*, H. Tizard, *A scientist in and out of the Civil Service*, London, Birkbeck College, 1955.

such toxic substances from various industrial works, whether by changing the manufacturing process, or more realistically, through improved treatment of the effluent, before it entered the watercourse. Maurice cited the impact of tar acids, derived from the effluent of coke oven plant along the Tees estuary. Were there practicable ways of reducing their toxicity 'to the lowest degree obtainable at a reasonable cost'? Tizard was sceptical as to whether the DSIR could help; a scientific solution, even if it could be found, might not be the most practicable economically.[30]

At Tizard's suggestion, a further application was made in July 1926, this time jointly by the Ministry of Agriculture and Fisheries, and Ministry of Health. It called for wider-ranging studies to be carried out in the laboratory and in the individual industries and sewerage works. The Advisory Council of the DSIR supported the application. As the Secretary of the DSIR, Sir Frank Heath, reported to Lord Balfour, in October 1926, the longer the problem of such pollution was left, the greater would be the cumulative loss, and the consequent cost of the measures that would ultimately have been taken. The trend in public policy was towards encouraging industrial development in rural districts, as part of the garden city schemes. This would be hampered if, for example, limits had to be imposed on the establishment of beet-sugar factories. Inquiries made through the Foreign Office indicated that no other country with a history of beet growing had found a way of avoiding such pollution.[31]

Lord Balfour agreed that a large-scale programme of research into the problems of water supply and effluent disposal should be initiated. He gave full support to the insistence of the Advisory Council that such research should be a standing function of the DSIR, and that an eventual level of funding of some £20,000 per annum should be assured. There would otherwise be no chance of attracting the services of suitably qualified scientists. It was at this point in the decision-making process that the DSIR learned of the memorandum received by the Prime Minister, and Baldwin's Private office became aware of the recommendation of the Advisory Council. It was the realization that these two separate initiatives were taking place that caused the Private office to suggest that Lord Balfour should meet the deputation. Balfour realized that Cabinet support for the full funding of the research programme could be made a condition of his meeting the deputation.

Sir Frank Heath found, from preliminary discussions, that the Treasury was likely to be sympathetic. Baldwin suggested that Lord Balfour might give an undertaking to urge on his colleagues the importance of making provision for research. As a Treasury internal minute pointed out, no definite figure could be given for supporting the new programme, but savings could be anticipated elsewhere in the DSIR budget. It had been rumoured

[30] PRO, DSIR 13, 1. [31] PRO, DSIR 13, 4.

that research on commercial forms of smokeless coal was a 'washout'. Savings might be made in building research. The DSIR gave assurances that economies might be expected, largely in the National Physical Laboratory, and from discontinuing research on alcohol oils and fats, and some grants to research workers and students.[32]

In a telephone conversation with the Prime Minister's Office, the DSIR reported that Lord Balfour had reached an understanding with the Treasury, and would take the deputation. Whilst he would not encourage hope for a central administrative authority, he would state that the Government intended to start on comprehensive research. The work was to be carried out under the aegis of a Water Pollution Research Board.

Conclusions

The management of water bodies, and in particular the conservation of fisheries, have received much attention in historical research on what has come to be known as 'the tragedy of the commons'. Models have been sought to explain the use and misuse of common-property resources.[33] One of the most important agendas to be explored are the interactions between the different forms of ownership, and the steps taken, if any, to acquire and utilize information relevant to the stewardship of that resource.

In his paper of 1958, MacDonagh discerned a paradox. On the one hand, the process of fighting a perceived evil depended on public and parliamentary opinion. On the other hand, the closer a subject engaged the attention of public opinion, politicians and civil servants, the more obvious was their commitment to the *status quo* in law, and misgivings as to whether the Government's limits of 'interference' should be extended. For Maurice, it seemed paradoxical that, at a time when the use and management of the river banks became ever more prescribed, the rivers themselves suffered from constant neglect. As he wrote, in his report for 1924 'they are nobody's children because they are everybody's children'. Whilst the Fisheries Department could hardly claim 'a monopoly of parental rights or functions', Maurice believed it was scarcely an exaggeration to say that the Department, and those who supported its goals, were alone in attempting to improve the state of the rivers. For as long as that remained the case, they would invite the gibe, 'What do a few fish matter ?' Fishing interests would be accused of seeking to hold back the revival and development of manufacturing industry, upon which the nation depended. There was an obvious need for other

[32] PRO, T 161, p. 272, S30808.

[33] B. M. McCay and J. M. Acheson, eds., *The question of the Commons. The culture and ecology of communal resources*, Tucson, 1987; F. Berkes, ed., *Common property resources: Ecology and community-based sustainable development*, London, 1989.

interest groups to join in pressing for improvements in river management.[34] In that respect, the appointment by the Minister of Health, and Minister of Agriculture and Fisheries, of a Joint Advisory Committee on River Pollution in 1927 (representative of a much wider range of interests) was at least a symbolic move in the right direction.[35]

Whether primacy was given to legislative safeguards or voluntary action, Maurice wrote of how

> It is our hope and belief that pollution can in great measure be controlled, checked and ultimately banished without prejudice to industrial developments.

Whether such aspirations were justified would depend mainly on what 'science can teach us'. There was no reason for despair if the scientific effort could be mobilized effectively. The country was, however, so tightly in the grips of economic recession that, as Maurice conceded, it was difficult to secure the funds needed for the many experiments.[36] In that context, the appointment of the Water Pollution Research Board in 1927 was a signal achievement.[37] But even then, the implementation of the findings of science was by no means assured. Writing in the late 1920s, Maurice commented on how the Ministry had hoped to secure a reasonable measure of protection without recourse to legal proceedings. Whilst it was too soon to despair of co-operation, members of SCORP had found on some occasions that nothing short of resort to the law, and the prospect of further costs, brought home to the polluting parties the need for a proper and more informed response.[38]

Elements of the five stages of MacDonagh's model, developed to explain a nineteenth-century situation, can be discerned by the late 1920s in the steps taken to protect inland fisheries from pollution. Whilst still the outcome of compromise, legislation had been strengthened. A body of expertise had been developed. In the Fisheries Department, the Minister could call upon the advice of his own scientific staff and the collective wisdom and knowledge of the relevant voluntary and professional bodies, as represented on the SCORP. Through the initiative of Maurice, other sectors of Government, and most notably the DSIR, had become involved in the search for ways of preventing, as well as curing, the effects of pollution.[39]

The emergence of such 'communities of expertise' within Government

[34] Ministry of Agriculture and Fisheries (MAF *Report*), *Report of proceedings under the Salmon and Freshwater Fisheries Acts*, London, 1926, pp. 3–4; MAF, *Report* (1930), pp. 21–6.

[35] PRO, MAF 41, 316.

[36] MAF, *Report* (1922), 3–5.

[37] PRO, DSIR 13, 2.

[38] MAF, *Report* (1930), 21–6.

[39] Parallels can be discerned between the contribution of Maurice and the approach, and variety of methods, adopted by Sir Lional Earle, the Permanent Secretary of the Office of Works, in respect of the preservation of ancient buildings and monuments (L. Earle, *Turn over the page*, London, 1935).

constitute an important agenda for further research on the machinery of Government, both for their positive and negative attributes. The interest groups and professional bodies concerned with fisheries were among the first to enter into a dialogue with Government, as attempts were made more generally to recover the momentum of pre-war campaigns, whether by individual lobbying or by association (as most outstandingly demonstrated in the creation of the Council for the Preservation of Rural England in 1926).[40] There were obvious attractions for representatives of those interest groups and professions in being able to tender advice and hear at first hand the preoccupations of Government. It was a dialogue of both political and practical value to the respective Government departments. There were, however, as those on the SCORP found in 1926, severe limits as to what they could be told of the thinking and initiatives of Ministers — the ultimate decision-makers. The need for confidentiality on the part of Ministers and their officials could so easily be misconstrued by 'outsiders' as a lack of interest or understanding on the part of Government.

A growing sense of frustration at their apparent inability to put the message across, no matter how well argued, caused many pressure groups (both during and since the inter-war period) to debate whether a more public, adversarial stance should be adopted. There was a strong temptation to abandon patient, behind-the-scenes dialogue for a more strident approach to campaigning for the environment.

[40] J. Sheail, *Rural conservation in inter-war Britain*, Oxford, 1981.

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'The smoke menace': Cinema, sponsorship and the social relations of science in 1937

Timothy Boon

Introduction: From environment to the history of the representation of smoke

> 'During the past quarter century, environmental conservation has rocketed from obscurity to a position of world influence. Its network of observers watch . . . Its researchers present embarrassing challenges . . . Its films, television and radio programs, and reports bring into many millions of homes authentic pictures of the natural world and its manmade problems . . . It is, in fact, very much a part of the action of the modern world, and has become so during the last twenty-five years . . .'[1]

Max Nicholson, the seasoned environmental campaigner, wrote these words in 1986. His sentiments are of value because it is, one suspects, the current dominance of 'green thinking' that makes the study of the history of environment so seductive. But Nicholson denies that environmentalism has more than a 25-year history; he doesn't even want to claim the passing of the Clean Air Act (1956) for the cause: 'although there was a kind of lobby in existence by this time, its campaign had not availed until [external factors] came to its support'.[2] If environmentalists do not wish to claim smoke abatement (campaigning to reduce the burning of raw coal) for their own, then it would certainly be curious if historians were to ascribe it to them. Environment is not a unitary theme 'running through history'; the subjects now contained within environmentalism were discussed within different scientific and social contexts in the past. We must respect these contexts

I should like to thank Chris Lawrence, Stephen Johnston and Alan Morton for reading and commenting upon earlier drafts of this paper. Seminar audiences at Manchester and at the Wellcome Institute and the Science Museum in London have witnessed the evolution of closely related material and provided helpful criticisms. I owe a debt of gratitude to my family for their support and forbearance. I gratefully acknowledge my indebtedness to the Science Museum for supporting this work. Stills from *The smoke menace* reproduced by courtesy of British Gas plc.

[1] M. Nicholson, *The new environmental age*, Cambridge, 1987, p. xi.

[2] Ibid. p. 24. The phrase 'kind of lobby' refers to the National Smoke Abatement Society, for which see below.

whether we are studying smoke abatement, impure water, or whatever. As a case study of the different contexts of smoke abatement in the late thirties, I present here a discussion of the 1937 documentary film *The smoke menace*. Work on the history of smoke abatement has tended to concentrate on its political context, and mainly the passing of legislation, but this paper shows that studying *The smoke menace* and its context reveals different stories about smoke abatement from those in the established accounts.[3]

Studying *The smoke menace* provides a different focus both from the whiggish distortions of a potential 'environmentalist' account and from the concentration on parliamentary legislation common in published work on air pollution. But there are theoretical problems with concentrating on films or other image artefacts: whilst they promise great potential for studies of science and the public, the evidence they contain is problematic and ambiguous. This paper proposes that a solution to this difficulty can be found in treating the film as the product of a heterogeneous alliance, much wider than simply film-makers, including all interested parties. Within this approach, *The smoke menace*, for example, serves as the occasion and site for study of an alliance gathered around the subject of smoke abatement. By using this approach it is possible to account both for the choice of the film's subject and for the precise way in which it is represented, a task which is not attempted in the established documentary literature.

There are general advantages in paying historical attention to image artefacts such as films. Studying a non-fiction film with a scientific subject may, for example, provide useful evidence of how groups of historical actors have sought to influence public perceptions of science. Shapin, in his study of science and the public suggests:

> We need more studies of the vehicles used to communicate between science and the public, not least because the conventions and distribution

[3] On smoke see: P. Brimblecombe, *The Big Smoke: A history of air pollution in London since medieval times*, London, 1987; Lord Ashby and M. Anderson, 'Studies in the politics of environmental protection: The roots of the British Clean Air Act, 1956: I. The awakening of public opinion over industrial smoke, 1843–1853', *Interdisciplinary science reviews*, (1976) 1, pp. 279–290; 'II. The appeal to public opinion over domestic smoke, 1880–1892', ibid., (1977) **2**, pp. 9–26; 'III. The ripening of public opinion, 1898–1952', ibid., (1977) 3, pp. 191–206, also C. Flick, 'The movement for smoke abatement in 19th-century Britain', *Technology and culture*, (1980) **21**, pp. 29–50, who gives more emphasis to industrial smoke abatement technologies. On water see: C. Hamlin, *A science of impurity: Water analysis in nineteenth-century Britain*, Bristol, 1990 and J.-P. Goubert, *The conquest of water*, Cambridge, 1986. On the history of environmental concerns in general see (although mainly concerned with the nineteenth century): B. Luckin, *Pollution and control: A social history of the Thames in the nineteenth century*, Bristol, 1986. Also idem, *Questions of power: Electricity and power in inter-war Britain*, Manchester, 1990.

> of these vehicles may have had an important bearing upon public perceptions of scientific claims and therefore their careers.[4]

It is essential if historians are to gain from studying films — or other media — that they replace the simplistic model of popularization of science — what Latour would call a 'diffusion' model[5] — with a more sophisticated account of the processes of communication. Shapin offers such an account by implying that, far from being disinterested emanations from the laboratory 'front', vehicles of communication used by scientists are a significant tool in the creation of favourable public opinion and all that follows in terms of funding, prestige and so on. This paper does not directly discuss how *The smoke menace* was used as such a tool but, acting on this assumption, looks at its constitution.

The study of image artefacts of any kind is a moderately new area for historians of science,[6] and raises many serious methodological issues, the most pressing of which for us here is their status as evidence of the past. If it is to be worth studying them for anything other than their own sakes — in an antiquarian sense, almost — then it must be to provide evidence of something. But difficulties arise when we scrutinize the question of where their evidence is reliable. Image artefacts used as illustrations are often assumed to give incontrovertible evidence of the reality of what they picture. But several authors have convincingly argued that images cannot offer a privileged window onto the past; the form they take is determined by social factors, and that we must therefore find other ways of talking about them.[7] Jordanova argues that one of the most important implications of this for historical procedures is the consequent requirement to find 'concrete

[4] S. Shapin, 'Science and the public', pp. 990–1007 in R. C. Olby et al., *Companion to the history of modern science*, London, 1990, pp. 1000–1001.

[5] B. Latour, *Science in action*, Milton Keynes, 1987. See also S. Hilgartner, 'The dominant view of popularization: Conceptual problems, political uses', *Social studies of science* (1990) 20, pp. 519–539.

[6] Except, perhaps, for areas where visual language is an integral part of basic science, in which of course the pioneering work is M. J. S. Rudwick, 'The Emergence of a visual language for geological science 1760–1840', *History of science*, (1976) 14, pp. 149–195. Studies of films from a history of science or medicine perspective are few; C. J. Lawrence, 'Cinema vérité ? The image of William Harvey's experiments in 1928', in N. A. Rupke, ed., *Vivisection in historical perspective*, London, 1987, is one of the few accounts of a non-fiction film. Michael Shortland has done pioneering work in the study of feature films; M. Shortland, 'Screen memories: Towards a history of psychiatry and psychoanalysis in the movies', *British journal for the history of science*, (1987) 20, pp. 421–452, ibid., *Medicine and film,* Research publications of the Wellcome Unit for the History of Medicine, 9, Oxford, 1989.

[7] Methodological issues are discussed in: D. M. Fox, and C. J. Lawrence, *Photographing medicine*, New York, 1988; see especially chapters 1 and 6; L. Jordanova, 'Medicine and visual culture (review article)', *Social history of medicine*, (1990) 3, pp. 89–99, is a discussion of some of the issues raised. The introduction to J. Tagg, *The burden of representation*, Basingstoke, 1988, analytically surveys many of the theoretical issues involved; see, especially, pp. 1–5.

occasions when the mediating process can come to the historian's attention'.[8] She is suggesting that when we replace the naïve view that 'the camera never lies', it is helpful instead to concentrate on the process and circumstances of the image's constitution. These arguments can be applied with equal validity to the moving images of the cinema film as to still photography.

The case of smoke abatement and *The smoke menace* provides an occasion to study the mediating process. In the first instance there is the matter of the selection of the subject of smoke abatement for a film. From the point of view of public health, it is clear that smoke abatement was a minor subject in the thirties; for instance, Newsholme does not refer to it at all in his *The last thirty years in public health* (1936), whilst the PEP *Report on the British Health Services* (1937) devotes only two pages of 414 to the subject. The only pieces of legislation to touch on the subject, The Public Health Act and The Public Health (London) Act (both 1936) are not mentioned by Ashby and Anderson in their historical survey of smoke abatement.[9] In the thirties, it is clear that smoke, although rarely now conceived as a sign of prosperity, could be represented as many things; as a cause of inefficiency and uneconomy both in the home and in industry, or as a danger to architectural heritage and public monuments. Its impact on agriculture and its interference in aviation were considered. Where it was considered as a public health issue, there was an implied question about whether more concern should be shown for the effect on the working classes, and children specifically, or whether it endangered wider groups. There was no clear agreement over the relative significance of lung disorders and other illnesses associated with smoke. No consensus existed over what should be considered the main locations of the problem — domestic, or industrial, or of whose responsibility it was: industrialists', scientists', or citizens' in general.[10] *The smoke menace* represents a selection from these possibilities, and to understand the processes by which the selection was made, we must look to the contexts of the film's production, contexts which can be shown to have been constitutive of its content.

But historians studying image artefacts frequently lack sufficient archival documentation on which to base a close examination of the mediating process. We have a choice, either to study only those examples supported by substantial archives[11] and leave many examples beyond the pale of scrutiny,

[8] Jordanova, op. cit. (7), p. 96.

[9] A Newsholme, *The last thirty years in public health*, London, 1936; Political and Economic Planning, *Report on the British Health Services*, London, 1937, pp. 47–9, Ashby and Anderson, op. cit. (3).

[10] Freely adapted, with additions, from the headings in; National Smoke Abatement Society, *The case against smoke: The evidence of authorities*, Manchester, 1936.

[11] As is the case with the Eugenics Society's films. See T. Boon, '"Lighting the understanding and kindling the heart?": Social hygiene and propaganda film in the 1930s', *Social history of*

or else to look for other ways of dealing with the problem. In the case of *The smoke menace*, with no contemporary correspondence, scripts, or policy files extant — even if it could be established that the production of the film occasioned that formality of communication in the first instance — the production process can only be scrutinized from a distance.[12] The solution to the problem adopted here is to argue that the film should be seen primarily to provide historical evidence about itself, about the context of its production and about the social relations under which it was made. We may see it as the product of an alliance of film-makers, sponsors, scientists and others, designed to enrol various audiences in support of its representation of the smoke problem. In other words, we may work backwards from the image artefact to its constitutive context, illuminating, as a result, some of the factors involved in the construction of vehicles of communication between science and the public. Within this approach, the film becomes the source of references to its constituting context, not only in terms of whom and what it pictures, but also in its iconography and conventions of representation.[13]

A side effect of looking at the wider alliance of interests is slightly to displace the film-makers from the centre of attention they occupy in most accounts, but this is entirely consistent with the view of the film's director that reform, and not film-making, was at the heart of documentary activity.[14] Self-consciously reforming attitudes were held by a much broader community than just documentarists. This wider group was drawn to documentary film, partially because it offered a vehicle for their political and social views, but also because those views were shared by the film-makers. Talking of a film as the product of an extended group allows us to see it as the result of negotiations and agreements between interested parties.

After a brief account of *The smoke menace* and its iconography, this paper will turn to look at the members of the alliance of interests associated with it production: the film-makers, the gas industry who sponsored it, and the scientists. It will examine the rather low level of association between the film and the National Smoke Abatement Society (NSAS), and suggest ways of thinking about this rather surprising aspect of the story.

medicine, (1990) **3**, pp. 140–1 (précis of conference paper given to the annual conference of the Society for the Social History of Medicine, 7–9 July 1989).

[12] None of the four organisations involved to varying extents (The Realist Film Unit, Film Centre, The British Commercial Gas Association and the National Smoke Abatement Society) has extant correspondence files. One reason for the particularly poor survival of records for this film lies in the fact that it was one of the first documentaries to be made outside government; both earlier and later films are well served by files at the Public Record Office.

[13] Though we should bear in mind Jordanva's caveat that conventions should be seen as actively made rather than passively present; Jordanova, op. cit. (7), p. 96.

[14] J. Taylor, interview with the author, February, 1991.

'The smoke menace'

The argument of the film *The smoke menace* is advanced by the use of caption boards. These divide the film into four sections, the last of which is followed by a concluding section. The film's introduction puts the subject into the immediate context of the time by referring to the economic upturn in industry, and its continued reliance on coal.[15] Here, the imagery is of a type familiar from earlier documentary films; shots of manual workers going to work, as found in *Shipyard*, of heavy industry, like some in *Industrial Britain*, of coal mining, like those in *Coal face*.[16] The commentary then turns to criticism of the dirt and inefficiency caused by coal's domestic and industrial use. Here are images of chimneys producing dark smoke (Fig. 1), industry swathed in smoke, of housewives toiling, and of industrial waste.

The second section discusses what it describes as 'the dirt trades'; chimney sweeping, laundering, window cleaning, cosmetics, and the cleaning of public monuments and buildings. Here, once again, the iconography is of 'honest working people' at their employments, typical of the early mode of documentary. This section includes a short speech from Arthur Warnes, 'the consulting chemical and structural engineer', on the deleterious effects of smoke on buildings.[17]

The middle section is concerned with public health; J. B. S. Haldane explains how smoke clouds prevent children from getting the sunlight they need to synthesize vitamin D and thus avoid rickets. Scenes of Haldane, at first in front of a curtain (Fig. 2), then walking into a children's ultraviolet light treatment session are intercut with shots of children playing in streets. Accidents in fog are mentioned against shots (probably stock footage) of a ship and pedestrians in smog. A doctor, as if talking to a patient, mentions asthma, bronchitis, and heightened mortality due to fog.

Then the film discusses the conversion of coal into 'modern fuels'; it allows a short section on electricity, and rather longer on the scientific basis of the gas industry and its 'social mission', presented as a lecture to sales representatives, intercut with shots of industry, smoking chimneys, coal tar, fertilizer, and so on.

[15] Historical contextualization is typical of documentaries of this period; see Swann, op. cit. (18) pp. 114–5.

[16] *Shipyard*, directed by Paul Rotha for the Orient Line, 1935; *Industrial Britain*, by Flaherty, Grierson, Anstey, 1933; *Coal face*, by Cavalcanti, Grierson et al, 1935. The credits for documentary films are recognised to be rather unreliable; see Swann, op. cit. (18), p. 114.

[17] Obituaries for Arthur Warnes (1878–1942) can be found in *Quarterly bulletin, Institute of Chemical Engineering*, (Oct. 1942) 70, p. 4, and *Journal and Proceedings, Institute of Chemistry*, (1942), pp. 181–2. I have not been able to prove or to disprove any links between Warnes and other members of the alliance. He did, however, contribute an article to the catalogue of the Science Museum exhibition; National Smoke Abatement Society, *Smoke abatement exhibition: Handbook and guide*, Manchester, 1936.

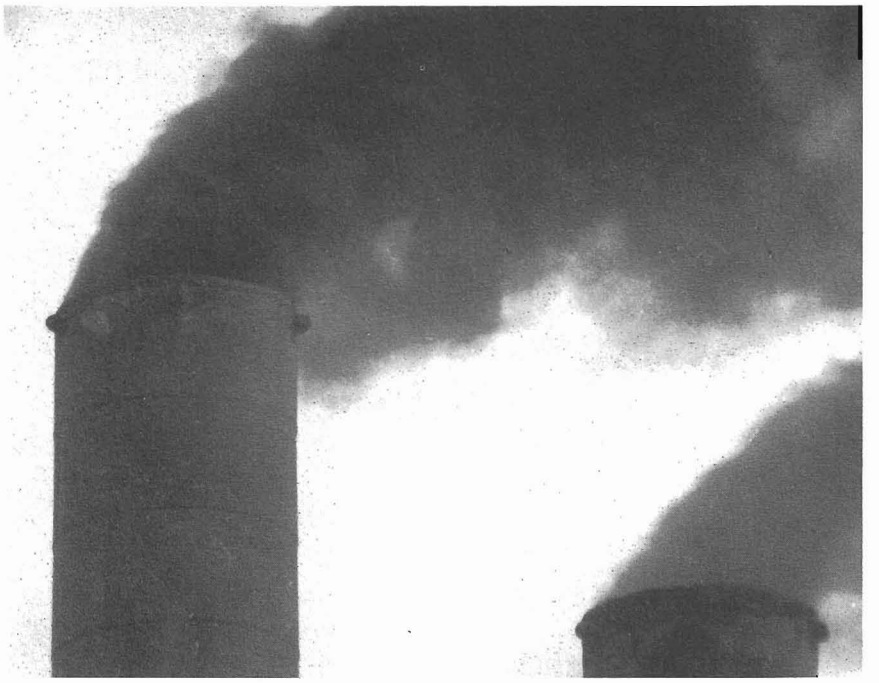


FIG. 1: '. . . progressive minds are not satisfied. There are people who say that coal is too valuable to be burned raw . . .' The iconography of smoke from the opening of *The smoke menace*.

The film's conclusion, echoing the opening, gives an account of progress in 'the battle against smoke', in terms of cleanliness and public health. We are told that 'far-seeing authorities are winning their battle against dirt and disease'. Against shots of new flats, mothers and children, we learn that housewives are happier and that 'children are being given a fair chance to grow up healthy and strong, because they're no longer shut-away from the sun by a blanket of darkness' (Fig. 3). Reference is made to improvements in industry too. The denouement adopts a less assured tone: 'But as more of these positive results are achieved, it becomes obvious that only the fringe of the problem has been touched'. The commentary stresses what still remains to be done, and who might do it: 'above all they ['people fighting waste'] look to the ordinary citizen who can in the home, the office and the factory, help to build a cleaner and less wasteful order'.

Considering that smoke pollution was acknowledged as being caused by both industrial and domestic smoke, it is striking that in the end it is the latter which is stressed by the commentary. The film's mode of address implies that the smoke problem is a subject for discussion amongst citizens, but also that the solution is in the hands of those citizens acting responsibly

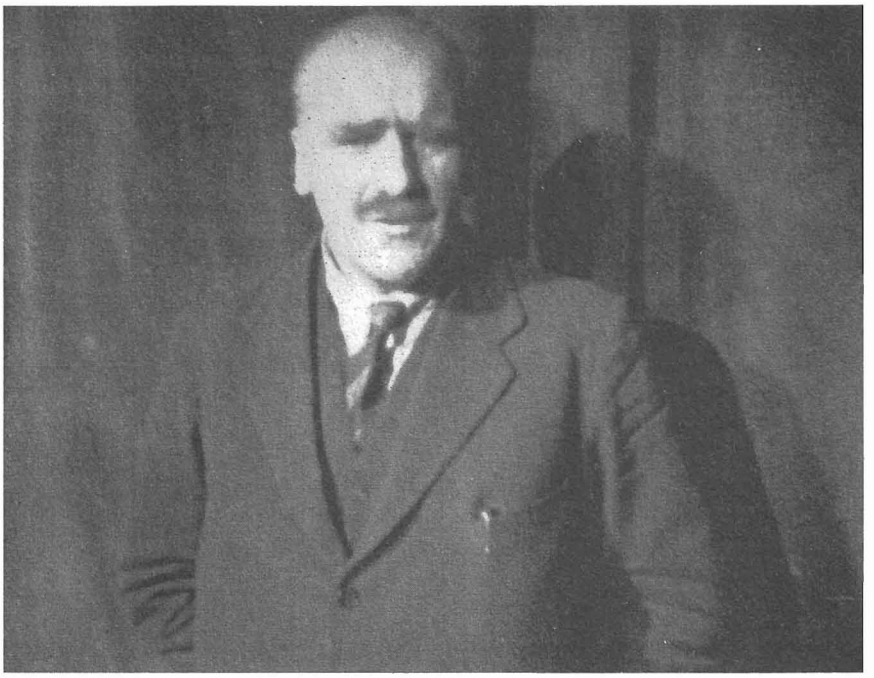


FIG. 2: 'I am a public enemy. I live in a house with several open coal fires.' J. B. S. Haldane in *The smoke menace*.

for the 'common good'. Although there are sequences on the work of the electricity and gas industries, this is not an exposé of industrial pollution, and industry is not being told to 'clean up its act', as might be the angle of latter day environmental documentaries. This presentation might now be represented as typical of distortions introduced by industrial sponsorship. But that would be to miss the different constitution of social problems in this and associated films, which I will discuss in the next section.

Contexts of 'The smoke menace': Documentary film

The smoke menace is a documentary. Understanding precisely what that means for a film made in 1937 is crucial to understanding the constitution of smoke abatement within it, and the film's subsequent career. The documentary film 'movement' is blessed with a large but not uniformly useful literature, unfortunately little of it concerned with the subjects of its films, or the factors determining them.[18] Equally, few of these accounts are concerned

[18] There are now some very good historical accounts; see P. Swann, *The British documentary film movement, 1926–1946*, Cambridge, 1989. I. Aitken, *Film and reform: John Grierson and the documentary film movement*, London, 1990, is a substantial intellectual biography of docu-



FIG. 3: 'Children are being given a fair chance to grow up healthy and strong, because they're no longer shut away from the sun by a blanket of darkness.' Children on a balcony at Kensal House, from the conclusion of *The smoke menace*.

mentary's leading light, but tends to subsume the whole movement to Grierson. Of the standard sources, Paul Rotha, *Documentary diary*, London, 1973, gives a very vivid account, though other participants have questioned details of his recollections. This book is usefully supplemented by P. Marris, ed., *Paul Rotha*, London, 1982. F. Hardy, *John Grierson: A documentary biography*, London, 1979, continues to be very useful, reflecting Hardy's close knowledge of his friend. E. Sussex, *The rise and fall of British documentary*, Berkeley, 1975, and E. Orbanz, *Journey to a legend and back: The British realistic film*, Berlin, 1977, are both reports of interviews with participants in the documentary movement; although useful, they suffer the usual problems of recollection accounts. R. Low, *Documentary and educational films of the 1930s*, London, 1979, is indispensable as a catalogue and reference source, though some of her historical judgements are coloured by the documentarists' rhetoric. A. Lovell, and J. Hillier, *Studies in documentary*, London, 1972, is still useful, if superseded in detail by Aitken. Contemporary texts may be found in: F. Hardy, ed., *Grierson on documentary*, London, 1946 and 1966 (abridged). P. Rotha, *Documentary film*, London, 1936 (1st edn) and 1952 (3rd edn), is an indispensable piece of documentary rhetoric. Some of the best early critical work on documentary is to be found in: A. Kuhn, *'Independent' film-making and the state in the 1930s*, in C. Johnston, ed., *Edinburgh '77 magazine*, (1977), ibid., 'British documentary in the 1930s and "independence": Recontextualising a film movement', in D. MacPherson, *British cinema: Traditions of independence*, London, 1980. R. Colls and P. Dodd, 'Representing the nation: British documentary film, 1930–45', *Screen*, (1985) **26**, pp. 21–33, usefully examines the ideology of

with the wider social groups involved in the films' production. Documentary was not the only type of non-fiction film made in the thirties. Newsreels, commercial, educational and propaganda (including health education) films, as well as political films on the Left and the Right, had all established support networks and public audiences by the end of the decade, not necessarily in conventional cinema settings.[19] So, what distinguished documentary? British Documentary is commonly described as a movement, and this can be justified in terms of institutional, theoretical and representational factors.

In the first place, documentaries may be defined as films made by people who had their roots in the government film units established by John Grierson in the early '30s, first at the Empire Marketing Board (EMB) and subsequently at The General Post Office. Thus we may describe as documentarists both John Taylor (director of *The smoke menace*) who remained close to Grierson throughout the thirties, and Paul Rotha, who left the EMB Unit after only five months and pursued a more independent career.[20] But the fact that these units were associated with government is also significant. It has been argued by Paul Swann amongst others, that the rise of the documentary film is very much a part of the rise of public relations, itself a response to the increasing power of the state in the first half of the twentieth century. With the extension of suffrage came the requirement for government to talk to the public. Within this view, 'the general public' is an artefact of the modern democratic state. Obviously it cannot be argued that universally across government all officials in all departments wanted to talk about all subjects to the public. But it is clear that groups of individuals within particular types of governmental organization were able to advance democratic rhetorics based on these types of assumptions.[21] The Empire Marketing Board is the key example, and therefore it is no surprise that it was here that the first documentary unit was formed.

The establishment of documentary was accompanied by the publication of many theoretical articles and books, often vividly rhetorical in tone, and frequently dedicated to elucidating what distinguished documentary from all other types of film. Both Grierson and Rotha, with slightly different emphases, made major contributions to this literature.[22] More than anything else, it was documentary's assumption of a realist rhetoric which set it apart from

documentary, but tends, like some of the older work, to underestimate historical changes in the form. J. Corner, ed., *Documentary and the mass media*, London, 1986, is a series of essays on documentary in the broader sense, also treating media other than film.

[19] In general, see R. Low, *Films of comment or persuasion of the 1930s*, London, 1979; N. Pronay, and D. W. Spring, *Propaganda, politics and film, 1918–1945*, London, 1982; for Left film-making see MacPherson, op. cit. (18); and B. Hogenkamp, *Deadly Parallels; Film and the Left in Britain 1929–39*, London, 1986.

[20] Low, op. cit. (18), p. 53.

[21] Swann, op. cit. (18), p. 2.

[22] See references in note 18 above, especially Rotha (1936), Hardy (1946/1966).

other types of non-fiction film-making in this period. When many instructional films used a fictional form to put across their content, Grierson advocated a type of philosophical realism which went beyond the world of appearances to a deeper underlying reality:

> In documentary we deal with the actual, and in that sense with the real. But the *really real*, if I may use that phrase, is something deeper than that. The only reality which counts in the end is the interpretation which is profound.

Aitken gives an example of the intercutting of shots of a herring trawler at sea with shots of the market place in Grierson's own film *Drifters*, forming a sequence which is designed to show their interdependence and the 'interrelation of social practices within a social reality'.[23] Not all documentarists were educated in philosophical idealism, and it is not sustainable to argue that all of them held to the precise view which Grierson articulates here. But it is clear that for many of them realism did entail a concern with the underlying social forces associated with a film's subject, whether in the more abstract sense expressed by Grierson, or in the more political, as often found in Rotha's work.

It is clear from this example that it is possible to relate the documentarists' apologetic writings with the appearance of the films. A recognizable documentary style, which Rotha later described as the 'poetic' style, developed in the first half of the thirties. In these films, as exemplified by *Industrial Britain* and *Night mail*, the documentarists were concerned to show the dignity and craftsmanship of the worker as a way of educating the audience in the nature of modern industrial and corporate society.

But both the institutional place and rhetorical style of documentary film were supported and nurtured within an alliance. Stephen Tallents, secretary of the EMB, was a key figure in the alliance which sustained the documentarists. He had previously held a series of semi-official appointments which 'placed him in positions where he was required to work with public opinion'.[24] Under his influence, the EMB used innovative publicity methods including the establishment of nation-wide poster hoardings and, of course, documentary films. Tallents had facilitated the establishment of the EMB film unit, but also recruited for the Board people schooled in public relations work, including Frank Pick, William Crawford, and A. P. Ryan (who later created the public relations department for the gas industry). On the dissolution of the EMB in 1933, Tallents transferred to the GPO and took the film unit with him.[25] Walter Elliot the Tory MP provides another example of the long lasting alliances underpinning documentary activity. He had known Grierson at Glasgow University, and was chairman

[23] Grierson quoted with discussion in Aitken, op. cit. (18), pp. 109–110.

[24] Swann, op. cit. (18), p. 23.

[25] Ibid., pp. 47–8.

of the EMB film committee when its first films were approved in 1928. He served on the GPO Unit's film committee and was a consistent supporter of the documentarists, taking trouble to visit them at their offices.[26] One final example, that of Ritchie Calder, serves to illustrate the breadth of documentary's sustaining alliance; as science correspondent of the *Daily Herald*, he consistently wrote in support of documentary film, and was, on occasion, involved more substantially in the production process.[27]

A series of institutional changes at mid decade encouraged a diversification of documentary activities. When Tallents ceased to be public relations officer at the GPO in 1935 and was replaced by a less sympathetic official, it became more difficult for the Unit to make films on subjects other than GPO services.[28] Increasingly from 1935, film-makers who had started at the EMB or the GPO began to work under different auspices.[29] The Strand Film Company operated as a documentary unit from 1935, and The Realist Film Unit, responsible for *The smoke menace*, was established in 1937. This diversification of production facilities was accompanied by other consolidating activities: with *World film news*, set up in 1936, documentary had a publication from which it could marshal its allies and tease its political opponents.[30] Associated Realist Film Producers (ARFP) was formed in January 1936 to serve the function of easing contacts between documentarists and potential clients.[31] It provided the institutional base outside government departments which the documentarists required to further the interests of their wider alliance. These changes were associated with the arrival of new members of the alliance, for example the nutritionist John Boyd-Orr, long an associate of Walter Elliot,[32] who appeared in the film *Enough to eat?* in 1936. Thereafter, and especially during the War, he was associated with several more films and closely collaborated on Rotha's *World of plenty* (1943).

It is important to note that despite the longevity of this sustaining alliance, documentary was not an unchanging form. Authors such as Nichols, who are keen to characterize an overall documentary form, underestimate the degree of change in the representational styles of documentaries, and do not see the historical specificity of the alliances of interest embodied in indi-

[26] Hardy, op. cit. (18), pp. 27, 49, and 73; Rotha, op. cit. 1973 (18), p. 43. See J. Boyd-Orr and S. Tallents, 'Walter Elliot', *Biographical memoirs of Fellows of the Royal Society*, (1958) 4, pp. 73–80. Elliot also appeared in the film *Enough to eat?* in 1936.

[27] For example in *Peace of Britain* (1936) and *They met in London* (1941). See R. Calder, 'We must show close-ups of life', *Daily Herald*, (7 Oct. 1936) London, p10. Also, E. Sussex, op. cit. (18), p. 64.

[28] Swann, op. cit. (18), especially pp. 76–7.

[29] Although Rotha had worked with British Instructional on the making of *Contact* from 1932; Rotha, op. cit. 1973 (18), p. 70.

[30] Swann, op. cit. (18), pp. 111 and 77.

[31] See P. Rotha, Letter to the editor, *Sight and sound*, 8, 1939, p. 81, and Anon., 'The documentary film: Consultative body formed', *The Times*, (10 Jan. 1936).

[32] See J. Boyd-Orr and S. Tallents, op. cit. (26).

vidual films.[33] Accounts such as these do not always pay due heed to the historical circumstances of the 1930s, which wrought enormous changes in documentary practice, just as, in different ways, they altered other discourses concerned with society and social problems, such as, for example, eugenics.[34]

From about 1935, documentary films also began to treat new issues and subjects, especially social problems and their reform. Some of the clearest early examples of this are to be seen in Rotha's *The face of Britain* and Elton and Anstey's *Housing problems* (both 1935). A feature of these films, which became increasingly prominent as the form developed, was the inclusion of expert opinion, usually deployed in the form of 'camera interviews'.[35] Where it is scientists who supply this expertise, it is most often to lend authority to the delineation of a problem. The presence of Arthur Warnes and J. B. S. Haldane in *The smoke menace* is typical of this. It is also characteristic of these films that they represent the social problems they portray as having solutions; often by means of the benevolent actions of corporations, or government agencies,[36] as Grierson enthused in 1938:

> In many of the documentary films, the country is shown tearing down slums and building anew, or facing up to unemployment and reorganising economically: in general passing from the negative to the positive. It is in this, precisely, that most of us have felt that the strength of democratic Britain is made manifest.[37]

But the expertise which is deployed to demonstrate such progress is typically represented by politicians and not scientists.[38]

The expansion in subjects was accompanied by changes in representational style. Rotha commented: 'the making of films about the task of social reconstruction was a great deal harder than the dramatization of the steelworker and the fisherman as symbols of labour'.[39] The new films experimented with what were all thought of as 'journalist' forms at the time.[40] *Housing problems* confronted the audience with slum dwellers; *Enough to eat?* adopted an illustrated lecture format; and *The smoke menace* borrowed its style of *reportage* from the American social issues newsreel *The march of time*, which ran a British office enjoying close links with the British docu-

[33] B. Nichols, 'Documentary theory and practice', *Screen*, (1976) 17, and idem, *Ideology and the image*, Bloomington, 1981.

[34] P. G. Werskey, 'Nature and politics between the wars', *Nature*, (1969) **224**, pp. 462–472; idem, 'British scientists and "outsider" politics, 1931–1945', *Science studies*, (1971) **1**, pp. 67–83; D. J. Kevles, *In the name of eugenics*, Harmondsworth, 1986.

[35] Swann, op. cit. (18), pp. 114–15. [36] Ibid., pp. 115–16.

[37] Grierson, *The battle for authenticity* in F. Hardy, op. cit. 1966 (18), pp. 83–85, p. 85.

[38] For example, Walter Elliot is seen and Herbert Morrison delivers a speech in *Enough to eat?* [39] Rotha, op. cit. 1952 (18), p. 196, a section written for the 1939 edition.

[40] Rotha, op. cit. 1936 (18), pp. 224–8.

mentary film movement from 1936.[41] This is how Rotha associated *The smoke menace* and the newsreel:

> The Fast Moving tempo of The March of Time, with its anxiety to find an edge to every item, was now to influence the British documentary film. Trying to put across facts in journalistic style, a film like *The smoke menace* showed small care for the visual importance of the medium. It used the screen as an illustration to its commentary, broken up here or there by a personal interview. It had nothing of the pictorial beauty of photography which was normally associated with documentary films.[42]

In all these films, subject and style were very tightly bound together, as is clear from Grierson's recollection:

> We worked together and produced . . . the poetic documentary [a reference to the earlier style of *Night mail*, for example]. But . . . there has been no great development of that in recent times. I think it's partly because we ourselves got caught up in social propaganda . . . got caught up with the problems of housing and health, the question of pollution . . . We got onto the social problems of the world, and we ourselves deviated from the poetic line.[43]

The older rhetoric of education in the nature of society became mixed with a social concern, and the language of citizenship, found occasionally earlier in the decade, was used with greater frequency. Here is Rotha, writing in 1935:

> Civilisation today, in fact, presents a complexity of political and social problems which have to be faced by every thinking person. . . . it rests with the ordinary person to act not merely as a passive voter but as an active member of the State. His political cooperation, criticism and even active opposition is demanded and must be intelligently equipped to meet that demand.[44]

It is, of course, documentary film that is intended to equip the citizen to act.[45]

As a documentary made in 1937, then, *The smoke menace* exemplifies

[41] J. Taylor, op. cit. (14); P. Rotha op. cit. 1973 (19) p. 158; R. Low, op. cit. (18) p. 44.

[42] P. Rotha op. cit. 1952 (18), p. 196, a section written for the 1939 edition. Of course, we do not have to conclude from this that the imagery of the film did not matter to contemporary audiences.

[43] Grierson quoted in Sussex, op. cit. (18), p. 79.

[44] P. Rotha, op. cit. 1936, (18), pp. 48–9.

[45] Rotha quoted approvingly a piece on adult education by Eva Hubbock: 'It should, then, be the aim of education for citizenship to equip the citizen for the "choice before him" so that he may support the better rather than the worse general policy. It should, indeed, be possible . . . to put before him the main facts and principles, whether these be moral, economic or political, which lie behind the chief questions of the day'; P. Rotha, op. cit. 1952 (18), pp. 50–1.

concerns about social problems typical of documentary at that stage in its development. It embodies representational practices intended to address those concerns. Associated with the emergence of this new phase of documentary film was the development of a new class of ally; the scientists, and architects for example who, both on screen and off, developed with the documentarists mutually advantageous alliances and strategies of publicity.

Contexts of 'The smoke menace': Sponsorship

The gas industry, sponsors of *The smoke menace*, named their film catalogue 'Modern Films on Matters of Moment'. It opened with the following assertions:

> The films in this catalogue are offered as dramatic accounts of some of the problems of modern Citizenship in which the general public and the Gas Industry have a common concern . . . Nutrition, Housing Reform and Public Health. Films have been made on all these subjects: some deal with the processes involved in the manufacture of Gas; some with the domestic problems of Housewifery and Cooking: some outline a planned approach to home economics and some discuss the National problems of Smoke Abatement, Nutrition, Housing and Education. We offer this account of matters of moment in the life of the community in the hope that it will serve not only to make known the activities of the Gas Industry and the responsibilities which the Industry has taken upon itself in matters of Public Health and general welfare, but also help to articulate the public knowledge in the major social problems.[46]

Each year from 1935, at least one high profile film on a social subject designed to 'appeal to the public conscience' was included in the programme.[47] Other than *The smoke menace*, the films in the group are: *Housing problems* (1935), *Enough to eat? (The nutrition film)* (1936), *Children at school*, *Kensal House* (both 1937) and *The Londoners* (1939). From this list the context of *The smoke menace* becomes clear, for in all these films public health issues act as the vehicles for reforming social concern; *Housing problems* and *Kensal House* are about life in the slums and in the flats built to replace them; *Enough to eat?* discusses working class malnutrition; whilst *Children at school* reveals the poor state of the nation's schools; and *The Londoners* celebrates the work of the London County Council in the alleviation of social ills. Smoke, then, is presented not only as a problem of waste perpetrated within industry and within the home, but also as a problem of public health, especially (in terms of the proportion of time devoted) for

[46] BCGA, *Modern films on matters of moment*, London, 1939, p. 3.

[47] H. De Mouilpied, 'The gas industry presents', *Co-partners' magazine (Gas Light and Coke Co.)*, (Nov. 1937) 27, p. 611.

children. These problems are presented as having two types of solution: in the first place, there is a series of technical solutions effected at the corporate level — presented in the penultimate section of the film. But there is also the direct address to the audience; that it is through the exercise of responsible citizenship, and the burning of less raw coal, that solutions to public health problems can be effected.

For the gas industry, these films were part of what was, for the period, a highly sophisticated public relations strategy. Gas was in competition with the electrical supply industry, especially for large installations in the slum rehousing schemes of the Local Authorities who, in S. C. Leslie's memorable phrase 'associated electricity with the Millenium'.[48] Leslie, the publicity manager at the Gas Light and Coke Company, and A. P. Ryan before him, sought to assert the modernity of gas by associating it with urgent public isues.[49] Hence the film catalogue title '*Modern* Films on Matters of Moment'. They achieved this not just through making and showing films but also in public exhibitions,[50] formal dinners with invited speakers,[51] and single theme advertising campaigns.[52] Even the building of slum clearance flats was celebrated as a triumph of public relations. Kensal House — the estate built by the Gas Light and Coke Company — proved, Leslie argued, that new accommodation could be contrived to be not just larger, cleaner and smoke-free, but cheaper to rent than slum properties, and left tenants, contrary to some arguments,[53] enough money to purchase a nutritious diet.[54]

By playing its part in the production of these films, the gas industry served its interests by keeping gas in the public eye, and especially, addressing those perceived to be influential members of society. David Milne-Watson, Governor of the Gas Light and Coke Company, in a letter to *The Times* about their films stated:

[48] P. Rotha, op. cit. 1973 (18), p. 155. For the nature of the electrical industry's campaign, see B. Luckin, op. cit. 1990 (3), especially pp. 9–22. [49] P. Swann, op. cit. (18), p. 101.

[50] For smoke abatement exhibition: Anon., 'Britain's burning shame and the gas campaign', *Journal of the National Smoke Abatement Society*, (Nov. 1938), pp. 101–3; For the nutrition exhibition: Anon., 'Food and national health: Exhibition at Charing Cross Underground', *The gas world*, (29 Jan. 1938), p. 97; The gas and coke industry also exhibited at the Science Museum smoke abatement exhibition; see NSAS, op. cit. (17).

[51] The same meeting is referred to in: Anon., 'British Commercial Gas Association 27th annual general meeting in London', *The gas world*, (22 Oct. 1938), pp. 338–345, and; Anon., 'The Minister of Health on smoke: Mr Herbert Morrison on London's problem', *Journal of the National Smoke Abatement Society*, (Nov. 1938), pp. 111–3.

[52] See the section below on the gas smoke abatement campaign of 1938.

[53] Anon., 'The Committee Against Malnutrition', *Lancet*, (1934) 1, p. 1358, G. C. M. M'Gonigle, and J. Kirby, *Poverty and public health*, London, 1936.

[54] S. C. Leslie, 'Kensal House: The case for gas is proved', *The gas world*, (2 Oct. 1937), pp. 299–304.

> we have found here a means of establishing relations of improved understanding and increased confidence between our industry and its public, and not least in the minds of those more highly critical and influential sections which are particularly difficult to reach through traditional channels of persuasion.[55]

This was partially achieved by manufacturing and sustaining a public reputation for laudable neutrality. For example, one of their own publications, speaking of *Enough to eat?* asserted that 'Such a film, naturally, contains no gas propaganda whatever',[56] a sentiment which is echoed in *The new statesman and nation*: 'The only aspect which causes any surprise is the absence of any kind of reference to gas, light or coke'.[57]

The documentarists actively sustained the gas industry's enrolment in the alliance of interests by, for example, Grierson attending and speaking at the 1937 annual conference of the British Commercial Gas Association (BCGA), where he maintained that 'It was not . . . the Ministry of Health which had improved the health of the nation last year, but the Gas Industry . . . In his view . . . nothing would keep the flag of gas flying more effectively than films, because they maintained public relations in a manner that no other method could do.'[58] Sustaining the alliance with the documentarists, the BCGA appointed Thomas Baird, formerly at the GPO film unit, as Film Officer in late 1937. That he was on the Associated Realist Film Producers lecturers' panel is explicitly mentioned in the press announcement.[59] Walter Elliot took the opportunity to stress the importance of documentary films when guest of honour at a BCGA luncheon.[60]

Equally, accounts of the production process state both that it was the industry which chose the subject,[61] and that 'early in 1935, the British Commercial Gas Association commissioned Elton and Anstey to produce a programme of five films, *inviting their collaboration in the choice of subjects*'.[62] If we take it that all those concerned were already members of a long-sustained alliance, these accounts are not incompatible.

Once again, as is clearly visible in the catalogue introduction, the common language of the alliance of interests can be seen to revolve around notions of

[55] D. Milne-Watson, 'Utility in public relations (letter to the editor)', *The Times*, (16 Jun. 1938), p. 12.

[56] Anon., 'Mr. Therm — film star: Gas Light and Coke Company's films', *Gas journal*, (5 August, 1936), p. 325.

[57] Anon., 'A London diary', *New statesman and nation*, (10 Oct. 1936), p. 499.

[58] Anon., 'The twenty-sixth national conference of the BCGA' *Gas journal*, (6 Oct. 1937), pp. 61–4 and 98, pp. 63–4.

[59] Anon., 'BCGA appoints film officer', *The gas world*, (27 Nov. 1937), p. 487.

[60] See *The gas world* reference op. cit. (51), p. 342.

[61] H. De Mouilpied, 'Films in the making', *Co-partners' magazine (Gas Light and Coke Co.)*, (Jul. 1937) **27**, p. 408.

[62] My emphasis. The Arts Enquiry, *The factual film*, Oxford, 1947, p. 53

'the problems of modern *Citizenship*' deployed as the ground for discussion by the public and the Gas Industry as equal partners with a common concern. This point is expanded in internal publications:

> In 1½ hours of films th[e] citizen can learn of the numerous activities of the Gas Industry and of its social significance . . . These films are part of an education in citizenship which will give to the ordinary man a better understanding of the social problems of the day. To do that is to give him a new conception of the Gas Industry. These two things are complementary. To raise the question of fuel costs in re-housing schemes or to discuss the national wastes and ravages of smoke is to bring the place of the Gas Industry into its true perspective.[63]

Or, as the *Gas Journal* put it: 'This sort of film is good citizenship and good business too'.[64]

Contexts of 'The smoke menace': Social Relations of Science

The last group I want to consider as members of the alliance of interests responsible for this film is that of scientists. I will argue that *The smoke menace*, and certain other films[65] can be seen as cinematic expressions of the views of scientists active in arguments over the social relations of science. Once again, evidence of agreement amongst a broad alliance is signified by the person of Ritchie Calder, whom we have seen was active in support of the documentarists, but was also involved in debates on the social relations of science.[66]

Certain scientists were very active within the 'Middle Opinion' culture of the thirties, notably Julian Huxley, Lancelot Hogben, Richard Gregory and J. B. S. Haldane.[67] The documentarists' organization Associated Realist Film Producers (ARFP)[68] provides important evidence of the involvement of

[63] H. De Mouilpied, 'The gas industry presents', *Co-partners' magazine (Gas Light and Coke Co.)*, (Nov. 1937) **27**, pp. 610–611.

[64] Anon., 'A review of the Gas Light and Coke Company's films', *Gas journal*, (14 Oct. 1936), p. 158.

[65] Notably *Enough to eat?* (1936) and *Peace of Britain* (1936), but also several films from the wartime period, including *They met in London* (1941) and *World of plenty* (1943).

[66] W. McGucken, 'The social relations of science: The British Association for the Advancement of Science, 1931–1946', *Proceedings of the American Philosophical Society*, (1979) **123**, pp. 237–264, passim.

[67] Several, for example, were on the editorial board of *The realist*, 'a journal of scientific humanism', which ran between 1929 and 1930, publishing articles across the scientific, political and artistic spectrum. For the general point, see: A. Marwick, 'Middle opinion in the thirties: Planning, progress and political "agreement"', *English Historical Review*, (1964) **79**, pp. 285–298. For a somewhat diffuse discussion of documentary in the context of middle opinion, see I. Aitken, op. cit. (18) especially, pp. 167–183.

[68] See above, 'documentary film' section, note 31; also P. Swann, op. cit. (18), p. 112.

scientists within cultural and specifically cinematic institutions. The membership of ARFP contained all the most prominent documentary film-makers, including John Taylor, director of *The smoke menace*.[69] The biologists Julian Huxley, J. B. S. Haldane and Lancelot Hogben, along with the architect Basil Ward, the designer McKnight Kauffer and the composer Walter Leigh, were all listed as advisers to this group. In the absence of any detailed records of ARFP's meetings[70] we must turn to other evidence of the ways in which it acted as a coalition of scientists and film-makers. We can, for example, look at the activities of scientists in support of documentary film. Each of them took a more or less active role: Hogben published and lectured,[71] Huxley appeared in many films, lectured, and wrote supportive letters to *The Times*,[72] and Haldane appeared in *The smoke menace*. The strength of this aspect of the alliance is demonstrated by the way in which, during a lengthy debacle over what sorts of films should be shown in the British Pavilion at the 1939 New York World's Fair,[73] all three scientists were amongst only eight signatories from ARFP to a letter to *The Times* advocating the choice of documentary films on account of their 'truthful approach to British life', rather than the 'British Newsreel' which the organizing committee was proposing.[74]

Haldane's presence in *The smoke menace* is at first a little puzzling; from a sun ray clinic at the Middlesex Hospital he gives a short lecture on the deleterious effects of smoke on sun exposure — and therefore vitamin D production — of children:

> I am a public enemy. I live in a house with several open coal fires. But I am not sufficiently ashamed when I see the smoke coming out of my

[69] He is not listed in the membership list printed in *World film news* in October 1936 (reproduced in P. Rotha, op. cit. 1973 (18), pp. 160–1), but he was subsequently its Treasurer. In the credits for *The smoke menace*, the letters 'ARFP' appear after his name, despite the fact that the film was made during the period when Film Centre (set up in August 1937; see ibid., p. 213) was taking over ARFP's role of acting as broker between organisations wanting to have documentary films made, and their producers.

[70] Rotha records that 'The minute books of the association were scrupulously kept but were subsequently lost'; Rotha, op. cit. 1973 (18), p. 161.

[71] L. Hogben, 'The new visual culture', *Sight and sound*, (1936) 5, pp 6–9; he spoke to the NUT Higher Education conference in December 1936; L. Hogben, 'Films and science: Mathematics', *Sight and sound*, (1936) 5, p. 150.

[72] Most notably *Enough to eat? (The nutrition film)* in which he is seen speaking the commentary. He appears on the list of lecturers organised by ARFP to 'give lectures . . . about the social, aesthetic and educational aspects of cinema'; *The Times*, (30 August 1937), p. 8. He also spoke in a series at the Royal Institution in January 1937 (two others were R. A. Watson-Watt and B. A. Keen); see 'Films and science: A symposium', *Sight and sound*, (1936) 5, pp. 150–1; J. Huxley, 'British films (letter to the editor)', *The Times*, (22 Jun. 1937), p. 17.

[73] P. Swann, op. cit. (18), pp. 136–143; Rotha, op. cit. 1973 (18), pp. 233–240.

[74] *The Times*, (7 November 1938), reproduced in part in Rotha, op. cit. 1973 (18), pp. 236–7.

> own chimney, and I ought to be ashamed of myself. But I know that my chimney is cooperating with a million other chimneys to make London not merely dirtier, but less healthy. Growing children need vitamin D, if their bones and teeth are to be properly formed. Now, vitamin D is produced when ultraviolet radiation falls on a waxy substance which is found in almost all living organisms including ourselves. Few children get all the vitamin D that they need in their food, but they can make all that is necessary if enough sunlight falls on their skin. If they don't get enough sunlight or enough vitamin, they will develop rickets and bad teeth. And my smoke is adding to the pall which hides the children of London from the ultraviolet radiation of the sun.
>
> Now these children here are getting artificial ultraviolet radiation in order to protect them from rickets. It may be as good as real sunlight; I don't know, I'm not sure, but I do know it's a great deal more expensive.

Haldane here is not 'popularizing' his own work,[75] but the tone is very similar to that of his newspaper and magazine essays, for example those collected and published as *Possible worlds and other essays*.[76] Here, as in many of the essays, he mobilizes an easy expertise, aristocratic authority and self deprecation as instruments of his scientific rhetoric. But, within this speech, it is Haldane's personal responsibility as a citizen, not as a scientist that is emphasized. This accords very well not just with citizenship motifs within documentary, but also with one of the conclusions of the long-running debates about the social responsibilities of scientists which had arisen during the depression, in 1930–31. During the 1930s, impelled by doubts about the impact of science upon society provoked by the experience of economic depression and mass unemployment, and recalling the effects of novel 'scientific' weapons in the First World War, many scientists had begun to doubt the traditional positivistic association between the accumulation of scientific knowledge and the progress of Mankind.[77] Whether scientists should be held responsible for the application of the 'pure' knowledge they perceived to be produced in the laboratory was the subject of sustained debate throughout the decade. McGucken argues that amongst the various

[75] Judged on the basis of his publications listed in the bibliography to N. W. Pirie, 'John Burdon Sanderson Haldane, 1892–1964', *Biographical memoirs of Fellows of the Royal Society*, (1966) **12**, pp. 219–49.

[76] J. B. S. Haldane, *Possible worlds and other essays*, London, 1927.

[77] See: P. G. Werskey, 'Nature and politics between the wars', *Nature*, (1969) **224**, pp. 462–472; ibid., 'British scientists and "outsider" politics, 1931–1945', *Science studies* (1971) **1**, pp. 67–83; idem, *The visible college; A collective biography of British scientists and socialists of the 1930s*, London, 1988 (2nd edn); McGucken, op. cit. (66); P. Collins, 'The British Association as public apologist for science, 1919–1946', pp. 211–230 in R. Macleod, and P. Collins, eds., *The parliament of science*, Northwood, IL, 1981.

groups of scientists involved in debates about the social relations of science, those connected with the British Association by 1938 had

> concluded that they were responsible neither for the positive nor especially for the negative social uses of science. In a democratic society such uses, they plausibly concluded, were the responsibility of every citizen, scientists and layman alike.[78]

It is intriguing that in the case of *The smoke menace* it is Haldane, ostensibly one of the most radical of scientists, who is found voicing a sentiment more typical of the far less radical British Association group, though at this stage he certainly considered himself to be a Marxist.[79] This example shows both how intricately we must look at the mediation process in examining vehicles of communication between scientists and the public, but also how complex are the alliances which underpin them. Certainly, as far as documentary films are concerned, nothing more radical than a meliorist reforming viewpoint, normally deployed around ideas of planning, is ever expressed regarding any of the public health issues on which they touch.[80]

Of the scientific advisers to ARFP, Hogben was the most actively concerned with issues of citizenship, and its role in relation to both science and film. By looking at his ideas we can investigate one scientist's view of the advantage of association with documentary film-makers. In a speech to the Blackpool meeting of the British Association in 1936, he argued that 'The cultural claims of science rest on the social fact that the use and misuse of science intimately affects the everyday life of every citizen in a modern community'.[81] But he went beyond the British Association view, explicitly arguing that scientists should be active as citizens: 'This nonsense that the scientific worker has no time to be a socially responsible adult, exercising his social responsibilities as a citizen, is due to be debunked'.[82]

Hogben's review of books by Rotha and Grierson in the film journal *Sight and sound*, can be seen as his manifesto for ARFP.[83] He argues:

> The willing co-operation of men of science, the financial support of persons belonging to different political parties (or to none at all) and the

[78] W. McGucken, op. cit. (66), p. 264.

[79] In the Muirhead Lectures on political philosophy delivered in the University of Birmingham in January and February 1938 he commented 'I have only been a Marxist for about a year'; J. B. S. Haldane, *The Marxist philosophy and the sciences*, London, 1938, p. 13.

[80] It is interesting too that scientific research is nowhere the subject of a documentary film in this period.

[81] Printed as L. Hogben, 'Naturalistic studies in the education of the citizen', pp. 111–132 in J. B. Orr et al., *What science stands for*, London, 1937. See p. 119.

[82] L. Hogben, *Science for the citizen*, London, 1938, pp. 9–11.

[83] It was published at very much the time that ARFP was established; L. Hogben, 'The new visual culture', op. cit. (71).

> creative work of film directors with the outlook of Grierson and Rotha could be enlisted to quicken the social imagination of England.

This is a process he describes, quoting Grierson, as bringing 'the new world of citizenship into the imagination'.[84] But for him, the cinema — 'the university of the future' — has a particular purpose in relation to science, and that is to sweep away the mystification normally effected by popular science,

> To make mankind aware that science teaches the possibility of plenty of the people by the people and for the people, we have got to translate it into the vernacular. . . . Education for the age of plenty means shewing people that science is not a mystery. It is organised workmanship. This can only be done if you shew people how science has arisen out of the common experience of mankind to meet the common needs of mankind.[85]

This can be seen in the light of his views on science education in general:

> education for citizenship demands a knowledge of how science is misused, how we fail to make the fullest use of science for our social well-being, and, in short, a vision of what human life could be if we planned our resources intelligently.[86]

Documentary, then, is the means by which education for citizenship, in which the instrumental potential of a demystified science is crucial, can be achieved.

It is intriguing, given his views, that Hogben was never more actively involved in film production; Huxley and Haldane actually appeared in films, after all. But his clarification of the roles of science and documentary does give an indication of the function that these scientific advisers to ARFP might expect film appearances by scientists to play. Within this view, 'camera interviews' by scientists in documentary films, are acts of scientific citizenship.[87]

But there are simpler things to be said about such appearances. At the minimum, we can argue that for the individual scientist to appear in a film implies assent to preliminary ideas about how the subject, in this case smoke abatement, is represented.[88] Beyond this, they assent to add to it a negotiated

[84] J. Grierson, Preface to P. Rotha, op. cit. 1936 (18), p. 5: 'Here, in the reporting and interpretation of fact [i.e. in documentary], was a new instrument of public influence which might increase experience and bring the new world of citizenship into the imagination.'

[85] L. Hogben, 'The new visual culture', op. cit., (71), p. 8.

[86] L. Hogben, op. cit. (83), p. 123.

[87] As Hogben wrote to Huxley about *Enough to eat?*: 'I want to tell you that I think your part in the Nutrition film was a really courageous piece of active citizenship.' LH to JH (20 Oct. 1936), Huxley Archive, Rice University, Houston, Texas.

[88] Although, a film may change markedly during the production process, as Anstey, director of *Enough to eat?*, describes; see E. Orbanz, op. cit. (18), p. 92.

contribution of their own: a statement of opinion, a display of expertise, or the presentation of information. Given this, what is Haldane doing in *The smoke menace*? It might be anticipated that he would be called upon to give a scientific definition of smoke, as he did later in a *Daily worker* column,[89] or to speak on smoke and cancer, a topic that he had raised in a magazine essay in 1925, and which had been taken up with alacrity as pro-electricity propaganda.[90] Of all the things which might be said about the effects of smoke on the public health, it is not at all clear that all interested groups would lay such stress on the prevention of vitamin production. The National Smoke Abatement Society, for example, in the five and a half pages concerned with smoke and health in their publication *The case against smoke: The evidence of authorities*, does not mention it at all. They concentrate on lung diseases and find space to speak of the psychological gloom induced by darkness.[91] Yet it is typical of those who championed the use of scientific method in medicine to stress vitamins, which for them proclaimed the validity of the scientific approach.[92] Haldane, who had spent ten years as a member of Gowland Hopkins' Department at Cambridge is clearly a member of this group.[93] The stress on the scientific approach to medicine and public health was built into discussions on the social relations of science. Haldane, Hopkins, Ryle and others were active participants in discussions which led to the establishment of the British Association's Division for the Social and International Relations of Science in 1938.[94] These scientists were arguing for an enlarged role for science in the solution of social problems and, for them, nutrition was a paradigm case. There is clear evidence in the different uses of science by the Smoke Abatement Society and the alliance formed around the production of *The smoke menace*, of scientific expertise and authority being mobilized within different interest groups for different

[89] Reprinted as 'Smoke' in J. B. S. Haldane, *Science in peace and war*, London, 1940, pp. 199–202. He explains: 'A smoke is a collection of particles, either solid or liquid, of a certain degree of fineness, suspended in air . . .'

[90] Reprinted as 'Cancer research' in J. B. S. Haldane, *Possible worlds and other essays*, London, 1927; Anon., 'Electricity and cancer (current topics)', *The electrician*, (30 Jan. 1925) **94**, 112–3. See: B. Luckin, op. cit. 1990 (3), p. 18.

[91] National Smoke Abatement Society, op. cit. (10).

[92] See: J. Austoker and L. Bryder, *Historical perspectives on the role of the MRC*, especially chapter 4 (pp. 83–108): C. Petty, 'Primary research and public health: The prioritization of nutrition research in inter-war Britain'.

[93] R. Clark, *J. B. S.: The life and work of J. B. S. Haldane*, Oxford, 1984, pp. 63–4. Haldane was active speaking in public on the nutrition issue, for example he addressed a meeting of the Committee against Malnutrition in 1934: Anon., 'The Committee against Malnutrition', *Lancet*, (1934) 1, pp. 1358–60.

[94] I am referring to the special supplement on the social relations of science published in *Nature* in April 1938 (141, pp. 723–42). See also P. Collins, op. cit. (77), pp. 225–8 and Werskey, op. cit. 1988 (77), pp. 245–7.

purposes. There are, perhaps, echoes here of the ideological divisions between scientists which had raged over the cause of rickets.[95]

We can see, then, that in *The smoke menace*, the substantial agreement between particular scientists, film-makers and sponsoring organization came together to produce a film which laid the stress on responsible action by informed and engaged citizens. The representational means used to convey this agreement combined images of industry, domestic life, children, urban landscape and 'expert voices' to produce a whole which could be received favourably as a socially-conscious documentary film.

'The smoke menace' and the National Smoke Abatement Society

One group scarcely mentioned so far is the National Smoke Abatement Society (NSAS), the organization most vocally concerned with smoke abatement in this period. Looking at their relation and reaction to *The smoke menace* shows how the interest group we have been following intersected with others. The NSAS was formed in 1929 by the merger of the Smoke Abatement League and the Coal Smoke Abatement Society.[96] Ashby and Anderson, the historians of smoke abatement legislation, characterize the first half of the 20th century as one of 'the ripening of public opinion'.[97] To achieve this 'ripening', the Society made use of the full range of media favoured by propagandist groups in the inter-war period; posters,[98] exhibitions (including a high profile event at the Science Museum in London),[99] a quarterly journal, and a series of other publications, both on the national and the local level. It is no surprise therefore that the NSAS also expressed interest in the use of film. Minutes of the NSAS executive committee reveal a low-level but continuing interest in the use of the medium from at least 1935, when they were approached by Gaumont British Distributors with a scheme for a film.[100]

[95] See D. Smith and M. Nicholson, 'The 'Glasgow School' of Paton, Findlay and Cathcart: Conservative thought in chemical physiology, nutrition and public health', *Social studies of science*, (1989) **19**, pp. 195–238.

[96] National Society for Clean Air, *Clean air: 90 years of progress*, Brighton, 1989, p. 3.

[97] op. cit. (3).

[98] Using the Empire Marketing Board poster sites.

[99] But mainly Local Authority health exhibitions; these are extensively discussed in the PRO files (MH82 series) which relate to the CCHE. See also NSAS, op. cit. (17) and Anon., 'Smoke at South Kensington', *Journal of the National Smoke Abatement Society*, (August 1936) 7, pp. 60–61.

[100] On 16 October 1935 there is a report of an offer from Gaumont–British Distributors to make at a minimum cost of £250 a smoke abatement film for non-theatrical use, along with a shortened version for inclusion in the weekly *Gaumont British magazine, an* educational newsreel which they claimed was seen by six million people. The committee agreed that 'organisations interested in the advancement of smoke abatement' should be approached for help with the £250 [Minutes of the NSAS executive committee (hereafter 'Minutes'), 16/10/1935]. A month later it was reported that this '. . . had not led to any satisfactory result . . .' and '. . . It

The gas industry had a history of support for the NSAS, as is indicated for example by their regular advertising in the NSAS journal, co-operation over the Science Museum exhibition and their care in reporting smoke abatement conferences in gas periodicals. Therefore, it might be anticipated that there would have been some sort of collaboration between the two organizations over the gas-sponsored *The smoke menace*. However, despite the film director's recollection that all the information for the film came from the NSAS,[101] there is no indication in the surviving records of the Society that any formal approach was made.[102] We may, perhaps assume that the research for the film was more by book than personal contact with the Society; certainly there is nothing within it which could not have been found either in the NSAS's publications,[103] or have come direct from the film's sponsors in the gas industry. The Society's attitude to *The smoke menace* is clear from the minutes of a meeting of the executive committee on 20th October 1937, fifteen days after its première.[104] Their reference to it is less than enthusiastic, even allowing for the tone of official minutes: 'it should be seen *what use could be made* of the film *The smoke menace* recently made by the Gas Light and Coke Company'.[105] In the same meeting there is an implied sense of urgency that the NSAS make their own film; it is stated that a Society member had agreed that an offer of £100, previously promised for radio broadcasts, be diverted to the making of a film. In the light of this activity, the tone of the editorial in the next issue of the Society's journal is intriguing:

> One of the most valuable methods of education and propaganda that a movement such as ours can possess is the film. Smoke abatement in particular is a subject that would lend itself magnificently to cinematic

was agreed that it be left to the secretary to make any further inquiries that might be of value . . .', the last which is heard of this particular project [Minutes, 27/11/1935].

[101] John Taylor, interview with author, February 1991.

[102] The only unpublished records surviving from the this period of the NSAS at the National Society for Clean Air are the Minutes of the executive committee. For a complete list, see P. Basset, *A list of the historical records retained by the National Society for Clean Air*, Birmingham and Reading, 1980 (Copy held at NSCA). These minutes record several other approaches by film companies, so we may expect contacts between The Realist Film Unit regarding *The smoke menace* also to be recorded there if they occurred.

[103] National Smoke Abatement Society, op. cit. (10), or NSAS, op. cit. (17) Certainly this tallies with Rotha's account of basing his gas-sponsored film of 1938, *New worlds for old* on written sources; 'PEP had just published an independent report on the then fierce competition between the respective virtues of the Gas and Electricity industries. This report I realized would be impossible to dramatize in human terms but it provided a mine of statistical information . . .'; Rotha, op. cit. 1973 (18), pp. 225–6.

[104] No evidence has come to light about who attended the première of the film, and it is not at all clear whether, at this stage, anyone from the Society had seen *The smoke menace*.

[105] My emphasis, Minutes, 20/10/1937.

> treatment and *with skilled and imaginative direction a vivid and convincing film of the case against smoke could be made.*[106]

The smoke menace is not mentioned in this piece, and indeed there is no reference to it in the *Journal* for the remainder of the decade. It is not as though the Society was opposed to documentaries; in the only review of a film of any kind during the decade it was most positive about Paul Rotha's *The Face of Britain*.[107] The *Journal* presents the problem of procuring a film as one of finance, tempered by qualitative considerations:

> There is only one difficulty which has prevented us from going ahead with what is almost an essential requirement, and that is, of course, the problem of cost. An adequate film — *adequate that is in both quality and length* — cannot be made under several hundred pounds, and today it has to be a 'talkie', which adds to the expense.[108]

The editorial continues by referring to the promised £100, and appeals for similar sums to be pledged by 'four or five more members'. This request only elicited another £100, which they considered spending on a film to be made by Short Film Productions Ltd.[109] In June,

> the question of the preparation of a film to cost £300 was further discussed. In view, principally, of the fact that at such a cost a *satisfactory* film could not be guaranteed and that at present the film *The smoke menace* made a further film *largely unnecessary*, it was agreed that no further action be taken on the matter, but that it be considered further in two years time'.[110]

Finally, in 1939 we find the NSAS executive committee turning to the amateur film-making community, providing the prizes for a competition in the magazine *Home movies and home talkies*.[111]

The tone the Society used in its descriptions of *The smoke menace*, a film involving them in no expenditure, is rather less enthusiastic than might be expected of an organization which was complaining of the costs of film

[106] Anon., 'Commentary', *Journal of the National Smoke Abatement Society*, (Nov. 1937) 8, pp. 200–201 (my emphasis).

[107] Anon., '"The face of Britain": A film to see', *Journal of the National Smoke Abatement Society*, (May 1936), p. 41.

[108] op. cit. (106), (my emphasis).

[109] Ibid. The Minutes report a month later that '. . . A further offer of £100 towards making a film had been received. It was agreed that inquiries should be made regarding the possibility of a film at a reasonable cost, if possible for commercial exhibition . . .' In February 1938, the committee '. . . agreed to investigate in detail the proposal for a film costing £200 made by Short Film Productions Ltd, the final decision being deferred . . .' [Minutes, 11/02/1938].

[110] My emphasis, Minutes, June 1938.

[111] Anon., 'The editor's newsreel / Big prizes for filming smoke!' *Home movies and home talkies*, (Jun. 1939), pp. 5–8. By this time the NSAS's relations with the gas industry and therefore the film-makers had changed; see next section.

production. We may gather from this that *The smoke menace* did not serve the interests of the Society. Certainly, there is no sign that the Society used the film in meetings in the way that, for example the Committee against Malnutrition used *Enough to eat?*[112] As we have seen, both minutes of the executive committee and the Society's *Journal* make reference to the cost of making a *satisfactory* film, which is defined in terms of quality and length: *The smoke menace* was obviously deemed 'unsatisfactory' on one or both counts. Equally, we are obliged to take the implication that they felt that *The smoke menace* lacked 'skilled and imaginative direction' and therefore failed to be 'a vivid and convincing film of the case against smoke'. However, given that press reviews were positive, their negative response demands some kind of explanation.[113]

One possibility is that they objected to the absence of reference to themselves; the film only refers to 'progressive minds', 'progressive people' and 'people fighting waste'. As we have seen, it certainly did not adopt the same view on smoke and health as they promulgated. But a more likely answer is to be found in the care the Society took to balance the commercial interests of gas and electricity, which they expressed in 1935, in an article in their *Journal*, 'Smoke abatement and commercial interests: The position of the NSAS':

> . . . [The] necessity [to advocate smokeless methods of fuel utilization] at once puts the movement in danger of unfair criticism, for it may easily appear to the uninformed that it is working on behalf of these particular industries, and that 'smoke abatement' may, indeed, be little more than a kind of advertising agency for them. For a long time, because of this danger, policy forbade the acceptance of subscriptions or other support from these interests. Finally, however, it was seen that it was justifiable for us to accept such support, provided that it did not also entail the acceptance of any form of control, and providing that care was exercised in maintaining impartiality as between one and another of the smokeless agents that it was desirable to recommend.
>
> . . . the various firms and associations which believe it is to their ultimate advantage to assist our work do so in the knowledge that their help does not entitle them to control the Society in any way. The relations between these subscribers and the Society are friendly but, wisely, not too intimate.[114]

We can take this idea further. We have seen that *The smoke menace* can be

[112] Anon., 'Medical news', *Lancet*, (1936) 1, p. 849.

[113] e.g.: Anon., 'Value of documentary film: Life as it really is', *The Times* (6 Oct. 1937), *Monthly Film Bulletin*, (1938) 5, p. 214.

[114] Anon., 'Smoke abatement and commercial interests: The position of the NSAS', *Journal of the National Smoke Abatement Society*, (February 1935), pp. 6–7.

viewed as an outcome of an alliance of documentary film-makers, the gas industry, and expert groups including scientists. We have also noted that the NSAS was not closely involved in its production. But equally, they can be seen as involved in other alliances. Certainly, the impression given by the lists of affiliates, subscribers and donors published in the NSAS Annual Reports is of a solid backing in Local Government and in the fuel utilities, particularly gas and electricity. Vice Presidents (presumably an honorary position) included the Bishop of London and assorted Knights of the Realm, including Leonard Hill and George Newman.[115] The NSAS was one of the active sponsoring organizations of the Central Council for Health Education (CCHE), a semi-official body, itself an alliance of central and local government, professional groups, social hygiene organizations, medical officers of health and the insurance companies.[116] As the official conduit of health education, the CCHE made use of many media of persuasion, including the cinema. But the types of films which they favoured used a mode of address which differed strongly from that of the documentarists.[117] They are much more didactic in tone; the viewer is addressed as the potential bearer of the health fault about which the films seek to educate. For example, in the *Giro the germ* series, produced by the Health and Cleanliness Council, cartoons and rhymes were used to inculcate the habit of frequent washing. VD propaganda films made for the British Social Hygiene Council[118] adopted the structure of melodrama, but also frequently included addresses from doctors or nurses about appropriate medical and moral behaviour for those with syphilis or gonorrhoea. In *The smoke menace*, by contrast, smoke is presented as a subject for discussion amongst informed citizens who, perceiving the positive efforts being made by 'progressive people' and corporations, are expected to respond by burning less raw coal, thereby making their contribution to 'a cleaner and less wasteful order'.

Denouement: The gas smoke abatement campaign of 1938

We have seen that *The smoke menace* was not received in a markedly favourable spirit by the NSAS, and I have suggested that part of the reason may lie in their membership of different alliances with different views from that

[115] National Smoke Abatement Society, *Eighth annual report, 1937–8,* London, 1937.

[116] Central Council for Health Education, *Health education yearbook, 1937–8*, London, 1937, pp. 9–21.

[117] Although the CCHE distributed a wider range of films than those made specifically for and by their constituent organisations. For a list, see Central Council for Health Education, *Health education yearbook, 1939–40*, London, 1939. The BCGA, adept at making use of existing organisation had secured distribution of their films through the CCHE, and some of them are listed here, although they also used the WEA, and several other distributors; see Anon., 'BCGA 27th AGM', op. cit. (51), p. 341.

[118] Such as, for example *Trial for marriage* (1937) or *A test for love* (1937).

gathered around the production of The *smoke menace*. That such alliances mattered is signified by their attitude to balancing the influence of the gas industry against those of electricity.

But in 1938, the gas industry made smoke abatement the focus of a large scale advertising campaign and the Society, whatever their wishes, found themselves enrolled by the sophisticated public relations strategy of the industry. A key element in the campaign was the booklet *Britain's burning shame*,[119] which uses a vivid photojournalistic approach — redolent of *Picture post* — to put across the smoke abatement gospel. A few examples will indicate the style. In one spread we learn 'the story of one girl — and thousands' whose marital bliss is shattered within six months by the constant labour of cleaning the dirt caused by smoke. Eight pages are given up to 'an experiment in clean living' in Kensal House, thinly disguised as 'Cleanliness House' for the sake of anonymity. Like the documentary film *Kensal House*, this section concentrates on individual experience; large photographs are accompanied by small quotations from the inhabitants of the flats; '"I save time on washing since I came to 'Cleanliness' House" says Mrs. Lancashire'.[120] This stress, in content and in style, implies very close involvement by the British Commercial Gas Association in the booklet's production, and possibly sole responsibility.[121]

The Society was sensitive to the fact that the style of the booklet was so radically different from their established publicity:

> If, by chance, any of our members should have felt that [it] is too popular in its method of presenting the smoke abatement case all that need be said is that it is not intended for the already converted. It supplements, but in no way displaces, our conference reports, the *Journal*, or the invaluable *Case Against Smoke*.[122]

In an extension of the policy of sustaining an air of 'laudable neutrality' in their publicity, which had been so effective with the films, the BCGA had agreed that 'It need hardly be said that in the first place it was made clear that the booklet could not in any way indicate any preference for gas as a smokeless medium'.[123] Yet, in fact, the BCGA immediately produced another booklet, *What* you *can do to get rid of SMOKE* which 'has gone into every room in the house and has said — "Get coke working here — and gas here —

[119] London, 1938. [120] Ibid., pp. 2–3, 21.

[121] The British Library Catalogue lists it as a NSAS publication, but some of the gas press (for example; Anon., 'On the selling side', *The gas world*, (15 Oct. 1938), p. 328) refers to it as a gas publication; certainly the implication was taken that this was the case. It mentions no electrical solutions to the smoke problem. The gas industry were experimenting with magazines as a publicity method at this time; the Gas Light and Coke Co. brought out one called *Modern living* in October 1938; see Anon., 'On the selling side', *The gas world*, (22 Oct. 1938), p. 355.

[122] Anon., '"Britain's burning shame" and the gas campaign', *Journal of the National Smoke Abatement Society*, (Nov. 1938), pp. 101–3. [123] Ibid.

and gas here — and coke here"'. They supplied the two booklets tied together; as an internal journal suggested: 'With these two books together in her hand the over-worked, smoke-ridden housewife will have the problem *and* the answer'.[124] The intention was clear:

> This is the strongest, most realistic sales platform the Gas Industry has ever had. It ties up gas sales policy with a burning social question that effects [sic] every home in Britain. The B.C.G.A. has made plans to support this national propaganda to the hilt, to plug gas and coke as the complete answer to the home smoke problem, and to turn newly roused public goodwill into SALES.'[125]

This campaign provided a new context for use of *The smoke menace*, which they recommended to be shown with *Kensal House* in 'cinemas and halls, and to Women's Institutes, Mother's Unions etc.' These were not new audiences for non-theatrical distribution of documentary films, but within this context, the woman's audience is integrated into a sales campaign which is addressed at women first and men second.

The NSAS was not entirely happy with this aggressive use of *Britain's burning shame*, as the executive committee minutes reveal: 'although its use by the gas industry had caused much comment in certain quarters, the booklet appeared to have been very favourably received by members of the Society'.[126] In their *Journal* they discuss the implications of so close an association with gas:

> We hope that with them it will not be merely the slogan of a season, and we hope even more earnestly that our other friends will each consider the ways in which they can each advance smoke abatement. The Society will willingly co-operate *in any way that is consistent with its impartiality and independence*. . . . Cannot the gas campaign be regarded not as something that concerns the gas industry only, but as the beginning of an intense and continuous effort, with the co-operation of all, towards the realization of the Society's objectives ?[127]

Given the context, this reads as a question inviting a sceptical reply; the Society's 'impartiality and independence' had been exchanged for an unwelcome alliance. This had been achieved, not at the first stage with the production of the film *The smoke menace*, but at the second stage when the gas industry came to apply much larger resources to the issue of smoke abatement. Within this, *The smoke menace*, with its central idea of concerned citizens working alongside scientists and corporations to solve a 'national problem', is put to work as a way of selling gas appliances. The fact that it

[124] Anon., *B. C. G. Announcer (magazine)*, (Sept. 1938), London, pp. 1 and 4.
[125] Ibid., p. 1. [126] Minutes, 16 Dec. 1938. [127] Anon., op. cit. (122), p. 103.

is Haldane's speech which is used to persuade local representatives to incorporate *The smoke menace* in local campaigns speaks volumes about the status of scientific expertise within these films, and also of the way they were put to work.[128]

Conclusion

The smoke menace served the interests of an alliance of different groups. For the documentarists it provided the opportunity to turn their reforming gaze on one of the minor public health discourses of the thirties. For the gas industry, it was in the first instance an opportunity to sustain their public profile as a socially conscious industry whilst dealing with a subject in which they could be seen as reformers themselves. In the second instance, it was knitted into one of their largest and most daring public relations strategies, in the campaign of autumn 1938. For scientists it was an opportunity to increase their visibility whilst demonstrating their social concern. For all involved it was a small instance of an alliance which was to be sustained into the war years with increasing influence.

The analysis of *The smoke menace* presented in this paper has several implications, in how we consider documentary films and public health, and for our understanding of the processes involved in scientists talking to the public. For documentary film it gives evidence that to understand the sorts of power which are involved in the production of realist representations of the world, it is first necessary to address the social relations underlying them. In the case of *The smoke menace*, the relative degrees of agreement between members of the alliance responsible for its production contrast clearly with the views of the NSAS, ostensibly the most powerful body concerned with smoke abatement in the period. Understanding that the NSAS was accustomed to operate within different alliances, and knowing how cool their response was, clearly indicates just how contingent an artefact a 'realist' film can be. The production of *The smoke menace* and the NSAS's failure to produce a film of any kind is an instance of the ascendancy of documentary over other types of propagandist film-making, a phenomenon which became more significant slightly later. During the second world war, the documentary group with its sustaining alliances, rather than any other single film-making group, came into power as the creators of official propaganda of all kinds, including those which represented public health issues.

Lastly, it gives an instance of a vehicle of communication between scientists and the public, not on the basis of the dominance of either group, but on the basis of agreement. In this case, agreement is not about the importance of science — perhaps too deeply buried to warrant comment — but

[128] Anon., op. cit. (124), p. 6.

about the need for social reform. Now that environmentalism rather than social reform is one of the dominant ideologies under which scientists speak to the public on television and radio, it would be interesting to think about the levels of agreement and disagreement which inform their appearances, and what that may imply about the status of expertise in our own society.

— 5 —

Conserving nature and building a science: British ecologists and the origins of the Nature Conservancy

STEPHEN BOCKING

Introduction

From a hill in Sussex, in the south of England, one sees a diverse landscape of woodland and farmland; further away is Chichester Cathedral, and beyond that, the seashore. Immediately below is Kingley Vale, a pocket of natural vegetation: a yew wood, of 'slow powerful trees that make their own cathedral-cool climate'; scrub and heath, with their characteristic and colourful flora; and a grassland, rich in herbs and flowers. A Sarsen Stone, placed there in 1957, overlooks this view. Its inscription reads:

> In the midst of this Nature Reserve which he brought into being this stone calls to memory Sir Arthur George Tansley, F.R.S., who during a long lifetime strove with success to widen the knowledge, to deepen the love and to safeguard the heritage of nature in the British Islands.

Tansley was said to have considered this view the finest in Britain.[1] Like many British ecologists, he combined his research with a deep affection for the countryside.

During the Second World War debate accelerated over the protection of Kingley Vale and other natural areas. Tansley and other ecologists played a major role in this, by insisting that ecological research was essential to maintaining these fragments of Britain's natural heritage. In 1949 a sympathetic government responded by creating the Nature Conservancy, an independent institution to support ecological research, provide advice

E. M. Nicholson, E. B. Worthington and Polly Winsor provided generous assistance and advice during my research, and an anonymous reviewer supplied useful comments on an earlier version of this paper. I should also like to thank Professor Henry Regier, and the Associates of the University of Toronto for research support. Additional support was provided by Social Sciences and Humanities Research Council of Canada Doctoral and Post-Doctoral Fellowships.

[1] See J. Hywel-Davies, V. Thom and L. Bennett, *The Macmillan guide to Britain's nature reserves*, London, 1986, pp. 397–398; the inscription, written by Max Nicholson, is in *Report of the Nature Conservancy, for the year ended 30th September 1958*, London, 1958, p. 91.

concerning natural flora and fauna, and maintain nature reserves. For ecologists, the Conservancy was an important milestone. While assuring the protection of many distinctive plant and animal communities, it also quickly became the leading ecological research organization in Britain, with several research centres and a program of grants to academic ecologists.

The Nature Conservancy however reflected more than ecologists' success in protecting the countryside, while gaining new support for their research. In this paper I explore the period immediately preceding the Conservancy, when ecologists were attempting to redefine their discipline, and demonstrate that they could provide practical, authoritative advice on the management of the natural landscape. These objectives were reflected in new techniques of experimentation and survey, new theoretical perspectives such as the ecosystem concept, and firm ideas on the relationship between government and science. Convinced that their science could not flourish within existing academic or government institutions, ecologists decided that a new institution, dedicated to ecology, was necessary. In the 1940s they pursued this by assuming leadership of the nature reserves movement, recasting its objectives to be consistent with the theory and practice of ecological research, and presenting reserves and nature conservation as an appropriate responsibility of government. Their success was apparent in the creation of the Conservancy, which was designed according to the proposals of leading ecologists, to fulfil the unique requirements of their research.

Applied ecology

By the late 1930s, British ecologists could look back on several decades of efforts to obtain support for their discipline. Even their transformation of the British Vegetation Committee into the British Ecological Society in 1912 had been partly motivated by a belief that government funds would thereby be more easily obtained.[2] This concern for funding was most apparent in discussions of the practical applications of ecological research. Tansley, until his death in 1955 the most eminent British ecologist, was also among the most persistent in demonstrating the utility of his discipline. Between 1924 and 1926, for example, he co-edited *Aims and methods in the study of vegetation*, which explained how ecology could be applied by foresters and agriculturists throughout the Empire.[3] In 1927, in his inaugural lecture as professor of botany at Oxford, Tansley presented his vision of the future of botany, discussing at length its relevance to agriculture, forestry, and pasture management. Each, he explained, depended upon an extensive understanding of plants and their environment. He also stressed that science itself

[2] J. Sheail, *Seventy-five years in ecology: The British Ecological Society*, Oxford, 1987, p. 39.

[3] A. G. Tansley and T. F. Chipp (eds.), *Aims and methods in the study of vegetation*, London, 1926.

would benefit from links to 'practical human needs'. Evidently, he remarked, 'it is perfectly hopeless to expect to develop or to maintain an efficient scientific department without the substantial monetary support that is rarely forthcoming unless a clear and unequivocal public utility other than a purely educational or academic utility can be demonstrated.'[4] Practical relevance would also assure support for graduates. As he noted several years later,

> As soon as the various 'powers that be' have realized more fully that field ecology is the best of all trainings for forestry, for pastoral science, and for all careers in which a knowledge of the relation of vegetation to the land is of prime importance, the opportunities and facilities for such work will increase and multiply.[5]

But Tansley also argued that botany itself benefited from insights provided by practical problems. These problems were 'an important part of the driving forces which are necessary for the fertile development of science, and if we try to divorce pure from applied science we drift almost inevitably into sterile academicism.'[6] As Tansley knew well, ecology had benefited from its contacts with agriculture. Research at Rothamstead Experimental Station, for example, led to more interest among ecologists in grasslands and the relations between plants and soil.[7]

But while benefiting from such contacts, ecologists were nevertheless not able to obtain from the Ministry of Agriculture sufficient support for studies of strictly ecological problems. As R. George Stapledon complained in 1928, study of grassland communities affected by grazing was only supported when it could be justified by benefits to agriculture; a similar study on vegetation strictly of interest to ecologists would only be possible when ecology had 'sufficient financial backing to found for itself a station unhampered by agricultural or other economic calls upon its methods of enquiry'.[8] By 1939, only Charles Elton had obtained significant support from the Ministry for ecological research, for studies by his Bureau of Animal Population at Oxford on pests of grain. This came after a decade of depending on modest, relatively uncertain grants from several organizations.[9]

[4] A. G. Tansley, *The future development and functions of the Oxford Department of Botany*, Oxford, 1927, pp. 4–5.

[5] A. G. Tansley, *The British islands and their vegetation*, Cambridge, 1939; reprinted 1953, p. x.

[6] Tansley, op. cit. (4), p. 5.

[7] E. J. Salisbury, 'The origin and early years of the British Ecological Society', *Journal of ecology*, (1964) **52** (suppl.), 16; J. Sheail, 'Grassland management and the early development of British ecology', *British journal for the history of science*, (1986) **19**, pp. 297–298.

[8] R. G. Stapledon, 'Cocksfoot grass (Dactylis glomerata L.): Ecotypes in relation to the biotic factor', *Journal of ecology*, (1928) **16**, p. 103.

[9] P. Crowcroft, *Elton's ecologists: A history of the Bureau of Animal Population*, Chicago, 1991, pp. 13–46; J. Sheail, 'Applied ecology and the search for institutional support', *Environmental review*, (1989) **13** (2), pp. 68–70.

In the 1940s Tansley led ecologists in an effort to demonstrate the relevance of their work to forestry. This culminated in July 1943, in a meeting between the British Ecological Society and the four British forestry societies, at which Tansley and other ecologists explained the assistance they could provide to forestry. The meeting had been precipitated, Tansley noted, by a recent article by Sir Roy Robinson, chairman of the Forestry Commission. Robinson had explained that new forests were most successful when their planting and management followed the natural succession of woodlands. It was in particular necessary to be aware of which species could colonize bare land, and which could replace pioneer species. An understanding of woodland ecology was therefore, Robinson believed, necessary to forestry management.[10] Robinson's comments apparently convinced Tansley that the Forestry Commission would be willing to provide opportunities for ecological research. The Commission, Tansley suggested, should permit the use of plantations as large-scale experiments. Ecologists should even be able to propose variations in forest planting, to make these experiments as useful as possible. In co-operating, both ecologists and the Forestry Commission would benefit: practical problems would provide starting points for research aimed at a general understanding of forest ecology; this understanding would then provide the basis for solution of specific problems.[11]

This effort to link ecology and forestry was especially timely because an increasing fraction of the countryside was being devoted to new forests. The month before the joint meeting, the Forestry Commission had released its plan for post-war forestry, in which it proposed to create within 50 years five million acres of intensively managed forests. This would require new forest plantations on three million acres of land.[12] Tansley was concerned about the aesthetic impact of these plantations, and the replacement of deciduous woodlands by conifers:

> Young coniferous plantations, straight-edged and neat in form with precisely aligned saplings, their floors bare of the familiar woodland plants, introduce into the landscape an alien feature which cannot but offend the lover of the luxuriant irregular beauty of the countryside to which he is accustomed. And it is not only his aesthetic sense which is outraged. Plantations of conifers profoundly alter the plant and animal life of a region. Not only do they destroy the vegetation of open ground, but they change altogether and greatly impoverish the character of the

[10] R. Robinson, 'Some ecological aspects of afforestation and forestry in Great Britain', *Forestry*, (1943) **16**, pp. 1–12.

[11] British Ecological Society, 'Ecological principles involved in the practice of forestry', *Journal of ecology*, (1944) **32**, pp. 83–87.

[12] Forestry Commission, *Post-war forest policy*, London, 1943.

> woodland flora and fauna when the plantation is made on a woodland site.[13]

These plantations, Tansley argued, could cause soil deterioration and declining productivity. Such problems, he implied, demanded the attention of ecologists. Ecologists could also advise the Forestry Commission of areas suitable for reforestation. The highlands of north-west Britain, for example, while unsuitable for agriculture, could be used for 'great forests of climax conifers developed and allowed to grow to maturity'.[14]

Foresters were not unswayed by these arguments. The Commission set up a few forest reserves, and permitted ecologists to study in its forests, supporting the research of a few of them. This support, however, was very limited. This may have been because while foresters did appreciate the insights of ecologists into such problems as forest succession and soil deterioration, they also had many concerns for which the expertise of ecologists was not immediately relevant, such as the effects of fire, wind and disease, and methods of ensuring soundness of timber.[15] In general, the Commission favoured short-term research on problems encountered in planting new forests, not the long-term studies of established plantations that ecologists were most prepared to participate in. A probable additional factor was the perception that foresters understood aspects of forest ecology better than did ecologists. In particular, Alex Watt argued that foresters were far ahead of ecologists in comprehending the patterns of distribution, and relations between, species within forest communities. 'Ecologists', Watt noted, '. . . have a long way to go before they know enough about the relations between species in the plant community to make generalizations which will be useful in the practice of forestry'.[16] The end result was that, in the 1940s, support for ecological research from the Forestry Commission, as from agricultural bodies, remained a largely unfulfilled hope.

But while unable to obtain adequate support, ecologists were nevertheless discussing research that would require more support and facilities than they had ever had access to. At Oxford, for example, Elton was developing plans for his ecological survey of Wytham Estate, with the long-term aim of understanding the dynamic interactions between all the species of organisms living within it. Such a survey, Elton believed, was essential to bridging the gap between plant and animal ecology, permitting synthesis of 'at least a

[13] A. G. Tansley, *Our heritage of wild nature: A plea for organized nature conservation*, Cambridge, 1945, p. 54.

[14] British Ecological Society, op. cit. (11), pp. 84, 88.

[15] H. G. Champion, 'The future of our new forests', *Advancement of science*, (1950) **6**, pp. 319–324.

[16] British Ecological Society, op. cit. (11), 103. This was the focus of Watt's own research, which he would later present in his influential paper, 'Pattern and process in the plant community', *Journal of ecology*, (1947) **35**, pp. 1–22.

working model of how whole ecosystems are arranged in nature, and how they work and interact'.[17]

Elton's reference to ecosystems recalled Tansley's development of the concept in 1935, both to serve as a basis for previous ecological study, and to suggest future research directions. In Tansley's view, this concept integrated the understanding that ecologists had gained since 1900 of succession, the interaction between animal and plant communities, and the development of an equilibrium between the biota and its physical environment.[18] The ecosystem concept could also provide the basis for new techniques, such as long-term studies of succession and field experiments, and the adaptation of ideas from physics and chemistry, climatology and pedology, through which, Tansley believed, ecologists could develop a fully scientific approach to the study of flora and fauna. And as, for ecologists, the 'basic unit of nature', the ecosystem concept, finally, asserted the distinctiveness of ecology, distinguishing it from the study of both individual organisms and inorganic systems.[19]

With these new ideas and techniques emerging, some ecologists felt that ecology was entering a new phase, in which new methods, concepts and problems would reshape the discipline, shifting its orientation from describing the patterns of vegetation and animal populations, to the causes behind these patterns. Elton, for example, told ecologists that he was 'absolutely convinced that we are on the threshold of a period when synthesis of animal and plant community facts and concepts will not only be possible but necessary, if we are to interpret ecological phenomena fully, and see them in the proper contexts'.[20] Tansley too foresaw the need to move from description, to understanding causes, and that new techniques would be necessary:

> Ecologists have been largely occupied hitherto in describing the facts of natural succession, but we can have no thorough knowledge of a succession until we have discovered the actual effective factors which enable one kind of a tree to succeed in the presence of another but not alone . . . Only carefully designed experiments will discover the actual factors at work, and this is essentially the business of the ecologist.[21]

This attitude was perhaps best demonstrated in the reaction of leading ecologists to Tansley's *The British islands and their vegetation*, published in 1939, which summarized the work of British plant ecologists over the preceding four decades. William Pearsall found special significance in how it

[17] C. Elton, 'Population interspersion: An essay on animal community patterns', *Journal of ecology*, (1949) **37**, p. 1; see also C. Elton & R. Miller, 'The ecological survey of animal communities: With a practical system of classifying habitats by structural characters', *Journal of ecology*, (1954) **42**, pp. 460–496.

[18] A. G. Tansley, 'The use and abuse of vegetational concepts and terms', *Ecology*, (1935) **16**, pp. 284–307.

[19] Tansley, op. cit. (18), p. 299.

[20] Elton, op. cit. (17), p. 1.

[21] British Ecological Society, op. cit. (11), p. 86.

'marks an epoch, the culminating point of a period in which the descriptive study of existing vegetation has been the leading interest of British ecologists'.[22] According to Edward Salisbury, the book

> may be said to mark the closing of the purely descriptive phase of the study of British vegetation. Future progress lies rather in the direction of causal ecology, of the study of the biological relations of species, and in the application of the principles of physiology and the factors of competition to the elucidation of the problems of plant and animal life under natural and semi-natural conditions.[23]

Watt also took Tansley's recent survey as a 'landmark', marking the end of one period of British ecology, and the beginning of another: 'It is the climax of a phase in which the main theme has been the relation of one plant community to another. The next stage, in my view, will be the intensive study and elucidation of the dynamic relations which hold between one species and another in the plant community itself.'[24] These views, it may be concluded, indicate that ecologists saw ecology as leaving one period of development, and entering another, marked by new methods and perspectives.

But this new phase of ecology depended on facilities that were not readily available. Flora and fauna of interest to researchers needed long-term protection. Tansley's research areas had twice been destroyed; he noted that

> One of the most troublesome and irritating hindrances to ecological observations intended to serve as a basis for the study of successional change . . . is the liability to interference with or destruction of the vegetation of the area under observation by such events as clearing, felling, draining, gravel digging, change of ownership, or 'development'.[25]

Suitable areas were also needed for experimentation; Tansley noted in 1943 that for ecology not 'the least of its difficulties has been the lack of sufficiently extensive opportunities for adequate ecological experiments, without which the science cannot progress.'[26] Ecologists themselves also had to be assured of long-term support, to complete intensive surveys or experiments. Secure employment and research funding, however, was not readily available. Tansley complained of 'the difficulty of placing the young ecologist in a bread-winning job. Compared with the mycologist and even with the plant

[22] W. H. Pearsall, 'A milestone in plant ecology', *Journal of ecology*, (1940) **28**, p. 241.

[23] E. J. Salisbury, 'The study of British vegetation', *Nature*, (1939) 144, p. 306.

[24] British Ecological Society, op. cit. (11), pp. 103–104.

[25] A. G. Tansley, 'British ecology during the past quarter-century: The plant community and the ecosystem', *Journal of Ecology*, (1939) 27, p. 527.

[26] British Ecological Society, op. cit. (11), p. 83.

physiologist or geneticist he is still at a grave disadvantage.'[27] As he later recalled, he had even, because of the lack of opportunities, discouraged students from studying ecology.[28]

It was in these circumstances — of innovative research strategies emerging, but hindered by a lack of secure support and opportunities — that a new occasion arose for demonstration of the practical benefits of ecology. This was the concern over the destruction of Britain's flora and fauna. To understand the approach that ecologists took to this issue, we must turn to the longstanding interest in nature reserves.

Preservation or conservation

As early as 1873 it had been suggested that the government set aside reserves to 'preserve spots of primitive land-surface of which the vegetation was especially interesting'.[29] In 1912, when Charles Rothschild founded the Society for the Promotion of Nature Reserves (SPNR), naturalists began a formal attempt to win support for the preservation of rare species and habitats through nature reserves. Rothschild and his colleagues at the National Trust and other preservationist societies were painfully aware of the gap between the few reserves they had acquired, and the 273 areas 'worthy of protection' that they had identified. They did not propose that the government take responsibility for reserves, only that it provide funds to voluntary societies, which would then select and protect reserves.[30] The concept of reserves did receive some official support. In 1931, for example, the Addison Committee, investigating the potential for national parks in Britain, recommended a system of National Reserves and Nature Sanctuaries.[31] The government, however, did not act, and, as John Sheail has observed, nature preservation made little progress before the Second World War.[32]

Nature preservation assumed special urgency as the war deepened. Military ranges, camps and airfields consumed land. Farmers, urged to produce as much food as possible, ploughed under grasslands and heaths. Forests were cut down for timber. Taxes continued to force the breakup of large estates that had protected valuable areas. In addition, the government, preparing for post-war reconstruction, had begun developing mechanisms for

[27] Tansley, op. cit. (25), p. 530.

[28] A. G. Tansley's statement in meeting of the Advisory Council on Scientific Policy, November 2, 1949, PRO CAB 132-66.

[29] W. T. Thiselton-Dyer, in D. E. Allen, 'Changing attitudes to nature conservation: The botanical perspective', *Biological journal of the Linnean Society*, (1987) **32**, p. 208.

[30] J. Sheail, *Nature in trust: The history of nature conservation in Britain*, Glasgow, 1976, pp. 196–98; H. J. Harvey, 'Changing attitudes to nature conservation: The National Trust', *Biological journal of the Linnean Society*, (1987) **32**, pp. 149–159.

[31] G. E. Cherry, *Environmental planning, 1939–1969*, London, 1975, II, pp. 13–15.

[32] J. Sheail, *Rural conservation in inter-war Britain*, Oxford, 1981.

planning and regulating development and land use. Lord Reith, the Minister of Works and Buildings, initiated studies of industry, agriculture and other factors affecting economic activities and land use.[33] One such study was by the Committee on Land Use in Rural Areas (the Scott Committee). The Forestry Commission and other government bodies also developed plans for the post-war development of the countryside.

The SPNR looked on these plans with some anxiety: 'Many naturalists were concerned lest efforts to preserve the native flora and fauna for the benefit of posterity should be neglected.'[34] In response, it campaigned to ensure a role for naturalists in the future of the countryside, by arranging in 1941 a Conference on Nature Preservation in Post-War Reconstruction. One conclusion of the conference was that planning must protect flora and fauna useful to scientists, with 'strict' nature reserves, in which 'any work involving the alteration of the configuration of the soil or the character of the vegetation, any act likely to harm or disturb the fauna or flora, and the introduction of any species of fauna or flora . . . shall be strictly forbidden.'[35]

Ecologists did not at this point play a major role in the nature reserves movement, possibly preferring to leave the initiative to amateur naturalists and recreational interests. In 1939 Tansley, while noting the efforts of the Commons and Footpaths Preservation Society and the National Trust to establish reserves, did not propose that ecologists become involved, beyond perhaps surveying the plants and animals of Trust properties. He apparently expected greater success to be achieved not through nature reserves, but through the national parks movement, which 'in some form seems likely to achieve a positive result in the immediate future'.[36] Thus, although the British Ecological Society was one of fifteen organizations taking part in the 1941 Conference on Nature Preservation, Herbert Smith, an expert on gems, and honorary secretary of the SPNR, provided its leadership.

By 1942, however, ecologists had decided to play a more active role in the nature reserves movement. Their decision may have been motivated by the interest the government was now demonstrating in this issue. That year, representatives of the Conference on Nature Preservation met with William Jowitt, chairman of the ministerial committee concerned with reconstruction.[37] Jowitt invited them to form a committee to advise the government on reserves. This, the Nature Reserves Investigation Committee (NRIC), was also led by Smith, but it included several ecologists, including Cyril Diver,

[33] Cherry, op. cit. (31), pp. 33–37.

[34] Nature Reserves Investigation Committee, 'Nature conservation in Great Britain', Draft copy, January 1943, 1, PRO HLG 92-18.

[35] Society for the Promotion of Nature Reserves, 'Nature preservation in post-war reconstruction', Conference memorandum no. 1, November 1941, PRO HLG 92-18.

[36] Tansley, op. cit. (5), p. 192.

[37] H. Smith to W. Jowitt, April 1, 1942, PRO HLG 92-18.

Pearsall and Salisbury. In its 1943 report, the Committee presented a list of proposed nature reserves, and advised that they could not be merely protected from human interference, but must be subject to a 'rigid and continuing control under scientific direction'. This would be the responsibility of experts on plant and animal communities, employed by a 'National Reserves Authority' that would be a special department of the Ministry of Town and County Planning.[38]

The ecologists had selected many of the reserves proposed by the NRIC. But their influence was especially evident in the committee's emphasis on active management of reserves. By this time ecologists had concluded that most British plant and animal communities had been affected by human activities. British woodlands, for example, were intermediate between virgin forest and plantations: exhibiting aspects of both the primeval forest, and their long history of care and exploitation.[39] Grasslands were also, as Harry Godwin noted in 1929, shaped by human uses. When subjected to continued pasturage or cutting, their succession may be deflected into a different, but nonetheless stable climax community. If this management ceased, succession might take a different course, eliminating the existing community. For example, the Wicken Fen nature reserve had to be periodically cleared of bushes to maintain its characteristic vegetation.[40] Through such insights, the effects of human activities had become an integral part of ecologists' theoretical understanding of plant communities. In defining his conception of ecology, Tansley stressed in 1935 that however overwhelming is the human impact, it remains a factor that ecologists must consider: 'Regarded as an exceptionally powerful biotic factor which increasingly upsets the equilibrium of preexisting ecosystems and eventually destroys them, at the same time forming new ones of very different nature, human activity finds its proper place in ecology.'[41] 'We cannot', he reminded his colleagues, 'confine ourselves to the so-called "natural" entities and ignore the processes and expressions of vegetation now so abundantly provided us by the activities of man.'[42]

In the NRIC report, as well as in a 1943 report of the British Ecological Society, ecologists insisted that nature reserves must be managed to maintain their distinctive plant and animal communities. To refer to the management of natural areas they adopted the term 'conservation', likely importing it from the United States and Canada, where, since the turn of the century, it had signified the scientific management of natural resources. By the 1940s,

[38] Nature Reserves Investigation Committee, op. cit. (34).

[39] A. G. Tansley, 'Natural and semi-natural British Woodlands', *Forestry*, (1940) 14, pp. 1–21.

[40] H. Godwin, 'The sub-climax and deflected succession', *Journal of ecology*, (1929) 17, pp. 144–147; H. Godwin, 'The "sedge" and "litter" of Wicken Fen', *Journal of ecology*, (1929) 17, pp. 148–160.

[41] Tansley, op. cit. (18), p. 303.

[42] Tansley, op. cit. (18), p. 304.

British ecologists were well aware of, and admired, North American conservation. Elton, for example, in a laudatory review of *Natural principles of land use*, by Edward Graham, the Chief of the Biology Division of the US Soil Conservation Service, praised the 'fine tradition of the United States Federal wild life services'.[43] In 1941–42 Julian Huxley visited, and later wrote about, the Tennessee Valley Authority, which he saw as the application of conservation and planning principles to an entire region.[44] In adopting its terminology, British ecologists evidently hoped to emulate the success of North American conservationists in achieving recognition and support for their work. Their willingness to learn from the North American example was also suggested by the work of the Wildlife Conservation Special Committee. I will discuss this committee below; it is appropriate to note here, however, that while developing a proposal for a nature conservation institution, it studied the work and organization of the US National Parks Service and the Fish & Wildlife Service.[45]

There were several implications of the insistence of ecologists on the necessity of scientific management, or conservation, of reserves. Most important, it established the authority of ecologists as best able to manage these reserves. Management required an understanding of the dynamic processes of natural communities, and especially of their successional development. Such an understanding had been accumulated by ecologists over the preceding four decades. Thus, as nature reserves moved closer to being a government responsibility, this authority brought ecologists closer to official recognition of the practical utility of their expertise. Effectively, ecologists sought to define nature reserve management as a professional activity. In doing so, they excluded others from this activity, and particularly, the amateur naturalists that had been a driving force in the nature reserves movement. By designating their research and its application in management as 'conservation', ecologists distinguished themselves from amateur naturalists and their demands for the 'preservation' of nature. Reliance on preservation, Tansley insisted, reflected an ignorance of ecology:

> Some nature lovers who have little actual acquaintance with the ways of nature and her reactions to human activities constantly advocate a policy of 'letting nature alone' in nature reserves. This is only justified in the few places where there has been little or no human interference . . . Over

[43] C. Elton, 'Land ecology', *Journal of animal ecology*, (1945) 14, p. 53; E. H. Graham, *Natural principles of land use*, London, 1944.

[44] J. S. Huxley, *TVA: Adventure in planning*, London, 1943.

[45] Report of conversations with Iva Gabrielson, Head of US Office of National Park Wildlife, Fish & Wildlife Service, November 25, 1945; 'Copy of a memorandum of agreement between the National Park Service and the Fish and Wildlife Service, of the Department of the Interior, September 11, 1943' in Wild Life Conservation Special Committee, 'Terms of Reference, Committee Papers, minutes of meetings, etc.', 1945–46, PRO HLG 93-48.

> most of the country the natural vegetation . . . has been decisively altered and more or less stabilised by man, has become in fact what we call 'semi-natural', and if the human regime is discontinued it will change into something else.[46]

The notion that intervention was necessary to maintain plant and animal communities was useful for ecologists in another way, by helping dissolve the distinction between natural and artificial communities. There was no, or very little, pristine nature left — only plant and animal communities affected to varying degrees by human activities. Most vegetation, in fact, was neither natural nor artificial, but 'semi-natural'. Knowledge gained through study of manipulated communities could therefore provide valid insights into communities once considered to be 'natural', but that in fact differed only in degree of human influence. This conclusion gave more credibility and authority to ecologists developing experiments, studying forest plantations, or arguing for the relevance of ecology to agriculture and forestry management.

Another advantage to ecologists of the notion of conservation was its compatibility with the government's intentions for post-war planning. Just as ecologists were arguing that management was necessary to maintaining valued features of Britain's flora and fauna, so were government planners and other experts arriving at a similar conclusion for the countryside as a whole. Patrick Abercrombie, Thomas Sharp, and others had often argued that the British countryside was not static, but had been, and continued to be, shaped by a dynamic interaction between human activities and natural features. This perspective achieved some official status in 1942, in the influential report of the Scott Committee:

> The landscape of England and Wales is a striking example of the interdependence between the satisfaction of man's material wants and the creation of beauty. The pattern and the beauty of the countryside as we know them to-day are largely the work of man during the past few centuries. Its present appearance is not by any means entirely the work of nature and it is not enduring, for nature is dynamic, not static . . . Experience has shown how quickly land can revert to an unkempt, wild and ragged condition, even when it is only neglected and not wholly abandoned. The beauty and pattern of the countryside are the direct result of the cultivation of the soil and there is no antagonism between use and beauty.[47]

The countryside, the Committee concluded, 'cannot be "preserved" . . . it must be farmed if it is to retain those features which give it distinctive charm and character.'[48] This view, it is evident, was consistent with the case made

[46] A. G. Tansley, op. cit. (13), p. 35.

[47] Committee on Land Utilisation in Rural Areas, *Report*, London, 1942, p. 4.

[48] Committee on Land Utilisation in Rural Areas, op. cit. (47), p. 47.

by Tansley and other ecologists that nature reserves should be not merely preserved, but managed. By arguing that they could guide reserve management and conservation, they effectively presented their expertise as essential to, and consistent with, government plans for the countryside as a whole.[49]

For ecologists conservation involved more than nature reserves. While essential, reserves were only one element in a broader program of research and practice. Although several ecologists were members of the NRIC, they decided in 1942 to prepare their own report on nature conservation. They did so because they felt 'that the interests of ecology as such and the views of ecologists ought to be independently formulated'.[50] The following year, the report of the British Ecological Society (drafted by Tansley) endorsed the NRIC's proposal of a system of reserves managed by experts, to be used for ecological research and other purposes. But the Society also recommended a 'National Wildlife Service', to conduct ecological research. This service would not be within the Ministry of Town and Country Planning, but would, rather, be an independent body under the Privy Council, comparable to the Medical Research Council. Ecologists feared that if the Ministry of Town and Country Planning became responsible for nature reserves, as the NRIC had proposed, then independent research would be impossible. It would also hinder the application of research results to other responsibilities of government, including agriculture and forestry. Only an independent entity, they believed, would be able to 'create its own policy and traditions'.[51] These ideas were developed further in a second publication of the British Ecological Society, released in 1945 as a 'Memorandum on wild life conservation and ecological research from the national standpoint'. It argued for an ecological research council, with independence and powers similar to those of the other research councils, and composed of at least one institute of terrestrial ecology, as well as a biological service responsible for management of nature reserves and advising on the conservation of plants and animals.[52]

Ecologists expended considerable energy preparing their proposal for an ecological research council. Tansley even wrote a small book explaining to a wider audience the merits of nature conservation.[53] They expected that such a council would fulfil their long-standing aim of a secure institution for ecology, while demonstrating its relevance to practical problems. But during

[49] The Conservancy's slow progress during the 1950s and 1960s in developing practical techniques of reserve management, and training staff to apply them, confirmed that the ecologists' original, confident presentation of their ability to manage nature reserves had not been grounded in extensive actual experience, or full understanding of its practical implications. For the present account, however, it is more significant that this presentation proved, in the 1940s, to be persuasive.

[50] British Ecological Society, *Nature conservation and nature reserves*, Cambridge, 1943, p. 2.

[51] British Ecological Society, op. cit. (50), p. 32.

[52] Sheail, op. cit. (2), p. 140.

[53] Tansley, op. cit. (13).

the 1940s they also devoted much attention to the apparently unrelated issue of the autonomy of science. Many ecologists, led by Tansley, believed strongly that scientists had to be free to set their own research priorities. To understand the role of this belief in the events leading up to the Nature Conservancy, it is necessary to examine the contemporaneous debate over the relationship between science, government and society.

Freedom in science

Economic depression, World War Two, and the condition of science under fascist and Soviet governments provoked debate in Britain over the place of science in society. During the 1930s several scientists and historians argued that it is social and economic conditions, not curiosity, that drives science, and that the power of science to transform society must be recognized and controlled. In 1931 Soviet historians visiting London insisted that the division between pure science and society was an illusion fostered by class structures, and that science should be planned in order to be of maximum benefits for society.[54] In 1939 John Desmond Bernal argued in *The social function of science* that only through rational organization and planning, as in the Soviet Union, could science avoid waste and inefficiency, and provide genuine social and economic benefits.[55] Such views had by then become widely accepted, even by leading scientific organizations, including the Association of Scientific Workers and the British Association for the Advancement of Science. Between 1932 and 1945 this 'Social Relations of Science Movement' exerted great influence within the British scientific community.[56]

But for one Oxford zoologist, Bernal's book provided both a stimulus, and a target for counterattack. In November 1940 John Baker sent a statement to 49 British scientists, expressing concern that 'an influential group of scientists' believed that researchers should be directed to study utilitarian problems, and to work within organized groups. According to Baker these views endangered both pure science, as an activity pursued as an end in itself, and individual research.[57] Many scientists answered his call, and together they formed the Society for Freedom in Science (SFS), to unite opposition to the direction of science towards social priorities. They argued that even in

[54] N. I. Bukharin et al., *Science at the crossroads: Papers presented to the International Congress of the History of Science and Technology, London, 29 June–3 July 1931, by the delegates of the U.S.S.R.*, London, 1931.

[55] J. D. Bernal, *The social function of science*, London, 1939.

[56] W. McGucken, *Scientists, society, and state: The Social Relations of Science movement in Great Britain, 1931–1947*, 1984; G. Werskey, *The visible college*, London, 1978.

[57] J. R. Baker, circular letter, November 2, 1940; reproduced in Society for Freedom in Science, 'History of the foundation of the Society for Freedom in Science', in *The Society for Freedom in Science: Its origin, objects and constitution*, London, 3rd edition, 1953, unpaginated.

the absence of immediate practical benefits, science, like literature and art, was worthwhile in itself; that discovery depends on unrestricted curiosity; that no planner could predict which areas of science would produce new discoveries; and therefore, that efforts to plan or control science would stifle creativity.

Tansley was very active in the SFS, sharing its leadership with Baker and Michael Polanyi. Tansley helped draft the society's statement of beliefs, was on its executive committee, served as its first acting chairman, and later, as vice-president. His 1942 Herbert Spencer Lecture at Oxford was an eloquent statement of the SFS's ideals.[58] He remained active in the SFS until shortly before his death in 1955. Baker wrote then of Tansley:

> He was always a good counsellor, urging restraint when wild schemes would have damaged our cause, energetically supporting strong action whenever it could be successful. He was an eminently wise man, thoughtful, full of experience, moderate, optimistic. . . . Whenever it was suggested that opinion was swinging in our favour and that we might relax our efforts, or even wind up the Society, he objected vigorously.[59]

While the social relations of science movement provided the immediate impetus to the SFS, Tansley had long considered freedom to be essential to science. In his 1927 lecture at Oxford, he reminded his audience 'that an essential condition of successful development is freedom from utilitarian *compulsion*'.[60] Further, 'good effective research cannot be done to order, nor can the spirit of investigation be manufactured artificially. That spirit depends absolutely on the driving force of natural curiosity.'[61]

Tansley, however, was not the only ecologist in the SFS. Over one quarter of the society's members, 17 out of 61, were ecologists.[62] This must reflect, in part, Tansley's selection of members from among his own colleagues. Nevertheless, the large number of ecologists in the SFS, disproportionate to their significance within British science, requires, I believe, a stronger explanation. It is particularly necessary because one might expect ecologists to have had little cause to be concerned about utilitarian compulsion. Because their research was considered to have limited practical significance, and received little funding, the government would be less likely to seek to direct it. In a sense, indifference could provide ecology with some security against interference.

But if indifference could ensure researchers' independence, it could also guarantee their poverty. In the freedom in science movement, Tansley was as

[58] A. G. Tansley, 'The values of science to humanity', *Nature*, (1942) 150, pp. 104–110.

[59] J. R. Baker, *Sir Arthur Tansley, 1871–1955*, London, 1955.

[60] Tansley, op. cit. (4), p. 4. Emphasis in original.

[61] Tansley, op. cit. (4), p. 17.

[62] J. R. Baker, 'List of members of the Society for Freedom in Science on 12 July 1941', typescript.

concerned about funding as about freedom; indeed, the two were inseparable:

> Pure science is necessarily the basis of the whole scientific fabric, because the original discoveries on which it is founded are unforeseeable, and cannot be provided to order or planned in advance; and it is for this reason that pure science, under the conditions of modern life, must . . . have money at its disposal which can be used by the people who spend their lives in making investigations in accordance with their own interests. They must have their freedom and they must have money; and that money must not be controlled by any outside authority which is activated by motives of practical value.[63]

Tansley and his colleagues were, as they had been for many years, anxious to arrange secure support for ecology. Bernal and the social relations of science movement, however, threatened progress towards this goal. While ecologists had argued strongly that their research had practical benefits, their lack of success in obtaining support indicated that few within government had been convinced. If, then, science was to be directed towards immediate economic or social concerns, ecology would inevitably be neglected. This, of course, had been the case before the war, when government support for forestry and agriculture research had provided only meagre opportunities for ecology.

The danger that the social relations of science movement posed to ecology heightened as ecologists tied their fortunes to nature conservation, and its explicitly non-economic objective of the protection of the British natural heritage. Tansley wrote in 1945: 'If we keep and manage [natural areas] properly we can hand down to our descendants a Britain which is not only economically prosperous, but retains something of its distinctive natural historic character, continues to delight our sense of beauty, and affords rich material for the increase of knowledge.'[64] The British landscape, Tansley insisted, had a value that transcended economic utility. And, therefore, so did ecology, because it could provide the scientific basis for the care of this landscape. Baker made the same point, in describing Tansley's massive compendium, *The British islands and their vegetation*, as of no immediate practical application, but of great cultural value.[65] If support for science was determined on the basis of immediate social and economic priorities, these more intangible benefits of nature conservation and related ecological research would perhaps be ignored.

Besides advocating autonomy and funding for science, Tansley also ensured that the principles of the Society for Freedom in Science would be

[63] A. G. Tansley, '[Discussion on fundamental research in relation to the community]', *Advancement of science*, (1946) 3, pp. 294–295.

[64] Tansley, op. cit. (13), p. 67.

[65] Tansley, op. cit. (5).

compatible with ecology. Baker had originally distinguished between organized groups working on projects of economic benefit, from individual scientists studying pure science. Pure science, he believed, was best performed by individuals. But when Tansley, Polanyi and Baker established the principles of the society, they concluded that researchers should be free 'to work separately or in collaboration as they may prefer. Controlled team-work, essential for some problems, is out of place in others.'[66] Tansley himself noted that some problems in pure science could be best approached through teamwork.[67] He was evidently responsible (with Polanyi) for the Society for Freedom in Science not being opposed to organized activity in science. Tansley may have taken this position because a reliance on individual research could hinder ecosystem study. Because most ecologists were specialists, an individual could not do a complete study of an ecosystem. It required, rather, 'collaboration between an animal and a plant ecologist, or by a team . . . such collaboration, to be really successful, must be genuinely joint work in which each partner fully follows and understands the contributions of the others. Only thus can a unified picture of the whole be built up.'[68]

These concerns of Tansley and other ecologists suggest a need to reinterpret the controversy over the freedom and planning in science. William McGucken has described it in terms of the opposing views of the Social Relations of Science Movement and the Society for Freedom in Science, and argued that the controversy ended when the government reaffirmed the autonomy of the scientific community.[69] But for ecologists autonomy was not the sole issue. They were also concerned about the demand of the Social Relations of Science movement that science be directed towards social and economic priorities, and the threat that this demand posed to support for ecological research. This controversy, therefore, was the consequence, in part, of economic and disciplinary concerns, and not simply differences in ideological commitment.

Tansley led ecologists in efforts both to defend scientific autonomy, and to gain an institution for ecological research. As the most eminent British ecologist, a leader of the Society for Freedom in Science, and drafter of the British Ecological Society's report on nature conservation, he used the issue of nature reserves and their management to demonstrate the practical implications of scientific autonomy. Other ecologists, and particularly Elton, shared with Tansley a conviction that ecological research must be

[66] Society for Freedom in Science, op. cit. (57). [67] Tansley, op. cit. (58), p. 110.

[68] A. G. Tansley, 'What is ecology ?', *Biological journal of the Linnean Society*, (1987) **32**, p. 10; originally published in 1951 as a pamphlet by the Council for the Promotion of Field Studies.

[69] W. McGucken, 'On freedom and planning in science: The Society for Freedom in Science, 1940–46', *Minerva*, (1978) **16**, pp. 42–72.

independent.[70] Led by Tansley, they established a detailed argument for a new, autonomous institution for ecological research.

Ecologists' insistence on this new institution, independent of the Ministry of Town and Country Planning, put them at odds with the Nature Reserves Investigation Committee, and also with John Dower, who was then shaping government policy on national parks. In 1945 Dower recommended a Wild Life Conservation Council to provide scientific advice on the management of nature reserves and national parks. This council would be within a National Parks Commission, which would report to the Minister of Town and Country Planning.[71] Dower's strong opposition to the ecologists' proposal that nature reserves be independent of the National Parks Commission reflected the lack of consensus in 1945 on the appropriate organization for reserves and ecological research. Besides Dower and other Ministry of Town and Country Planning officials, the Agricultural Research Council and the Treasury also opposed an independent institution for nature conservation.

That year, the Minister of Town and Country Planning, refused permission by the cabinet to establish a National Parks Commission, instead appointed a committee under Sir Arthur Hobhouse to review the issue. The Wild Life Conservation Special Committee was also established, because the minister wanted 'to have the proposals of the Dower Report on Nature Reserves and Wild Life Conservation examined by a small group of experts', which could then recommend an appropriate response.[72] The committee, chaired by Julian Huxley, and after May 1946 by Tansley, and composed primarily of leading ecologists selected by Huxley, was charged with determining the organization that should be responsible for nature conservation. Within the committee Tansley was far from alone in his beliefs regarding ecological research, its organization, and its relevance to conservation. Diver had prepared large parts of the memoranda of the Nature Reserves Investigation Committee, while Elton had served on the British Ecological Society's committee on nature conservation.[73] His recent Bureau of Animal Population work on rodent control had also convinced him that ecological research would be best fostered by an independent research council. Another prominent member was the ornithologist, Max Nicholson. In his first book, *Birds in England* (1926), he had argued that policies of bird protection should adhere to an ecological perspective. Since then, he had been involved

[70] Elton had also been convinced of the need for autonomy by his experience in the Bureau of Animal Population that the greatest progress is made by individuals free to engage in their own innovative research; Sheail, op. cit. (9), pp. 74–75.

[71] J. Dower, *National Parks in England and Wales*, London, 1945.

[72] J. B. Bowers to H. Smith, 21 August 1945; in National Parks Committee, 'Correspondence with the Secretary of the NRIC (Dr. Herbert Smith)', 1945–46, PRO HLG 93-39.

[73] J. Sheail, 'Nature Reserves, National Parks, and post-war reconstruction, in Britain,' *Environmental conservation*, (1984) 11 (1), pp. 29–34; Sheail, op. cit. (9), pp. 71–72.

in the Oxford Bird Census, the Edward Grey Institute of Field Ornithology, and the British Trust for Ornithology. Each reflected his ambition to develop a more systematic organization of the efforts of amateur ornithologists.[74] While appointed to the committee to provide an ornithological perspective, Nicholson would also, as I will note, make an essential contribution to the practical success of the committee. Each of these ecologists made important contributions to the work of the Wild Life Conservation Special Committee. The committee itself, although officially reporting to the Hobhouse Committee, functioned virtually independently.

The appointment of ecologists to this expert committee indicates that ecologists had taken the place formerly occupied by amateur naturalists as the government's advisors on nature reserves. This, and the bypassing of the NRIC, which had been established three years before to provide just such advice, was at first not well received. When J. B. Bowers, secretary of the Hobhouse committee, requested from Herbert Smith the NRIC's materials, Smith answered curtly:

> I may remind you that the . . . [NRIC] were appointed . . . to advise government departments on matters relating to nature reserves and natural history generally. It would appear as if your ministry had ignored, or at least overlooked, the existence of the committee.[75]

Throughout the two years of its work, the Wildlife Committee sought to reconcile two objectives: the preference of Tansley and other ecologists for an independent ecological research council under the Lord President, and their official duty of advising the Ministry of Town and Country Planning. In a first attempt at a compromise, they proposed that a wildlife conservation advisory body or research council, reporting to the Lord President, be responsible for research, with reserve management to be the task of a Wild Life Conservation Administrative Body within a National Parks and Wild Life Conservation Authority, reporting to the Minister of Town and Country Planning.[76] The committee presented this as an interim proposal in November 1945, with the proviso that it would require close ties between research and management: 'If the primary purpose of [nature reserves] is to provide field laboratories for scientific research, and if Wild Life Conservation policy is to be framed on the best available scientific advice, it will be essential that the Wildlife Conservation Commission should be closely

[74] E. M. Nicholson, *Birds in England: An account of the state of our bird life and a criticism of bird protection*, London, 1926. D. Allen discusses Nicholson's ornithological work in *The naturalist in Britain: A social history*, London, 1976, pp. 252–258.

[75] J. B. Bowers to H. Smith, 21 August 1945; H. Smith to J. B. Bowers, 24 August 1945, in PRO HLG 93-39.

[76] Minutes of first meeting of Wild Life Conservation Special Committee, 6 September 1945, in PRO HLG 93-48.

integrated with a National Research organization that is or may be set up in the Biological or Ecological field.'[77]

The committee, however, quickly came to consider this compromise to be unacceptable. Placing reserve management within one government body, and research within another, would make their co-ordination much more difficult. And as the committee explored the implications of conservation the necessity for such co-ordination became ever more evident.[78] Reserves could be fully useful for research only if scientists were involved in all aspects of their management. This was especially important because of the growing interest among ecologists in experimental research. In February 1946 one committee member, John S. L. Gilmour, described to his colleagues the opportunities that reserves presented for botanical experiments, such as studies on the growth of plants outside their normal range, or the effects of exclosure of rabbits and other herbivores from plant communities.[79] Two months later, Nicholson presented the case for experimental bird reserves, arguing that they would enable both studies of the effects of changes in environment on birds, and training of the ecologists that would be needed by future nature conservation and research bodies.[80] In providing these contributions to ecological research, reserves were, as Huxley expressed it, the 'essential laboratories' of the nature conservation body.[81] It was also appropriate that they be administered separately from national parks because while parks were to be for public use, access to reserves would be restricted, to protect the flora and fauna and prevent interference in research.

By February 1946 the committee had, accordingly, decided not to compromise on its preference for an independent body for reserves and ecological research. Just as the British Ecological Society had two years before, it chose to recommend that both management and research within nature reserves be recognized as a matter of science, not planning, and that it be the task of a single body reporting to the Lord President, not the Ministry of Town and Country Planning. A biological service would manage the reserves, conduct related research, and respond to requests from other agencies for advice on conservation. In addition, one or more 'terrestrial research institutes' would be dedicated to 'fundamental and generalized' ecological

[77] Wild Life Conservation Special Committee, 'A Suggested Pattern for the Administration of Wild Life Conservation and Nature Reserves in Relation to National Parks and National Research', 9 November 1945, 2, in PRO HLG 93-48.

[78] Wild Life Conservation Special Committee, 'Draft Letter in reply to Mr. Flett's letter to Mr. Bowers', 27 November 1945, in PRO HLG 93-48.

[79] J. S. L. Gilmour, 'The Use of Nature Reserves for Botanical Experiments', 28 February 1946, in PRO HLG 93-48.

[80] Wild Life Conservation Special Committee, 'The case for experimental bird reserves', 8 April 1946, in PRO HLG 93-29.

[81] Wild Life Conservation Special Committee, 'Note on a joint meeting with the National Parks Committee, February 20, 1946', in PRO HLG 93-48.

research. Entirely separate from the biological service, these institutes would ensure the complete independence of the researchers.[82]

This decision to depart from the terms of its appointment by the Minister of Town and Country Planning, and recommend a scientific body under the Lord President, reflected the committee's perception of what had become politically possible. Immediately after the war, science enjoyed unprecedented prestige. The scientific community and government now agreed that science, having helped assure victory, should be permitted to set its own priorities. The Lord President (Herbert Morrison) told scientists in 1946 that 'Science and scientists must have an independent life and an independent existence of their own', although government could guide the application of new discoveries.[83] The committee therefore had a greater likelihood of having its proposals accepted, if conservation and ecological research were defined as a strictly scientific matter.

The Wild Life Committee had even been publicly advised to link conservation with science. An editorial in *Nature* had suggested that 'the word "nature" has come to be associated . . . with a somewhat childish or eccentric form of botanizing, bird-loving and butterfly-hunting. If the more fashionable word "science" could replace "nature" on the subcommittees' notepaper they would probably find the task of co-operating with local authorities less uphill work.'[84] Ecologists already knew the value of such a strategy. Elton had told the Committee that the political difficulties in acquiring nature reserves 'might be mitigated by calling them "scientific nature reserves", thus using the present momentous prestige of "the scientist", rather than the slightly shabby reputation of "the naturalist" . . .'[85] The committee evidently recognized that its recommendations would carry more weight if cast in terms of science. But if an independent nature conservation body was politically realistic, to argue for a full research council, similar to the Agricultural Research Council and the Medical Research Council, was not. Committee members felt it 'inadvisable to ask for everything at once, and that they were more likely to succeed with a modest demand. They hoped, however, that both grant-giving powers and full status as a Research Council would in the long run accrue to the proposed advisory board'.[86]

A further advantage of defining nature conservation as a scientific task, and therefore necessarily independent of a National Parks Commission, was that this proposed Commission had begun to encounter political opposition.

[82] Wild Life Conservation Special Committee, 'Notes from meeting of February 20 & 21, 1946', in PRO HLG 93-48.

[83] H. Morrison, 'Planned research', *Advancement of science*, (1946) 3, p. 296.

[84] 'Nature preservation in England', *Nature*, (1946) 157, p. 278.

[85] C. Elton, 'How a system of Nature Reserves might be run', in PRO HLG 93-48.

[86] Wild Life Conservation Special Committee, 'Note on meeting with the Scientific Advisory Committee of the Cabinet', March 1, 1946, in PRO HLG 93-48.

Even as the Hobhouse and Huxley committees conducted their inquiries, a plan was being drafted to give county councils broad authority over land use and development. This resulted in the Town and Country Planning Bill, which received royal assent in August 1947. With their newly acquired authority over planning, these councils now viewed the proposed National Parks Commission as a potential rival. Their opposition to the Commission obliged Lewis Silkin, the Minister of Town and Country Planning, to limit its role within the land use planning process to providing advice to the councils.[87] By proposing that nature conservation be independent of the National Parks Commission, therefore, the ecologists could lessen the risk that opposition to national parks would extend to nature reserves.

While preparing its report, the Wildlife Committee took steps to ensure its acceptance by the government, meeting with the Cabinet's Scientific Advisory Committee, in part to gain its support for their recommendations. They especially wanted the Advisory Committee to agree that conservation be the responsibility of the Lord President, because the government's acceptance of this recommendation was by no means inevitable. Proposing new responsibilities for the Lord President, when the committee had been created by another minister, was obviously a situation of some 'delicacy'.[88] In addition, Dower, still influential within the Ministry of Town and Country Planning, remained entirely opposed to the separation of national parks from nature reserves. To counter this, the committee hoped for support from the Advisory Committee 'in order that the National Parks Committee might be satisfied that the Wild Life Committee had a satisfactory alternative to the Dower scheme'.[89]

The Advisory Committee had already begun considering the nature reserves issue. Prompted by the British Ecological Society report and one from the Royal Society, it invited John Fryer, secretary of the Agricultural Research Council and a member of the Advisory Committee, to summarize the issues involved. According to John Sheail, Fryer's note formed the basis for a report by the Advisory Committee to the Lord President, advocating a system of nature reserves, one or more institutes of terrestrial ecology, and a Service, composed of scientific officers, who would manage the reserves and provide advice on conservation.[90] The Wildlife Committee, however, by meeting with the Advisory Committee, and sending it at least one memorandum, may have also helped convince it to make such a proposal. This is also suggested by the Advisory Committee's adoption of the Wild Life Committee's advice to recommend that the conservation body be associated with the Agricultural Research Council. It is at least clear that the Advisory Commit-

[87] Sheail, op. cit. (30), pp. 200–206.

[88] Wild Life Conservation Special Committee, op. cit. (78).

[89] Wild Life Conservation Special Committee, 'Notes from meeting of March 8, 1946', in PRO HLG 93-48.

[90] Sheail, op. cit. (30), p. 211.

tee's proposal, being very similar to the proposal prepared by the Wild Life Committee, gave it considerable credibility.

In July 1947 the Wildlife Committee presented its report. A detailed rationale for nature reserves and conservation areas, a biological service to manage these reserves, conduct research, and provide advice, and four terrestrial research institutes for fundamental studies, the report summarized the deliberations of the committee over the previous two years. Two general principles formed the basis of its recommendations. The first was that conservation was pre-eminently a matter of science. While the conservation of nature was, as the committee noted, a very difficult problem in a crowded, highly industrialized nation, they confidently asserted that 'The solution of the problem can be expressed . . . in three words: Research and Experiment.'[91]

The second principle was that the proposed Nature Conservation Board must be independent. The committee explicitly recommended that it not be attached to a National Parks authority, the Ministry of Town and Country Planning, or any other government department. The preferred option was for the board to be a research council under the Lord President. However, the committee, aware, as I have noted, of opposition to creation of another council, and determined to be politically realistic, recommended that the board be attached to the Agricultural Research Council. The Committee did not anticipate interference by the ARC in the board's activities. Within the board, the terrestrial research institutes would also be independent.

A striking aspect of the Wildlife Committee's recommendations was that they could overcome difficulties that had hindered ecologists and their research. Most obviously, a biological service and research institutes could employ many ecologists. In addition, an independent Nature Conservation Board would enable ecologists to avoid the problems encountered in obtaining support from bodies, such as the Agricultural Research Council or the Forestry Commission, that had concerns not always compatible with ecological research. Research sites protected from disturbance or destruction were equally important. Nature reserves provided many such sites, as well as examples of every type of plant and animal community of interest to ecologists.

The Wild Life Committee's recommendations also solved problems that the academic world posed for ecologists. From Tansley's complaint in 1914 that ecology 'occupies a very small, in many cases almost a negligible, place in our University curricula', to Cambridge ecologist Harry Godwin's recollection of being told by 'more than one leading botanist' in the 1920s that ecology had no future, to Elton's difficulties in obtaining support for his Bureau of Animal Population at Oxford in the 1930s, resistance from

[91] Wild Life Conservation Special Committee, *Conservation of nature in England and Wales*, London, 1947, p. 3.

universities and other biologists was for many ecologists a familiar story.[92] Another problem was the difficulty in arranging co-operative studies by scientists from several disciplines and university departments. Tansley and other ecologists considered such co-operation to be essential to future ecosystem studies. The academic division between botany and zoology, however, reinforced, as Elton complained, the 'split between the plant and animal ecologists' outlook', hindering an integrated perspective on ecosystems.[93] Finally, academic duties presented a problem. Tansley noted that 'a minor but quite real academic drawback is the frequent necessity of spending long working periods in the field, which does not fit in well with existing academic regulations'.[94]

Each of these problems facing ecologists could be solved by the proposal of the Wild Life Committee for independent, multi-disciplinary research institutions, working within protected and expertly managed nature reserves. If acted upon, this proposal would make possible the innovations proposed by leading ecologists: in techniques, including a greater use of experimentation and intensive survey; in new concepts, especially the ecosystem concept; and in the replacement of descriptive studies by a focus on the causal relations behind observed patterns in nature.

The Wild Life Committee's proposals were strikingly similar to Tansley's ideas on the organization of science. Both Tansley and the committee insisted that pure research and its application could profit from interaction, but only if appointment of researchers, selection of research projects, and allocation of funds remained in scientific hands. Under Tansley's chairmanship, the report of the Wild Life Committee became a practical expression of his views on the organization of science. The committee's members were certainly prepared to following his views: the majority were ecologists, well aware of the needs of ecology and nature conservation, and respecting Tansley as the most distinguished member of their discipline. They also shared his views on scientific autonomy; of the ten committee members, six were also members of the Society for Freedom in Science.

In January 1948 Morrison, as Lord President, accepted responsibility for acting on the recommendations of the Wild Life Committee. On April 29 he told parliament that the government had accepted its recommendations for a Nature Conservation Board and Biological Service, to be associated with the Agricultural Research Council. After, as Nicholson described it, these plans were 'steered gently through mechanisms which would normally have quietly buried them', in February 1949 Morrison announced that the Conservation Board and Biological Service would be united within a single body,

[92] A. G. Tansley, 'Presidential address', *Journal of ecology*, (1914) 2, p. 195; H. Godwin, *Cambridge & Clare*, Cambridge, 1985, p. 150.

[93] Elton, op. cit. (17), p. 2.

[94] Tansley, op. cit. (25), p. 530.

to be called the Nature Conservancy.[95] Accorded research, advisory and conservation duties by Royal Charter in March 1949, and statutory power to designate nature reserves by the National Parks and Access to the Countryside Bill of 1949, the Conservancy became an independent research council similar to the Agricultural Research Council and the Medical Research Council, but holding as well powers of land tenure and control.[96]

Several factors abetted the government's rapid adoption of the Wild Life Committee's recommendations. As I have noted, they were considered separately from the proposals for national parks, and they therefore avoided opposition from local governments anxious to protect their own planning authority. Nicholson also played a central role in the adoption of the Committee's recommendations. As both a member of the Wild Life Committee, and head of the Office of the Lord President, he contributed both to the report, and to the formulation of the government's response to it. Perhaps most important, however, was the prestige of post-war science, and the interest in applying science to government responsibilities. The post-war Labour government increased support for civil science from £6.5 million in 1945 to £30 million in 1950. It also arranged new mechanisms, such as the Advisory Council on Scientific Policy, to provide it with scientific advice.[97] The importance the government attached to such advice was reflected in the Lord President's explanation to Cabinet of the need for access to expertise on nature conservation:

> the Government is constantly taking action liable permanently to affect the fauna, flora and even the geography of the country without having at its disposal any channel of authoritative scientific advice about the probable results of its action, such as is available in all other fields of natural science.[98]

Because they had stressed that conservation was a scientific matter, to be approached through 'research and experiment', the new authority of science extended to the Wild Life Committee, and the advice they offered on conservation.

Conclusion

The Nature Conservancy owed itself to a remarkable convergence of circumstances. Before the Second World War, neither the government nor universities, in spite of ecologists' efforts, were willing to provide them with

[95] M. Nicholson, *The new environmental age*, Cambridge, 1987, p. 93.

[96] *Reports of the Nature Conservancy for the period up to 30th September, 1952*, London, 1953, pp.1–3; Sheail, op. cit. (29), pp. 215–216.

[97] H. Morrison, *Government and Parliament: A survey from the inside*, London, 1954, pp. 329–332.

[98] Quoted in Sheail, op. cit. (2), p. 143.

the support they considered necessary. In the early 1940s, however, a new opportunity for support emerged, in the nature reserves movement and government interest in land use planning. Ecologists successfully argued that nature reserves, and effective land use and management generally, required a commitment to nature conservation and a new institution for ecological research. Committed to the principle of scientific autonomy, ecologists, led by Tansley, insisted that this institution be independent. In obtaining government support for their research, ecologists were aided by the post-war prestige of science, and the interest in scientific solutions to problems facing British society. It is evident, therefore, that political, economic and ideological factors were instrumental in the ecologists' campaign for institutional security, and in the shaping of the eventual Nature Conservancy.

But interwoven in these events were ecologists' conceptions of their own identity: as scientists with a distinctive perspective on the natural environment, combining experimentation and survey in ways unique to ecology. This perspective was itself guided by a view of nature as complex communities of plants and animals interacting with each other and their physical environment, that is, as ecosystems. By the early 1940s, several ecologists had realized that ecosystem study, and new forms of ecological research generally, required both protected areas of land, and an institution unencumbered by demands for immediate practical relevance. This realization helped motivate ecologists in their advocacy of nature conservation and freedom in science. In effect, the conceptual development of their discipline shaped ecologists' efforts to obtain institutional support, even as their success in obtaining this support would eventually shape post-war British ecology. This episode, therefore, provides a demonstration of the interpenetration of cognitive and social factors, of ideas and institutions, that is an essential feature of twentieth century science.

— 6 —

Of desert and watershed: The rise of ecological consciousness in Victoria, Australia

LIBBY ROBIN

Conservation is an overworked word. It has carried myriad meanings over the years, from its beginnings as a technical term in water resource management to its present-day green flavour. Whether it has been used to describe a utilitarian notion of wise use without waste or a green environmentalist earth-centred management rationale, it has always carried *positive* overtones. Diametrically opposed views have come forward under the banner of conservation, if our past century's history is considered. Yet whatever the so-called conservationists have been saying has in some sense occupied the high moral ground. Whether its virtue was progress through efficiency or ecological understanding of the earth its virtue was never in doubt.[1]

Because conservation is, by definition, virtuous work, its proponents feel passionately about it. Conservation may be defined rationally, but the motives for practising it are complex, moral and informed as much by cultural values as by science.[2] However, there is and has always been a strong link between conservation and science, and many conservation managers are trained scientists. This paper explores some of the interrelationships between science and cultural values which create tensions in conservation.

The focus of this paper is a major conservation dispute in the state of

I wish to acknowledge the assistance of all those who shared their memories of the Little Desert dispute with me. The paper also benefited from those who read and commented helpfully on earlier drafts, including Malcolm Calder, Tom Griffiths, Rod Home and three anonymous referees.

[1] For an account of the first conservationists in the United States see Samuel P. Hays, *Conservation and the gospel of efficiency*, Cambridge, MA, 1959 (esp. p. 5). The earliest use of the term 'conservation' in Victoria was also by water resource managers. In 1881 the Water Conservation Act was passed to establish irrigation trusts in rural Victoria. J. E. Lovelock, *Gaia: A new look at life on earth*, Oxford, 1979; also Edward Goldsmith, 'Gaia: Some implications for theoretical ecology', *The ecologist*, (1988) 18 (2), pp. 64–74.

[2] Richard Mabey, in *The common ground*, London, 1980, argued that the nostalgic appreciation of the countryside should be regarded as a valid factor in current conservation assessments. He commented that a personal psychological need for pleasing landscapes and 'natural nooks' should not be overlooked by planning authorities just because it is difficult to quantify.

Victoria, Australia, which came to a head in 1969. The dispute concerned a proposal for an agricultural settlement scheme in Victoria's Little Desert, an area which had not been previously developed. The developers were opposed by a broad cross section of the Victorian public, from those who saw the area as ideal for a National Park, to those who were concerned only that the agricultural scheme itself was not economically viable. The study has benefited from the availability of oral sources. I have met with or interviewed formally over forty of the Little Desert protagonists. Most would have described themselves as conservationists. Their ideas about conservation were diverse but they were unanimous in the view that this dispute was critical to conservation in Victoria. Their shared rhetoric reflected their unanimity. The Little Desert affair was described as a 'watershed' by almost every conservation activist, irrespective of age, political views or definition of conservation.[3] Oral sources have been balanced with a plethora of contemporaneous documents, the most comprehensive of which was the transcript of evidence given at a lengthy Parliamentary inquiry into the dispute.[4]

The Little Desert dispute was a major media event at the time. It was also the catalyst for two important bureaucratic innovations for the state of Victoria which are still important to conservation over two decades later. The first was a private sector initiative: the formation of a new conservation umbrella group, the Conservation Council of Victoria (CCV), which gave diverse private conservation organizations an official voice with government. The second was a government initiative: the establishment of a new statutory advisory body to government, the Land Conservation Council (LCC). The LCC was established by act of Parliament to review the use of all public (Crown) land in Victoria. Public land at this time (1970) constituted over a third of the state (about 88,000 sq. km., more than the total area of Scotland).[5]

[3] Oral source material for the project is based primarily on typescripts of taped interviews undertaken by the author between 1989 and 1991. Copies of the typescripts are in the possession of the author, the informant and the University of Melbourne (Botany School). The term 'watershed' occurs in many of the transcripts, and also occurred in informal discussion about the Little Desert dispute. It was also used recently on ABC radio (interview by Doug Aiton, 3LO, 22.VIII.91) by the former state premier, Sir Rupert (Dick) Hamer, referring to 1970 as 'a watershed in our culture'. He specifically mentioned a rising culture which valued the environment and concern for quality of life. With the hindsight of 20 years, he saw these as replacing an earlier culture which made heroes of the pioneers, those who 'battled the elements'.

[4] Little Desert Settlement Committee, *Report upon the proposal to open the Little Desert to settlement (together with Appendices)*, Melbourne, 17 March 1970; Little Desert Settlement Committee, 'Transcript of evidence', unpublished typescript available through the Clerk of Legislative Council Papers, Spring Street, Melbourne.

[5] *Land Conservation Act* (No. 8008) 24 November 1970; LCC Statewide assessment of public land use, 1988, p. 8; *The international geographic encyclopedia and atlas* (area of Scotland approx. 79,000 sq. km.).

Land management in Australia is controlled by state governments. Public or 'Crown' land is

In 1969 the conservationists gloried not in their monuments to posterity, the CCV and the LCC, but in a new-found political power. As Falk has argued in a different context, the success of loosely linked local organizations in opposing centralized government (or non-government) decisions can stimulate further activity: confidence is gained from example.[6] The conservation victory was the unseating of the Minister for Lands, Sir William McDonald, who was responsible for an agricultural development proposal put forward for the Little Desert. The watershed concept was at the time used metaphorically in its (American) sense of catchment zone. It was about the confluence of ideas, ideas running as rivulets and streams from different points toward the valley floor, the political arena, where their total volume was sufficient to topple a cabinet minister and close friend of Sir Henry Bolte, the Premier of Victoria (1955–1972). The watershed concept also has appeal for those who come to the story over 20 years later, but in the sense of a dividing line between metaphorical river systems: the Little Desert dispute was the last great campaign of many of its protagonists. Soon after, there was a divide in conservation thinking, a parting of the ways. The unity of the 1960s, at its peak in the Little Desert dispute, was special but short-lived. New alliances and new enmities characterize the 1990s conservation lobby. However, no politician doubts its power.

The Little Desert is situated in Victoria's Wimmera region (see Fig. 1). It extends for about 120,000 hectares from the South Australian border eastwards toward the pastoral centre of Horsham. It is flat country interrupted by distinctive parallel ridges believed to have resulted from the westerly recession of the inland sea millions of years ago.[7] The ridges provide low vantage points from which to view the heathland vegetation which seems to extend like a blue-green sea. Early travellers sometimes carried this metaphor further, describing such country as 'Bay of Biscay' country, presumably a reference to the rough passage.[8] Soils in the Little Desert vary from shallow sands in which ironstone pebbles have formed over a heavy clay base, to deep sands which have been podzolized (leached of nutrients through a process of stratification). As the dense, prickly heathland vegetation suggests, the soils lack minerals critical to agriculture. It was probably

therefore the responsibility of the state, not the Commonwealth government. Similarly, resources such as forests, water, national parks, etc. are organized at a state level. Thus the conservationists in this story focused their political activity on the Victorian, rather than the Federal government.

[6] Jim Falk, *Global fission: The battle over nuclear power*, Melbourne, 1982, p. 290.

[7] Biological and geological details of the Little Desert are summarized in Peter Attiwill's statement to the Little Desert Settlement Committee, 'Transcript of evidence', 26 November 1969, op. cit. (4), especially p. 72.

[8] Tom Griffiths, ed., *The life and adventures of Edward Snell*, Sydney, 1988, entry for 24 February 1852, p. 276.



FIG. 1: Map showing the Little Desert and other relevant places. (Map by Peter Sloan)

called a desert because of its lack of agricultural potential. Its desert sands have been thickly vegetated for longer than European memory.

Between 1963 and early 1967, the Australian Mutual Provident (AMP) Society considered subdivision in the Little Desert for agricultural development, but eventually abandoned the scheme for economic reasons.[9] Within months of the AMP's final decision, a new Minister of Lands, Sir William McDonald, was appointed. He was already a knight because of his long service to the Victorian Parliament as Speaker (since 1955). He was also a farmer, with properties on both sides of the Victorian/South Australian border, one of which abutted the Little Desert. Premier Bolte had wanted 'Jack' McDonald in cabinet, and gave him his choice of portfolios. Sir William chose the Lands portfolio because of the opportunity it offered for land development, a cause dear to his heart (see Fig. 2).[10]

Less than two months after his appointment, McDonald addressed a public meeting in Kaniva on the issue of developing the Little Desert, which was south and south-east of that remote town.[11] McDonald felt he knew the area

[9] Little Desert Settlement Committee, *Report*, op. cit. (4) pp. 5–9.

[10] Sir William McDonald, transcript of interview, 12.XI.90, p. 8.

[11] Departmental report of public meeting at Kaniva on 28 June 1967, in LDSC, *Report*, op. cit. (4), Appendix B, pp. 14–15.



FIG. 2: Sir William McDonald (arm outstretched) extols the virtues of developing the Little Desert to his parliamentary colleagues. (Photo from the Melbourne *Age*)

well, as a local. He also knew the deep sands country to its west, at Bangham on the other side of the South Australian border. The deep sands of Bangham had been developed in the late 1940s and 1950s through research into soil nutrients and trace elements undertaken by CSIRO, the Waite Agricultural Research Institute and the South Australian Department of Agriculture. The South Australian experience testified that even deep sands could grow lucerne, a recommended pasture crop, given enough agricultural lime and superphosphate (see Fig. 3). Copper, zinc, molybdenum and cobalt also needed to be added to ensure the health of stock.[12] The local people of Kaniva were initially enthusiastic about the proposal, hoping that an influx of new settlers might make it possible to maintain the local high school and other amenities, which were threatened by the decreasing population of the area.[13]

[12] Newton S. Tiver (pastoral consultant) statement to the LDSC ('Evidence', op. cit. (4), pp. 60–70); R. J. Newman, 'Pasture Improvement in the Little Desert', *Journal of agriculture*, January 1959, pp. 1–9.

[13] Evidence presented by Alan Judge Holt, Secretary of Lands, to the Little Desert Settlement Committee, Exhibit H, Approaches of municipalities (to the Department of Lands), and p. 15. Written submission on 'Little Desert Development' from Kaniva Council, 5 pp. (typescript). (Holt's full submission (40 pp. and 11 Appendices) and the Kaniva Council's written submission did not appear in the final transcripts referred to above (note 4). Copies of these



FIG. 3: 'How much would it cost to land a farmer on that . . . ?' Cartoon by 'Jeff' from the Melbourne *Sun*, published 31.VII.69, with the Apollo mission to the moon fresh in the public mind. The figures are the Victorian Premier, Sir Henry Bolte (seated) and the Minister for Lands, Sir William McDonald.

In 1968, McDonald announced a Government-backed subdivision proposal for areas in the Little Desert.[14]

Plans for closer settlement and decentralization had been elements of political platforms in Victoria for over a hundred years. The public had clamoured in the 1850s for the land to be released for settlement, and the Land Acts of the nineteenth century were the mechanism for unlocking land from squatting pastoralists to settlers who would fill the country, and utilize its potential. The popularity of the schemes grew out of the English concept of the yeoman farmer, the small-time farmer who came to be regarded nostalgically as the backbone of the old style rural tradition when the Industrial Revolution swept the country.[15] William Lines has attributed the enthusiasm for the schemes to the need to dominate nature, to fully occupy the land.[16] In the early decades of the twentieth century, decentralization and

were, however, held in the private archive of Valerie Honey, an unaffiliated individual who became sufficiently concerned about the Little Desert Scheme to attend all sessions of the Parliamentary Committee's Inquiry.)

[14] LDSC *Report*, op. cit. (4), Appendices E and F, pp. 20–23.

[15] Tony Dingle, *The Victorians: Settling*, McMahons Point, NSW, 1984, p. 58.

[16] William J. Lines, *Taming the great south land*, Sydney, 1991, p. 94.

plans to fill the great outback gained in popularity as a defence measure against 'threats from the north'.[17]

McDonald enthusiastically endorsed decentralization. He had no personal sympathy with the view that land might have a value in an undeveloped form:

> I'm horrified at the attitude that is being adopted today [1990] to the development of land. How anybody in their senses could believe that the development of land to carry more stock was wrong, is beyond my comprehension . . . If the people who founded Australia had adopted that point of view, Australia would be a pretty miserable place today. Most of it is due to development. In one way or another the outback has been developed by putting more water points on it. The inside country has been developed, originally by the use of phosphate, latterly by trace elements. The AMP scheme in the desert in South Australia was . . . the result of phosphate and trace elements.[18]

McDonald's philosophy echoed that of one colonial Victorian who wrote '. . . the man who furnishes his fellow beings with the staff of life increases the *real wealth* and prosperity of his country'.[19] In a recent paper focusing on Victoria in the 1850s (its first decade of separation from New South Wales), David Goodman observes that the language of agrarianism had appeal to politically diverse positions. The conservative elements of the new colony were keen to recreate the old English yeomanry in the new land, whilst the radicals sought a rightful share of the land for all. These views coincided in a major cry to 'unlock the land'.[20] The Department of Crown Lands and Survey was established in 1855 to do this. The 1860s marked its development into an important aspect of government, with three major Land Acts being passed in less than a decade.[21] Like his predecessors in the Lands department, McDonald saw land as there to be developed, not wasted. The Little Desert was a last land frontier, for which there was at last the knowledge and technology to improve for agricultural purposes. He expected the Victorian people to be supportive of a new settlement opportunity, as they had traditionally been for over a century (see Fig. 4).[22]

[17] Tom Griffiths, 'In search of classical soil: A bicentennial reflection' *Victorian historical journal*, (November 1988) **59** (3–4), pp. 21–38.

[18] McDonald op. cit. (8), p. 8.

[19] Emphasis original. S. Wekey, Secretary to the Victorian Philosophical Institute, 1853, quoted in David Goodman, 'Gold fields/golden fields: The language of agrarianism and the Victorian gold rush', *Australian historical studies*, (1988) **23** (90), pp. 19–41, 24.

[20] Goodman, ibid., esp. pp. 36–8.

[21] Peter Cabena, Heather McRae and Elizabeth Bladin, *The lands manual*, Melbourne, 1989 pp. 2–4; see also Ray Wright *The bureaucrats' domain: Space and the public interest in Victoria*, Melbourne, 1989, chs. 3–5.

[22] Goodman argued that the Victorians were particularly susceptible to agrarian rhetoric, contrasting them with New South Welshmen in this regard: 'Very few [1850s] Victorians . . .



FIG. 4: Cartoon by 'Tanner' from the Melbourne *Age*, 9.IX.69. The 'Sheikh' is the Victorian Premier, Sir Henry Bolte.

Distinguished economists and agricultural scientists, including senior university academics and officials of the state Department of Agriculture, did not share this view. They argued that the McDonald proposal for the Little Desert was not economically viable. The agricultural climate of 1969 was not unlike that of the early 1990s, with poor wool and wheat prices, and problems of overproduction. The cost of improving such poor land was prohibitive, and the rainfall was lower than on the comparable country in South Australia (see Fig. 5).[23] It might have been expected that the opinions of so many distinguished experts would stop the scheme. The Minister was, however, a committed developer: 'We can't afford *not* to develop this land' was

were prepared to argue publicly that agriculture was simply one among many modes of land use, to be judged only by its profitability.' op. cit. (19), p. 36.

[23] LDSC, 'Evidence', op. cit. (4), included pastoral and agricultural consultants: Newton S. Tiver (26.XI.69), Christopher K. Shearer (16.XII.69) and William A. Beattie (19.XI.69); senior academics from the University of Melbourne: John S. Holden (30.X.69), research fellow, Institute for Applied Economic Research, Egon Donath (19.XI.69), lecturer in economic geography, and Alan G. Lloyd (23.XII.69), professor of agricultural economics; and officers of the Department of Agriculture: Rex J. Newman (22.XII.69), agrostologist, and Desmond R. Meadley (22.XII.69), agricultural economist. ABC 'Country Hour' programmes in mid-1968 also voiced opposition to the scheme predominantly on economic grounds. Speakers included Lloyd and Holden, as well as Hugh Wynter, a farm management consultant from Nhill, Jack Vallance from Pier Millan, Victoria's largest wheat producer, and emeritus Professor Sir Samuel Wadham, formerly professor of agriculture at the University of Melbourne.



FIG. 5: The border between South Australia (right) and Victoria, in the Little Desert. The South Australian side of the border had been developed in the 1940s and 1950s, and was well established. The Victorian side carried sizeable yellow gums, which were regularly used by apiarists for wintering bees. (Photograph taken in August 1969 by Peter Attiwill)

his line.[24] He was prepared to gamble that in the medium to long term, the scheme would pay its way.

The arguments against the scheme on economic grounds could have offered the area a temporary reprieve until there were changes in the economic situation. However, a significant group opposed the scheme absolutely. Conservation activists argued that the agricultural scheme would ruin the opportunity for making a major National Park in the Little Desert, preserving habitat for its many rare species, and providing a tourist attraction for the area. Sir William McDonald had perhaps not expected a fight about National Parks. He, like many locals, did not view the 'scrub' country of the Little Desert as scenic. His idea of beauty was, in the words of one of his critics, to convert the area into grass — as far as the eye could see.[25]

National Parks had been a hot political issue of the 1950s. It was a Bolte Liberal (conservative) Government which in 1956 had passed a National Parks Act after strong campaigning by the Field Naturalists' Club of Victoria

[24] Departmental report of public meeting at Kaniva, op. cit. (13), p. 14.
[25] W. G. D. Middleton, transcript of interview, 9.11.90, p. 32.

(FNCV), the newly established Victorian National Parks Association (VNPA) and other groups, including the unlikely Australian Primary Producers' Union (APPU).[26] Since the previous eight Victorian governments (all between 1947, when the campaign had begun, and 1956) had failed to agree on a National Parks Bill, the Bolte team felt they had reasonable credentials in this area. Initially it was not clear whether the conservation protest about the Little Desert in 1969 was, in political terms, electorally representative, or if it was just a few keen birds-and-bugs fanatics.

By mid 1969 the Minister acknowledged that National Parks were supported by a significant proportion of the electorate by declaring that a National Park would be created in the eastern section of the Little Desert (the part least suitable for agricultural purposes).[27] This political solution failed to satisfy the conservation movement. The eastern Little Desert was not biologically representative of the whole area, they argued. No settlement scheme should be contemplated in any part of the area until flora and fauna surveys had been undertaken so that more would be known about what would be lost.[28]

The Minister was, however, determined to pursue some form of the Little Desert agricultural settlement scheme and also a similar scheme near Portland in south-west Victoria. He felt that he had made significant concessions to the conservationists, scaling down his original plan for forty-four farms to a mere twelve. However, he had underestimated the force which lay behind the conservation protest. The Little Desert represented a last frontier. McDonald wanted to tidy it up, to civilize it. The conservationists wanted it as a monument to lost wilderness, an element of the past retained for the future.[29]

New attitudes to frontiers had developed in Australia, as they had overseas. Frontiers, because of their increasing scarcity, required protection rather than conquest. This dispute coincided approximately with the publication in the United States of Roderick Nash's *Wilderness and the American mind*.[30] Australians, like Britons and Americans, had a tradition of nature

[26] The campaign for national parks is well documented in early VNPA newsletters (1952–1956), and in the transcripts of Crosbie Morrison's radio broadcasts (Crosbie Morrison Archive, Series 3, Australian Science Archives Project). See also the magazine *Wild life* and Libby Robin, 'Visions of nature: *Wild life*, 1938–1954', *Victorian naturalist*, (1985) 102 (5), pp. 153–61. APPU involvement can be traced through the Archive of D. W. Goode (State Library of Victoria).

[27] *National Parks Act (Amendment)*, No. 87928, 16 December 1969.

[28] For example, Peter Attiwill (LDSC, 'Evidence', op. cit. (4) pp. 71–7).

[29] It was not in fact pristine wilderness — it was crossed with tracks, some dating back to the mid 19th century when the 'Gold Route' from Adelaide passed through the region. All parts of the Little Desert had been grazed by pastoralists for over 100 years.

[30] Roderick Nash, *Wilderness and the American mind*, 3rd edn, New Haven, 1982 (1st edn 1967).

writing which counterpointed Nature with the evils of civilization.[31] At the end of the 1960s, after unprecedented boom times for materialist society, it was understandable that some citizens would be seeking a sort of balance to the excesses of consumerism. In times of extreme trouble, for example, the Depression of 1929 and the wars (1914–18 and 1939–45), many Australians had found solace in nature, and in writings about nature. Those who felt that the new society cushioned people too much from the reality of life, who perhaps could not believe that the good times would keep on happening, were attracted to the solace of nature. Just as nature had provided a touchstone before when the affairs of the world had become stressful, so it seemed important again in the Brave New World of consumer society. The conservationists, many of them old enough to remember the Depression and the exigencies of at least the Second World War, decided to make the Bolte government aware that many voters were not concerned only with expanding a materialist society.

Melbourne-based conservationists affiliated themselves into a new group, the Save Our Bushlands Action Committee (SOBAC). The affiliation represented the united forces of eight metropolitan conservation groups, the largest of which were the Field Naturalists' Club of Victoria (FNCV) (established 1880), the Natural Resources Conservation League (NRCL) (established 1944) and the Victorian National Parks Association (VNPA) (established 1952).[32] Local views had been expressed earlier in a document prepared by the Wimmera Regional Committee, which had been addressing the issues raised by the earlier AMP scheme for the Little Desert. Their detailed recommendations about Nature Reserves were included in 1968 in a major government report on the Little Desert, and were known to SOBAC.[33] The metropolitan base was essential to the demonstration of the extent of the opposition to the scheme. Only in a city as big as Melbourne (over 2 million in 1969)[34] could a major demonstration be organized. As in

[31] Richard Mabey, 'Richard Jefferies', in *In a green shade: Essays on landscape, 1970–1983*, London, 1983, pp. 133–149; Frederick Jackson Turner, 'The Significance of the Frontier' (Chicago, 1893) in *Early writings of Frederick Jackson Turner*, ed. E. E. Edwards, Madison, WI, 1938; Tom Griffiths, '"The natural history of Melbourne": The culture of nature writing in Victoria, 1880–1945', *Australian historical studies*, (1989) **23** (93), pp. 339–365; Robin, op. cit. (26).

[32] SOBAC membership is listed in *Save our bushland*, Melbourne, 1969. Gwynnyth Taylor, who with her husband, Ron, had founded the group, commented that the active members of SOBAC had been drawn together from people located relatively close to each other (mostly in the eastern and outer-eastern suburbs of Melbourne), who were well known for their commitment to conservationist causes. (Transcript of interview with G.Taylor, 5.11.90)

[33] Wimmera Regional Committee, *Need for reservations in desert settlement*, Horsham, 1964; Land Utilization Advisory Council internal report on land use in the Little Desert, 14 March 1968.

[34] At the 1966 census, Melbourne's population was 2,110,168 out of a total Victorian population of 3,219,526 (66%).

the United States, it was the urban dwellers who felt most sharply the deprivation of wild country.[35]

The President of the NRCL, the retired Soil Conservation Authority chairman George Thompson, chaired and convened SOBAC. The NRCL's membership comprised municipal councils throughout the state, as well as state government authorities, so the voice of the NRCL had a support base in both rural and urban areas, and amongst public servants.[36] Gwynnyth Taylor, horticulturalist, field naturalist and President of the VNPA, and her husband Ron, a city lawyer, were other driving forces behind SOBAC, representing the clubs of amateur nature lovers. SOBAC organized two major public meetings in Melbourne in August and October of 1969, each of which was attended by over one thousand people. These meetings were supported by a sympathetic coverage of conservation issues, both by journalists and in letters to the editor in the three metropolitan daily newspapers.[37]

SOBAC also sponsored a deputation in September to the Premier of the State, Sir Henry Bolte.[38] Bolte apparently heard them out but took no action to stop the scheme. Bolte believed and frequently said that ministers should be allowed autonomy to make their own decisions. 'Why keep dogs and bark yourself?', was his standard comment on the subject.[39] Bolte was personally sympathetic to the agricultural scheme, being a farmer himself. He saw no conflict of interest between McDonald's two portfolios of Lands and Conservation, though many conservationists did. Bolte was comfortable with McDonald's decision to treat the Conservation portfolio as relating exclusively to the practical farming aspects of soil conservation. This was exactly the way he himself had been expected to treat it when he assumed responsibility for the State's first Ministry for Conservation in 1950.[40]

By late 1969 the Little Desert development scheme had few supporters,

[35] Nash, op. cit. (30), see graph and text p. 346; Samuel P. Hays, *Beauty, health, and permanence: Environmental politics in the United States, 1955–1985*, Cambridge, 1987.

[36] Libby Robin, *Building a forest conscience*, Springvale, 1991.

[37] SOBAC, *Save our bushland* and *Bushlands Magna Carta*, 1969; WVCC *Kentbruck Heathland* (December 1969) recorded the attendance at the 29 August 1969 meeting as 1100 and the 26 October 1969 meeting as 1500.

[38] SOBAC *Save our bushland*, Melbourne, 1969, pp. 25–31.

[39] W. A. Borthwick, transcript of interview, 8.VI.90, p. 12.

[40] Bolte was chosen for the new Ministry of Conservation in 1950 because of his practical farming experience. He always regarded the prime purpose of conservation to be the improvement of agricultural resources. In an article written for *Victoria's resources* in 1960, Bolte as Premier and Minister for Conservation chose for his subject the conservation of grass, 'a vital Natural Resource', urging that we should 'realise the value of grass . . . a bounty of Nature which we must cherish — or perish' *Victoria's resources*, (1960) **2** (3) , p. 5.

Graham Pizzey, a journalist and naturalist who accompanied Claude Austin on a 'Little Desert briefing' requested by Bolte late in 1969, confirmed that this was still Bolte's notion of conservation at that time (Pizzey, personal communication, 21.III.91).

especially in the metropolitan media. The ABC radio *Country hour* programme had interviewed a number of experts at length about the scheme. All had dismissed it as unworkable, mostly for economic reasons. The three Melbourne daily newspapers, the *Age*, *Herald* and *Sun*, had all been inundated with letters to the editor about the proposed scheme, almost all of which were opposed to development.[41] Sir William McDonald had become the source of cartoonists' satire, and was increasingly caricatured by strident journalists as an 'enemy of conservation', concerned only with furthering the prospects of wealthy farmers.[42]

A Parliamentary inquiry was set up in October 1969 by the combined Labor and Country Party opposition forces in the Legislative Council, the Victorian Parliament's upper house, and this heard evidence stretching over more than 250 pages of transcript. Some of the expert witnesses were the same agricultural resource managers who had advised the Minister against the scheme when it was first mooted. Bureaucrats frustrated because the government was not taking their well considered advice, unofficially and officially supplied much of the data presented both at the inquiry and in the media. The staff of the Department of Agriculture were particularly active in opposing the scheme, insofar as public service etiquette allowed. McDonald actually would not speak to the Minister for Agriculture for some time because of the 'leaks' in his department.[43]

Even before the report of the inquiry was published (in March 1970), evidence and opinion mounted sufficiently for the Upper House to vote to block funding to the scheme. The decision to block the relevant supply bill

[41] The country newspapers such as the Kaniva *Times*, the Wimmera *Mail-Times* and the Portland *Observer* (which was more concerned about the parallel agricultural settlement scheme proposed for Kentbruck Heath near Portland), were much more supportive of settlement schemes than the Melbourne papers. The agricultural weekly *Stock and land* published an article about the Nhill Chamber of Commerce supporting Sir William McDonald ('Locals want Little Desert developed', 26.VI.69, p. 5) but soon after this the mood changed. Another weekly, the *Countryman*, published an article 'Many people have doubts about the future of the Little Desert', publicizing the views of the President of the Victorian branch of the Australian Institute of Agricultural Science on 10.VII.69, and from then on even the country papers were circumspect about the scheme, generally for economic rather than ecological reasons.

[42] The *Age* conducted a particularly personal campaign directed at the Minister, and finished up losing an expensive court case about an article (4.X.69) that alleged that the new developments in the Little Desert were being pursued because of their likely benefit to the Minister's brother-in-law (J. M. Powell, 'Action analysis of resource conflicts: The Little Desert dispute, Victoria, 1963–1972', in J. M. Powell, ed., *The making of rural Australia*, Melbourne, 1974, pp. 161–78). The 'wealthy farmers' were those who could afford the capital to develop the proposed farms, those for whom the tax concessions might make the farms economically viable. In Melbourne these were referred to disparagingly as 'Collins Street farmers', Collins Street being the business address of many professionals, especially doctors, dentists and lawyers. A characteristic of tax-farmers was that they were unlikely to be resident on the new farms, and therefore that the scheme would do nothing to 'decentralize'.

[43] Borthwick, op. cit. (39), pp. 12 and 30: Borthwick described it as 'bitter in Cabinet'.

was taken in December 1969, the same week that a by-election in metropolitan Dandenong (a long way from the Little Desert, as one cartoonist pointed out) yielded a negative result for the Bolte Liberal government.[44] This led to a halting of the Little Desert development, but only temporarily.

Sir Henry Bolte was old-fashioned in many ways, but he was politically astute enough to sniff the winds of change. It seemed that it was the new conservation vote that had decided the Dandenong by-election result unfavourably for him. So he 'discovered' a more electorally appealing type of conservation, just in time for the general election the following May. The post-election Minister for Lands, Bill Borthwick, recalled it thus:

> The Little Desert was a turning point . . . it caught Bolte — Bolte came to me for the 1970 policy speech and got me to write a segment on conservation — never been in a policy speech before. He knew it was time — Bolte was a great politician — he knew that things and attitudes had changed. I wrote the conservation issues . . . I took [the section] to [Dick] Hamer [who succeeded Bolte as Premier of Victoria in 1972] because I got it down to seven pages and I knew it should be seven paragraphs and Dick blue pencilled it for me.[45]

The departure by Bolte from earlier policy did not go unremarked. Borthwick went on to recall wryly the comments of the press in an Ararat pub after the speech:

> We walked in on the press, and they were saying 'Who wrote Bolte's speech? — He's saying things he doesn't understand'.[46]

Borthwick and Hamer had Bolte promising that land management decisions would be taken in a new way which involved more public consultation via a 'Land Resources Council'. He also specifically promised that at least 5% of the State would be reserved for National Parks, Wildlife Reserves and Forest Parks.[47] This was not difficult, given that over 30% of the State was unalienated (public) land. It was designed simply to woo the conservation vote.

The speech was persuasive. Bolte's government was re-elected, losing only two seats. The Liberal primary vote slipped only slightly. However, one of the two seats lost was Dundas, the seat of Sir William McDonald.[48] This did

[44] Cartoon by Tanner, the *Age*, 9 December 1969, 'I never knew the Little Desert was in Dandenong'; Victoria, *Parliamentary Debates (Hansard)*, (9 December 1969) 9, p. 2563.

[45] Borthwick, op. cit. (39), p. 13. [46] Ibid.

[47] The election promise was cited by W. A. Borthwick in his introduction to the second reading of the Land Conservation Bill, Victoria, *Parliamentary Debates (Hansard)*, (15 September 1970) 299, p. 147.

[48] Liberal first preference votes throughout the State fell from 37.5% to 36.7% (Electoral information supplied by Dr Paul Thornton-Smith, Projects Branch, State Electoral Office, Victoria, personal communication, 7.VIII.90).

not necessarily mean the end of the Little Desert scheme. Many conservationists at the time were anxious about what would happen after the election.[49] But the defeat of Sir William had to be attributed at least in part to the Little Desert controversy. The Government had also lost the scheme's most passionate proponent. It emerged that there was not a high level of support from other members of cabinet, and even the Premier had shown himself to be sufficiently concerned to take conservation interests seriously leading up to the election.

'The public interest' and a credible bureaucratic mechanism

The Little Desert dispute was not simply a case of conservation or preservation versus development. It was, rather, a rare moment when economists and conservationists found themselves arguing the same case. The public, which in the first half of the twentieth century had been very supportive of development and decentralization schemes, was sceptical about this particular scheme. Some felt that it was to cost the taxpayer too much, while others remembered individuals who had suffered because of the inadequacy of the land provided under previous such schemes.[50] A new group was emerging which was concerned about the cost to the land itself. However, the majority of letters to the editor about the Little Desert agricultural scheme published in major metropolitan newspapers in 1969 were concerned not so much with conservation values and economic forces, but rather with political processes.[51] There was much concern expressed about the unbridled powers of a single minister to create a land use pattern with which future generations had to live. There was a strong call for more consultation and accountability. It was this which motivated a review of the whole system of public land management. Leaving options for future generations was a quality-of-life issue, part of an emerging ethic which crystallized in the 1970s.

Nevertheless, there was recognition of a 'conservation victory' in the

[49] For example, C. S. Elliott, 'A new era?' (editorial) *Victoria's resources*, (1970) 12 (1), p. 1.

[50] Borthwick commented that anyone who had witnessed the tragedy of the Victorian Mallee region (where there was extensive soldier settlement after the First World War) in the 1930s (when he was growing up) would be sympathetic to conservation. He poignantly described his memory of financially stricken farmers skulking off their land in the dead of night ashamed to confess their plight to their neighbours (transcript of interview 8.VI.90, p. 5).

[51] Survey (1990) by the author that looked at letters to the editor in the *Age*, *Herald*, *Sun* and several local papers between April and October 1969. A total of 164 letters related to the Little Desert or Lower Glenelg settlement schemes. Overall, 153 were against and 11 in favour of development. Letters stopped abruptly on 10 October 1969 because of litigation taken by Sir William McDonald against the *Age*. Of the 153 letters opposed to the development, 33% gave as their prime concern some combination of conservation, tourism or science, 20% gave economics as the reason and 47% expressed concern about the 'public interest' in land management matters (including 8% congratulating the media on their role in the publicizing of the dispute).

outcome of the election.[52] Conservationists realized that they were being listened to by parliamentarians as a unified pressure group. The successful result for the Little Desert lent confidence to the whole movement.[53] The Conservation Council of Victoria was established on the wave of this enthusiasm, to act as an umbrella organization for all conservation groups.[54] The CCV took over from the informal SOBAC in making Parliament aware of conservation concerns. Its purpose was to liaise formally with the government of the day on all conservation matters affecting the state.

Bolte handed the mantle of the Lands portfolio to Bill Borthwick after the 1970 election. Although Borthwick had not wanted that task, he understood the political importance of making the incoming Minister of Lands very different from McDonald, and reluctantly accepted the portfolio.[55]

The Premier's election promises of a Land Resources Council had to be honoured. Borthwick needed to ensure that the Council would be credible to the concerned general public, and (in Jim Falk's words) 'impart to government policy a sufficient level of legitimacy to enable it to be implemented'.[56] The Little Desert dispute was important in Victoria because it created a climate where a Land Conservation Council (the name change was significant) was established and set in concrete by Act of Parliament.[57] The incoming government chose to confront the criticisms of accountability and public consultation with a mechanism which was apparently above politics. This authoritative, independent (although of course government-approved) body was charged with the responsibility for inquiry into all matters of public land management. If Falk is correct in arguing that it is only a potentially divisive issue that will drive a government to risk a public inquiry for the sake of legitimizing government policy, then the Little Desert issue stands out as a very divisive issue. For the result of the Little Desert in practical terms was not just an inquiry, but rather a permanent mechanism for inquiry.

The Land Conservation Council took over from an earlier council of Heads of Department, the Land Utilization Advisory Council, but it was

[52] For example, a photographic exhibition entitled 'The Little Desert: A conservation victory' was held in the foyer of the Botany School at the University of Melbourne early in 1970.

[53] See Falk, op. cit. (6), p. 290.

[54] The first meeting suggesting the possibility of a body like the Conservation Council of Victoria was held on 24.IV.69 at the NRCL headquarters at Springvale. An ad-hoc committee was set up at this time, but the Council was not formally constituted until the height of the Little Desert dispute, when a meeting held on 30.X.69 at Brymay Hall, Richmond (an inner suburb of Melbourne), voted that it be formally constituted. This meeting was attended by representatives of 76 clubs and organizations concerned about conservation issues in Victoria. See *Victoria's resources*, (1969) **11** (2), p. 7; (1969–1970) **11** (4), p. 31; and G. T. Thompson, 'Conservation Council of Victoria', *Victoria's resources*, (1970) **12** (1), pp. 27–29.

[55] Borthwick had wanted the Health Ministry, because of a personal concern he felt about mental retardation and the state of asylums. (Transcript of interview, 8.VI.90, p. 15)

[56] Falk, op. cit. (6), p. 149.

[57] *Land Conservation Act*, op. cit. (5).

given more independence and power. The important difference between the LUAC and the new LCC was its openness and active involvement of the general public. The LCC had an independent chairman and three independent members (from the general public), two of whom were nominated by the conservation community (via the Conservation Council of Victoria). The third member was a person experienced in developing land for agricultural purposes. The first person nominated for this position, Claude Austin, was in fact an ornithologist and president of the active conservation group, the Western Victorian Conservation Committee, as well as being a Western District grazier. Another important addition to the LCC was the Head of the National Parks Service. The structure of the earlier LUAC had been established before the National Parks Authority, and the Director of National Parks had not been added to the Council subsequently.[58] Borthwick reinforced the status of the new LCC when he elected to defer all alienations of Crown Land (except for trivial ones) until it had made recommendations on the area concerned.[59] The LCC's published reports (in draft and final form) were another element of its public accountability.

The LCC divided the state into sixteen geographical regions and in turn reviewed the uses of public land in each. The first area to be considered was South West Victoria and the last (completed in 1986, before a second round of reviews of the regions commenced) was the Wimmera, with the Little Desert. The two areas targeted by SOBAC, the Lower Glenelg and the Little Desert, politically the most sensitive regions in the state, were to be the starting and finishing points for the LCC's deliberations. Professor John Turner, a foundation member of the LCC, saw the order of the reviews as symbolic.[60] No one had expected that the reviews would take sixteen years to complete, but the decision to leave the Little Desert until last was undoubtedly to give it a 'cooling-off' period. Following the recommendation of the LCC in 1986, the total area of the Little Desert National Park (as declared in May 1988) is 132,000 hectares.[61]

The South West region of Victoria (including the controversial lower reaches of the Glenelg River) was a good place for the LCC to start. It satisfied conservationists that the LCC would take on land management issues which involved their concerns. A major Lower Glenelg National Park had been proposed in various forms for over twenty years, and sections of the Lower Glenelg area had already been declared National Parks.[62]

[58] The National Parks Authority established in 1956 was replaced in 1970 with a National Parks Service through the *National Parks Act* Amendment (No. 87928, 16 December 1969).

[59] Borthwick, op. cit. (39), p. 24.

[60] Professor John S. Turner, transcript of Tape 3 of interview, 28.VIII.90, p. 3.

[61] Victoria: Department of Conservation and Environment, *Little Desert National Park and Wail State Forest proposed management plan*, Horsham, November 1991, p. 1.

[62] FNCV expedition to the Lower Glenelg, Easter 1947, sponsored by Portland Field

However, the Forests Commission and the Department of Agriculture were also concerned land users in the area. There had been a long-running dispute between them as to whether the Kentbruck Heath should be used for pine plantations or farms. Meanwhile, the Portland Field Naturalists' Group, one of the most active and influential in the state, set about documenting all the flora and fauna in the area, as well as the natural geographic boundaries, which were likely to be violated by the farms scheme. The conservationists' principal concern was that even if the sensitive wet heathland of Kentbruck was spared, the scheme would intrude on the catchment area of Mole Creek, which feeds it.[63] SOBAC was able to pick up this cause, documented in a sophisticated way by the regional group, the Western Victorian Conservation Committee, and publicize it in metropolitan Melbourne. Juxtaposed with the Little Desert scheme, it received powerful urban support. Active conservationist and Portland farmer Fred Davies commented on the importance of the Little Desert in dictating the outcome of the Lower Glenelg dispute:

> With all credit to other people I must concede that if it hadn't been for the Little Desert I don't think we would have got Kentbruck Heathland because the Desert appeared to be a much bigger objective and it seemed to involve city people more than our little plot down here . . . I really think because of its size and because of media support the Little Desert was the glamour one. But Kentbruck was the most important although [small] in size, and hardest to get. The developers wanted Kentbruck even more than they wanted the Little Desert because of its better rainfall and its greater productive potential . . . If McDonald had opened the Little Desert there would have been a lot of similar country left. But if the farmers here had got the Heathland, that was it — there was no more Heathland.[64]

The LCC's first recommendations included a sizeable expansion to the Lower Glenelg National Park. Virtually all the LCC's recommendations for land use in South West Victoria were eventually adopted by Parliament. As well as substantial additions to the Lower Glenelg National Park, the National Parks Act 1975 included the LCC's new, tighter definitions of what

Naturalists' Club. Ina Watson et al., 'The proposed Glenelg National Forest and Sanctuary' *Victorian naturalist*, (1947) 64, pp. 62–91.

[63] Western Victorian Conservation Committee, *The case for a Lower Glenelg National Park*, Portland, June 1968; *Boundaries of Kentbruck Heathland*, Portland, April 1969; *Kentbruck Heathland*, Portland, December 1969.

[64] Fred Davies, revised transcript of interview, 20.III.89, p. 7. Davies' reference to the uniqueness of the Kentbruck Heathland was supported by Frank R. Gibbons and R. G. Downes, *A study of land in south-western Victoria*, Melbourne, 1964, which argues that the distinctive property of the Kentbruck Heathland was that it was a wet heathland — other Australian mainland heaths are sandy and dry, and therefore have different vegetation and ecology (p. 231).

National Parks should be.[65] The government was seen to take the LCC very seriously. This was essential to its continuing credibility. It also served to legitimize further the LCC itself.

A final postscript to the Lands Ministry was the demotion in 1973 of the Lands Portfolio by Borthwick in favour of a new broader Conservation Ministry. From 1973 Borthwick styled himself 'Minister for Conservation' rather than Minister for Lands.[66]

The Little Desert: Impact on the conservation movement

The conservationists were unanimous in their delight about saving the Little Desert. They were also delighted about their unanimity. Those who joined forces to work for the Little Desert cause came from philosophically and politically different backgrounds. There were utilitarian conservationists, nature lovers, preservationists. They voted Labor (left), Liberal (conservative–urban) and Country Party (conservative–rural). In some cases it seemed that their only common ground was their cause. They did, however, share other commonalities. The three most powerful organizations within SOBAC were the Natural Resources Conservation League, the Field Naturalists' Club of Victoria and the Victorian National Parks Association. All of these groups were led by experienced campaigners, mostly middle-aged, approaching retirement or recently retired. The young activists who are commonly associated with conservation in the 1970s, 1980s and 1990s were conspicuous by their absence from the 1969 scene. An exception to this was a small group of science students, the Monash University Biological Students' Society, who distributed leaflets and bumper stickers protesting about the scheme in the Melbourne City Square.[67]

[65] The first Lower Glenelg National Park of 9069 hectares was declared in 1969. This was expanded to a total of 27,300 hectares in 1975 on the basis of the Land Conservation Council's *Final recommendations, South Western Study Area, District 1* (1973). See also Sandra Bardwell, *National Parks of Victoria*, Macquarie Park, NSW, 1985, pp. 9–14; and Department of Conservation, Forests and Lands (Portland Region and National Parks and Wildlife Division), *Lower Glenelg National Park: Proposed management plan*, Portland, December 1988, p. 6.

[66] The *Ministry for Conservation Act* (No. 8364 of 1972) came into operation on 23 January 1973. The Lands Ministry was finally abolished in 1983, its land administration functions being transferred to the new Department of Conservation, Forests and Lands (CFL), and its survey and land sales functions to the Department of Property and Services. The word 'Lands' no longer appears in the title of any Ministry. In 1990, the Victorian Government created a new 'superministry', Conservation and Environment, which included the functions of CFL in its brief. A change of government in October 1992 led to the Superministry being divided and regrouped as Conservation and Natural Resources — with Forestry under the 'Natural Resources' and National Parks under 'Conservation'. It is too early yet to tell what the implications of this will be.

[67] Geoff Edwards, transcript of interview, 30.v.91; dictated letter to the author, 31.viii.91). Edwards commented that the year after the Little Desert dispute the Monash University

The importance of the older leaders was the fact that it was their contemporaries, and in some cases friends, who held the reins of power within the government. Claude Austin, who later became a foundation member of the Land Conservation Council, was chairman of the Western Victorian Conservation Committee. He campaigned tirelessly for the Lower Glenelg and Little Desert National Parks. He had also campaigned for the Liberal party, on behalf of Malcolm Fraser, his local Federal member, who went on to become Australia's Prime Minister, 1975–1983. Austin played golf regularly with the Premier, Sir Henry Bolte. He was also President of the Melbourne Club.[68] Even Sir William McDonald recalled him as a 'friend', although he disagreed with him passionately about conservation.[69] Ros Garnet, secretary of the Victorian National Parks Association from its inception for twenty years, was well known to many politicians as one who had been promoting the cause of National Parks since the 1940s, and was well informed of their historical development.[70] Gwynnyth Taylor was an important linchpin in the conservation movement because of her active membership in eleven different amateur organizations (mostly concerned with natural history and nature conservation).[71] Professor John Turner, who held the Chair of Botany and Plant Physiology at the University of Melbourne from 1938 to 1974, was another with multiple contacts: he worked with the Forests Commission in Forestry education, with the Victorian National Parks Association as Vice President, he was a member of the Melbourne Club, and he was deeply involved with both the 'Save the Dandenongs League' and the Landscape Preservation Committee of the National Trust.[72] Austin, Garnet, Taylor and Turner were not alone.

Some of the most important campaigners for the Little Desert chose to remain anonymous. Public servants, constrained by their positions, were nevertheless responsible for much of the hard data publicized by the activists.[73] For those whose concerns were economic rather than conservationist,

Biological Students' Society split and a group formed a more radical 'Environment Society'. At the time of the dispute, however, it was very much a scientific society.

[68] The Melbourne Club is regarded as a centre of influence in conservative (masculine) circles.

[69] McDonald, personal communication, 12.XI.90.

[70] J. Ros Garnet, revised transcript of Interview 1, 20.IV.89, pp. 12–13.

[71] Gwynnyth Taylor, notes of discussion, 16.IV.90.

[72] Turner, transcripts of interviews, 28.VIII.90 and 28.I.91.

[73] W. G. D. Middleton, public address, 24.VI.89, at the formal ceremony which handed over Broughton's Waterhole in the Little Desert to the State for a National Park by the family of P. L. Williams, one of the great local campaigners. Middleton, as the Wimmera's senior forester, recalled being specifically barred from 'being seen to be involved in the Little Desert dispute' by the Chairman of the Forests Commission. So he, 'like a lot of other public employees went underground' (p. 12 of transcript).

'leaking' was the only way to make their feelings about the Little Desert scheme known.[74]

For public servants motivated by conservation concerns, the Natural Resources Conservation League provided a way to have a legitimate voice in the debate. The NRCL's membership comprised organizations, rather than individuals, so it was relatively easy to lend anonymous support to a cause. The NRCL had begun as the 'Save the Forests Campaign' in 1944, and its original spirit and some of its first members were still active in the 1960s. It had been the brainchild of a senior industrialist, Sir Herbert Gepp, who was then managing director of Australian Paper Manufacturers (APM) and the Hon. Cyril Isaac, teacher, nurseryman and Member of the Legislative Council. Although it was an independent organization, it had the strong support of the Forests Commission and also many local government bodies.[75] Its first aim had been to build a public 'forest conscience' in the wake of the devastating Black Friday bushfires of 1939, which the Royal Commissioner, Judge Leonard Stretton, found had been lit 'by the hand of man'.[76]

The NRCL remained true to its utilitarian conservationist origins. It published a major journal, *Victoria's resources*,[77] which contributed much to raising the conservation consciousness of Victorians. The conservation of the journal was and still is 'wise use'. Even in the 1990s many of the NRCL's members do not accept any other definition.[78] Its contribution to the establishment of the Conservation Council of Victoria was significant, supplying all the administrative support and a Secretary (Lewis Godfrey) for the CCV's first five years.

The Field Naturalists' Club of Victoria was a much more diverse group, its definition of conservation less easily characterized. Because it was an amateur group, albeit with many distinguished scientists as members, there was less pressure on it to have an official line on what conservation was. Certainly, it included those who sympathized with utilitarian conservation,

[74] Borthwick, op. cit. (39), p. 12: 'The Ag Department started leaking madly . . .'.

[75] Robin, op. cit. (36), ch. 1.

[76] Victoria, *Report of the Royal Commission to inquire into the causes and measures taken to prevent the bush fires of January 1939* (by L. E. B. Stretton), Melbourne, 1939; the fires of Friday 13 January 1939 burned much of Victoria, including its best mountain ash (*Eucalyptus regnans*) forests, and took seventy-one lives.

[77] Begun in 1959 and renamed *Trees and natural resources* in 1985; see Robin, op. cit. (36), pp. 78–82.

[78] Group interview, NRCL, 24.XI.89, and comments on drafts of my history of the NRCL in 1990 by members revealed that alternative definitions of conservation were to be resisted. In D. S. White and C. S. Elliot, eds., *Man, the earth and tomorrow*, Springvale, 1969, soil conservationist R. G. Downes stated that 'wise use' conservation was the modern definition, replacing the definition 'of 30 years ago' which corresponded to 'preservation' (p. 96). This blatantly ignored historical patterns of usage, including the naming of the Authority of which he was Chairman, which was named the Soil Conservation Board in 1940.

but it also had natural historians well informed on ecological systems, and its fair share of genuine romantic nature lovers. Its Portland branch, for example, had members with sufficient scientific knowledge to mount the full-scale scientific survey of flora and fauna in the 1940s and 1950s mentioned above. Yet as amateurs, 'Field Nats' were free to acknowledge the romantic and cultural motivations for undertaking such work. In a 1949 report of a trip to Rushworth in north central Victoria published in the FNCV journal, the *Victorian naturalist*, one member wrote:

> It is the whole pattern of nature which attracts me — colour, form and relationship. The symmetry and balance in every wild flower, the spider zigzagging down its web, the great Emperor Moth hanging on a twig drying its wings, the snake under the stone, the tingling adventure of an hour-to-hour existence of the scampering cotton-tail — all these are the facts that fit into a great pattern and that to me is the story of any trip.[79]

The author of this piece, Jean Blackburn, was a strong supporter of the conservation movement in Victoria right up until her death in 1983, when she left much of her estate to conservation causes.[80] Her commitment to nature conservation was cultural, supported by a literary tradition going back to the late eighteenth century, the Arcadian tradition of nature writing described by Donald Worster in *Nature's economy*.[81] Arcadian cultural values were also explicit in the founding of a Landscape Preservation Committee of the National Trust of Victoria in 1955.[82] The Landscape Preservation Committee sought to broaden the Trust's concerns with old buildings and urban artefacts to incorporate the preservation of whole landscapes in rural and bush areas, following closely the pattern of the British National Trust for Places of Historic Interest or Natural Beauty.[83] Aesthetic considerations were paramount in the classification of likely sites. Natural and pastoral landscapes alike were identified.

Like the earlier campaigns of the 1940s and 1950s, the Little Desert campaign was supported by both utilitarian conservationists and Arcadian nature lovers. It was exceptional in that it was a conservation campaign also supported by economists. A further important difference between the Little Desert and earlier campaigns was the role it gave to science and scientists. Earlier campaigns had involved scientists, both from the University of Melbourne (until the 1960s the only University in Victoria) and from govern-

[79] Jean Blackburn, writing of her trip to Rushworth, *Victorian naturalist*, (1949) **65**, p. 236.

[80] Geoff Durham, executor of the Blackburn Estate, personal communication, 10.VIII.90.

[81] Donald Worster, *Nature's economy*, Cambridge, 1985 (1st edn 1977) esp. pp. 2 and 378.

[82] Norman Wettenhall, foundation member of the Committee, personal communication, 25.III.91, supported by comments from other key members in interviews (Dewar Goode, (20.XI.89), J. S. Turner, (tape 1, 28.VIII.90, pp. 15–16)).

[83] Sir Dudley Stamp, *Nature conservation in Britain*, London, 1969, pp. 32–35.

ment research organizations such as the Commonwealth Scientific and Industrial Research Organisation (CSIRO). It was just that in the 1950s and earlier, the scientists' contributions had been in their spare time, as members of a Field Naturalists Club or other amateur nature lovers group. By 1969, as elsewhere in the world, they held a special status within the conservation movement, as providers of rational arguments, additional ammunition for the conservation cause.

The prominence given to scientific values of National Parks was distinctively different from the 1950s campaign which focused on amenity values. The arguments for National Parks mirrored those in Britain, especially those put forward by the influential ecologist Sir Arthur Tansley.[84] In the 1950s, the natural history preserved in a National Park was part of the amenity value of the park for the naturalists who visited. Parks were for people. The motto of the Victorian National Parks Association, established in 1952 is 'For all the people, for all time'.[85]

By the late 1960s, there was a greater variety of arguments in favour of National Parks. In fact there was a strong tendency in popular publications to amass every possible argument, without evaluating their mutual consistency. For example, one publication advocating a National Park for the lower reaches of the Glenelg River in south western Victoria, suggested that the National Park status would enhance the opportunities for 'speed boat enthusiasts'.[86] Yet the same publication was opposed to clearing the forests on the banks of the river because of the erosion it would cause. The erosion caused by power boats was acceptable (perhaps because it was a public right), whilst erosion which only benefited the timber industry was unacceptable. Arguments about parks as reference areas for the benefit of science also featured. Scientists, although an elite group, were seen as good users — perhaps because they were expected to generate useful conservation arguments. Yet in many cases scientific purposes would limit or exclude other users of National Parks.[87]

The publication in 1966 of a collection of essays by scientists entitled *The great extermination* (edited by A. J. Marshall, the Foundation Professor of

[84] Robert P. McIntosh, *The background of ecology*, Cambridge, 1985 (reprinted 1987), pp. 298–9.

[85] Sandra Bardwell, '"For all the people for all time": National Parks in Victoria, 1866–1956', PhD thesis, Monash University, 1974.

[86] WVCC, *The case for a Lower Glenelg National Park*, op. cit. (63), p. 4.

[87] Recommendations for public land use in Victoria by the Land Conservation Council include a category 'Reference Areas'. Reference Areas are 'tracts of public land containing viable samples of one or more land types that are relatively undisturbed . . . [which] are reserved in perpetuity as a reference to be used by people concerned with studying land for particular comparative purposes . . . [where] human interference should be kept to the essential minimum'. Abridged from *Lower Glenelg National Park: Proposed management plan*, op. cit. (65), p. 6. The purpose of Reference Areas is explicit: they are 'a control group' for scientific purposes.

Zoology at Monash University) was undoubtedly important in giving the scientific values of National Parks a high profile.[88] It aimed to make the general public concerned about vanishing biological species throughout Australia, but it also served to draw attention to the important role of scientists as 'whistle-blowers'. Following soon after the internationally acclaimed *Silent spring* (1963 to Australian audiences), *The great extermination*'s local content made it particularly relevant and disturbing. This book was read and cited by active conservationists. It was responsible for technical terms like 'gene pools' finding their way into popular documents.

Scientific values of parks became such a useful factor in debates that they did not have to be proven. Species of unknown value, or even unknown species, could exist in the Little Desert and many other parts of Australia where only limited scientific surveys had been conducted. Arguments were framed in terms of the need for Science to have access to organisms which were not yet even known. Natural history no longer needed to be discovered for it to be valued.

Scientists too, were seen in a different light. The earliest conservationists, water resource managers and foresters, used scientific principles to conserve specific resources for human use. Science conferred professional status on the irrigation engineers and foresters and served to render their works a 'black box' for the general public, although there was public support for so-called progressive conservation in Australia as in the United States.[89] The next generation of conservationists, the agricultural resource managers and soil conservationists, was the first to become interested in applying the rising science of ecology to conservation problems. In the United States, Frederic Clements, a founder of the academic discipline of ecology, was concerned in the 1930s with the practical problems raised by the Dust Bowl tragedy in the Midwest. Victoria's agricultural resource managers sought involvement from scientists in dealing with similar problems with severe dust storms in the Mallee region of Victoria's North-West.[90] The dramatic bushfires of 1939 compounded the State's soil conservation problems, and forced the government of the day to establish a Soil Conservation Board. Maisie Fawcett, a University of Melbourne botanist, was called on by the SCB to give scientific evaluations of fragile areas and make recommendations. Ecological principles were the basis for her studies although she would not have called herself an ecologist at the time.[91]

[88] A. J. Marshall, ed., *The great extermination*, London and Melbourne, 1966.

[89] The Conservation League of America was established by Gifford Pinchot in America in 1908, specifically to promote progressive conservation. Several important American progressive conservationists (for example, Elwood Mead) worked in Australia and influenced both forest and water managers in the period from about 1910 to 1925. The general progressivist movement in Australia is described by Michael Roe in *Nine Australian progressives*, St Lucia, 1984, esp. pp. 10–13.

[90] Dingle, op. cit. (15), pp. 188–90.

[91] Fawcett's specialties were plant pathology and mycology.

Nearly thirty years later, in 1969, ecology was an established science in Australia, with a flourishing local professional association.[92] Ecologists were secure about their professional status, which had been enhanced within biology through the International Biological Programme (IBP).[93] The public had developed a growing understanding of what ecology was, and there was recognition for the place of ecological evidence in land management decision-making processes. The evidence given by ecologists to the parliamentary inquiry into the Little Desert reflected this. Ecological expertise was so acceptable to the general public that it conferred on ecologists freedom and status to speak outside their discipline.

Peter Attiwill, a forest ecologist from the University of Melbourne's Botany School, began by presenting a scientific argument for the preservation of the biological diversity of the region. He explained carefully his technical definition of 'diversity' as the probability of finding particular species in ten plots placed randomly throughout a plant community. He also explained that his research in the area had been very preliminary, based on the work done by a class of third year students on a week-long excursion undertaken during the August vacation in 1969. The plots were evaluated using the technique of minimal area quadrats (see Fig. 6).[94] Attiwill reported that he had established that many species were found only at one or two of the ten plots examined. This indicated that the Little Desert was not just biologically rich but also diverse. The problem with alienating some of the area for farming was therefore that some species would almost certainly be lost, even if a substantial area were retained for a National Park. On the evidence of his brief survey, species did not occur throughout the region, but were often found only in certain parts of the region. In summing up, he argued for preservation of all of the remaining unalienated Little Desert, but

[92] The Ecological Society of Australia was founded in 1961. Its publication, the *Australian journal of ecology*, did not commence until 1976, but Australian ecological studies were published in a range of American and British journals at this time, as well as the *Australian journal of botany*.

[93] By the time of the Paris meeting of IBP delegates (23–25 July 1964), the decision to invest IBP's effort in ecology was a *fait accompli*, although this meeting was the first opportunity for Australians to contribute to the discussion on a face-to-face basis. Prof J. S. Turner, who was one of the two Australian representatives at the meeting at the time, did not believe that 'the subject of Biology . . . was ripe for an international scheme' (Turner Archive, University of Melbourne Archives, Box 26, ICSU file, draft report by JST, p. 4) a view shared by many, especially later in the 1970s. Nonetheless Turner and others promoted its work, and IBP conferred a new respect on the sub-discipline of ecology.

[94] Minimal area quadrats (plots) were used to evaluate diversity. The quadrats were of increasing size: 1, 2, 4, 8, 16 and 64 sq. m. The number of species in each quadrat was counted. In an area of vegetational diversity, the graph of number of species versus size of plot is linear, where in a vegetationally uniform area, the graph of the number of species per plot flattens out (Peter Attiwill, personal communication, 18.IX.91).



FIG. 6: Agricultural botany students examine 'minimal area quadrats' in heathland country in the Little Desert, measuring for biological diversity in August 1969. The background is brown stringybark forest. (Photograph by Peter Attiwill)

the basis for his call had moved away from his specific expertise, to what he called 'a new morality':

> [The] recognition of the need for conservation is part of a world-wide movement which, in essence, appears to be related to the problem of over-population and greatly increased mobility. We now recognise that a finite world can support a finite population. The goal of 'the greatest good for the greatest number' is simply not possible — we cannot maximise two variables at the same time. I believe we must maximise 'goodness', or the quality of life. It is the desire to maximise the quality of life — to make the world a fit place in which to live — that has brought to our attention problems of pollution, of contamination, and of conservation. The need to control the quality of our environment is, I consider, part of a new morality which is now man's urgent responsibility.[95]

Malcolm Calder, a reproductive biologist also from the University of Melbourne's Botany School, argued that field studies which develop the

[95] P. M. Attiwill, in LDSC, 'Evidence', op. cit. (4), p. 75.

practical skills used by ecologists were of value to the whole population 'as a cultural and aesthetic discipline, by bringing an increasingly urbanized population into closer touch with natural phenomena and rural life'.[96]

These submissions represented, in some sense the watershed of the Little Desert. Their arguments that the Little Desert should be a National Park appealed to all the conservationist traditions of their time, not just the discipline in which they could be regarded as expert witnesses. Attiwill's quality control had utilitarian overtones, whilst Calder appealed to the sense of loss of things Arcadian felt by the urban population. Both submissions were marked by a sense of social responsibility. The ecologists were conscious of their contribution of important expertise to a popular debate, but were also aware that other (non-scientific) arguments would hold sway with parliamentarians.

It was easier for ecologists to accept social responsibility for their actions than it was, for instance, for nuclear physicists. But the risk taken by the ecologists was that by making their science accessible to the general public, for political purposes, they would in some sense lose control of their science. Dorothy Nelkin has analysed the role of American ecologists' professional associations in this period, which was environmentally turbulent throughout the world, and has concluded that by the early 1970s American ecologists preferred to return to the isolation of their laboratories, rather than try to keep up with the mountain of socially responsible work which was accumulating for the few trained specialists in the field.[97]

Nelkin's analysis rests on the division between the pure and the applied aspects of the science: 'the pure and applied research of the scientist takes place in a different social context from the practical work of a professional involved in public service'.[98] This division was not as marked in 1960s Australia as in America. If anything, the opposite was the case. Tim Ealey, an ecologist working in Zoology at Monash University, actively involved senior scientifically trained resource managers in the teaching of his honours students. The lectures given by these public servants were responsible for the enthusiasm of the one group of young activists, Monash University Biological Students Society, about the Little Desert issue.[99]

Likewise, the School of Botany at the University of Melbourne (which employed both Attiwill and Calder) had a long history of strong links with public service professionals. Botanists taught and controlled aspects of the curriculum for foresters and agricultural scientists, had co-operative research

[96] D. M. Calder, ibid., p. 190A.

[97] Dorothy Nelkin, 'Scientists and professional responsibility: The experience of American ecologists', *Social studies of science*, (1977) 7, pp. 75–95.

[98] Ibid., p. 76.

[99] Geoff Edwards, personal file 'Conservation and applied ecology' (notes of fourth-year seminar, department of Zoology, Monash University); also dictated letter to the author, 31.VIII.91.

ventures with the Soil Conservation Board (later Authority), the National Parks Authority and the Melbourne and Metropolitan Board of Works. In addition, the Professor of Botany, John Turner (a foundation member of the Land Conservation Council), encouraged his staff to take an active and executive role in private groups like the Victorian National Parks Association. In the 1960s, the VNPA sponsored Judith Frankenberg, a young botanist, to undertake a major scientific survey of National Parks in Victoria. This was paid for by private subscription, each member being called on to donate 2 pounds (= $4).[100]

Mutual co-operation between scientists inside and outside universities was common in Australia. Co-operation between the general public and the scientific community as instanced by the Frankenberg survey was rarer. But sponsorship of that type redoubled the scientists' social obligations. It would not have been easy for the Australian ecologists to retreat to their laboratories as Nelkin described for the American ecologists. There were too few ecologists for anonymity to be an option. There has been substantial discussion of the problems of isolation in Australian science, sometimes described as the 'Tyranny of distance' debate (after the title of a book by Geoffrey Blainey). One of the products of isolation is unexpected alliances.[101] If there are few experts, then barriers between university and other scientists cannot be maintained.

The green environmental movement

The persistence of the watershed metaphor for twenty years after the Little Desert dispute in memories of the surviving conservationists has another, unexpected, explanation. The Little Desert dispute drew together diverse conservation philosophies in 1969 but, with hindsight, it also marked a parting of the ways. The Little Desert dispute was the pinnacle of the campaigning career of the older conservationists. After that, many felt them-

[100] Judith Frankenberg (edited by J. S. Turner), *Nature conservation in Victoria*, Melbourne, 1971.

[101] See for example: R. W. Home, 'The problem of intellectual isolation in scientific life: W. H. Bragg and the Australian scientific community, 1886–1909', *Historical records of Australian science*, (1984) 6 (1), pp. 19–30; John G. Jenkin, 'Frederick Soddy's 1904 visit to Australia and the subsequent Soddy–Bragg correspondence: Isolation from without and within', *Historical records of Australian science*, (1985) 6 (2), pp. 153–169; David Wade Chambers, 'Does distance tyrannise science?' in R. W. Home and Sally Gregory Kohlstedt, eds., *International science and national scientific identity*, Dordrecht, 1991, pp. 19–38. In a conference paper, Rod Home described the unexpected alliance between the University of Western Australia's Physics Department and the staff of the Carnegie Institution of Washington at the Watheroo Magnetic Observatory in outback Western Australia (R. W. Home, 'Humboldtian science revisited: An Australian case study', paper presented at the annual conference of the Australasian Association for the History, Philosophy and Social Studies of Science, Perth, 6 July 1991).

selves marginalized. For it was in the early 1970s that a new breed of conservationists emerged, the 'greenies'.

The utilitarian conservationists and resource managers, enthusiastic supporters of the 1960s campaign were the big losers in the new style conservation movement which developed in the 1970s. The first greenies — and the diminutive is an original Australianism[102] — were building workers. Members of the New South Wales Builders Workers Federation used their collective power to institute 'green bans' (a variant of the union term 'black ban') prohibiting projects endangering areas or buildings of historic or environmental significance. The first of these focused on a housing development in Kelly's Bush, near Sydney, beginning late in 1970. After two decades of relatively full employment, Australian unions were (by international standards) in an unusually strong position to pursue overtly political causes, and the environment, including such issues as uranium mining, was one cause pursued vigorously throughout the 1970s.[103] The first green activists were concerned with urban environmentalism rather than nature conservation. The union activists, in particular, were concerned about political power as well as quality of life.

It was significant that the first of the green environmentalists emerged with a political agenda out of the radical end of the trade-union movement. They abhorred the old networks — the 'Old Boys' clubs, and the elitist organization of power which professionals in the early twentieth century had worked to build up. Their organizational style was that of a flat hierarchy, anathema both to senior bureaucrats (who had 'served time' to build up their seniority) and to patriarchal older community leaders.

A left-wing model which gave voice to the 'little people' was foisted on to a movement which had run for more than two decades on a 'who-knows-whom' basis. The obvious advantage of the 1950s and '60s organizational style (to those in the system) was its 'efficiency'. A second advantage was that, because of the apolitical nature of such networks as the Field Naturalists, political differences were blurred and the movement was free to lobby all political parties, having in no sense a preconceived voting preference. The conservationists concentrated their efforts on unseating the problem minister, not toppling the government. It was a rare movement indeed which united the voices of communist sympathizers and Liberal (conservative) Party campaign managers. It was hardly surprising that such an alliance would prove to be temporary.

The Vietnam Moratorium years (of the late 1960s and early 1970s) had given the younger generation a grand organizational vision and a sense of power to achieve its aims. When military conscription was abolished in 1972,

[102] William Ramson, 'Wasteland to wilderness: Changing perceptions of the environment', in D. J. Mulvaney, ed., *The humanities and the Australian environment*, Canberra, 1991, pp. 5–19, esp. pp. 17–18. [103] Falk, op. cit. (6), pp. 247–55; Lines, op. cit. (16), pp. 228–30.

a whole radical generation found itself free to seek another cause. The environment beckoned.[104] The cost of importing moratorium-style organizational structures into the existing conservation movement was, however, the loss of many of its older supporters. Some of these were at retirement age anyway, but a significant number were in senior positions of authority, and would have liked to continue to make incremental changes from within the system. Alfred Dunbavin Butcher, Director of Victoria's Department of Fisheries and Wildlife, bemoaned the fact that the conservation movement was suffering from a backlash 'prompted by the inability of environmental activists to recognize a victory when they scored one'.[105] Butcher was one of seven members of council of the Australian Conservation Foundation who resigned in 1973 because of concern that a radical takeover had occurred within that body.

Organizational structure was a significant problem. The so-called takeover of the Conservation Council of Victoria by young members of new green groups like Friends of the Earth in the mid 1970s was as much a vote of no confidence in the older style of organization as a protest on philosophical grounds.[106] Strong evidence for this came from the CCV nominations for the conservation vacancies on the Land Conservation Council in 1978. The CCV is responsible for nominating five candidates from which the Government of the day chooses two LCC members to represent conservation interests. John Turner and John Landy, both foundation members of the LCC and senior members of the conservation community with many decades of experience, stood for nomination, but failed to be chosen by the CCV. The CCV wanted younger, more accountable members who kept the Council closely informed of the LCC's deliberations. As a university professor, Turner was used to having the authority to make executive decisions on behalf of his department. He expected the same in his role as member of the Land Conservation Council representing the conservation movement. Turner and others like him did not see a need for the endless committee level discussions and decisions demanded by a truly open democracy.[107] The young committed conservationists whose other responsibilities were less burdensome, wanted more radical changes than the older leaders could (or

[104] The women's movement was also growing at this time, and it appears that the earliest post-Vietnam environmental groups such as Friends of the Earth and Movement Against Uranium Mining were in fact predominantly male groups until the latter part of the 1970s, when feminist enthusiasts joined both movements. (personal communication, Pat Jessen, Maria Georgiou (Friends of the Earth) 26.IV.91).

[105] A. D. Butcher, interview by Jennifer Byrne, the *Age*, 23.II.78, p. 4. When he was appointed Director of Fisheries and Wildlife in 1949, Butcher was the first science graduate to head a state government department in Australia.

[106] Lewis Godfrey, first secretary to the CCV (seconded from the NRCL), personal communication, 24.XI.89. Confirmed by Geoff Wescott, Director CCV 1979–81, personal communication, 18.XII.90.

[107] Turner, transcript of Tape 2 of interview, 28.VIII.90, pp. 9–10.

perhaps wanted to) deliver. They also wanted a share of the power, and had a deep-seated scepticism about any sort of centralized structure.[108]

However, at least in some cases, organizational structure was a symptom rather than the cause of the fracture of the conservation movement. Environmental philosophy had taken a new turn. Practical philosophers concerned to give voice to oppressed minorities looked anew at the relationship between humans and Nature. The oppression seemed in this case to relate to the biota itself.[109] This radical view percolated down to more moderate thinkers. Suddenly utilitarian conservation views became unfashionable. 'Wise use' and 'intensified production' were seen as exploitative concepts.

The new environmentalists were anxious to distance themselves from an anthropocentric view of natural resources. In the 1960s 'ecology' was rescued from the relative obscurity of a sub-speciality within biology and popularized as a more general interdisciplinary science of the interaction of all biological species in an ecosystem. It drifted further from its scientific roots to emerge as a new world view. 'Deep ecology' and 'geophysiology' were manifestations of the radical end of the public spectrum.[110] The green rhetoric reflected a new-found geocentrism and a promotion of the earth itself to the status of a living being.

New earth-centred philosophies have excited some of the older campaigners. Gwynnyth Taylor, founder of the Save Our Bushlands Action Committee, is optimistic about the twenty-first century. She feels that we are living in stirring times in the 1990s. She commented that there is still a need for more consciousness of what 'saving the environment' means, a fuller understanding of habitat, not just of saving a few species, but she says that the new activists are on the 'right track'.[111] Ros Garnet, veteran of the 1950s campaign for National Parks, feels they did not go far enough in the 1960s, although he thought they were doing the right thing at the time. With hindsight, he wishes that he had had the vision to conceive the large National Park that the Little Desert area became in 1988.[112]

However, the new greens have antagonized many committed older conservationists. Some still refuse to acknowledge that conservation could mean anything other than the wise use of resources by humans. Many of these were, like Butcher, conservation professionals. The group most seriously alienated by the new environmentalists are the foresters. Professional foresters were at the heart of the conservation movement in the 1940s, '50s and '60s, some working very hard behind the scenes to block the Little Desert

[108] Falk, op. cit. (6), pp. 287–90.

[109] This case is neatly made for the discipline of environmental history in Donald Worster, 'Doing environmental history', in Donald Worster, ed., *The ends of the earth*, Cambridge, 1988, pp. 289–307.

[110] Lovelock, Goldsmith, op. cit. (1).

[111] Taylor, transcript of interview, 25.11.90, p. 9.

[112] Garnet, transcript of interview, 20.IV.89.

development. Their profile was not high in this work because as public servants they felt limited in what they could say. The 1970s marked a drift in opinion which left professional foresters stranded on the wrong side of conservation debates. By the late 1970s the forestry profession was feeling decidedly defensive: 'Pity the poor forester' was the title of one paper at the 1977 Australian Institute of Foresters conference in Adelaide.[113] In another paper at the same conference, L. M. Duffy stated:

> Forests and timber industries are criticised in pro-rata terms to a much larger extent than other comparative land-users. So much so, that they appear to rate only a little behind mining industries in popular censure.[114]

When the Forests Commission itself was absorbed into a larger Department (of Conservation, Forests and Lands) by the newly elected Victorian Labor government in 1983, some felt the profession itself had been betrayed.[115] I asked one retired forester to define a greenie for me since he clearly felt such antipathy towards them. His response was:

> a radical — people with radical views who . . . in case of the forest, for example, their aim is ultimately to stop all utilization in all native forests. Now that is impossible and it is ridiculous and it is just not done. They're the ones that have been listened to and they don't know what they are talking about — if you see them interviewed on television they just don't know what they are talking about. And they're just bloody ratbags to my mind. But they're the ones that governments are listening to.[116]

This sort of antagonism is a product of the new environmentalism. The new conservation movement itself adopts an aggressive stance. It has now identified many 'enemies of conservation'. For example, Friends of the Earth blacklists products which are made by companies with environmentally unsound policies.[117] The conservationist of the 1990s has a confidence that there are enough people on the side of conservation to exclude some, to create outcasts. In the 1960s there was no such certainty, and more embracing alliances were possible.

The conservationists of 1969 left two very important legacies which have had an impact on the management of conservation in Victoria in the past

[113] *Focus on the forester*, proceedings of the 8th annual conference of the Institute of Foresters of Australia, Adelaide, 1977.

[114] Ibid.; L. M. Duffy, 'Impacts and Influences', Section 2, p. 1.

[115] Middleton, transcript of interview, 9.11.90; the term 'forester' was dropped when the Forests Commission became part of the Department of Conservation, Forests and Lands (later the Department of Conservation and Natural Resources).

[116] Ibid., p. 16.

[117] One example of this was the A3-size insertion in the Victorian branch of the Friends of the Earth newsletter *Nuke news*, July 1990, entitled 'Killing corporations'.

twenty years. The Land Conservation Council and the Conservation Council of Victoria have made contributions of which all conservationists are proud. Although criticisms have been made (especially of decisions relating to the Victorian Alps),[118] there was general agreement amongst those that I have interviewed that the Land Conservation Council has worked reasonably well as a body for ensuring full public consultation about the uses of Crown land. Other Australian states have shown interest in its procedures, and in the way it allows for community participation in decision-making processes.[119]

Disputes about the use of public land resources are generally difficult to resolve. The more voices and views considered, the more unlikely a satisfactory resolution will be found easily. Yet the LCC, with its extensive public consultation and community representation, has in fact managed to generate considerable support for its recommendations. It has relied heavily on the so-called objective and undoubtedly socially acceptable evidence of scientists and technical experts in the conflict resolution process. Even the Aboriginal community condoned the advice of scientists with respect to the Little Desert. David Anderson, Tribal Advocate for Wimmera Aborigines, called for political and economic interests to be put aside 'so that the public world interest can be dealt with by the most competent of the incompetent, the scientists'.[120] The Little Desert dispute made decision-making about land development public property. No longer could scientific advice be ignored if the Minister so chose.[121] The dispute legitimized scientists and technical arguments as a dimension of the public interest in land management matters. The LCC processes ensure that the evidence of biological scientists contributes to every major land management decision, as a matter of course.

Economists have always provided evidence for decision making processes in land management. What the Little Desert dispute suggested was that ecologists could and should also offer evidence of a different nature, and that such evidence should be freely available to the general public. This was particularly acceptable in 1969 because of the unusual congruence of opinion between economists and ecologists with respect to the Little Desert scheme. Scientific consultation became enshrined in the procedures designed for the

[118] Community Research Action Centre, *Wilderness to waste: The ecology, politics and economics of the Victorian Alps*, Clayton, 1981; David Mercer, 'Victoria's Land Conservation Council and the Alpine region' *Australian geographical studies*, (1979) 17 (2), pp. 107–130.

[119] In 1991 the Queensland government was considering a similar body, and sought advice from Victoria's LCC. Tasmania also has had discussions with the Chairman of the LCC, David Scott. (Calder (member of LCC since 1982) personal communication, 8.v.91)

[120] NRCL News, *Victoria's resources*, (1970) 12 (2), p. 33.

[121] The earlier Land Utilization Advisory Council had access to scientific and technical advice from within the Victorian Public Service, but if the advice was ignored, as McDonald chose to do with the LUAC's 1968 report on the Little Desert proposal, there was no mechanism for making the scientific recommendations available to the public.

LCC. Thus one result of the passionate and emotional campaign fought over the Little Desert was the promotion of dispassionate scientific opinion as a tool for the resolution of conflict. This was not unique to Victoria or Australia. All over the world scientists and technical experts were increasingly being called on to resolve environmentally sensitive issues.[122] In Victoria the Little Desert dispute was the trigger for this process.

The term 'conservation' continues to have multiple meanings. Conservationists may be 'light green', environmentally conscious consumers, or 'dark green', radical deep ecologists.[123] Other conservationists eschew the designation green altogether. But for people holding all these shades of opinion the Little Desert became a cultural icon. The *Macmillan dictionary of the Australian environment*, predominantly a dictionary of technical terms, lists 'Little Desert Scheme' as an item.[124] It is there because it was abolished 'by public pressure'. Another conservation issue considered to be of national importance at about the same time as the Little Desert was the dispute over the flooding of Lake Pedder in Tasmania by the Tasmanian Hydro-Electric Commission. But despite concerted efforts by Tasmanian and mainland activists for seven years, the lake was dramatically increased in size in 1974, flooding its distinctive sandy beaches.[125] While 'Lake Pedder' is an icon of loss for Australian conservationists (like Hetch Hetchy in the United States), the Little Desert is a symbol of hope, a reassurance that pressure can work.

The Little Desert also coincided with and reinforced a shift in ecological consciousness. Utilitarian conservation was on its way out, with a growing opposition world-wide to technocratic quick fixes. But this issue allowed it

[122] The introduction of technical experts did not always in fact resolve the issue in question. For example, in a contemporaneous American dispute near Cornell University in New York state (1968–1973), Dorothy Nelkin wrote of the problem of conflicting technical advice given by scientists working for a local community group and those working for the New York State Electric and Gas Company, which was attempting to build a Nuclear Power Station on the shores of Cayuga Lake (Nelkin, 'Scientists in an environmental controversy', *Science studies*, (1971) 1, pp. 245–261; and eadem, 'The political impact of technical expertise', *Social studies of science*, (1975) 5, pp. 35–54). However, the dispute illustrates the way in which scientific advice was deemed (by both sides) to be the tool to resolve the question, despite the new level of problems it created.

[123] The 'shades of green' terminology has become common in Australia over the past couple of years, and appears to be used by writers with very different environmental objectives. For example, in the *Age* (1.v.90, p. 17), Geoffrey Blainey is reported as using the light/dark green distinction in an address concerned with the interference by green activists with what Blainey considered to be good economic management of the country. In a politically different context, Rosaleen Love reports British environmentalist Jonathon Porritt using the distinction at an ecopolitics conference in Sydney in April 1991 ('Should Australia have a green party?' *Australian society*, May 1991, pp. 6–7).

[124] David Meagher, *The Macmillan dictionary of the Australian environment*, South Melbourne, 1991, p. 180.

[125] The Save Lake Pedder Committee was founded in 1967. The period from 1972 onwards marked the peak of opposition. See William J. Lines, op. cit. (16), pp. 222–4, for details.

a last 'fling', and the older-style conservationists were the leaders of the movement. It is not accurate to describe the opponents of the Little Desert as 'eco-activists' (as William Lines has done).[126] They came from a different conservation tradition. Their style of operation was lobbying, negotiating, chatting with friends in high places. There was no direct action, no radical or 'Earth First!' element which opposed the government by non-violent or violent means. In Samuel Hays' terms, they were still concerned with nature conservation rather than environmentalism.[127]

As has been shown, the Little Desert dispute marked both a convergence and a divide in the conservation movement. It was indeed a watershed. But the very appropriation of the watershed metaphor itself described a certain type of ecological consciousness. A technical utilitarian conservationist term from the language of scientists moved into the realm of widespread metaphor. The choice of metaphor itself has dated. Latter day greens label their conservation victories as battles fought and won (though often by non-violent means).[128] The Little Desert dispute was about negotiation, not war, and working the system, not revolution.

[126] Ibid., p. 225. [127] Hays, op. cit. (35), esp. ch. 1, 'From conservation to environment'.

[128] Ian Watson, *Fighting over the forests*, Sydney, 1990 (for example, 'Sally Johnston', pp. 83–4); and J. G. Mosley and J. Messer, eds., *Fighting for wilderness*, Sydney, 1984 (Part 1: 'Battles lost and won'; Part II: 'Winning future battles') are two Australian examples.

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An elusive science: Ecological enterprise in the Southwestern United States

SHARON E. KINGSLAND

1. *Introduction*

Ecology in the United States began in the late nineteenth century, but its viability as a separate area of research remained problematic for scientists for several decades, despite promising beginnings. Ecologists saw their work as a contribution to basic biological research, which might include, but was not limited to, research relevant to problems in agriculture, resource management and conservation. Certainly the reform of botany that preceded the development of ecology was tied to agricultural practice,[1] and ecologists addressed practical problems in part to attract funding and to reinforce the central importance of ecological study. But ecology was conceived as a kind of inquiry that went beyond these pragmatic concerns. Ecologists also argued for research centres distinct from publicly funded agricultural colleges and experiment stations. Indeed, a division of labour between privately endowed research laboratories and publicly endowed agricultural stations was essential to the promotion of ecology as a discipline. This article is about one of the earliest ecological research centres in the United States, the Desert Botanical Laboratory founded in 1903 near Tucson, Arizona, by the Carnegie Institution of Washington.

The first director of the Desert Laboratory was Daniel Trembly MacDougal (1865–1958), a physiologist and ecologist who oversaw botanical research for the Carnegie Institution from 1906 to 1928, after which he continued as research associate until 1934. In 1909 the laboratory extended its operations to a second laboratory, located at Carmel, California, which

I thank Eugene Cittadino and Paul Romney for their comments on the manuscript. Research was supported by National Science Foundation summer scholar grant no. 9022280.

[1] The relation between botanical reform and agriculture, especially in connection with Charles E. Bessey's school of botany at the University of Nebraska, is discussed in R. C. Tobey, *Saving the prairies: The life cycle of the founding school of American plant ecology, 1895–1955*, Berkeley, 1981. Bessey was suspicious of the new ecology, which he regarded as faddish, but his students went on to establish the discipline along more rigorous lines, focused on the analysis of successional change.

from 1920 became MacDougal's main base. By 1940 the Carnegie Institution's commitment to ecological research was waning. Under growing financial pressures the Desert Laboratory was closed and the buildings handed over to the US Forest Service.

MacDougal is not well remembered because he did not write a seminal work in ecology. Despite his absence from the ecological canon, he was important for ecology as an institution builder whose entrepreneurial drive made it possible for ecologists to secure a niche, however briefly, as research scientists. MacDougal's laboratory differed from universities and agricultural colleges in allowing scientists to pursue research unfettered by teaching duties or the need to provide practical advice to farmers. The laboratory was dedicated to a range of subjects, including experimental research in physiology and biochemistry, experimental and field investigations in ecology, plant geography and evolutionary biology, and studies of climate, geography and geology. In addition to serving as a microcosm of science in America, the laboratory offers us a glimpse of biology in its pioneer phase, when aggressive promotion of basic research was combined with the voyager's delight in adventure for its own sake.

The history of the Desert Laboratory provides an exceptionally good illustration of themes that historians of science have raised in other contexts. Eugene Cittadino, who has studied the origins of American ecology in the light of the flowering of German botanical research in the post-Darwinian period, has focused especially on the indebtedness of American ecologists to late-nineteenth-century German studies of adaptation and biogeography.[2] The present essay complements Cittadino's work by elaborating important themes that he touches on more briefly, concerning the American context of this science. Those addressed in the first section deal with the relationship between economic development and the growth of science, the professionalization of botany around the turn of the century, and the crucial role of the wealthy business patron in the establishment of new scientific institutions. Therefore we begin by looking westward from the eastern seaboard and considering the relationship between western exploration and the rise of scientific institutions in the east. The section ends with a discussion of the creation of the New York Botanical Garden in the 1890s, which stood as a model of entrepreneurial activism. Its success stimulated further institutional expansion, which in turn gave impetus to ecological research.

From New York we follow the trail back west again to the founding of the Desert Laboratory, an offshoot of the Botanical Garden. I shall describe the nature of the ecological research pursued at the desert laboratory and its implications for the study of evolution, a field fraught with controversy. By

[2] E. Cittadino, 'Ecology and the Professionalization of Botany in America, 1890–1905', *Studies in history of biology*, (1980) 4, pp. 171–198; idem, *Nature as the laboratory: Darwinian plant ecology in the German Empire, 1880–1900*, Cambridge, 1990.

touching on the relationship between MacDougal and western writer Mary Austin, I shall consider the way ecology and the mystique of the Southwest have been used to promote American mythology about the relationship between environment and national character. Finally, the reasons for the laboratory's closing are discussed and some generalizations are drawn about the fate of ecology in relation to competing scientific fields.

2. *An imperialist enterprise*

Beginning with William Goetzmann's vivid descriptions of American westward expansion and science, historians have revealed that what came to be called ecology in the 1890s was the culmination of a century of exploration that sent explorers, armies, and engineers to make surveys, capture territories and prepare for the development of the country's mineral and agricultural wealth.[3] Artists and naturalists, European and British as well as American, took part in the conquest and occupation of the West. Inspired by the monumental work of European scientists like Karl Ritter, whose studies of human populations in their physical environment eventually grew to 23 volumes, and Alexander von Humboldt, whose atlas of the region they relied on, they drew and described, surveyed and mapped, and collected specimens for scientists working back east.

International rivalries provided further impetus to exploration and information gathering. In the Southwest, decades of conflict between the United States and Mexico culminated in the Mexican-American War of 1846–48. The Treaty of Guadalupe-Hidalgo ceded most of the disputed territory of the Southwest to the United States in 1848, with the final boundary being fixed, after more accurate surveys, by the Gadsden Purchase of 1853. This left the Indian nations as the last rivals to be conquered. By about 1890, warfare and disease had reduced the indigenous population of the Southwest to less than one-third its number three centuries earlier. For the white races, the vast interior region, once labelled the Great American Desert, became not just a place to cross on the way to the Pacific, but a site for settlement. Thousands of settlers entered the territories of Arizona and New Mexico in the late nineteenth century.

Science also entered the new territories with a military escort. The Army Corps of Topographical Engineers was organized in 1838, partly in order to make scientific assessments of the country's natural and human resources. During the Mexican War the Topographical Engineers were assigned to each of the major field commands. The unit headed by Lieutenant William H. Emory, a self-styled savant who hob-nobbed with the leaders of the eastern

[3] W. H. Goetzmann, *Exploration and empire: The explorer and the scientist in the winning of the American West*, New York, 1966; also, D. Worster, *Nature's economy: A history of ecological ideas*, 2nd ed., New York, 1985.

scientific establishment, made an expedition in 1846 that provided the army with a more accurate map of the Southwest and yielded much new information on geology, botany, zoology, as well as studies of the Native American peoples and their history. The army would use this information to try to contain the Indian nations that came under American control at the end of the war. In the late 1840s, railroad surveys were conducted to determine the best routes for a transcontinental railroad, which was completed in 1869.[4] The Topographical Corps sent out several expeditions designed to lay out military and emigrant routes. The science that emerged from these surveys was the product of collaboration between explorers and the specialists in the east who advised the engineers, sometimes accompanied the expeditions, and worked up the collections housed in eastern institutions.[5]

From the late 1860s through the nineteenth century, geographical surveys continued in conjunction with military campaigns and the railway surveys. The major national surveys conducted under Clarence King, Ferdinand V. Hayden, and John Wesley Powell were modelled on earlier state geographical surveys, the most important being the California Survey conducted from 1860 to 1874 under Josiah Dwight Whitney. These surveys were designed to open up the country, make an inventory of its resources, and prepare the way for land use management.[6] Civilian surveys conducted under the Interior Department, such as the United States Geological Survey of the Territories, formed in 1869, competed with the army surveys. A scientific committee formed in 1878 under the auspices of the National Academy of Sciences recommended to Congress greater civilian control over settlement and economic development in the far west. After this time scientific surveys were mostly civilian rather than military.[7]

By the early twentieth century the emphasis was on conservation and efficient use of resources, with the government's resource policies being shaped by a new breed of professional scientific manager and conservationist.[8] These men would in turn help to steer private patronage towards the kind of

[4] Goetzmann, op. cit. (3), ch. 8.

[5] Goetzmann, (op. cit. (3), ch. 9) labelled this passion for collection and classification 'Humboldtian science' because of the spirit of Romanticism with which it was imbued. He was critical of the science for falling prey to a profusion of descriptive studies with little theoretical synthesis, as compared with the later accomplishment of Darwin in the *Origin of species*. Cannon rebutted this thesis by arguing that Humboldtian science was more than Romanticism, for it provided a model of scientific method based on the use of instruments and exact measurement. In fact the young Darwin might be considered the very model of a Humboldtian scientist. Winsor's study of the history of classification underscores how essential it was to advance the monumental labour of classification before Darwin could furnish a convincing synthesis. S. F. Cannon, *Science in culture: The early Victorian period*, New York, 1978, ch. 3; M. P. Winsor, *Reading the shape of nature: Comparative zoology at the Agassiz Museum*, Chicago, 1991.

[6] Goetzmann, op. cit. (3), chs. 12, 13, 14.

[7] Goetzmann, op. cit. (3), ch. 13.

[8] S. P. Hays, *Conservation and the gospel of efficiency: The progressive conservation movement, 1890–1920*, Cambridge, MA, 1959.

basic research in ecology that government agencies lacked the resources and mandate to pursue. There developed a symbiotic relationship between government experts, scientists, and the entrepreneurs made wealthy by the country's expansion. This convergence of interests created a demand for ecological research and ultimately provided the resources necessary to make ecology a viable field. The way these mutually reinforcing interests served to stimulate ecology is especially well illustrated in the creation of the Desert Laboratory, which arose as an offshoot of the New York Botanical Garden, itself a product of economic expansion and scientific entrepreneurship.

The force behind the Botanical Garden was Nathaniel Lord Britton, student and protégé of John Strong Newberry. Newberry had been the first geologist to study the Grand Canyon, which he visited with an expedition in 1857, and was professor of geology and palaeontology at the Columbia School of Mines. He was also an active discipline-builder, who had served on the National Academy of Sciences committee of 1878 that had recommended greater civilian control over scientific surveys. As president of the New York Academy of Sciences, he campaigned to promote public interest in scientific research, especially research related to the arts, manufacturing, and agriculture.[9] The professionalization of science in the late nineteenth century was the result of astute promotional activity by scientists like Newberry who were on the receiving end of western explorations.

Learning from his teacher, Britton took up the campaign in the botanical sciences. During the early 1880s, Britton had worked as botanist on the Geological Survey of New Jersey, earning in the process a PhD from Columbia College (later University). He joined the faculty in 1887, advancing to the rank of professor of botany in 1891. Recognizing the need for professional societies, Newberry and Britton transformed the Torrey Botanical Club from a loose association of naturalists, first organized in the 1850s by botanist John Torrey, into a formal society of national prominence backed by a national journal. The Torrey Herbarium, later donated to Columbia College, likewise became the nucleus for Britton's crowning achievement, the creation of the New York Botanical Garden, which was chartered in 1891 after intensive politicking and fundraising.[10] Inspired by a visit to the Royal Botanic Gardens at Kew in 1888, Britton envisioned a great public garden in New York that might serve as a research arm of Columbia. Fundraising was sluggish until Britton's wife and a group of society women 'took to the warpath' to raise the minimum endowment of a quarter of a million dollars.[11] Britton's success owed much to his intimacy with New York's

[9] D. Sloan, 'Science in New York City, 1867–1907', *Isis*, (1980) 71, pp. 35–76; S. Baatz, *Knowledge, culture, and science in the metropolis: The New York Academy of Sciences, 1817–1970*, New York, 1990. [10] Sloan, op. cit. (9); Baatz, op. cit. (9), pp. 142–47.

[11] E. D. Merrill, 'Nathaniel Lord Britton, 1859–1934', *Biographical memoirs of the National Academy of Sciences of the USA*, (1938) 19, pp. 147–202, especially p. 152.

elite, for whom the Garden became a pet philanthropic project: the officers of the board of managers were Cornelius Vanderbilt, Andrew Carnegie, J. Pierpont Morgan, and Britton himself.

In 1896 a second charter enabled the Garden to expand its functions to serve as the botany department of Columbia, at which time Britton resigned his chair of botany at Columbia to devote his full energies to the direction of the Garden. Over the next thirty years he converted a sizeable tract of raw land into a major garden and research centre that included a museum, greenhouse and conservatory, an extensive library and reference herbarium, and well equipped laboratories. Much of the Garden's work centred on collecting; in this respect its geographical reach expanded with that of the country. After the United States took possession of Puerto Rico following the Spanish-American War of 1898, and as American interest turned to Cuba, the Caribbean became a major focus for Britton's botanical expeditions. The Garden also made collecting expeditions into Central and South America and the Philippines.

Lucile Brockway has argued that Kew played a crucial role in empire-building by advancing scientific research and development of economically useful plants in the British colonies.[12] Cittadino points out that German botanists admired Kew as a model of how an experimental garden might advance colonial interests by cataloguing plants and conducting research abroad in colonial gardens.[13] But he cautions us not to assume that imperialist expansion made botanists focus especially on practical problems in the colonial territories. To the contrary, German academic botanists were just as interested, if not more so, in basic problems of plant physiology and evolutionary biology. The new environments they encountered in their colonial travels provided botanists with opportunities for testing general theories about adaptation that had been formulated in studies of European flora.[14]

The New York Botanical Garden was created as the United States was undergoing a similar expansion of territorial interests, coupled with the development of scientific agriculture and the search for exotic new crops. But with the proliferation of publicly funded agricultural experiment stations throughout the states and territories, there were abundant opportunities for research on economically useful plants outside New York. Britton's goals, like those of the German academics whom he saw as his competitors, were directed toward a different kind of empire-building: the creation of a major institution that would compete with the established European taxonomic museums. He wanted to build the Garden into a centre of taxonomic research whose reputation rested on the credit earned by the discovery,

[12] L. Brockway, *Science and colonial expansion: The role of the British Royal Botanic Gardens*, New York, 1979.

[13] Cittadino, *Nature as the laboratory*, op. cit. (2), p. 137.

[14] Ibid., pp. 134–45.

description, and publication of the new species brought to light by the Garden's expeditions.

Britton's grab for world power was achieved by means of a veritable taxonomic Blitzkrieg, an observer recalled.[15] Since 1888 he had been involved in debate over the need to reform the rules of nomenclature, that is, the rules used by taxonomists to standardize the practice of naming new species. No sooner had the Garden received its charter than he proceeded to write his own rules. At a botanical meeting in Rochester, New York, in 1892 he moved for a committee to report on nomenclature. Shepherded by Britton, who probably had the recommendations ready in his pocket, the committee took only a day to draw up a report which advocated a uniform system of nomenclature based on priority of publication and set out rules to determine whether a name was properly published. Most of its proposals were adopted at once, though amendments were made a year later at a meeting of botanists in Madison, Wisconsin.

Britton's new code, called the American Code, was defended on the ground that it would impose greater rigour on scientific work, but its strict implementation would have required changes in the names of many species and created confusion. It was criticized in Europe and caused a schism at home, with Harvard botanists adhering to the older Paris Code while Britton insisted on the American Code in the publications of the New York Botanical Garden. Britton, however, was less interested in uniform standards than in priority of publication. Having turned the Maginot Line of established procedure, he proceeded to rout the enemy by lightning-fast publication of new species descriptions, securing for his herbarium and his country the advantages that came from priority in publishing the vast volume of new discoveries made by his various expeditions. His motto was 'Get it into print'. He would send duplicate collections to his long-time European rival, Ignatz Urban of the Botanical Museum at Berlin, confident that before Urban could publish his erudite descriptions, based on careful dissections, his own brief diagnoses would already be in print and the type specimens, 'christened with a Brittonian name, safely deposited in the herbarium at the New York Botanical Garden.'[16]

While Britton was aggressively building the botanical sciences from his base in New York, ecology was coming into its own as a separate field of study. Immediately after the Madison meeting of 1893 at which Britton's new rules were debated, another committee of botanists convened to discuss nomenclature in plant physiology. They decided to adopt the term 'ecology' to distinguish their interest in plant adaptation from the narrower kind of physiological study normally undertaken. The inspiration for their work

[15] H. A. Gleason, 'The scientific work of Nathaniel Lord Britton', *Proceedings of the American Philosophical Society*, (1960) 104, pp. 205–26, especially pp. 208–15.

[16] Gleason, op. cit. (15), p. 222.

was a group of German botanists who were prompted by Darwin's theory to focus on the analysis of adaptation.[17] The Germans had called their work simply 'biology', but the Americans wanted a term more descriptive of their specialized type of research. Ecology thus began as a branch of physiology, one whose agenda was stimulated by evolutionary theory and experimental science, and whose focus was the relationship between organism and environment. Legitimation for this new field of ecology would rest on its claims for the superiority of experimental and quantitative methods in the study of adaptation, geographical distribution, and plant succession.

Ecological work prompted demands for research facilities; the expeditions that were uncovering so many new plant species in the late nineteenth century led to further demands that plants be studied in their native habitats and that research institutions be formed in areas that had interesting flora.[18] The government, involved in biological surveys and agricultural research, also wanted to encourage private spending on basic research that might be of use to the agricultural stations. In this respect the New York Botanical Garden stood as a conspicuous example of what could be accomplished with an aggressive, expansionist strategy. Scientists like Newberry and Britton served as models of scientific activism, whose rhetorical strategies could be emulated in building research programmes. As Douglas Sloan has pointed out, the New York activists employed a mixed rhetorical strategy designed to appeal to as wide an audience as possible.[19] They constantly stressed the payoffs of practical science in order to garner public support, but in fact were equally interested in fostering specialized or pure research, meaning research which would not be constrained by the need to solve specific practical problems. Within new sciences like ecology that emerged at this time, scientists would also have to develop strategies to negotiate the boundary between practical applications and basic research, maximizing the freedom of the scientist to set the research agenda, but appealing also to concrete or practical goals to attract patronage.

As with the Garden, co-operation between scientists and wealthy patrons was an important condition of success as the new science of ecology emerged. Just as Andrew Carnegie's patronage had been important for the New York Botanical Garden, so the Carnegie Institution of Washington, which he created with a ten million dollar endowment, would be seen as a crucial source of support for botanical research, especially in ecology, after 1901. And it must have appeared natural that the scientists behind the New York venture would have roles to play in the Carnegie Institution ventures

[17] Cittadino, *Nature as the laboratory*, op. cit. (2), especially ch. 8.

[18] On the interest in tropical biology in this period see J. B. Hagen, 'Problems in the institutionalization of tropical biology: The case of the Barro Colorado Island Biological Laboratory', *History and philosophy of the life sciences*, (1990), **12**, pp. 225–47. The New York Botanical Garden operated a tropical research station in Jamaica.

[19] Sloan, op. cit. (9).

as well. To follow the story from botanical garden to ecological laboratory, we must follow the trail back out to the Southwest, led by Britton's second-in-command, Daniel Trembly MacDougal.

3. A scientific empire in the desert

Daniel MacDougal, a midwesterner of Scottish ancestry, was every bit as energetic and aggressively entrepreneurial as Britton. While an undergraduate at DePauw University in Indiana in 1890, he had taught himself the rudiments of the new physiological botany from the laboratory exercises developed by Joseph Charles Arthur, professor at Purdue University. MacDougal worked through the exercises on his own, setting up on a windowsill laboratory his home-made apparatus made from lamp chimneys, remodelled alarm clocks, kitchen appliances and tinware.[20] From 1890 to 1893 he worked as an instructor in Arthur's laboratory and eventually received a doctoral degree in plant physiology from Purdue in 1897. The research that earned the degree was completed under Wilhelm Pfeffer at Leipzig, where MacDougal had gone to get more experience in advanced research.

An enthusiastic traveller, MacDougal had worked in 1891/92 as an agent for the US Department of Agriculture (USDA). Equipped with Navajo blanket, tin cup, and a jar of beef extract, he roamed and collected through Arizona and Idaho. In 1898 he studied the desert and alpine flora of Arizona for the USDA and experimented at the western edge of the lava desert near the Little Colorado River. In 1899 Britton brought MacDougal into the New York Botanical Garden as a research assistant. In this capacity he explored parts of Idaho, Montana, Arizona and Mexico, bringing back from the Southwest four tons of cacti to enrich the Garden's collections. In 1904 he was promoted to assistant director of the Garden's botanical laboratory.[21]

The government-sponsored botanical surveys in which MacDougal had participated departed from the norm. Before the 1890s these had focused on the geographic distribution of animals, but in 1890 Congress enlarged their scope to include botany. That same year, Frederick Vernon Coville of the USDA made a botanical survey of Death Valley, California, and was surprised to discover, among other things, that the perennial shrubs and annual plants of the desert appeared to have few adaptations for water storage.[22]

[20] I have inferred this description of MacDougal's first laboratory from comments made in D. T. MacDougal, 'A half century of plant physiology', *Annals of the Missouri Botanical Gardens*, (1932) **19**, pp. 31–43.

[21] A. D. Rodgers, *John Merle Coulter, missionary in science*, Princeton, New Jersey, 1944, pp. 227–28; G. T. Moore, C. S. Gager, and F. Shreve, 'Daniel Trembly MacDougal, pioneer plant physiologist', *Plant physiology*, (1939) **14**, pp. 191–202.

[22] F. V. Coville, *Botany of the Death Valley expedition*, Washington, 1893; A. D. Rodgers, op. cit. (21), pp. 225–27.

Such observations made botanists question how closely plants were adapted to their environment, and how to assess the adaptational significance of specialized parts in general. Studies of adaptation were crucial for ascertaining the role that natural selection played in originating species, a subject of great controversy at that time. Upon returning from the Death Valley expedition, Coville hoped to create an ecological research centre specifically to study desert vegetation. For a partner in this enterprise he turned to MacDougal, esteemed by his friends as 'a desert specialist of the first magnitude, and a jolly good fellow all the time and everywhere', not to mention a mighty good shot with his old Winchester carbine.[23]

An opportunity presented itself when the Carnegie Institution of Washington was founded in December 1901, with a mandate to seek out exceptional men and promote their research. Coville chaired the Carnegie's advisory committee on botany, aided by Britton and two other botanists of note, John M. Macfarlane and Gifford Pinchot, US chief forester and prominent conservationist. They recommended as a major project the establishment of a desert laboratory.[24]

The strategy for opening a research niche emulated the arguments being used by Newberry and Britton in their campaign for public support of science in New York. There were practical reasons for setting up a scientific laboratory in the desert, for the National Reclamation Act of 1902 provided for the construction of irrigation projects using funds from the sale of public lands in the west.[25] Pinchot and Coville were both active in establishing government policy in conservation and resource management. Coville's special area of expertise was range conservation; his studies supplied crucial technical advice for the Public Lands Commission, which turned to the problem of formulating a range policy in 1903.[26] The committee report for the Carnegie Institution drew attention to the need for scientific studies that would help to assess the prospects for population expansion and agriculture in the Southwest:

> The economic ground for the establishment of such a laboratory is the enormous development of population and industries that is bound to take place in our arid region during the next hundred years. The basis of that development is agriculture, both with and without irrigation. At the present time comparatively little is known about the peculiar fundamen-

[23] W. T. Hornaday, *Camp-fires on desert and lava*, New York, 1914, pp. 4, 23; Cittadino, 'Ecology and the professionalization of botany', op. cit. (2), pp. 182–84.

[24] 'Report of the Advisory Committee on Botany', *Carnegie Institution of Washington yearbook*, (1902) 1, pp. 3–12.

[25] The consequences of this act are discussed in D. Worster, *Rivers of empire: Water, aridity, and the growth of the American West*, New York, 1985.

[26] Hays, op. cit. (8), pp. 55–56.

> tal processes of plant growth under the unusual conditions surrounding plant life in that region.[27]

Coville did not intend that the future Carnegie laboratory should be tied exclusively to practical concerns, however. Such work could be left to the agricultural experiment stations that were being established in the South-west. The practical subjects pursued at those stations, even when aimed at studies of soil and climate and the effect of irrigation on plant growth, did not constitute a research programme in ecology, which addressed broader questions about the relationship between plants and their environment with the aim of clarifying the nature of adaptation and the mechanisms of succession and evolution. Coville's idea was to provide basic research that might be used by the agricultural experiment stations in the arid states but would not be limited to the practical needs of these stations.

The combination of an appeal for pure research to advance scientific knowledge, but with the promise of eventually contributing in a practical way to economic development, appealed to the Carnegie officers. The Board of Trustees approved the plan in 1903 with an appropriation of $8,000 to establish a desert laboratory and pay for a resident scientist. Coville and MacDougal were immediately dispatched west to find a suitable site. After surveying a large area along the Mexican boundary they settled on a small mountain near Tucson, Arizona, described by an early-twentieth-century visitor as

> a wide-spreading, wide-awake little city on a level, sub-tropical plain that is encircled by granite mountains; a city with a strong Mexican accent, a city neither fast nor slow; a city with wide, clean streets, good buildings, abundant electricity and all the respectable concomitants of a metropolis — this is Tucson, Queen City of cactus-land.[28]

It was originally the site of a Papago Indian village, the Papagos being accomplished desert farmers well before the arrival of Spanish missionaries in the seventeenth century. The land was acquired from Mexico as part of the Gadsden Purchase of 1853. The Southern Pacific Railway put the town less than four days' travel from New York and just over one day from San Francisco, and by MacDougal's time Tucson had grown to a town of 10,000. The laboratory location combined the advantages of a varied desert flora, including mountains and plain, with easy communication access by telephone to Tucson and by telegraph and cable to the greater world. Geographically it was well placed for scientific expeditions to the deserts of the south-western United States and northern Mexico.[29]

[27] 'Report of Advisory Committee', op. cit. (24), p. 5.

[28] Hornaday, op. cit. (23), p. 14.

[29] F. V. Coville and D. T. MacDougal, *Desert Botanical Laboratory of the Carnegie Institution*, Washington, 1903, pp. 12–17. For brief sketches of the laboratory's history, see the

MacDougal and Coville were gratified to find the 'progressive Americans' who ran the city well disposed toward science. Their appreciation was expressed by subsidies of land for the building site and a nature preserve, the installation of a water system, telephone, electricity, and a rough road from Tucson.[30] The convenience of the location was enhanced by the presence of an agricultural experiment station, created in 1890 in conjunction with the opening of the University of Arizona in 1891.[31] A one-storey laboratory was duly built in 1903 and botanist William Austin Cannon from the New York Botanical Garden was installed as its resident investigator.

MacDougal's initial connection to the laboratory was as a visiting researcher. However in December 1905 the Carnegie Institution created a Department of Botanical Research in order to correlate the work of the Desert Laboratory with botanical work conducted at the other Carnegie stations, in particular with the work of the Station for Experimental Evolution, founded in 1904 at Cold Spring Harbor, on Long Island just outside New York City. In January 1906 MacDougal became the full-time director of this department and of the Tucson laboratory. He spent only part of the year in Tucson, returning regularly to New York to supervise experiments still in progress at the Botanical Garden.

In Tucson MacDougal at once set about improving the buildings and grounds and in general extending the range of operations of the laboratory. He brought in his friend and fellow explorer Godfrey Sykes, the English civil engineer and geographer, whom he had accompanied on explorations of the Southwest while employed at the Botanical Garden. Sykes had lived in the Southwest since 1895, indulging a scientific interest in the Colorado River, its delta, and the Gulf of California, regions that were relatively unknown at that time.[32] He applied his engineering skills to expanding the laboratory building, adding a machine shop, and surveying and fencing the 800 acres of mountain and mesa land around the laboratory to prevent the destruction of vegetation by grazing cattle and return the land to its original state. The laboratory grounds were converted into a nature preserve to fulfil the scientific goals of studying ecological relations in an environment where the effects of human activity had been erased. The preservationist ethos embodied in the laboratory extended beyond its borders as well. MacDougal was interested in the preservation of Native American pueblo culture, and

pamphlet by Ray Bowers, 'Mr. Carnegie's plant biologists: The ancestry of Carnegie Institution's Department of Plant Biology,' Carnegie Institution of Washington, Washington, 1992; R. P. McIntosh, 'Pioneer support for ecology,' *BioScience*, (1983) 33, pp. 107–12.

[30] Coville and MacDougal, op. cit. (29), p. 14.

[31] M. Mitchell, 'The founding of the University of Arizona, 1885–1894,' *Arizona and the West*, (1985) 27, pp. 5–36.

[32] G. Sykes, *A westerly trend . . . : Being a veracious chronicle of more than sixty years of wanderings, mainly in search of space and sunshine*, Tucson, 1984.

resident ecologist Forrest Shreve became active in the local desert conservation movement in the inter-war years.[33]

Those who wished to avoid commuting to Tucson lived in tents on the laboratory grounds, so the laboratory grew into a small community of families, with the wives of the staff sharing in the scientific work.[34] Ecological and physiological studies were pursued by Cannon and by resident botanists Burton E. Livingston and Volney M. Spalding. Spalding retired in 1909 and Livingston left for Johns Hopkins University, returning to the desert only in the summers. In 1908 MacDougal brought ecologist Forrest Shreve into the laboratory and in 1910 hired physiologist Herman Augustus Spoehr. Spoehr eventually achieved eminence for his studies of carbohydrate metabolism and photosynthesis. Shreve had been working at the New York Botanical Garden's tropical research station in Jamaica. Impressed by this work, MacDougal offered him a research position that would enable him to quit teaching and finish it. Afterwards, enticed by the prospects of the Tucson environment that MacDougal laid before him, Shreve concentrated on desert ecology. The laboratory also controlled the journal *Plant World*, which under Spalding's and Shreve's editorship was transformed from a magazine for high school teachers into a respected scientific journal. In 1919 the journal was handed over to the Ecological Society of America and continued under the name *Ecology*. MacDougal, Cannon, and Shreve had all been founding members of the Society, created in 1915.[35]

Most of the daily work was limited to a 25-mile radius, representing a single day's travel for the riding and pack animals the scientists used, but MacDougal had his sights on more distant terrain. In 1906 he set up two experimental sites on the slopes of the nearby Santa Catalina mountains, a distance of about 30 miles or two days' ride on horseback. This gave them, in addition to the desert climate of the Tucson region, a dry mountain and a temperate mountain environment. Occasionally longer expeditions were undertaken to desert regions further afield. In 1906 MacDougal, Sykes, and other laboratory workers began a long-term study of the Colorado River delta and the Salton Sea, a temporary lake formed by the overflow of the Colorado River and located 300 miles to the west of Tucson. In 1907 MacDougal and Sykes, along with William T. Hornaday, the director of the New York Zoological Park, explored the Pinacate Mountains and deserts of northern Mexico; a detail of the Mexican fiscal guard ensured their safe passage across the border.[36] Sykes mapped these regions for the first time. In

[33] J. E. Bowers, *A sense of place: The life and work of Forrest Shreve*, Tucson, 1988.

[34] Effie Spalding and Edith Shreve were regular scientific contributors.

[35] Bowers, op. cit. (33).

[36] D. T. MacDougal, 'Department of Botanical Research', *Carnegie Institution of Washington yearbook*, (1909) 7, pp. 70–72.

1910 Cannon made an expedition to the Algerian Sahara, followed by MacDougal and Sykes, who explored the deserts of Egypt and Libya.

As the Tucson operation expanded, the more congenial climate of California beckoned to the north-west. Cannon had gone to Carmel in 1907 and 1908 to investigate the work of Luther Burbank, the private breeder whose much-publicized creation of new plant varieties had attracted the interest of the Carnegie Institution, which had granted him support for five years starting in 1905.[37] The potential economic importance of Burbank's work was sufficient reason for support, but as Burbank operated intuitively, writing little down, the Carnegie Institution insisted on sending one of its own scientists to work alongside the illustrious breeder, with the aim of interpreting his methods in modern scientific terms. MacDougal had been involved in these negotiations to organize the Burbank project. In fact Robert S. Woodward, president of the Carnegie Institution in 1906, had used the Burbank study to advocate hiring MacDougal as permanent director of the Tucson laboratory, on the grounds that he would be better able to co-ordinate and oversee the scientific investigations into Burbank's results.[38] MacDougal originally recommended that Woodward send geneticist George Shull from Cold Spring Harbor, but Burbank did not get along with him and Cannon was sent in his place.

While in California Cannon made contact with the Carmel Development Company, which provided him with laboratory space and expressed an interest in helping to set up a coastal laboratory there. MacDougal took the idea to Woodward, who accepted the company's offer. In 1909 the Carnegie Institution established a coastal laboratory at Carmel, a centre of artistic and intellectual life that served as a summer retreat from the scorching desert heat for the Tucson staff.[39] The Carmel laboratory added a cool, damp coastal environment to those of desert and mountain. These areas were used for transplantation experiments, in which species from outside the region were introduced and their survival charted. In this way the factors limiting the distribution of plants and the ability of plants to adjust to new environments could be studied.[40]

Some of the expenses accumulated from the constant expansions and expeditions prompted questions from the Carnegie Institution. As early as 1906 Woodward reprimanded MacDougal for what he believed was an in-

[37] N. Reingold, 'National science policy in a private foundation: The Carnegie Institution of Washington', in A. Oleson and J. Voss, eds., *The organization of knowledge in modern America, 1860–1920*, Baltimore, 1979, pp. 313–41.

[38] S. E. Kingsland, 'The battling botanist: Daniel Trembly MacDougal, mutation theory, and the rise of experimental evolutionary biology in America, 1900–1912', *Isis*, (1991) **82**, pp. 479–509.

[39] W. G. McGinnies, *Discovering the desert: Legacy of the Carnegie Desert Botanical Laboratory*, Tucson, 1981, pp. 10–12.

[40] D. T. MacDougal, 'The reactions of plants to new habitats', *Ecology*, (1921) **2**, pp. 1–20.

flated budget. Woodward cast a jaundiced eye on such expenses as a team of draught horses and mounted assistants to help maintain the laboratory and accompany the expeditions.[41] By 1910 MacDougal was using a second-hand car, which he and Sykes specially outfitted for their expeditions. Car travel extended the radius of operations by sixty to a hundred miles. In 1913 a private telephone system had to be installed, after MacDougal fell out with 'progressive Americans' of the Tucson Chamber of Commerce over who should pay for the original public telephone extension to the laboratory. The frequent demands for funds provoked Woodward to remark that MacDougal's efforts to 'work the Institution for all it is worth' reminded him of a Tammany politician: 'To a greater extent than in any other department you and Mr. Sykes have manifested a disposition to overreach your opportunities instead of seeking to establish terms of generous reciprocity with the Institution.'[42] MacDougal's entrepreneurial style, however it may have grated on Woodward, was an important factor in the laboratory's success. It was also important for the development of the science of ecology, which was a prominent activity in the first decade of the laboratory's life. By 1916 MacDougal could boast that his laboratory's reach stretched over 5,000 square miles.[43] With this extended reach, its scientists could begin to transform ecology from an activity often derided for its lack of rigour into a more systematic quantitative science.

4. Ecology in the desert

Ecology at Tucson embraced both the descriptive field sciences and experimental laboratory science. One of the largest long-term projects was the study of a lake that had been accidentally created in 1905 in the Salton Basin of southern California when an irrigation canal running parallel to the Colorado River flooded, resulting in the uncontrolled diversion of the river into the irrigated valley. For more than two years the Colorado River flowed directly into this basin, covering 500 square miles of desert before engineers could redirect its course. The Salton Sea, which reached its maximum height in 1907 and then gradually evaporated, formed an exceptional ecological experiment, a chance to study the desert while it was going through a series of rapid physiographical changes. In 1906 Sykes built a boat on the shores of the rising lake so that he and MacDougal could survey the 150 miles of shoreline, preparatory to two years of intensive field work and subsequent short expeditions to chart the progress of the vegetation as the lake receded.[44]

[41] McGinnies, op. cit. (39), pp. 9–10; Bowers, op. cit. (33), pp. 44, 69.

[42] Quoted in Bowers, op. cit. (33), pp. 69–70.

[43] D. T. MacDougal, 'Department of Botanical Research', *Carnegie Institution of Washington yearbook*, (1916) **15**, p. 95.

[44] D. T. MacDougal et al., *The Salton Sea: A study of the geography, the geology, the*

Fieldwork entailed studies of species abundance, distribution, migration patterns, and successional development, in order to assess the relationship of plants to their physical and organic environment. Physiological studies of plant metabolism were conducted at the Tucson laboratory in conjunction with the field research. The desert studies were early recognized as representing a new direction in ecology. A reviewer praised the laboratory's work in 1910 as the epitome of a 'new era' of geographic ecology because it placed the study of plant relations on the same quantitative and experimental basis as the study of plant physiology.[45] Basic observational work was a major preoccupation at the laboratory as well. Forrest Shreve and Burton Livingston devoted several years to mapping vegetation and compiling climatic data.[46]

The laboratory also provided research opportunities for visiting scientists, some from the Tucson area and others from further away. Of the research associates, the one who would have the greatest impact on ecology was Frederic E. Clements, who was almost single-handedly forging the discipline of plant ecology in America. Far more than MacDougal, Clements emphasized the distinctiveness of ecology as a science and insisted on using the word 'ecology' to describe his research. MacDougal saw the advantages of forming an alliance that would free Clements from teaching and administrative duties at the University of Minnesota and enable him to publish his research. He secured for Clements a position as research associate in 1913.

Humourless and obsessively hard-working, Clements put a damper on laboratory life with his puritanical disdain of smoking and drinking. But in MacDougal Clements found a strong supporter, for despite their temperamental differences they shared the same intense ambition to build scientific empires. Clements was one of the few people who had a clear vision of ecology as a distinct subject synthesizing experimental physiology and the field sciences.[47] He forged the link by developing the concept of the plant community as a 'complex organism', which interacted with its environment and had a developmental history like an individual organism. This organismic viewpoint, which Clements interpreted quite literally, allowed him to argue that methods similar to those used to study individual organisms could be applied to the study of plant communities.

At the Desert Laboratory, Clements completed the research for his monumental study of plant succession, which used the concept of the complex

floristics, and the ecology of a desert basin, Washington, 1914.; G. Sykes, *The Colorado delta*, Baltimore, 1937.

[45] W. L. Bray, 'Desert Plants', *American naturalist*, (1910) **44**, pp. 443–48.

[46] Bowers, op. cit. (33).

[47] Tobey, op. cit. (1); J. B. Hagen, 'Organism and environment: Frederic Clements's vision of a unified physiological ecology', in R. Rainger, K. R. Benson, J. Maienschein, eds., *The American development of biology*, Philadelphia, 1988, pp. 257–277.

organism to structure his analysis of historical change in plant communities. The study earned MacDougal's highest endorsement and was published by the Carnegie Institution in 1916.[48] With MacDougal's backing Clements then became a full-time Carnegie researcher, working partly at Tucson, partly at an alpine laboratory in the Rocky Mountains, and later at Santa Barbara, California.

The other projects undertaken at the laboratory did not manifest the same unified vision of ecology or the unremitting promotion of the subject that one sees in Clements's work. In part this difference resulted from different styles and preferences. Shreve and Livingston were not impressed with Clements's domination of ecology; his writings appeared to them to be dogmatic, jargon-laden, and incorrect in details, especially about desert vegetation. But Shreve eschewed dogmatism at the cost of neglect: his studies of the desert, with their oblique criticisms of Clements's ideas, made little impact on his contemporaries during his lifetime.[49] His general lack of entrepreneurship would eventually weaken the competitive position of the desert laboratory compared to other Carnegie divisions. After MacDougal left Tucson in the 1920s, Shreve had great difficulty keeping the laboratory operating.

Collaboration itself was an obstacle to unity: ten scientists contributed to the Salton Sea research, for instance, so that the ecological portion of the study was simply one component of a multidisciplinary study that also embraced geology, geography, and floristics (census of plant species). There was no attempt to superimpose on these individual contributions a set of ecological principles that would provide an analytical framework or a distinctive concept of the ecological system comparable to Clements's 'complex organism'. Nor was there any attempt in the annual reports of the department to unify the various investigations under the rubric of ecology. We should not see this work as a foreshadowing of ecosystem ecology, focused on the concept of the integrated ecological system, except in the most superficial sense that it involved a long-term, multidisciplinary study of a single region. Such research promoted ecology not by proclaiming it as a discipline with a definite agenda, but simply by collecting ecological data on succession in an environment that had not been well studied. It was Clements who saw these individual analyses as collectively representing a distinctive ecological approach. He argued that the collaborative enterprise at the Salton Sea was a sign of the new ecology of the future, where growing quantitative sophistication demanded the co-operation of several specialists.[50]

Then again, a unified approach should not be expected because the laboratory was not exclusively an ecological research centre, nor did MacDougal

[48] F. E. Clements, *Plant succession: An analysis of the development of vegetation*, Washington, 1916.

[49] Bowers, op. cit. (33), especially pp. 56–62.

[50] Clements, op. cit. (48), p. 32.

conceive it as such. In presenting his work to the Carnegie Institution and to the public in the laboratory's early years, MacDougal's strategy was to highlight the laboratory's work on basic physiological processes, especially those dealing with the water relations of plants. This strategy reinforced the relationship between ecology and physiology, for MacDougal's definition of his laboratory's purpose implied a definition of ecology as a science closely related to physiology. He explicitly denied that the purpose of the laboratory was to study desert vegetation per se: the study of life in arid regions as a branch of science, he said, made no more sense than 'mountain astronomy'. The scientists were engaged in the study of the region not for its own sake, but in order to shed light on fundamental scientific problems. The desert location was simply a convenient one for the analysis of certain kinds of physiological processes.[51] This conception of the laboratory would later imperil the ecological research that remained centred at Tucson after the Carnegie Institution transferred its physiological work to California in the 1920s.

MacDougal was also careful to deflect any suggestion that the purpose of the laboratory was mainly economic. Here again there was a difference between MacDougal's and Clements's vision of ecological work. Clements placed great emphasis on the relevance of ecology to problems of land use and resource management, especially after the First World War when agricultural problems in arid regions were becoming more pressing. The Carnegie Institution was interested in practical ventures in co-operation with other research groups. In 1918 the laboratory entered into joint research in grazing and range management with the Forest Service, Biological Survey, and the University of Arizona. Clements touted this work as demonstrating ecology's relevance to agriculture, although he also saw these co-operative projects as a means of building up the body of knowledge about ecological succession, and hence contributing to basic research. In his annual reports to the Carnegie trustees, he rarely missed a chance to highlight the practical importance of ecology and the need for more research.

For MacDougal, agricultural problems were more an incidental feature of laboratory life. W. T. Hornaday, who wrote a book describing the Mexican expedition he had made with MacDougal, noted MacDougal's insistence that the laboratory should not overlap with the practical work of the agricultural stations but should be the basic research arm for these stations.[52] Agricultural work was the function of the government and MacDougal was satisfied to maintain this division of labour. He reiterated in 1914 that the basic research undertaken at the laboratory would eventually find its appli-

[51] D. T. MacDougal, 'The measurement of environic factors and their biologic effects', *Popular science monthly*, (1914) **84**, pp. 417–33. This article was based on a lecture given to the trustees of the Carnegie Institution in December 1913.

[52] Hornaday, op. cit. (23), p. 21.

cation as the need for more intensive agriculture increased with population growth in the southwest.[53] Only one agricultural project was undertaken, an attempt to cultivate guayule, a wild desert rubber plant, to see whether commercial rubber production would be feasible in the United States.[54] These experiments were not conducted at the desert laboratory, however; the breeding work and genetic analysis were done on grounds purchased near Tucson by a private company. MacDougal made it clear that the laboratory was not evolving into an agricultural station.

5. Evolution and adaptation

Another aspect of basic research, one that linked several of the laboratory's projects, was the study of evolution. Ecological research focused on the response of the organism to the environment; this emphasis naturally created interest in neo-Lamarckian theories of evolution. Conversely if the environment were shown to affect the organism directly and permanently, this would demonstrate the central importance of ecology in evolutionary studies. Ecologists were among the last supporters of Lamarckian inheritance. Clements, for instance, remained a neo-Lamarckian throughout his career, long after others had abandoned the theory. MacDougal was also intrigued by the Lamarckian hypothesis; he offered the desert laboratory as an ideal testing ground for this thesis. Thinking about the relationship between individual and environment raised questions about human nature as well. The laboratory's work, with its Lamarckian emphasis, stimulated speculation about the relation of human character and environment, a theme already well rehearsed in American popular and scientific writing.[55]

The value of working in the desert was that it was an extreme environment, where the organism's ability to survive was put to the harshest test. At a time when Darwinian and Lamarckian views competed with the newer claims of the mutation theory and Mendelism, scientists recognized that choosing among alternative mechanisms would require more careful study of adaptations. G. J. Romanes had argued in the late nineteenth century that small differences between closely allied species were not adaptive and were

[53] MacDougal, op. cit. (51).

[54] D. T. MacDougal, 'Department of Botanical Research,' *Carnegie Institution of Washington yearbook*, (1916) 15, p. 86; D. T. MacDougal, 'Can we grow our own rubber? Guayule, a native American rubber, is being cultivated on a large scale in California,' *Scientific American*, (1928) **139**, pp. 16–19. Laboratory scientists F. E. Lloyd and W. B. MacCallum were involved in the guayule research.

[55] A full history of Lamarckianism and related controversies awaits its historian. We need more evidence to assess how the ecological work pursued at Tucson may have served either to prolong interest in Lamarckian theory, or conversely to cast doubt on its validity, and how the biological debate was related to the social Lamarckianism that Americans admired into the 1920s.

therefore not produced by natural selection. Arguments of this kind led scientists to make a distinction between how species were first formed — the origin of minor systematic diversity — and why they continued to exist. Natural selection, it was thought, could perhaps account for the survival of the fittest, but not the arrival of the fittest. A. R. Wallace represented the adaptationist position, arguing that apparently useless characters might turn out to be useful on closer scrutiny. Those who followed the adaptationist line taken by Wallace were accused of assuming the existence of adaptation without experimental proof. Experimental biologists extended this doubt about natural selection to the analysis of adaptation itself, arguing that even traits that appeared to have adaptive value might not originate by natural selection.[56]

On this question American ecologists diverged significantly from their German counterparts who had opened up the study of ecology in the late nineteenth century. The German school had arisen when Darwinian theory was at its height, and those who pursued evolutionary studies of adaptation did so as confirmed Darwinians seeking to show how Darwin's theory of natural selection explained the presence of adaptations.[57] The Americans, on the other hand, used their arguments with the opposite goal, to show that structures previously assumed to have adaptive significance might not in fact be adaptations at all. It was this problem of adaptation that had stimulated Coville to lobby for a desert laboratory. The desert ecologists at Tucson likewise centred their studies on the problem of adaptation. They wanted to open the problems of evolution to experimental analysis. In doing so they approached them largely as non-Darwinians.

This agnostic approach to adaptation was reinforced by the latest theoretical developments in evolutionary science, in particular by the mutation theory of Hugo de Vries. His ideas had ignited controversy in the United States, generating in the process much research on the production of mutant species. At the centre of this debate, acting as de Vries's most enthusiastic advocate, was MacDougal. Firmly convinced from the start that de Vries's experimental approach was the most promising not only for understanding evolution, but more importantly for controlling the process by experimental manipulation, MacDougal had embarked in 1902 on a series of experiments to confirm de Vries's conclusions.[58] Drawing on his own results, as well as the various experimental and field studies of his colleagues

[56] A typical expression of this point of view was Thomas Hunt Morgan's review of adaptation and evolution in 1903. His critique had begun with experimental studies of regeneration, which had made him doubt that natural selection could have originated this particular adaptation. T. H. Morgan, *Evolution and adaptation*, New York, 1903. For a survey of non-Darwinian thought at this time see P. J. Bowler, *The eclipse of Darwinism: Anti-Darwinian evolution theories in the decades around 1900*, Baltimore, 1983.

[57] Cittadino, *Nature as the laboratory*, op. cit. (2).

[58] Kingsland, op. cit. (38).

in New York and Tucson, MacDougal argued strongly for the validity of de Vries's claims. He saw the mutation theory as a way to manipulate the evolutionary process itself through physiological studies of the cause of mutation. This promise of controlling evolution through studies of mutation had drawn enthusiastic support from the Carnegie Institution. In this respect the goals of the Carnegie Institution's botanical divisions were in tandem with those of the Cold Spring Harbor laboratory, which was set up expressly for the experimental study of evolution and had evolved into a centre of genetic and cytological research. MacDougal's mutation studies were done in collaboration with Cold Spring Harbor scientists.

MacDougal's campaign for the mutation theory and for an experimental approach to evolution in general was distinguished by the same combination of belligerence, guile, and perseverance that had marked Britton's strategy for elevating the New York Botanical Garden to world status.[59] His support of de Vries had brought him into conflict with naturalists, both traditional Darwinians and Lamarckians, who were sceptical of the bold claims of the newer mutation theory, which they disparaged as a caricature of science. The debate over the mutation theory began to wane by 1912, when a decade of experimental work led the majority of biologists to conclude that de Vries had incorrectly interpreted his observations. But MacDougal was not going to let his naturalist adversaries have an easy victory: he continued to assert that neither Darwinian nor Lamarckian groups had grounded their arguments in sufficiently rigorous experimental analyses of adaptation. Nor had they proved the common assumption that extinction meant lack of adaptation. As the mutation debate wound down, MacDougal directed his colleagues' attention to other evolutionary problems that still needed more rigorous experimental tests.

Chief among these was the Lamarckian hypothesis that organisms could make permanent, adaptive responses to the environment. Although many experimental biologists questioned the validity of Lamarckian views by this time, the possibility of direct environmental action on the organism was still an active area of research and discussion. It was not out of place for MacDougal to call for an experimental attack on this problem to resolve the lingering debates and clarify the ambiguous evidence that seemed to support the Lamarckian position.

MacDougal presented his Department of Botanical Research, with its four experimental environments, as ideally set up for studies of transplanted species, Lamarckian evolution, and the general problem of assessing adaptive value.[60] Shreve's work also dealt with the problem of adaptation.[61] Related to the Lamarckian theory was the question of whether environmental

[59] Ibid.

[60] D. T. MacDougal, 'Organic Response', *American naturalist*, (1911) 45, pp. 5–40.

[61] Bowers, op. cit. (33).

stresses could produce changes directly on the germ-plasm, if not indirectly through their effect on the somatoplasm. MacDougal's mutation studies had led him to think that chemical or radiation treatments might directly affect the chromosomes of plants and hence produce permanent mutations. Because these mutations would not represent any kind of direct adaptive response to the environment, the experiments did not confirm Lamarckian theories of evolution, but they did seem to confirm the value of continuing experiments on the impact of the environment on the germ-plasm.

Field studies by the Chicago biologist William L. Tower, partly conducted at the Tucson laboratory, appeared to corroborate these experimental results.[62] Working on transplanted beetle populations, Tower found that his beetles adapted themselves to the desert environment over several generations. He interpreted these results in non-Darwinian terms, that is, as demonstrations that the environment had acted directly on his beetles, rather than that selection had acted on random variations. These results were ambiguous, however, and by the time Tower published his full study in 1918 he was losing credibility among his peers.[63] In 1924, when Paul Kammerer was in the United States lecturing on his work in support of Lamarckian inheritance, MacDougal admitted that although the inheritance of acquired characteristics remained a 'most alluring possibility' to him, there still was not conclusive evidence to support it. By this time MacDougal had come to believe that even the direct action of the environment on chromosomes was probably not very important as a mechanism of change.[64]

Lamarckianism was not only a biological theory, however: it supported a view of social change that buttressed Americans' sense of their cultural uniqueness. The talk of transplanted species and adaptation had human implications as well, ones only hinted at in MacDougal's ecological studies, where the effects of human activity on the landscape were occasionally and briefly acknowledged. But the converse, the effect of the landscape on the transplanted races of pioneers who had travelled west, was a subject of much fascination to Americans.

Social Lamarckianism reached its height in late-nineteenth-century America, when historian Frederick Jackson Turner framed his seminal 'frontier hypothesis', which linked the formation of American character and society to the frontier experience and to the closing of the frontier in 1890. Turner was directly influenced by the new science of evolutionary human geography; from this science he adopted the metaphor of the social organ-

[62] W. L. Tower, 'Genetics and taxonomy,' *Carnegie Institution of Washington yearbook*, (1916) 15, pp. 80–83.

[63] W. L. Tower, *The mechanism of evolution in Leptinotarsa*, Washington, 1918.

[64] MacDougal to Paul Kammerer, 7 January 1924, Carnegie Institution of Washington papers, University of Arizona, Tucson. See MacDougal's views on Kammerer's work, quoted in the article 'Glands used to aid deficient children', *New York Times*, 2 January 1924, p. 19.

ism and the Lamarckian evolutionary thesis that lay behind it.[65] In American social thought Lamarckian ideas remained influential long after they lost credibility as biological explanations. Popular writing, even in the age of eugenics, was infused with environmental determinism, and with concern about the American character and the dangers of decline as civilization progressed.

Ecological studies at the Desert Laboratory encouraged an environmental interpretation of human history that echoed these broader cultural debates. Ellsworth Huntington, geographer and for several years research associate at Tucson, became interested in the relationship between climate and human civilization. He developed his ideas into a global hypothesis, arguing that the highest civilizations sprang up only in certain kinds of climates, because climate affected one's physical and mental energy.[66] Putting down the appearance of character flaws ranging from idleness to immorality to the influence of bad climate, he concluded optimistically that 'If we can conquer climate, the whole world will become stronger and nobler.'[67] He later turned to championing human ecology.[68]

MacDougal's influence on these cultural debates was more subtle, being refracted through the visionary writing of his close friend, Mary Austin, the author of a number of poetically evocative stories of western life and landscape. She had met MacDougal in Carmel, where she had a house. They shared a common ecological perspective and an interest in the landscape and Native American cultures of the southwest.[69] In 1919 MacDougal arranged for Austin to give several lectures in Tucson; she spent a month in Arizona and determined to write a series of sketches of the region's history and potential, a book of prophesy, as she called it, of the progressive acculturation of the land's people. MacDougal agreed to help her with the book, which appeared in 1924 as *The land of journeys' ending*.[70]

Austin, encouraged by MacDougal, was involved in the regional movement to preserve the native pueblo culture. She enlisted the aid of reformer John Collier in the preservation movement and both Austin and MacDougal became involved in Collier's American Indian Defense Association, which in the 1920s was working to secure better treatment for Native Americans, especially in the matter of land rights.[71] Austin's book, which grew from the discussions and trips she took with MacDougal, used a Lamarckian frame of

[65] W. Coleman, 'Science and symbol in the Turner frontier hypothesis', *American historical review*, (1966) **72**, pp. 22–49.

[66] E. Huntington, *Civilization and climate*, New Haven, 1915. [67] Ibid., p. 294.

[68] See Eugene Cittadino's essay on human ecology in this volume.

[69] A. Fink, *I-Mary: A biography of Mary Austin*, Tucson, 1983, especially chs. 14, 15.

[70] M. Austin, *The land of journeys' ending*, New York/London, 1924.

[71] K. R. Philp, *John Collier's crusade for Indian reform, 1920–1954*, Tucson, 1977.

reference to link the evolutionary fate of the white man's culture to that of the Native American, showing how the one evolved from the other.

Austin's book was a celebration of life in the desert and an account of human migration to the Southwest, beginning with the missionaries who had left a 'dull, debasing smear over the lovely and aesthetic culture of the pueblos'.[72] Moving from descriptions of the native desert plants that lived 'fulfilled, triumphant' in the desert, she built a contrast between the adaptation of the Indian peoples to the land and the violence of the white invader, who had not yet understood the reciprocal relations of humans and environment. The imported European-American culture was made to seem insignificant in its rampant materialism: in Indian dramas, she wrote, the presence of a white man was indicated by a pile of tin cans.

The lesson to be learned did not concern native preservation for its own sake, but also for the sake of the white race's survival, for new races were made out of old races by a continual process of adaptation to the environment. Where different races mixed, or where one race tried to graft itself on an earlier arrival, the determining factor in the new design was always the land, especially the 'magnificently shaped and colored scene'.[73] What the white man had to understand was that he was growing in an 'aboriginal topsoil' to which had been added a Spanish element and then a Nordic-American element. The new American population could not simply impose its previous culture on the new land; rather, a new culture had to grow from this soil, using the existing alphabet of cultural expression that came from the land itself and its earlier inhabitants. The image Austin used was of grafting onto an aboriginal root, an 'energetic blond engrafture on a dark, earth-nurtured race'. Americans had obtained material wealth from the land; now they needed to develop spiritual strength, which meant appropriating the cultural and spiritual legacy of the Indians. She ended with an appeal to grow spiritually by recovering the level of understanding of the ancient cultures.

Where MacDougal wanted to understand the relationship of organism to environment in order to control biological evolution, and Huntington sought by controlling the climate to raise human evolution to the high level attained by the European nations, Austin pursued a similar key to the spiritual evolution of the American people. In each case, whether dealing in the material or the spiritual domain, the interest in exploring the relationship between organism and environment was linked to the affirmation of American power, and to a belief in the ability of the American people to strengthen themselves and shape their destiny. Just as the creation of the laboratory had been an expression of American expansionism and entrepreneurial drive, so the ecological point of view embodied in the laboratory, with its Lamarckian

[72] Austin, op. cit. (70), p. 197. [73] Ibid., p. 438.

overtones, could be turned into an affirmation of continued growth, extending onto the spiritual plane. At the Desert Laboratory, a microcosm not only of American science but of American culture, science and national ideology met and reinforced each other.

6. The triumph of utility

In the end the Desert Laboratory succumbed to those considerations of utility which MacDougal, determined to uphold his laboratory as a place for basic science, had always tried to keep at arm's length. By the 1920s, even while MacDougal was guiding Austin around the desert and encouraging her as spokeswoman for the conservation of native culture, he had almost completely transferred his base of scientific operations to Carmel, where Herman Spoehr had moved in 1920. This move ultimately shifted the Carnegie Institution's centre of botanical science from Arizona to California. In 1928 Spoehr, having achieved solid respectability for his work on photosynthesis, succeeded MacDougal as chairman of the renamed Division of Plant Biology and moved his physiological laboratory to Stanford University. MacDougal remained at Carmel, retiring from the Carnegie Institution in 1934. By this time, Carnegie Institution president John C. Merriam was decidedly cool towards the desert research.[74] Tucson's growing marginality was also reflected in the separation of the two positions MacDougal had combined: Shreve succeeded him as director of the Desert Laboratory, but Spoehr became head of the entire division.

Once the centre of physiological research had moved to California, it became increasingly difficult to justify the expense of the Desert Laboratory, which suffered also from its geographical isolation. Shreve could point to the laboratory's success in opening up a new field in desert research and attracting over fifty investigators to these problems over two and a half decades. But from the Carnegie Institution's point of view Shreve's ecological studies were less fundamental, and hence less significant, than the experimental research on plant metabolism and photosynthesis being done in Spoehr's laboratory. Shreve had never possessed the aggressive drive of MacDougal and Clements and he was simply outmanoeuvred by Spoehr. In 1940 Vannevar Bush, the new president of the Carnegie Institution, finally closed the laboratory, handing its buildings to the US Forest Service. In 1960 the Forest Service sold the laboratory to the University of Arizona.

Uncertainty as to the kind of science ecology was and should be, and whether it was worth funding, had dogged the science from the start. Ecology was a product of economic development, in that interest in ecological study was stimulated by territorial expansion and its funding came from the

[74] Bowers, op. cit. (33), pp. 142–145.

wealth created by industrial growth. Arguments in favour of new research centres such as the Desert Laboratory appealed directly to economic motives. In this respect ecology was no different from other sciences at this time. But what these scientists really wanted was the freedom to study basic problems in science as they saw fit, without the constraints imposed by the need to solve practical problems. This was why, although agricultural experiment stations were being created all over the country, scientists still pressed for privately funded laboratories to do basic biological research. Through the efforts of Coville and MacDougal, following in the footsteps of entrepreneurs like Britton, ecology was defined as what was *not* pursued at the agricultural stations, that is, as basic research into problems of physiology and succession, with reference to broader evolutionary questions. The division of labour brought about by the creation of the agricultural stations suited MacDougal's goals perfectly, for the applied side of science could be seen as the special function of government.

MacDougal hoped that the public and the Carnegie trustees would recognize that his laboratory had little in common with the experimental farm but was more like a nature preserve. Setting up the laboratory entailed fencing in the land, in order to prevent the depredations of grazing and return the biota to its pristine state. The value of the preserve had little immediate relevance to the practical needs of the local population; it was a source of intellectual enlightenment, of the kind that came from basic research and which might one day find its applications. The conservationist ethos represented by the fencing of the grounds extended also to the indigenous inhabitants of the region, whose aboriginal culture had to be protected as well, not for its own sake but for the spiritual enlightenment it gave to the immigrant population. MacDougal's science could sit comfortably with Austin's conservationist ethic: his botanical reservation, like the Indian reservation as Austin conceived it, was an attempt to preserve and study nature for fundamental intellectual and spiritual goals.

Having distanced himself from immediate practical applications, however, MacDougal still had to defend the importance of his research by appealing to its economic and intellectual significance in other ways. The most tangible expression of ecology's importance was in physiology, that is, in experimental science, where scientific progress in understanding plant function was relatively easy to chart. From time to time, moreover, MacDougal still appealed to the possibility that his research would yield practical benefits in the long term, that it would help to resolve debates in the forefront of evolutionary biology, and that it might offer the possibility even of controlling evolution through studies of the effect of the environment on the germ-plasm.

These prospects were sufficiently alluring to retain the interest of the Carnegie Institution, even when the officers chafed at the mounting research expenses. But what had seemed promising in the early 1900s seemed less so

by the 1920s. The Burbank project had not yielded significant results. The mutation theory of de Vries had been discredited, and MacDougal had come to see his own work on experimental production of mutation as less important than he had first hoped. Lamarckian research in general, including some of the work conducted by William Tower in Tucson, had been discredited in part by flawed technique and ambiguous results. The Carnegie Institution continued its support of species transplantation experiments in the 1920s in its section of 'experimental taxonomy'. But this section, under botanist Harvey Monroe Hall, was based at the University of California at Berkeley and in 1929 transferred to the central laboratory at Stanford directed by Spoehr. In 1931 a cytogeneticist from Copenhagen, Jens Clausen, joined the division. He took over the transplantation research on Hall's death in 1932. Still under the rubric of 'experimental taxonomy', the research shifted in orientation toward cytogenetics and physiological ecology, experimental research that was closely related to work at the Cold Spring Harbor laboratory and that now seemed the most promising route for understanding and controlling evolution. The link between ecology, physiology, and cytogenetics thus grew closer at the Stanford laboratory. The greater weight placed on experimental science increased the isolation of the Tucson laboratory. Once the centre of physiological research had moved to California, it became more difficult to justify the desert laboratory as a locus for the pursuit of basic research. Shreve was not able to convince the Carnegie officers that the descriptive, climatological aspects of ecological work were as fundamental, and hence as valuable, as the experimental programme at Stanford.

Even those who were more aggressive than Shreve were having trouble maintaining ecological research in this tight economic environment.[75] Clements, whose research formed a separate unit of the botanical division and who became a leading government adviser during the Dust Bowl in the 1930s, was also finding it difficult to compete with Spoehr's reductionist programme. By the mid thirties he was using the environmental crisis to argue for the importance of ecology in the public service, presenting ecology not as a specialized field, but as a 'point of view and a plan of attack', a synthetic science that 'embraces all problems in which life and its environment are concerned.'[76] These visions of ecology had little impact in a scientific milieu where fundamental research was valued for being the very antithesis of this kind of synthetic undertaking. His own research division at the Carnegie Institution did not survive his retirement in 1942. For a number of reasons, ably charted by Ronald Tobey, the Clementsian programme

[75] On the low status of ecology as an academic subject in the 1930s, see P. B. Sears, *Deserts on the march*, Norman, Oklahoma, 1935; reprinted, Washington, 1988.

[76] F. E. Clements, 'Experimental ecology in the public service,' *Ecology*, (1935) 16, pp. 342–363, quoted p. 342.

declined and grassland ecology faced increasing difficulties as a field of basic research.[77]

Ironically, these looming environmental problems provided reasons to shift the laboratory's burden to the government. Spoehr's remarks in 1940 encapsulated the dilemma of the Desert Laboratory and the difficulty that ecology has had in maintaining a competitive position in science.[78] Setting the scene for the laboratory's closing, he recalled Coville's emphasis on economic considerations back in 1902 and noted that the laboratory's investigations had been aimed at understanding problems involved in rational use of the land. Moreover, he acknowledged, the problems underlying the use of arid and marginal lands had intensified in the 1930s and were now to be considered urgent. But this very urgency had caused the government to increase its expenditure on these problems to such an extent that the Carnegie's commitment to this research was deemed to be no longer required. In the end, with practical problems overwhelming basic science, the laboratory's work became the government's business. Unfortunately the government's interest soon waned in its turn. Government funding boosted applied fields such as range management, but did little to further basic ecological research once the Dust Bowl ended.[79]

The history of the Desert Laboratory raises some complex issues that face the historian of ecology. The problem of defining ecological research so as to justify spending money on it, while differentiating ecology from practical research programmes designed for agricultural and resource management purposes, was one that ecologists would constantly have to confront. The desire to secure a niche for ecology, in competition with other sciences, has inspired recourse to many different strategies for defining and advancing ecology as a science. As ecology grew and diversified, different branches of the science competed with each other and continued to fight for authority in an environment of limited resources. The increased use of mathematical methods in the 1950s, the building of ecosystem ecology in the 1960s, and the evolutionary synthesis of genetics, behavioural biology, and ecology known as socio-biology in the 1970s, represent but three adaptive strategies that helped ecologists to compete and survive after ecology's slump in the 1930s. It has often been noticed, and it was noticed even in the earliest days of ecology, that there is something oddly elusive about ecology as a subject. The difficulty is not just that the subject matter is eclectic, but that the quest for identity and legitimacy involved so many contradictory tactical moves.

The Desert Laboratory's story points to a larger problem that confronts historians as they follow these shifting strategies, namely, the relationship between ecology and what we would now call environmentalism. Environ-

[77] Tobey, op. cit. (1).

[78] H. A. Spoehr, 'Division of Plant Biology', *Carnegie Institution of Washington yearbook*, (1941) **39**, pp. 146–147.

[79] Tobey, op. cit. (1), ch. 7.

mentalism can refer to a preservationist ethos that has aesthetic and moral justifications, or to pragmatic interest in resource conservation, planning and management, so that we may live in a healthy environment. We have seen that the desert scientists were imbued with a preservationist ethos, were involved in conservation efforts, and expected their research to have various practical applications outside the laboratory. However, the ecology pursued there did not derive its legitimacy either from this ethos or strictly from the needs of applied science, but from its contributions to basic research. Nor does modern ecology derive its legitimacy mainly from environmentalism, although the two are related.

To be sure, funding of ecology can reflect society's concern about environmental problems and ecologists have often been active environmentalists in both of the above senses. Yet ecology and environmentalism can also be antagonistic. Applied scientists and resource managers may deride the 'ologists' whose interests lie in esoteric subjects, while ecologists have been known to disparage practical work as uninteresting. The strategies used to bolster ecology as a science do not necessarily strengthen the link to environmentalism. Moreover, the biological training one receives to become an ecologist may bear little relation to environmental activism. Popular 'ecology' movements can be openly hostile towards science, just as scientists have been suspicious of critics who take their concerns directly to the public, bypassing the self-protective conventions of scientific debate. Unravelling this complex relationship between ecology and environmentalism poses special problems for historians, who must grapple with a subject that has been challenged and redefined through its century of existence to a greater degree than its more reductionist cousins. Yet it is important to understand this relationship as we move from a regional to a global perspective on environmental problems and ask what role the science of ecology, and the ecologist as expert, may play in their resolution.

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On 'environmentalism': The origins of debates over policy for pest-control research in America, 1960–1975

Paolo Palladino

1. Pest control and 'environmentalist' ideology

During the 1950s, synthetic insecticides such as DDT, which only ten years earlier had seemed to many excited American entomologists to usher a new age in the practice of pest control, were proving quite ineffectual: the insects these insecticides were meant to kill were becoming quite resistant to them.[1] More alarmingly, the environmental disruption wrought by the widespread and ecologically indiscriminate use of these insecticides, described so vividly by Rachel Carson in *Silent spring*, resulted in a campaign in America during the 1960s to ban their use.[2] Entomologists responded to the situation by presenting 'integrated control' and 'sterile-male release' programs as novel pest control programs that would eventually remedy the agricultural and environmental disaster for which they were being held responsible. By the late 1970s, however, these entomologists were sharply divided over the merits of these two innovations. The historian John Perkins has argued that the division was so sharp because 'integrated control' and 'sterile-male release' programs were based on views of the relationship between humanity and nature that are mutually incompatible.[3] Apparently, those entomologists supporting the 'sterile-male release' approach had no reservations about

I wish to thank Vernon Ruttan and Waldemar Klassen for their guidance through the mazes of American agricultural politics and agricultural research policy, and Jon Harwood for his insightful criticism of an earlier draft of this paper.

[1] See P. Palladino, 'The "insecticide crisis" and entomological research in the United States in the 1960s and 1970s: Political, institutional, and conceptual dimensions', Ph.D. dissertation, University of Minnesota, 1989, pp. 17–99.

[2] R. Carson, *Silent spring* [1962], Boston, 1987; and T. R. Dunlap, *DDT: Scientists, citizens, and public policy*, Princeton, 1981.

[3] J. H. Perkins, *Insects, experts and the insecticide crisis: The quest for new pest management strategies*, New York, 1982; see also the shorter accounts of Perkins' thesis in the following two papers: 'The quest for innovation in agricultural entomology, 1945–1978', in D. Pimentel and J. H. Perkins, eds., *Pest control: Cultural and environmental aspects*, Boulder, 1980; and 'The boll weevil in North America: Scientific conflicts over management of environmental resources', *Agriculture, ecosystems, and environment*, (1983) 10, pp. 217–45.

both helping farmers and stopping any needless destruction of the natural environment by simply 'eradicating' pest populations down to the very last individual. This attitude, according to Perkins, was grounded in a belief that humans' capacity to reshape the natural environment to meet their material needs is unlimited. The proponents of 'integrated control' believed instead that entomologists should limit themselves to 'managing' the use of insecticides so as to limit any damage to the natural environment, while still helping farmers to reduce economic losses. This belief stemmed supposedly from the view that humans are an integral part of nature and have no right to eliminate any other species. Given these opposing ideological commitments, it was perhaps inevitable that any discussion between the proponents of the two programs should result in sharp divisions with no room for compromise.[4] This explanation, however, is at odds with the occasional willingness of some of the chief proponents of 'integrated control' to argue that the two programs were quite compatible. Still other proponents seemed to oppose 'eradication' programs on grounds that had far more to do with funding for entomological research than with the unity of humanity and nature.

In this paper, I want to suggest that the divisions observed by Perkins are better understood by considering that antagonism between federal and local authority which has characterized so much of the history of the United States. The 'sterile-male release' approach was devised by entomologists in the United States Department of Agriculture, while the development of 'integrated control' was the work of entomologists in some of the largest state agricultural experiment stations. The relationship between these two branches of the American agricultural research establishment, one federal and the other controlled by state legislatures, has never been an easy one.[5] But this tension reached a particularly high point during the 1970s, as the Department of Agriculture became a prime target of a program initiated by the Nixon Administration to transfer the burden of government from the federal to the state level. The passage of parts of this program (and Nixon's re-election in 1972) was facilitated by a temporary alliance between the administration and an increasingly powerful 'environmentalist' movement. This alliance was sometimes mediated by supporting programs of research that were particularly appealing to both parties. By this time, the 'integrated

[4] I use the term 'ideology' quite deliberately because what Perkins describes as competing 'philosophies', are *normative* sets of propositions, which, as we shall see, serve particular social interests; on the meaning of 'ideology', see T. Eagleton, *Ideology*, London, 1991.

[5] For other examples of this rivalry, see Deborah Fitzgerald's discussion of the efforts between 1915 and 1925 by the Bureau of Plant Industry of the Department of Agriculture to transform the plant breeding programs of the state agricultural experiment stations (*The business of breeding: Hybrid corn in Illinois, 1890–1940*, Ithaca, 1990, pp. 63–9); and R. Sawyer's study of the relationship between the federal Bureau of Entomology and entomologists employed by the state of California ('Monopolizing the insect trade: Biological control in the USDA, 1881–1951', *Agricultural history*, (1990) 64, pp. 271–85).

control' and 'sterile-male release' programs considered here had reached crucial phases in their development: they had been recognized as promising alternatives to the current, ecologically indiscriminate use of DDT and other similar insecticides, but needed further support to implement the promises. The appeal by the researchers in the experiment stations to those 'environmentalist' values that so captured Perkins' attention must be viewed also as an effort to mobilize the alliance between the Nixon Administration and the 'environmentalist' movement to act in their behalf during their competition with researchers in the Department of Agriculture for the expansion of their respective programs of research. This is not to say, however, that none of these researchers were deeply committed to the values of the 'environmentalist' movement. We should instead view the promoters of 'integrated control' as a coalition drawn together by a variety of different concerns, some of which assumed greater importance at a particular point in American history. In sum, the aim of this paper is to show how ideological considerations can interact with political and institutional ones to shape the evolution of particular scientific research programs.

2. *Insects and chemical insecticides*

In the 1930s and 1940s, the intensive use of arsenic- and lead-based insecticides resulted in large toxic residues on crops and in the soil, which were hazardous to consumers and also reduced the productivity of the soil. The economic repercussions of this situation halted the increasing use of these insecticides.[6] However, the synthetic ones developed during the 1940s, like DDT for example, seemed less toxic to both plants and humans, and were used therefore much more liberally. At the same time, their relative cheapness promoted their use where 'biological control' or 'cultural control' had been preferred before. For example, instead of searching for and introducing predators and parasites of insect pests (biological control), and instead of merely checking their depredations by removing infested plants, by destroying the pests' refuges, or by altering cultivation practices to make the crop less attractive (cultural control), it now seemed feasible to completely eliminate any pest by simply showering insecticide throughout its natural range.[7] Federal and state departments of agriculture proceeded to 'eradicate' pests such as the gypsy moth, the spruce budworm, and even the elm bark beetle, by spraying the new insecticides over millions of acres, crossing property, municipal and sometimes even state lines.

This approach had tremendous repercussions on the ecology of both noxious and beneficial insect species. In the short term, the use of the new insecticides resulted in the very rapid selection of resistant insect populations

[6] J. Whorton, *Before 'Silent spring': Pesticides and public health in pre-DDT America*, Princeton, 1975.

[7] Dunlap, op. cit. (2), pp. 76–97.

(resistance).[8] In the longer term, however, the indiscriminate destruction of all insects, pests and their natural enemies alike, allowed any surviving pests to grow quite explosively after each insecticidal treatment (resurgence). Moreover, other species, which formerly existed at naturally low densities because they were held in check by natural enemies, could now obtain far greater densities; whereas formerly these species had been secondary pests, they now became pests of primary economic importance (secondary outbreaks).[9] Although these problems raised serious questions about the effectiveness of DDT and other insecticides like it, farmers' typical response was simply to increase the frequency and doses of the treatments. In so doing, resistance, resurgence, and secondary outbreaks were only worsened. This destructive cycle was a cause of great concern to a number of American entomologists, and, from the mid to late 1950s onward, they began to discuss the need for some better way out of this situation.[10] 'Integrated control' and the use of artificially sterilized males to 'eradicate' insect populations seemed to be the most promising solutions.

3. Managing and eradicating pest populations

By the late 1950s, American entomologists who had staked their career on the use of natural enemies to control insect pests and who, with the advent of DDT, had then found themselves relegated to the margins of their profession, were suddenly back in the limelight. These entomologists, especially those in the Division of Biological Control at the University of California, injected new life into their programs in biological control by proposing to develop schedules for the use of chemical insecticides that would take into better account the ecology of the pests and their natural enemies.[11] For example, they proposed to control the alfalfa aphid by spraying the crop with an insecticide that was more lethal to the aphid than its major natural enemies. With such an insecticide it would no longer be necessary to destroy all the aphids, but it would simply suffice to reduce their numbers just enough to enable the surviving natural enemies to lower the abundance of aphids to a tolerable level.[12]

Although these entomologists sought to emphasize the central role of

[8] A. W. A. Brown, *Insecticide resistance in arthropods*, Geneva, 1959.

[9] D. Pimentel, *Ecological effects of pesticides on non-target species*, Washington, 1971.

[10] See R. Doutt and R. F. Smith, 'The pesticide syndrome: Diagnosis and suggested prophylaxis', in C. B. Huffaker, ed., *Biological control*, New York, 1971, pp. 3–15; and Entomological Society of America, 'Theme of meeting', *Bulletin of the Entomological Society of America*, (1959) 5 (3), p. 103.

[11] See R. Sawyer, 'To make a spotless orange: Biological control in California', Ph.D. dissertation, University of Wisconsin, 1990.

[12] See V. M. Stern and R. Van den Bosch, 'Field experiments on the effects of insecticides', *Hilgardia*, (1959) **29**, pp. 103–30.

natural enemies in any 'integrated control' program, in some ways, their approach was not terribly at odds with that of most of their contemporaries.[13] In the early 1960s, they viewed the future of 'integrated control' as contingent on the development of more, and better 'narrow-spectrum' and 'systemic' insecticides, a forecast which was in line with the well-established rule among American entomologists that the proper response to the failure of one class of chemical insecticides was to replace it with a new one.[14] In another respect however, 'integrated control' was based on a rather significant departure from current practice. Ever since the emergence of arsenic insecticides at the turn of the century, and even more so after the advent of DDT, insecticides tended to be used on a fixed schedule. That is to say, insecticides were usually used if the cost of treatment was less than the potential losses that might result from failure to do anything at all about the presence of some noxious insects. Since it was very difficult to predict whether their abundance was going to remain at the observed level, or whether it was going to decrease naturally, or whether it was going to grow and reach destructive levels, the best policy was to always take some action. Indeed, as long as the value of the crop saved each year exceeded the cost of the insecticides, it seemed wise to use them repeatedly throughout the growing season without regard for the ecology of beneficial and noxious insect species.[15] The proponents of 'integrated control' broke with this tradition by arguing that control measures should be adopted when, and only when, the projected economic losses incurred by the growers exceeded the costs of treatment.[16] Furthermore, unlike entomologists of an earlier age (or their contemporaries, for that matter) their equation was not based simply on the cost of the insecticides against immediate gains, but also included the long-term cost of disrupting the whole crop fauna. The value of the crop saved following the insecticidal treatment was supposed to be weighed against future damage due to resistance, resurgence, and secondary outbreaks. Under this broader set of considerations, the proponents of 'integrated control' hoped that the attractiveness of insecticides would be severely dampened and that 'biological control' would carry the day. This also implied that the goal of total freedom from insect pests should be abandoned as an unobtainable one.

[13] In fact, Rachel Carson did not include 'integrated control' programs among the alternative approaches to pest control, which she thought might one day lead to a 'new spring'; instead she referred to them in the context of a general discussion of the impact of insecticides on the natural mechanisms regulating insect populations, see Carson, op. cit. (2), pp. 258–61 and 277–97.

[14] See, however, R. L. Doutt, 'Ecological considerations in chemical control: Implications to non-target invertebrates', *Bulletin of the Entomological Society of America*, (1964) 10, p. 87.

[15] Since the costs for crop protection often represent only a small fraction of the total costs of production, this simple cost-benefit analysis, in most cases, would favour the use of insecticides. R. Riley (USDA Cooperative State Research Service), personal communication.

[16] V. M. Stern et al., 'The integrated control concept', *Hilgardia*, (1959) 29, pp. 81–101.

There was at hand, however, a far more radical approach to pest control, which promised to dispense eventually with all uses of insecticides; Rachel Carson described it in the following manner:

> about a quarter of a century ago Dr. Knipling startled his colleagues by proposing a unique method of insect control. If it were possible to sterilize and release large numbers of insects, he theorized, the sterilized males would, under certain conditions, compete with normal wild males so successfully that, after repeated releases, only infertile eggs would be produced and the population would die out.[17]

Edward Knipling, director of the Division of Entomology in the Agricultural Research Service of the United States Department of Agriculture from 1953 to 1970, had striven from 1950s onward to shift the agenda for federally funded entomological research away from the development of new insecticides (and uses thereof), toward a greater emphasis on non-chemical strategies. Among the latter was the use of artificially sterilized males, an approach which he had been thinking about ever since the late 1930s. Knipling believed that if an insect population were so sparse that a female faced comparable odds of mating with either a normal and fertile male or an artificially sterilized one, then it would eventually be possible to eliminate completely any local insect population; that is to say, it could be 'eradicated'. The screwworm, which existed naturally at very low population densities and yet was a cause of significant economic losses to the cattle industry in the southwestern United States, seemed to Knipling the perfect candidate for the approach. In 1954 and 1955 his idea was put to test on the Caribbean island of Curaçao, with remarkable success: after treatment, no screwworms could be found anywhere on the island. This approach was then used by the United States Department of Agriculture to eliminate the screwworm from Texas and Florida, and in the early 1960s, Knipling suggested that perhaps it could also be used against the most intractable pest of American agriculture, the cotton boll weevil.[18]

4. Eradicating the boll weevil

In the late 1960s, the United States Department of Agriculture obtained the financial backing of the National Cotton Council and political support in Congress to implement a Pilot Boll Weevil Eradication Experiment and test Edward Knipling's ideas on a grand scale.[19] The experiment would be very

[17] Carson, op. cit. (2), p. 279.

[18] See Perkins, op. cit. (3), pp. 97–126; and E. F. Knipling, 'Potentialities and progress in the development of chemosterilants for insect control', *Journal of economic entomology*, (1962) 55, pp. 782–6.

[19] E. F. Knipling, *The basic principles of insect population suppression and management*, USDA Agriculture handbook no. 512, Washington, 1979, pp. 528–38.

expensive, $5.25 million, but, as cotton protection accounted for almost half the use of insecticides in the United States, and two thirds of them were used against the boll weevil alone, this large investment seemed worthwhile.[20] Certainly, if successful, the experiment would transform the 'sterile-male release' approach into the most revolutionary innovation in crop protection since the advent of DDT.

The plan for the experiment was firstly to reduce the weevil population over a selected area to a very low density. Thus, starting in the fall of 1971, parts of Mississippi were subjected to a drastic control program, which included the destruction of crop residues at the end of each growing season to reduce the number of overwintering refuges for the weevils, intensive insecticidal treatments before and after the dormant season, and the use of pheromones to trap most of the insects remaining after this preliminary phase. Artificially sterilized male weevils were then released in the second phase to eliminate any survivors.

As this trial run reached completion in 1973, many entomologists began to express serious doubts about the effectiveness of the 'sterile-male release' method because a small number of weevils were found in the interior of the experimental site.[21] A special panel of the Entomological Society of America was called upon to review the accomplishments of the experiment, and the panellist immediately sought to establish from where these weevils had come. If they had simply survived the entomologists' onslaught, then the 'sterile-male' technique was far less effective than promised. The amazingly low weevil densities achieved by this effort could only be maintained by continuously monitoring the crops and quickly treating local infestations. It was of course possible that the weevils had simply immigrated into the area after the onslaught. This second, seemingly less damaging explanation, however, drew attention to the discretely overlooked fact that, although the screwworm was completely eliminated on Curaçao, a long-term commitment of financial resources was needed in Texas to prevent any re-invasion from Mexico and other infested areas. Such re-invasions might be particularly common in the case of the boll weevil, which was known to migrate over relatively long ranges.

Those entomologists in United States Department of Agriculture who had championed the revolutionary achievements of the 'sterile-male release' method were caught in a bind. Not only had they had failed to fulfil the promise that had allowed them to obtain from their public and private

[20] See US Senate, *Integrated pest management*, Hearings before the Subcommittee on Agricultural Research and General Legislation. 99th Congress, First Session, Washington, 1977, p. 211; E. F. Knipling, 'Boll weevil and pink bollworm eradication: Progress and plans', *Ginners' journal & yearbook*, (1971), pp. 23–30; and Perkins, op. cit. (3), p. 120.

[21] W. G. Eden et al., 'The Boll Weevil Eradication Experiment', *Bulletin of the Entomological Society of America*, (1973) 19, pp. 218–21.

sponsors a previously unimaginable fund for any kind of entomological research and development, they had also angered entomologists outside the Department. It seemed to these other entomologists, and especially to those advocating 'integrated control', that their colleagues in the Department of Agriculture had obtained their funding by simply misrepresenting the goals of their project, by promising to repeat the achievements on Curaçao, while knowing quite well that they were in fact aiming to obtain a situation similar to that established against the screwworm in the Texas.[22] They were not prepared to let the Department of Agriculture off the hook.

5. Two paradigms for pest control

Five years after the Pilot Boll Weevil Eradication Experiment, the Entomological Society of America convened a round-table discussion on the merits of 'eradication' programs.[23] The mediator, entomologist H. C. Cox from the United States Department of Agriculture, opened the discussion by pointing out that even graduate students knew that, 'eradication is a politicians' term. They really mean control'.[24] Nevertheless, the North Carolina State University entomologist and most articulate proponent of 'integrated control', Robert Rabb, took the term 'eradication' to be something more than a 'politicians' term'. He began his criticism by considering the different aims of 'integrated control' and 'eradication'. He did not believe that

> man can devise large-scale productive and persistent agro-ecosystems in which he is the sole beneficiary of the energy and materials involved. The goal of pest management is not to eliminate pests, but rather to reduce their take of biomass as low as possible without jeopardizing the stability of production.[25]

For Rabb (and other proponents of 'integrated control'), entomologists should aim to maximize the long-term productivity of farmers by balancing any benefits from the use of insecticides against the damage to the natural checks on the growth of pest populations; at times, this might even mean that no action should be the appropriate response to the presence of pests.[26] But more crucially, Rabb argued that

[22] H. C. Cox, 'Comments of the moderator', *Bulletin of the Entomological Society of America*, (1978) **24** (1), p. 35.

[23] Entomological Society of America, 'Eradication of plant pests — Pro's and con's', *Bulletin of the Entomological Society of America*, (1978) **24** (1), pp. 35–54.

[24] Cox, op. cit. (22), p. 35.

[25] R. L. Rabb, 'Eradication of plant pests — Con', *Bulletin of the Entomological Society of America*, (1978) **24** (1): 43.

[26] On this last point, see C. B. Huffaker, 'Ecological management of pest systems', in J. A. Behnke, ed., *Challenging biological problems: Directions towards their solution*, New York, 1972, p. 319.

> the most rational general approach to pest problems is to learn to live with them . . . the use of [technological] power [against them] is a tremendous responsibility and must be done without arrogance and with subtle sensitivity, if not reverence, for the value of all life.[27]

Rabb's views of the aims of pest control were sharply different from those of Edward Knipling. For Knipling, 'integrated control' was nothing more than the systematic assembly of a number of different tactics to maximize the control of insect populations, while seeking to reduce dependence on chemical insecticides.[28] The chief problem was to establish the proper balance between chemical control, whose efficiency is proportional to the density of the pest population, and the release of sterilized males, whose effect on the target population is inversely proportional to its density. That the adoption of control measures should be contingent on economic deliberations of the type involved in the formulation of the 'economic thresholds' advocated by Rabb and other supporters of 'integrated control' was to Knipling absolute nonsense. In fact, he argued firstly that entomologists should strive to afford humanity with the 'maximum protection from pests that affect [its] health and comfort', and then proceeded to express his 'great confidence in the ingenuity of our young scientists to perfect the technology necessary to put sound principles of insect suppression [eradication] into practice in future years'.[29]

In sum, Knipling preferred a terminal solution to an expensive coexistence with insect pests, and professed an unlimited confidence in the abilities of entomologists to obtain this goal.[30] This, according to Perkins, was the expression of a more fundamental belief, namely that humans' capacity to mould nature to suit their own needs was limitless and proper. Rabb disputed this faith on both points. Given such fundamentally incompatible perspectives on the interaction of humans and nature, it is not surprising that any disputes between the two groups over the relative merits of their respective programs should be intractable. Yet there are good grounds for suspecting that this is not a very satisfactory account of the controversy, as there are too many exceptions, ambiguous statements about the relationship between 'management' and 'eradication', and arguments against the latter, which do not fit easily with Perkins' explanation.

[27] Rabb, op. cit. (25), pp. 40 and 44.

[28] E. F. Knipling quoted in Perkins, op. cit. (3), p. 56.

[29] E. F. Knipling, 'Eradication of plant pests — Pro', *Bulletin of the Entomological Society of America*, (1978) **24** (1), pp. 44–52, quotations on pp. 45 and 51.

[30] In his latest and comprehensive discussion of pest control strategies, Knipling completely ignores the notion of 'economic thresholds'; he is far more concerned with what he views as the detrimental economic and environmental impact of a long term commitment to treating pest outbreaks with pesticides, which is an implicit and undesirable feature of any 'integrated control' program. See Knipling, op. cit. (19).

For example, Paul De Bach, a member of the group of researchers from the University of California who played a central role in the development of 'integrated control', was totally dedicated to the development of biological control as a self-standing strategy of pest control, and a long-time opponent of the 'eradication' campaigns so criticized by Rachel Carson.[31] Yet, like Carson, De Bach did not find the notion of 'eradicating' an insect species particularly reprehensible if it was accomplished by biological rather than chemical means.[32] In fact, he proposed that the ecological mechanism of 'competitive exclusion' could be exploited to totally eliminate an undesirable species. By introducing into a given area a species that was closely allied to the target pest by way of its habits (except for the undesirable ones) and was a superior exploiter of at least one shared resource, De Bach believed, that the ensuing competition between the two species would result in the displacement and extinction of the noxious one.[33] For yet another important figure in the early development of 'integrated control', the Canadian entomologist Bryan Beirne, 'eradication' was one among many ecologically sustainable approaches to pest control; permanence, as opposed to the fleeting and very destructive action of insecticides, was for him the one goal toward which all entomologists should aim. In fact, in 1967 Beirne wrote that, 'the most desirable level of results [of pest management practices] is permanent pest eradication'.[34] In sum, De Bach's and Beirne's critical references to 'eradication' were indictments of the indiscriminate use of insecticides rather than indictments of the ultimate goal: the complete elimination of the pest population.[35] Thus it seems that in 1960s 'integrated control' and 'eradication' were viewed by some entomologists as compatible approaches to crop protection.

[31] See Dunlap, op. cit. (2), p. 194, and also P. De Bach, *Biological control by natural enemies*, Cambridge, 1974.

[32] For Carson's opposition to the use of insecticides to 'eradicate' insects, see Carson, op. cit. (2), pp. 155–72; and on her attitude toward the elimination of insect species by any other means, see pp. 277–91 (especially pp. 284–5).

[33] See P. De Bach, 'Some ecological aspects of insect eradication', *Bulletin of the Entomological Society of America*, (1964) 10, pp. 221–4, and P. De Bach, 'The competitive displacement and coexistence principles', *Annual review of entomology*, (1966) 11, pp. 183–212. See also De Bach, op. cit. (31), pp. 265–9. On De Bach's general views on the relationship between ecological theory and the practice of biological control, see P. Palladino, 'Ecological theory and pest control practice: A study of the institutional and conceptual dimensions of a scientific debate', *Social studies of science*, (1990) 20, pp. 255–81.

[34] B. P. Beirne, *Pest management*, London, 1967, p. 71.

[35] A further indication that this may in fact be the case is offered by one of Ray Smith's early discussions of 'integrated control', where he stated that, '[growers] must now be reconditioned to tolerate pest populations at sub-economic levels and refrain from attempts at eradication *through chemical control*' (emphasis added), R. F. Smith, 'Integration of biological and chemical control: Introduction and principles', *Bulletin of the Entomological Society of America*, (1963) 8, pp. 188–9.

It could be argued, however, that De Bach and Beirne contributed more to the technical rather than the conceptual development of 'integrated control', and that therefore their views are not very significant. Yet, even Carl Huffaker, the director of the Division of Biological Control at the University of California in Berkeley, and one of the conceptual founders of 'integrated control' whose views provide most of the material for Perkins' contrast between the ideological foundations of 'eradication' and 'integrated control', could sometimes accept the goal of eradication. In 1970, during a conference of seminal importance for the development of 'integrated control', Huffaker first asserted quite clearly that, 'eradication is outside the philosophy of pest management by definition', but then went on to concede that the complete elimination of a pest population was, sometimes, a legitimate goal.[36] He suggested that small, recently established, and circumscribed populations, especially if they were potentially so pernicious as to effectively prevent the implementation of management programs to control them and other secondary pests, could be targeted for eradication.[37] Interestingly, the criteria for legitimating the goal of eradication — small, recently established, and circumscribed populations — were so vague, that it was not at all clear why a program to eradicate the woodwasp in Australia discussed during the 1970 conference satisfied them while the boll weevil program did not.[38] Similarly, United States Department of Agriculture entomologists often argued that the intensive insecticidal programs required to protect cotton from the boll weevil prevented the implementation of effective management schemes to control a secondary, but very destructive pest on cotton, the cotton boll worm.[39] Huffaker had suggested that in a situation such as this one, eradication of the more pernicious pest might be a legitimate goal; yet, when it actually came to the boll weevil, he rejected this rationale. Huffaker's was not a very consistent position.

Huffaker's position on 'eradication' raises some doubts about Perkins' account of the controversy. Confidence in it is eroded still further upon consideration of the explicitly economic and professional motives for opposing 'eradication' offered by some other participants in the crucial round-table discussion in 1978. Dale Newsom, an entomologist from Louisiana State University and yet another central figure in the development of 'integrated control', was called upon to discuss the economic aspects of the controversy. He prefaced his position by stating that

[36] C. B. Huffaker, 'Summary of a pest management conference — A critique', in R. L. Rabb and F. E. Guthrie, eds., *Concepts of pest management*, Raleigh, 1971, p. 232.

[37] Huffaker, op. cit. (36), pp. 232–3; see also R. L. Rabb, 'Introduction to the conference', in Rabb and Guthrie, op. cit. (36), p. 3.

[38] See P. W. Geier, 'Temporary suppression, population management, or eradication: How and when to choose', in Rabb and Guthrie, op. cit. (36), pp. 170–190.

[39] R. L. Ridgway (USDA Agricultural Research Service), personal communication; R. Jones (University of Minnesota), personal communication.

> I do not consider it morally wrong and ecologically disastrous to attempt to eradicate some pest species. To the contrary, I would argue that such attempts may be appropriate pest management strategies in some cases, worthy of substantial, continuing research effort. However, the accomplishments of eradication efforts have been so meager and the costs so great that an assessment of the eradication strategy is long overdue. In addition to the investments of manpower and money in eradication efforts by the [United States Department of Agriculture] . . . substantial amounts are contributed by other agencies of the . . . Federal Government, State agencies, grower organizations, and individual growers. Considering funds from all sources, the amount spent for eradication is much too high a percentage of the total available for pest control. When judged by the extremely high ratio of failure to success, application of the eradication principle is a poor investment in pest management.[40]

Thus, although he acknowledged that some entomologists objected to 'eradication' on ethical grounds, Newsom himself did not view 'eradication' and 'integrated control' as mutually exclusive approaches. He was rather more concerned about the appropriation by the proponents of 'eradication' of a large fraction of the total allocations for research on pest control, mainly by making promises which, for the most part, were unfulfilled.[41] Ultimately, Newsom warned that the 'wishful thinking' characteristic of eradication projects presented a threat to public support for all entomological research. He did not believe that,

> society can indefinitely continue to pour money and manpower into ill-conceived eradication programs that produce nothing but continued dangling of the carrot of eventual eradication before society. Eventually our profession will lose credibility.[42]

Such concerns about the economic and professional implications of 'eradication' programs were not of marginal importance. Indeed, in his comments during this round-table discussion, Robert Rabb himself pointed out that

> there is a finite amount of resources (talent and money) to expend on entomological activities. How much of this is it prudent to expend on eradication efforts? . . . And what will be the effect of expensive large-scale eradication programs on basic research?[43]

These links between ideological, economic, and professional concerns

[40] L. D. Newsom, 'Eradication of plant pests — Con', *Bulletin of the Entomological Society of America*, (1978) **24** (1), pp. 36 and 38. Newsom's opposition to eradication programs has a long history, which goes back to the 1950s, see Carson, op. cit. (2), p. 172.

[41] For an alternative view on the accomplishments of eradication programs, see W. Klassen 'Eradication of introduced insect pests: Theory and historical practice', (manuscript).

[42] Newsom, op. cit. (40), p. 39.

[43] Rabb, op. cit. (25), p. 43.

suggest a much more complicated struggle than Perkins would have his readers believe, a struggle which can be understood more fully only by attending firstly to those political changes which so transformed America during the 1960s and early 1970s, and by then examining how this transformation affected the institutional organization of American entomological research, and especially the work of those entomologists who sought to develop 'integrated control' into a major new approach to pest control.

6. 'Silent spring', the politics of pest control, and the funding of agricultural research

Prior to the publication of *Silent spring*, the control of insect pests and the use of insecticides were matters of debate among a very small group of parties, primarily growers, usually represented by the American Farm Bureau Federation, commodity groups like the powerful National Cotton Council, and the chemical industry.[44] These groups were extremely influential in shaping the policies of the United States Department of Agriculture because until then agriculture and agricultural policy were an isolated and privileged domain that functioned according to very different rules from those pertaining to other areas of American politics and society.[45]

The chairman of the congressional subcommittee overseeing budgetary appropriations for national agricultural policy from 1947 to the present day, Congressman Jaimie Whitten of Mississippi, for example, has been likened to a 'feudal lord' because he has run agricultural policy according to his own priorities and those of his mostly cotton-growing constituency.[46] Thus, when the United States Department of Agriculture requested $25,000 to study new ways of addressing the boll weevil problem, Whitten rejected the request by appealing to the need for fiscal restraint and by questioning the necessity for any further research on this problem because DDT did the job quite well.[47] But at the same time he quickly approved the appropriation of $1.1 million for the construction of the Boll Weevil Research Laboratory requested by the same department.[48] He did so only because the laboratory

[44] For an analysis of the agricultural policy-making process in the 1950s and 1960s, see C. J. Bosso, *Pesticides and politics: The life cycle of a public issue*, Pittsburgh, 1987, pp. 21–78; and D. F. Hadwiger, *The Politics of Agricultural Research*, Lincoln, 1982, pp. 115–68. With regard to farmers' associations like the American Farm Bureau Federation, see Bosso, op. cit. (44), pp. 35–8; on commodity groups and agribusiness, see L. Busch and W. B. Lacy, *Science, agriculture and the politics of research*, Boulder, 1983, pp. 26–9.

[45] For an analysis of the place of agriculture in American society, see A. Mayer and J. Mayer, 'Agriculture, the island empire', *Daedalus*, (1974) 103 (3), pp. 83–95.

[46] Bosso, op. cit. (44), pp. 65–71; Hadwiger, op. cit. (44), pp. 130–32.

[47] Hadwiger, op. cit. (44), pp. 162–7. See also Bosso, op. cit. (44), p. 130; Whitten was one of the staunchest supporters of DDT during the years that eventually witnessed its ban, and his book, *That we may live*, (Princeton, 1966) was a true celebration of the marvels of DDT.

[48] Perkins, op. cit. (3), pp. 114–15.

was to be built in his own congressional district; had the location of the laboratory been different the outcome would not have favoured the plans of the Department of Agriculture.[49]

From the 1960s onward, however, the American political landscape was radically transformed as growers, commodity groups, the chemical industry and elected officials like Whitten had to come to terms with a new actor, the 'environmentalist' movement.[50] The disastrous impact of the indiscriminate use of insecticides during the 'eradication' campaigns on bird and fish populations, which often were far removed from the areas targeted by these programs, resulted in considerable opposition among an increasingly numerous and prosperous suburban middle class.[51] The members of this middle class had very little understanding of those economic pressures that led American farmers to support the widespread use of insecticides like DDT.[52] But, for all their opposition to what was happening on American farms, with all the implications it carried for the survival of wildlife, they were excluded from those political circles that controlled agricultural policy. This situation seemed to them profoundly anti-democratic.[53] This disquiet was exacerbated by a more general concern: here, as with fall-out from testing of nuclear weapons, the government seemed unwilling to do anything to protect the public from an invisible but ever-present menace.[54] The political consequences of these concerns were manifold.

[49] J. Whitten quoted in Bosso, op. cit. (44), p. 67.

[50] See Bosso, op. cit. (44). Most probably, this transformation of American politics was due to the changing character of the American economy: By the end of the second world war, it had become a predominantly industrial rather than agricultural one. This shift altered the composition of the American electorate — most Americans now lived in urban centres rather than on a farm — and thus also altered the balance of power in American politics.

[51] On public reactions to the highly publicized 'fire-ant' eradication campaigns, see Bosso, op. cit. (44), pp. 85–90; and Dunlap, op. cit. (2), pp. 89–90. For a contemporary assessment of the impact of these 'eradication' campaigns on non-target species, see O. B. Cope and P. F. Springer, 'Mass control of insects: The effect on fish and wildlife', *Bulletin of the Entomological Society of America*, (1959) **4** (2), pp. 52–6.

[52] For a review of the politics of growth and the 'environmentalist' movement, focusing especially on the class composition, see J. R. Logan and H. L. Molotch, *Urban fortunes: The political economy of place*, Berkeley, 1987, pp. 200–247, especially 209–23. Logan and Molotch reject the thesis that the 'environmentalist' movement is a middle-class phenomenon, but it is interesting that the earliest literature they cite on the subject tended to defend the opposite position. This suggests that the 'environmentalist' movement was originally middle-class and that its program was later accepted by lower social classes. On the relationship between the 'environmentalist' movement and the farming community, see the account (rather heavily slanted in favour of the first group) in S. P. Hays, *Beauty, health, and permanence: Environmental politics in the United States*, Cambridge, 1987, pp. 287–97.

[53] On the undemocratic aspects of these campaigns, see Carson, op. cit. (2), p. 127; with regard to the general political aims of *Silent spring*, see D. Fleming, 'Roots of the new conservation movement', *Perspectives in American history*, (1972) **6**, pp. 28–34.

[54] R. H. Lutts, 'Chemical fallout: Rachel Carson's *Silent spring*, radioactive fallout, and the environmental movement', *Environmental review*, (1985) **9**, pp. 211–25.

In 1962, the President, John Kennedy, whose election in many ways epitomized the political triumph of the new suburban middle class, requested that the issues raised by Rachel Carson in *Silent spring* be investigated by his Science Advisory Committee; the very decision to turn to this body rather than the National Academy of Sciences symbolized the fundamental change under way.[55] Although it had been usual practice to turn to the 'Biology and Agriculture Board' of the Academy for technical and scientific advice about agricultural issues, this board was now seen as too closely allied to the interests of the agricultural establishment to provide a balanced assessment of the problem.[56] In fact its 1963 report, *Pest control and wildlife relationships*, was scathingly criticized for being unduly sympathetic to the interests of the agricultural establishment, and to those of the chemical industry in particular.[57] The Science Advisory Committee, on the other hand, could hardly be said to represent the interests of this establishment, as only one of the nine panel members was affiliated with either the chemical industry, or an agricultural institution.[58] Apart from the differences in attitude and tone, however, the reports prepared by the two bodies agreed on the need to support more research on improving existing pest control strategies and to develop alternative ones. But such uncontroversial advice was bound to be without effect as long as funding for such research was not forthcoming from the congressional committees in charge of agricultural matters.

Even this situation began to change in the aftermath of *Silent spring*, as the sudden burst of public attention to the previously uninteresting politics of agriculture accelerated the ongoing transfer of political power from rural to urban America.[59] Many legislators rallied behind the 'environmentalist'

[55] President's Science Advisory Committee, *Use of pesticides*, Washington, 1963. See also Bosso, op. cit. (44), pp. 117–18. On environmentalist agenda of the Kennedy Administration (and also of later Administrations) see H. P. Caulfield, 'The conservation and environmental movements: An historical analysis' in J. P. Lester, ed. *Environmental politics and policy: Theories and evidence*, Durham, 1989, pp. 13–56 (especially pp. 26–48).

[56] Archives of the National Academy of Sciences Archives: Biology and Agriculture: Committee on Pest control and wildlife relationships: 1962–1963. The total budget for the study was $31,000, of which total, $10,750 was contributed by the pesticide industry, $15,500 by government agencies, and the rest by other interest groups.

[57] For an overview of the reception of the National Academy of Sciences (NAS) study, see Dunlap, op. cit. (2), p. 116. For a broader assessment of this and other related NAS studies, see P. M. Boffey, *The brain bank of America: An inquiry into the politics of science*, New York, 1975, pp. 199–226.

[58] The President's Science Advisory Committee panel responsible for the preparation of the 1963 report was composed of the following people: C. M. MacLeod, New York University, Chair; H. S. Bennett, University of Chicago; K. Clark, University of Colorado; P. M. Doty, Harvard University; W. H. Drury, Massachusetts Audubon Society; D. R. Goddard, University of Pennsylvania; J. G. Horsfall, Connecticut Agricultural Experiment Station; W. D. McElroy, Johns Hopkins University; and J. D. Watson, Harvard University.

[59] On the importance the media attached to this issue, consider the statement by the influential CBS News commentator, Eric Sevareid, following the release of the presidential report.

banner to challenge the power of established figures such as Whitten. For example, in 1963, the newly elected senator from Connecticut, Abraham Ribicoff, circumvented the agricultural committees by bringing the pesticide issue before an especially organized subcommittee of the Government Operations Committee, which then called on a wider range of witnesses than ever before, some representing interest groups that had previously been deliberately excluded from the debate.[60] In this changing atmosphere it became possible to obtain more federal funding for research on alternative pest control strategies, and in fact, the budget for all entomological research in the United States Department of Agriculture was increased very substantially over the period 1964–1965.[61]

7. *Funding for research on alternative pest control programs*

The allocation of these new federal funds affected the development of federal entomological research programs and those in the state agricultural experiment stations very differently. Until the early 1970s, when the United States Department of Agriculture underwent a radical reorganization, federal funding for agricultural research was channelled chiefly through the Department. Its Agricultural Research Service received about four-fifths of the allocation. These funds were then distributed in a manner such that the directors of the various divisions of the Service could exert extraordinary control over any areas of agricultural research that were not likely to be funded by the private sector or by the more fiscally conservative state legislatures.[62] It was thanks to this power that Edward Knipling could redirect the federal research agenda for entomology toward the development of alternatives to chemical control programs. He also used this power to garner political and financial support for his program to 'eradicate' the screwworm. The fifth of the federal funds for agricultural research remaining after the Agricultural Research Service got its share was divided between the fifty

Sevareid announced that, 'Miss Carson had two immediate aims. One was to alert the public; the second, to build a fire under the Government. She accomplished the first months ago. Tonight's report by the Presidential panel is prima facie evidence that she has also accomplished the second', quoted in F. Graham, *Since 'Silent spring'*, Boston, 1970, p. 79; see also Bosso, op. cit. (44), p. 125.

[60] Graham, op. cit. (59), pp. 74–5, 176–81, and Bosso, op. cit. (44), p. 121.

[61] Perkins, op. cit. (3), p. 36, and Bosso, op. cit. (44), p. 121.

[62] V. W. Ruttan (University of Minnesota), personal communication. See F. W. Maxwell et al., 'Report of the committee on impact of ARS-USDA reorganization on the profession of entomology', *Bulletin of the Entomological Society of America*, (1974) 20, pp. 50–53; and K. L. Knight et al., 'Report of the 1975 ESA committee on impact of ARS-USDA reorganization on the profession of entomology', *Bulletin of the Entomological Society of America*, (1976) 22, pp. 65–73, for the reactions of the Entomological Society of America to the reorganization of the USDA-ARS during the 1970s.

state agricultural research systems. This resulted in a very thin distribution of funds to researchers outside the United States Department of Agriculture.[63] State scientists therefore relied largely on state legislatures and the private sector for support. In the case of entomology, the very practical demands of these sponsors resulted in very narrowly defined research programs that emphasized above all improvement of chemical control.[64] Thus, by the 1960s, entomological research programs in federal and state research centres were very different: research in the state systems had hardly been affected by Knipling's reforms.[65] This difference was exacerbated in the years following the publication of *Silent spring*. More federal funds for research on alternative pest control strategies were now available, but the largest share went to federal rather than state research programs. Furthermore, the largesse of the federal government was not matched by the state legislatures in most agricultural states, as the environmental movement carried considerably less political clout there.[66] Thus, the very active, but quite exceptional research programs led by Carl Huffaker, Robert Rabb, and Perry Adkisson, in California, North Carolina, and Texas, programs which aimed to reduce agricultural producers' reliance on insecticides just as much as Knipling's, benefited very little from this increase.[67]

[63] See E. L. Peterson, *The research program of the USDA*, Statement before the federal council on Science and Technology, 28 April 1959; L. E. Hawkins et al., *Fund support to the state agricultural experiment stations*, Statement before the Division of agriculture, Association of State Universities and Land-Grant Colleges, November 1964; and US Department of Agriculture, *Pesticide program for fiscal year 1965*, Washington, 1965.

[64] See Palladino, op. cit. (1).

[65] US Senate, *Inter-agency coordination in environmental hazards*, Hearings before the Subcommittee on Reorganization and International Organizations, 88th Congress, Second Session, Washington, 1964.

[66] For a review of the relationship between federal and state responses to environmentalists' concerns, see C. E. Davis and J. P. Lester, 'Federalism and environmental policy' in Lester, op. cit. (55), pp. 57–84.

[67] Detailed data on the distribution of federal funds for agricultural (environmental) research in the state agricultural experiment stations during the period prior to 1966 are not available. Furthermore, since the state budgets of California, North Carolina, and Texas are very large, state allocations for environmental protection are relatively unaffected by any developments at federal level, and collection of data relevant to my argument is even more difficult. Therefore, I rely here largely on interviews with Edward Knipling and Richard Riley of the USDA Cooperative State Research Service, who by virtue of their positions are capable of providing a general overview of what was happening in the different states. Since the Cooperative State Research Service — established to link the federal and state agricultural research system — is staffed by personnel on leave from the state agricultural experiment stations, the service tends to be much more sympathetic to peculiar priorities of researchers in the experiment stations than is the Agricultural Research Service, I believe that between the two of them, Knipling and Riley provide a balanced picture of the situation.

8. *The search for support of research on 'integrated control'*

Given a political and institutional setting that favoured the research agenda of scientists in the service of the United States Department of Agriculture, those in the state agricultural experiment stations who did not enjoy any substantial support beyond what was allocated to them by the federal government and state legislatures, had to seek support from agencies not usually associated with agricultural research. Thus, the National Association of Land-Grant and State Universities suggested in 1964 that they should turn to the National Science Foundation, the National Institutes of Health, the Atomic Energy Commission, or the Department of Defense for support.[68] But such diverse organizations could not support projects of a scale comparable to the screwworm program or the Pilot Boll Weevil Eradication Experiment. However, with the establishment of the International Biological Program during the mid 1960s, the problem facing the proponents of 'integrated control' seemed to be resolved.

The International Biological Program was conceived in the early 1960s by a number of biologists researching fields that had failed to expand as successfully as molecular biology and other bio-medical sciences.[69] The American participants in this program, especially the members of one particular school of ecology, argued widely that government support for their cause was necessary to fully develop the unique potential of ecology to 'manage' the problems of environmental degradation that so alarmed the American public during this period. Following the publication of the 1965 President's Science Advisory Committee report, *Restoring the quality of our environment*, these ecologists were able to bring their case to the attention of the House of Representatives Subcommittee for Science, Research and Development.[70] Their rhetoric was so attuned to prevailing public and political concerns that, in 1970, the President, Richard Nixon, signed a bill allocating $40 million to the National Science Foundation, to support the American effort in the International Biological Program.

Carl Huffaker was an early participant in this monumental endeavour to

[68] Hawkins, op. cit. (63). The pioneering work of Vernon Stern and his colleagues in the Agricultural Experiment Stations at Riverside and Berkeley in developing an effective 'integrated control' program during the late 1950s was supported by the National Institutes of Health, as was also that of Robert Rabb and his colleagues at North Carolina State University.

[69] See also C. Kwa, 'Representation of nature mediating between ecology and science policy: The case of the International Biological Programme', *Social studies of science*, (1987) **17**, pp. 413–42 (see also his 'Mimicking nature: The development of systems ecology in the United States, 1950–1975', Ph.D. dissertation, University of Amsterdam, 1989); and P. Palladino, 'Defining ecology: Ecological theories, mathematical models, and applied biology in the 1960s and 1970s', *Journal of the history of biology*, (1991) **24**, pp. 223–43.

[70] President's Science Advisory Committee, *Restoring the quality of our environment*, Washington, 1965. For an overview of these developments, see Kwa, op. cit. (69).

transform the place of ecology in the American republic of science.[71] From its very outset, Huffaker sought to develop a research program which would demonstrate the effectiveness of an ecological approach to pest control. For him, crop protection should be viewed as one among many different aspects of crop production (albeit the most interesting to insect ecologists), which could be 'managed' properly only by understanding the reciprocal interactions of large, complex agricultural ecosystems and the human decisions regarding their use.[72] Thus, he proposed to examine the structure of five separate crop production systems and then to develop mathematical models that could eventually serve as guides for managing the use of pesticides and biological agents to control the various species attacking these crops. However, this plan to transform 'integrated control' into 'integrated pest management' ran into problems due to its explicitly agricultural character.[73]

The rules governing the American side of the International Biological Program stipulated that, like all the proposals submitted to the American National Committee of the International Biological Program, the one proposed by Huffaker should be supported by the National Science Foundation.[74] The National Science Foundation, however, was reluctant to back Huffaker's plan since it viewed it as agricultural research, which, in its opinion, fell within the purview of the United States Department of Agriculture.[75] At this time, however, the relations between the Department of

[71] Huffaker was highly respected among many ecologists for some classic ecological experiments in the 1950s, and Fred Smith, one of the principal figures in the conception and development of the International Biological Program, considered him as 'one of us', F. E. Smith (University of Michigan), personal communication.

[72] C. B. Huffaker (University of California), personal communication; O. Loucks (Butler University), personal communication. For a brief account of the development of Huffaker's plan in the context of IBP, see US National Committee for the IBP, *Research programs constituting US participation in the International Biological Program*, Washington, 1968, pp. 16–17, 38–41, US National Committee for the IBP, *Research programs constituting US participation in the International Biological Program*, Washington, 1971, pp. 75–7, US National Committee for the IBP, *US participation in the International Biological Program*, Washington, 1973, pp. 36–7, and US National Committee for the IBP, *US participation in the International Biological Program*, Washington, 1974, pp. 62–4. See also C. B. Huffaker and R. F. Smith, 'The IBP program on the strategies and tactics of pest management', *Proceedings of the Tall Timber Conferences*, (1972) 4, pp. 219–36.

[73] Although the 'integrated control' approach is now generally known as 'integrated pest management', to avoid unnecessary confusion I will continue to use the older term, except when referring to Huffaker's project.

[74] J. Neuhold (Utah State University; formerly employed by the National Science Foundation) to Paolo Palladino, 17 June 1988. Other projects developed outside the US/IBP National Committee could be included under the IBP umbrella, but the funding would have to come from outside the National Science Foundation.

[75] During the late 1960s, the National Science Foundation came under considerable political pressure to support more 'applied' research, but this pressure was met by equally considerable opposition from many of the scientists traditionally supported by National Science Foundation. The outcome of this struggle was the organization of a Research Applied to National Needs

Agriculture and the proponents of the national Integrated Pest Management project were rather tense.[76] Huffaker had expressed reservations about the role the Department might want to play in the project, fearing as he did that it would try to control its future development.[77] The Department was equally suspicious of its prospective partner. It argued that it was already involved in programs that closely paralleled many of the activities contemplated by Huffaker and his colleagues. In fact, a number of projects relating to the ecological aspects of agricultural production, including pest control, had been funded by the Department during the 1960s, and were later incorporated into International Biological Program in the hope of thus providing some coherence to the overall effort. The Department of Agriculture therefore viewed the national Integrated Pest Management project simply as an attempt by state scientists to assert their institutional independence.[78] Given the attitude of the National Science Foundation and the United States Department of Agriculture, it seemed that Huffaker and his colleagues would

program within the National Science Foundation. However, there were no provisions within this new programme to support agricultural research. See US House of Representatives, *A history of science policy in the United States, 1940–1985: Report prepared for the Committee on Science and Technology*, 99th Congress, Second Session, Washington, 1986, pp. 57–60, and M. Lomask, *A minor miracle: An informal history of the National Science Foundation*, Washington, 1976, pp. 237–250. Also D. Bottrell (University of Maryland), personal communication; T. Davies (Conservation Foundation), personal communication; W. Dickinson (Environmental Protection Agency), personal communication; W. Muir (private consulting firm), personal communication. Davies and Dickinson were in the White House Office of Management and Budget when these issues were being debated and the Nixon administration was seeking to bring its weight to bear on the various agencies involved in funding Huffaker's project; Muir instead served on the Council of Environmental Quality.

[76] From here on, I will refer to Huffaker's project as the 'national Integrated Pest Management project'.

[77] Huffaker had hoped that the participation of the National Science Foundation would act to preserve the independence of state scientists, see the files of Waldemar Klassen (henceforth Klassen Files) 'NSF–IBP/RANN–Dr. Huffaker': C. B. Huffaker to Steering Committee, 14 October 1971; Minutes of Steering Committee Meeting, 2 December 1971. (Klassen, now director of the USDA Science and Education Administration, was in the service of the Agricultural Research Service at the time of the events discussed here). See also Perkins' discussion of the composition of the Steering Committee in op. cit. (3), p. 151. The leaders of the national IPM program also succeeded in antagonizing the USDA-ARS by not inviting Knipling to the 1970 conference in Raleigh on the 'Principles of Pest Management' — see Rabb and Guthrie, op. cit. (36). B. P. Beirne (Simon Fraser University) has suggested that the Raleigh conference was of seminal importance to the development of IPM in the United States. B. P. Beirne to Paolo Palladino, 12 February 1988. See also Rabb's remarks about the exclusion of Knipling from this conference, in R. L. Rabb, review of *Insects, experts, and the insecticide crisis: The quest for new pest management strategies*, in *Agriculture and human values*, (1984) 1 (1), pp. 34–7.

[78] The Department also resented the implication that it was not responding to the environmental crisis in an adequate manner, and that it was ignoring the role of state agricultural scientists, Klassen Files: 'NSF–IBP/RANN–Dr. Huffaker': H. C. Cox to T. W. Edminster, 27 September 1971.

be unable to obtain either political or financial support for their activities in the United States, notwithstanding the fact that 'integrated control' had been endorsed in 1965 by the Food and Agriculture Organization of the United Nations as one of the most promising answers to the problems of crop protection in the lesser developed countries.[79] Huffaker and his chief collaborators, Ray Smith, from the Department of Entomology of the University of California at Berkeley, and Perry Adkisson, from the Department of Entomology of Texas A & M University, had to seek their support in the halls of Congress and the White House.

9. *Tapping the environmental movement*

In the late 1960s and early 1970s, environmental issues had become major items on the American political agenda. Both the (Republican) Nixon Administration and the Democrats in the Congress were vying with one another to capture the support of the environmentalist movement, and 'ecology' and 'environment' became central items in the arsenal of political rhetoric. Furthermore, the Nixon Administration was aggressively pursuing a new federalist policy of decentralizing the powers of the federal government.[80] This policy was closely connected with Nixon's effort to bring about a greater degree of political accountability in all areas of governmental activity, including support of science.[81] At the outset, these developments enabled Huffaker and his colleagues to surmount the opposition of the National Science Foundation and United States Department of Agriculture to the national Integrated Pest Management project. Perry Adkisson could successfully turn for support to his Texas congressman and powerful chairman of the House of Representatives Appropriations Committee, Congressman George Mahon. Ray Smith instead could turn with equal success to Nixon's chief of staff, John Erlichman, who then ordered the National Science Foundation to support the Integrated Pest Management project under the umbrella of the International Biological Program, and directed the United States Department of Agriculture to facilitate this operation.

The long-term effect of these political changes however was even more significant for the development of 'integrated control'.[82] In 1970, the

[79] Food and Agriculture Organization, *Report of the FAO symposium in integrated pest control*, Report No. PL/1965/15, Rome, 1966.

[80] See M. D. Reagan and J. G. Sanzone, *The new federalism* [2nd ed.], New York, 1981, pp. 3–29.

[81] J. E. Katz, *Presidential politics and science policy*, New York, 1978, pp. 180–88; and S. D. Fries, 'The ideology of science during the Nixon years, 1970–1976', *Social studies of science*, (1984) 14, pp. 323–41.

[82] R. L. Ridgway (USDA Agricultural Research Service), personal communication; R. Sauer (University of Minnesota), personal communication; and J. Neuhold to Paolo Palladino, 17 June 1988. It was also very useful to the progress of the national Integrated Pest Management

competition to capture the vote of the environmental movement between Senator Edmund Muskie, who was preparing the ground for his 1972 presidential bid, and Richard Nixon, resulted in congressional legislation and executive orders leading to the establishment of the Environmental Protection Agency and the Council of Environmental Quality.[83] Soon after their establishment, these organizations turned their attention to the issue of insecticides in the environment. Responding to the controversies surrounding DDT, the Administration requested the Council of Environmental Quality to develop guidelines for amending the Federal Insecticide, Fungicide and Pesticide Act.[84] One of the most significant recommendations made by the Council was the transfer of power to regulate the insecticide industry from the United States Department of Agriculture to the Environmental Protection Agency. While responding to public mistrust of the Department as a captive of the chemical industry, incapable, if not unwilling, to enforce existing regulations, this transfer of responsibilities was also part of the Administration's plan to debilitate all independent federal centres of power, and the Department of Agriculture was foremost among them. A second, very important recommendation of the Council of Environmental Quality, which had the same political effect, was to channel through the Environmental Protection Agency all federal support for research on 'biologically integrated alternatives for pest control'.[85] The definition of the legitimate beneficiaries of such support written into this report (with the aid of Huffaker, Smith, Robert Van den Bosch, and Dale Newsom) was so worded that only 'integrated control' programs could qualify. It laid great emphasis on the role of biological control and economic thresholds in these 'biologically integrated' approaches to pest control.[86] Other techniques, central to 'eradication' programs were given but a cursory treatment, and, although 'eradication' was contemplated as one possible pest control tactic, its applicability was very severely restricted.[87] Since 'eradication' was a major component of the United States Department of Agriculture research brief, this definition of acceptable alternatives effectively excluded the Department

(IPM) project to have an ally such as William Caldwell in the White House Office of Science and Technology, Klassen Files: 'NSF–IBP/RANN–Dr. Huffaker': C. B. Huffaker to Steering Committee, 14 October 1971.

[83] Bosso, op. cit. (44), pp. 151–4, and E. J. Burger, *Science at the White House*, Baltimore, 1980, pp. 89–90.

[84] Bosso, op. cit. (44), pp. 143–77.

[85] Council on Environmental Quality, *Integrated pest management*, Washington, 1972. See also US Senate, *Federal environmental pesticide control act*, Part II, Hearings before the Subcommittee on Agricultural Research and General Legislation, 92nd Congress, Second Session, Washington, 1972, pp. 56–7. For a more detailed insight into the politics of IPM during this period, see US Senate, *Pest control research*, Hearings before the Subcommittee on Agricultural Research and General Legislation, 92nd Congress, First session, Washington, 1971.

[86] Council of Environmental Quality, op. cit. (85), p. vii.

[87] Council of Environmental Quality, op. cit. (85), p. 7.

from obtaining any new funds until it redefined its program. Not surprisingly, it was highly critical of the report.[88] But more significantly, the Environmental Protection Agency was now competing directly with the Department of Agriculture for congressional allocations to cover the costs of funding environmental and related entomological research, thus diminishing the power the latter could exercise over the national research agenda for these fields.

Yet, any mandates to address the environmental crisis were meaningless in the absence of support for them in the congressional committees overseeing budgetary appropriations. The chairman of the House of Representatives Appropriations Committee, Mahon, and his colleague, Jaimie Whitten, the chairman of the Subcommittee for Agricultural Appropriations, were both staunch supporters of the 'iron triangle' linking their committees, the United States Department of Agriculture, and the various agricultural interest groups, and they were not very sympathetic to the intrusions of the environmental movement (represented by the Council of Environmental Quality and the Environmental Protection Agency) into this domain.[89] They fought a bitter battle against the amendment of Federal Insecticide, Fungicide and Pesticide Act, and Mahon's eventual assignment of budgetary responsibilities for this legislation to Whitten gave this staunch supporter of the pesticide industry partial control over the budget of the Environmental Protection Agency, even though one of the many reasons for creating this agency had been to subvert the close alliance of the United States Department of Agriculture and the industry. This situation, in addition to Whitten's noted fiscal conservatism, did not promise well for the development of 'biologically integrated alternatives for pest control'. Yet, if any program to support research on 'ecological' approaches to pest control was going to receive any funds at all (as was demanded by the Administration), it was far more likely that congressional representatives from agricultural districts, such as Mahon and Whitten, would support the Integrated Pest Management project rather than the one proposed by the United States Department of Agriculture. Their support for the Department of Agriculture was tempered by general suspicion of all federal programs as somehow interfering with the rights of individual citizens. The Pilot Boll Weevil Eradication Experiment was virtually designed to transform this suspicion into active opposition by requiring the co-ordination of agricultural practices of independent growers on a regional basis, and by relying on both a highly centralized organization and a great deal of coercion to do so. These measures seemed to violate the basic right of growers to do as they pleased on their own land. 'Integrated control', on the other hand, seemed to lend

[88] R. L. Ridgway (USDA Agricultural Research Service), personal communication.

[89] Bosso, op. cit. (44), pp. 6 and 154.

[90] W. Klassen (USDA Science and Education Administration), personal communication;

itself more easily to implementation on a farm-by-farm basis, and thus fit much better with prevailing individualist, agrarian ideology.[90] Furthermore, while the active role taken by the Administration in promoting 'integrated control', with all the accompanying 'environmentalist' rhetoric, served very usefully to steal some of the thunder from Muskie's presidential bid, it did not ruffle unduly the feathers of the chemical industry. Insecticidal control was, after all, a central and permanent feature of 'integrated control', and the national Integrated Pest Management project was a research program and nothing more.[91] Eventually, this combination of powerful (if reluctant) political support in Congress and the 'environmentalist' and 'neo-Federalist' agenda of the Nixon Administration provided for the smooth passage through Congress of the Federal Insecticide, Fungicide and Pesticide Act and the annexed line item mandating federal support for 'integrated control' through both the Environmental Protection Agency and the United States Department of Agriculture.

10. *Crisis and conflict*

The social and political alliances which had supported the new environmentalist agenda suddenly collapsed in 1973. In the wake of the 'oil crisis' and the general downturn of the American economy, federal spending was severely curtailed and the legitimacy of this agenda now had to be judged in strictly economic terms.[92] Pest control research was not immune to this change. Carl Huffaker's program faced severe financial difficulties as the three agencies supporting it attempted to divest themselves of any responsibility for its continuation. The Environmental Protection Agency questioned the propriety of its involvement, viewing the program as falling outside its scope of responsibilities; it was, after all, a regulatory agency.

R. L. Rabb (North Carolina State University), personal communication. See also Perkins, op. cit. (3), p. 140; and Rabb, op. cit. (77). For a different view of the relationship between 'integrated control' and the organization of agricultural production, see P. De Bach, 'Successes, trends, and future possibilities', in P. De Bach, ed., *Biological control of insect pests and weeds*, New York, 1964, p. 713.

[91] W. Dickinson (Environmental Protection Agency), personal communication. See also 'The President's Message on the Environment', 8 February 1972, in Council of Environmental Quality, *The President's 1972 environmental program*, Washington, 1972, pp. 6–7. For the views of the chemical industry on 'integrated control', see M. Y. Demaret, 'Déclaration faite par M. Y. Demaret, sécrétaire général du GIPAF, au nom de l'industrie des pesticides au cours de la conférence FAO sur l'écologie en relation avec la lutte contre les parasites des plantes', in *Proceedings of the FAO conference on ecology in relation to plant pest control*, Rome, 1974, pp. 321–6

[92] Bosso, op. cit. (44), pp. 178–181. Another important factor in the reductions of budgets for environmental initiatives was that Nixon's landslide victory in the 1972 presidential elections removed the imperative that had motivated the earlier challenge of entrenched political powers, see Bosso, op. cit. (44), p. 174.

At most, the Agency was interested in supporting research on economic thresholds, research which would directly support its regulatory functions.[93] But any greater emphasis on such 'economic' studies would have made the National Science Foundation even more aware of the fact that it was supporting research outside its traditional domain.[94] In fact, with the termination of the International Biological Program, the National Science Foundation sought to end its involvement in the Integrated Pest Management program. The United States Department of Agriculture, on the other hand, had been placed in an unusual situation when it was ordered to support Huffaker's project. Since at the outset of the project its leaders had been leery of any direct involvement of the Department, fearing that they might lose control over it, they had sought to obtain support from the Department only in the form of extramural research grants. However, this category of funds had been severely reduced by the Nixon Administration, as part of its campaign to restrict the power of federal agencies.[95] If forced to continue supporting Huffaker's program, the Department of Agriculture would have then had to provide logistical and manpower support for a research program over which it had minimal control, and do so out of funds designated solely for its own projects. The Department dug in its heels.[96] Thus, in 1973 Huffaker and his colleagues found themselves having to cut $500,000 from their budget. The Environmental Protection Agency and National Science Foundation suggested that the cotton sub-project, which was directed by Perry Adkisson, should absorb the brunt of it, since 'cotton research is funded from a variety of sources'.[97] Yet, in 1973 the National Cotton Council, the most important organization of American cotton growers and most significant source of funding for cotton research outside the federal government, was already supporting the Pilot Boll Weevil Eradication Experiment and was committed to supporting the even more ambitious and expensive

[93] Klassen Files: 'NSF–IBP/RANN–Dr. Huffaker': W. Klassen to R. J. McCracken, 30 October 1973. See also National Academy of Sciences–National Research Council, *Pest control: An assessment of present and alternative technologies*, Vol. I, *Contemporary pest control practices and prospects: The report of the Executive Committee*, Washington, 1975, pp. 168–9.

[94] Klassen Files: 'NSF–IBP/RANN–Dr. Huffaker': C. H. Hoffman to H. O. Graumann, 26 September 1973.

[95] W. Klassen (USDA Science and Education Administration), personal communication. Following the 1972 reorganization of the USDA-ARS, which resulted in the decentralization of its activities, the costs of establishing additional regional and area offices were paid out of the ARS divisional reserve funds. Prior to the reorganization, these funds had been used by Knipling to improve the connections between the Entomology Research Division of the Department of Agriculture and the scientists in the state agricultural experiment stations and land-grant universities.

[96] Klassen Files: 'NSF–IBP/RANN–Dr. Huffaker': W. Klassen to R. J. McCracken, 30 October 1973.

[97] Klassen Files: 'NSF–IBP/RANN–Dr. Huffaker': W. Klassen to R. J. McCracken, 30 October 1973, p. 2.

Boll Weevil Eradication Trial.[98] Adkisson was caught between federal agencies, which were cutting their appropriations, and the only alternative sponsor, the National Cotton Council, which was already in the pocket of the proponents of 'eradication'. Moreover, as a member of the technical committee overseeing the Pilot Boll Weevil Eradication Experiment, Adkisson was well aware that the National Cotton Council was supporting a program that had failed to fulfil its ambitious promise to eliminate the boll weevil once and for all time, and that many members of the Entomological Society of America felt the same way.[99] At this juncture, what had been simmering discontent among the leaders of the Integrated Pest Management project erupted into an open challenge of the United States Department of Agriculture pest control research program. And, once again, the prevailing political environment provided them with an opportunity to challenge the federal establishment with great effectiveness.

In the early 1970s, the entire research program of the United States Department of Agriculture had come under intense scrutiny. Up until that time, state agricultural experiment stations were apportioned federal funds for research in strict proportion to the farming population of each state.[100] The Nixon Administration sought to eliminate such 'formula-funding' so as to reduce the power granted to the Department of Agriculture through this funding mechanism. A controversial report by the National Academy of Sciences provided a cover for this political campaign. The so-called 'Pound Report' suggested that 'formula-funding' led to excessive duplication of efforts and low quality research, and that a competitive grant system would be far more effective.[101] This report was soon followed by another study by the National Academy of Sciences of current pest control strategies and prospects for the development alternative ones.[102] The chairman of the new investigative team, Donald Kennedy of Stanford University, was very sym-

[98] Perkins, op. cit. (3), pp. 139–141; Perkins, 'Boll weevil', op. cit. (3).

[99] Perkins, op. cit. (3), pp. 127–39.

[100] See V. W. Ruttan, *Agricultural research policy*, Minneapolis, 1982, pp. 81–2, and 255–6.

[101] See National Academy of Sciences–National Research Council, *Report of the Committee on Research Advisory to the United States Department of Agriculture*, Washington, 1972. For a brief review of the report, see Hadwiger, op. cit. (44), pp. 78–79, and H. T. Huang, 'The USDA's competitive grant program and agricultural research', *Agricultural history*, (1988) 62, pp. 270–278. However, for a more critical assessment, see N. Wade, ' Agriculture: NAS panel charges inept management, poor research', *Science*, (1973) 179, pp. 45–7, N. Wade, 'Agriculture: Critics find research stunted and wilting', *Science*, (1973) 180, pp. 390–91, and R. Levins, 'Fundamental and applied research in agriculture', *Science*, (1973) 181, pp. 523–4. Interestingly, the authors of the 'Pound' report took research in molecular biology as a major criterion for judging the quality of the research funded by the Department. At this time, molecular biologists, who had enjoyed virtually unlimited support throughout the 1960s, were also facing a reduction of support for their research through cuts in the National Institutes of Health, see Katz, op. cit. (81), pp. 194–200.

[102] National Academy of Sciences–National Research Council, op. cit. (93).

pathetic to the findings and recommendations of the 'Pound Report', and Perry Adkisson figured very prominently in this project as a member of the Executive Committee, as did John Perkins in the role of Principal Staff Officer.[103] The final committee report, *Pest control: An assessment of present and alternative technologies*, appeared in 1975 after three years of exhaustive investigations. Not unexpectedly, it was quite critical of United States Department of Agriculture research programs in the area in general, and in particular, it was extremely critical of the boll weevil 'eradication' program. The Executive Committee complained that,

> the agricultural research enterprise now lacks adequate capacity to conduct experiments on the large scale necessary to evaluate the impact of changes in practice on a particular crop over a wide area . . . [T]he only project of such scale to have drawn significant support to date, unfortunately, are eradication programs.[104]

The authors of the report then went on to endorse 'integrated control' resoundingly, striving to emphasize the fundamental differences between 'eradication' programs and 'integrated control'.[105] Finally, the Committee expressed the belief that,

> the promise offered by this general approach [integrated control] makes it an appropriate candidate for high-priority attention by federal funding agencies.[106]

The report left the researchers in the United States Department of Agriculture very embittered because it seemed to imply that they were wholly opposed to 'integrated control', even though during its preparation they had pointed out repeatedly how they had made significant contributions to the development of alternative approaches to chemical control that were both 'integrated' and 'biological', and had also argued that the merits of the boll weevil eradication program had been judged unfairly.[107]

Notwithstanding the protestations of the Department of Agriculture, the

[103] J. H. Perkins, 'Strategies for pest control: The NAS report', *Proceedings of the Agricultural Research Institute*, (1979) **28**, pp. 105–21; J. H. Perkins (Evergreen College), personal communication; D. Kennedy (Stanford University), personal communication. From the very outset, the committee responsible for this study was viewed as biased against the 'agricultural establishment.' National Academy of Sciences Archives. A: C & B: ESB: X: 1971–1973. CNR/ESB: Problems of Pest Control: Executive Committee, 1972: General. J. H. Perkins to R. A. Carpenter, 28 August 1972.

[104] National Academy of Sciences–National Research Council, op. cit. (93), p. 18.

[105] National Academy of Sciences–National Research Council, op. cit. (93), pp. 380–99.

[106] National Academy of Sciences–National Research Council, op. cit. (93), p. 386.

[107] R. Riley (USDA Cooperative State Research Service), personal communication. See also National Academy of Sciences Archives. A & C: CNR: ESB: X: 1973–1976. CNR/ESB: Problems of Pest Control: Review Process: 1975. E. F. Knipling to R. E. Green, 29 April 1975; E. F. Knipling to P. Handler, 24 June 1975; R. W. Long to T. Shad, 15 July 1975.

National Academy of Sciences report was very influential in the White House Office of Management and Budget and also in its Office of Science and Technology, and had an important impact in redirecting the efforts of the United States Department of Agriculture away from 'eradication', and toward greater support for 'integrated control'.[108] Thus, although the Department of Agriculture still managed to obtain funds during the mid 1970s to proceed with its Boll Weevil Eradication Trial, such funding was conditional on support for an Optimum Pest Management Trial.[109] This last program was essentially identical to the one developed by Adkisson in Texas during the 1960s and further refined in the context of the Integrated Pest Management program headed by Huffaker.[110] But the tide was running against the supporters of 'eradication' programs.

Although the first (1972) Council of Environmental Quality report to the President on problems of pest control, prepared with the advice of the chief proponents of 'integrated control', recommended that control programs should not aim to completely eliminate any insect population, even 'eradication' in the classical sense — the broadcast use of chemical insecticides — was still considered a sometimes acceptable approach. This relatively catholic attitude was completely absent from the second (1979) Council report on pest control, even though it was written with the aid of virtually the same supporters of 'integrated control' responsible for the first one.[111] It portrayed a far more antagonistic relationship between 'eradication' and 'integrated control'; for example, the first principle defining the last strategy was that

> potentially harmful species will continue to exist at tolerable levels of abundance. The philosophy is to manage rather than to eradicate the pests.[112]

Here the term 'eradication' referred specifically to the Pilot Boll Weevil Eradication Experiment, not any classical spraying program.[113] This second report served as the basis for President Jimmy Carter's directive to the

[108] P.M. Smith (National Academy of Sciences), personal communication; D. Kennedy (Stanford University), personal communication.

[109] R. L. Ridgway and E. P. Lloyd, 'Evolution of cotton insect management in the United States', in R. L. Ridgway et al., eds., *Cotton insect management with special reference to the boll weevil*, USDA Agriculture Handbook No. 589, Washington, 1983, pp. 3–27; J. L. Hamer et al., 'Optimum pest management trial in Mississippi', in Ridgway, *Cotton insect management*, op. cit. (109), pp. 385–407.

[110] D. R. Rummel et al., 'An appraisal of a 10-year effort to prevent the westward spread of the boll weevil', *Bulletin of the Entomological Society of America*, (1975) 21, pp. 6–11.

[111] This report was in large part the work of Dale Bottrell, a close associate of both Carl Huffaker and Perry Adkisson, C. B. Huffaker (University of California), personal communication.

[112] Council of Environmental Quality, Council of Environmental Quality, *Integrated Pest Management*, Washington, 1979, p. vi.

[113] Council of Environmental Quality, op. cit. (112), p. 105.

relevant federal agencies, chiefly the Department of Agriculture, to 'modify as soon as possible their existing pest management research, control, education, and assistance programs and to support and adopt IPM [integrated control] strategies wherever practicable'.[114] The proponents of 'integrated control' appeared to have won the day; they had marshalled a far more powerful coalition than the Department of Agriculture ever could, and were then able to force the Department to adopt their own agenda for pest control.

11. Conclusion

The historian John Perkins has argued that during the 1970s the American entomological community was deeply divided over the merits of the two principal programs developed during the 1960s and 1970s to address the agricultural and environmental problems resulting from an ecologically indiscriminate use of insecticides, because these programs were based on incompatible appreciations of the proper relationship between humanity and nature. The systematic development and use of artificial means to create an environment completely free of any economically destructive insect species supposedly reflected a deep desire on the part of the supporters of the 'sterile-male release' approach to the absolutely dominate nature. The proponents of 'integrated control' instead sought to use biological means to maintain insect depredations at an economically acceptable level. This more restrained approach, according to Perkins, was motivated by a basic belief in the equal right to existence of all species alike. For this last group of entomologists, the absolute control sought by their rivals was not only an unobtainable goal, it was also an unethical one.

Perkins' account of the division between these entomologists in terms of incompatible ideological commitments is at odds with the inconsistent arguments against 'eradication' offered by the chief proponents of 'integrated control', and with the equally inconsistent assessments of 'eradication' programs in the two reports on pest control by the President's Council of Environmental Quality, which they had helped to prepare. These inconsistencies can be explained by considering the political and institutional environment in which the debates over 'eradication' took place.

The organization for funding of American agricultural research up to the 1960s had resulted in a tense relationship between researchers in the United States Department of Agriculture and in the state agricultural experiment stations, and especially between entomologists working to replace existing practices of pest control with economically more sustainable and environmentally less disruptive ones. However, the mounting concern of the American public during the 1960s over a seemingly unarrestable destruction of

[114] J. Carter quoted in Council of Environmental Quality, op. cit. (112), p. 116.

their natural environment relieved some of this tension as more funds became available for any research programs aiming to halt such destruction. But later in the decade this atmosphere of relative conviviality began to vanish, and finally disappeared in the mid 1970s, as the American economy began to falter. Those researchers in the state agricultural experiment stations of California, Texas, and North Carolina, who led a national program for the development of 'integrated control', were able to shore up their program by drawing some very sharp distinctions between what they were doing and the 'eradication' program promoted by the Department of Agriculture. By drawing attention to a particularly popular view of the relationship between humanity and nature, which seemed compatible with 'integrated control' alone, they were able to enrol on their side an ally previously shared with the researchers in the Department of Agriculture, namely the 'environmentalist' movement. But this move could not have succeeded if the appeal to 'environmentalist' values to distinguish between the two programs had not attracted the support of the Nixon Administration. The Administration was already predisposed to support the national program for the development of 'integrated control' as a ploy to undermine the power of the United States Department of Agriculture, and was also seeking to capture the support of the 'environmentalist' movement. The appeal to 'environmentalist' values served to cement the exclusive alliance between the Administration and the proponents of 'integrated control'. This account in terms of political alliances, however, does not necessarily mean that all the appeals to 'environmentalist' values were motivated by political opportunism alone. It is quite likely that some of the proponents of 'integrated control', such as Robert Rabb, genuinely believed that the their strategy and 'eradication' programs were predicated on fundamentally different conceptions of the relationship between humanity and nature. Carl Huffaker and Ray Smith certainly expressed some reservations (albeit very ambiguously) about the legitimacy of eradication in writings for their fellow supporters of 'integrated control' well before the 1970s. However, the importance of these reservations must be tempered by considering H. C. Cox's view that no one truly believed in the idea of 'eradication' — the complete elimination of an insect population or species — and the more pragmatic, institutional concerns voiced by such important figures as Dale Newsom.

That the ideological differences between the chief proponents of 'integrated control' and 'eradication' programs, by themselves, were of limited importance to the long-term evolution of more ecologically sophisticated and less environmentally disruptive pest control programs becomes rather clear by taking stock of the achievements of the two programs by the late 1970s. 'Eradication' was not a 'once and for all' solution to pest problems, as had been promised before Congress. The Pilot Boll Weevil Eradication

Experiment and Boll Weevil Eradication Trial were in practice long-term suppression programs based on the use of multiple tactics, and the exploitation of crucial features of the reproductive ecology of insect populations. This divergence between promised and effective achievements had been the main source of criticism by the Entomological Society of America and National Academy of Sciences panels. The term 'eradication' was eventually dropped in favour of the more cumbersome, but more accurate term 'area-wide population management'. Was 'integrated control' any different? The notion of 'economic thresholds' certainly lent distinctiveness to the approach developed by the proponents of integrated control. Yet, these thresholds were more an ideal than a practical reality.[115] In their absence, all that remained of 'integrated control' was a pest control program based on the use of multiple tactics, and greater consideration of the ecology of insect populations.[116] In sum, notwithstanding the intense arguments about the fundamental differences between 'eradication' and 'integrated control', from a practical standpoint, these strategies could not be distinguished from one another (except, of course, by looking at who was implementing them). They had a lot more in common than some of the parties to the controversy believed was the case. They both aimed to establish a new approach to crop protection, which was informed by an awareness of the enormous environmental disruption wrought by the careless use of pesticides. In fact, at present there is a growing recognition among most entomologists involved in 'integrated control' and 'area-wide population management' programs of this essential unity of purpose. This rapprochement, whose social roots are beyond the scope of the present study, is captured in a looser definition of 'integrated control' that is less specific about tactics and goals, except for the aspiration to reduce the adverse social, economic and environmental impacts of pesticide use.[117]

In sum, entomologists need to take stock of David Pimentel's recommendations for the future development of pest control. Motivated by concern over the continuing, explosive growth in the use of pesticides throughout the

[115] See Klassen Files: 'NSF–IBP/RANN–Dr. Huffaker': H. Hoffmann to H. O. Graumann, 26 September 1973. It is very difficult to determine what variables should be taken into account in the economic decision making model, and this problem is aggravated by the lack of critical knowledge of population dynamics. Under such circumstances, it is even more difficult, if not close to impossible, to build the integrated ecological and economic models needed to esitmate these thresholds. Council of Environmental Quality, op. cit. (112), pp. 21–3; B. A. McCarl, *Economics of integrated pest management*, Special Report 636, Agricultural Experiment Station, International Plant Protection Center, and the Department of Agricultural and Resource Economics, Oregon State University, 1981.

[116] D. Pimentel (Cornell University), personal communication.

[117] B. Croft (Oregon State University), personal communication. See also B. A. Croft, 'Integrated pest management: The agricultural environmental rationale', in R. E. Frisbie and P. L. Adkisson, eds., *CIPM: Integrated pest management on major agricultural systems*, College Station, 1986, pp. 712–28.

world, this senior figure of American entomology and early champion of the 'environmentalist' cause, has recently written that,

> progress has been made on pesticide problems, but *Silent spring* is not entirely behind us. Pesticide use continues, and the quantities of pesticides applied grow annually despite support for IPM control ['integrated control']. In future decades, as the world population grows rapidly and agricultural production is stretched to meet food needs, we should not forget Carson's warnings.
>
> Pesticides will continue to be effective pest controls, but the challenge now is to find ways to use them judiciously to avoid the environmental hazards and human poisoning that exist today. With this goal for research and development we can achieve effective, relatively safe pest control programs.[118]

But the answers to the growing use of pesticides and the environmental problems caused by their use do not lie solely in future conceptual advances in entomology. They also lie in greater sensitivity to fundamental significance of the ever-changing interplay of the institutional, political, and economic factors shaping these advances. There are lessons here for entomologists, environmentalists and, not least, for historians of science.

[118] D. Pimentel, 'Is *Silent spring* behind us?' in G. J. Marco et al., eds., *'Silent spring' revisited*, Washington, 1987, pp. 184–5.

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Radiation ecology, systems ecology and the management of the environment

CHUNGLIN KWA

This paper is about two subdisciplines of ecology, radiation ecology and systems ecology, and about two institutions, the ecology groups at Oak Ridge National Laboratory and at the University of Georgia. In these two institutions, systems ecology emerged out of radiation ecology. Yet, it seems these two subdisciplines could not be more different. Radiation ecology is problem and technique oriented. In contrast, systems ecology can best be described as a social movement. A general emphasis on newness combined with varying degrees of missionary zeal, rather than precisely stated goals, served to excite newly won disciples in an effort to reform an outdated discipline. Systems ecologists meant to 'revolutionize'[1] ecology, its concepts as well as its practices. How can we understand the nature of the transition of radiation ecology to systems ecology?

Eugene Odum, Professor of Zoology at the University of Georgia, coined the term 'radiation ecology' in 1956, a number of years after it came into existence.[2] It arose in response to problems of radioactive waste and fallout, which slowly, too slowly, emerged as problems at the end of World War II. The term 'systems ecology' was also first used by Eugene Odum, who hailed it in 1964 as the 'new ecology'.[3] A paper published in 1962 at Oak Ridge

This paper is a revised chapter of 'Mimicking nature: The development of systems ecology in the United States, 1950–1975' (PhD thesis, University of Amsterdam, 1989). I am grateful to Professor Eugene Odum for a copy of his unpublished manuscript on the early history of the Savannah River Ecological Laboratory, and to Dr Vince Schultz for various articles relating to the history of ecology. I am much indebted to Drs Stanley Auerbach, D. A. Crossley, Frank Golley, Eugene Odum, Jerry Olson, William Osburn, Frederick E. Smith and Richard Wiegert for the talks I had with them about various aspects of the history of ecology. Auerbach and Olson provided helpful comments on earlier versions of this paper.

[1] R. P. McIntosh, *The background of ecology*, Cambridge, 1985, p. 224.

[2] E. P. Odum, 'Consideration of the total environment in power reactor waste disposal', *Proceedings of the International Conference on Peaceful Uses of Atomic Energy*, (1956) 13, pp. 350–53. The name found acceptance immediately. See F. W. Whicker and V. Schultz, 'Introduction and historical perspective', in *Radioecology: Nuclear energy and the environment*, i, (Whicker and Schultz, eds.), New York, 1982, p. 4.

[3] E. P. Odum, 'The new ecology', *BioScience*, (1964) 14, pp. 14–16.

National Laboratory has retrospectively been called the first systems ecological paper.[4]

To define systems ecology seems harder than to give its chronology. Indeed, many different definitions have been offered. The most elusive points out that it is a 'new ecology', characterized by 'the accumulation of the techniques, instruments and wherewithal for analysis of ecosystems as a whole'.[5] One aspect of this definition highlights systems ecology as a modernization movement. Another aspect links it to ecosystem ecology, and this is indeed what 'systems ecology' in the United States has come to denote; as a 'systems science' it is more than anything else connected with the analysis of large and complex 'wholes' such as ecosystems. But systems ecology has many other connotations as well, as appear for example in the definition of two other system ecologists: 'Systems ecology is a robust hybrid of engineering, mathematics, operations research, cybernetics, and ecology.'[6]

Systems ecology in the United States, however, has been a much more coherent endeavor than the above definitions at first suggest. A few years after systems ecology came into existence, systems ecological projects constituted the bulk of the American contribution to the International Biological Programme (1968–1974), and a separate programme for systems ecology was set up at the National Science Foundation that equalled or surpassed the one existing NSF programme for the rest of ecology.[7] The ecological programmes at the University of Georgia and Oak Ridge National Laboratory were among its most important beneficiaries. One might say that the history of these two institutions coincide to a large extent with the early history of systems ecology, i.e. until the beginning of the IBP, the final point to which the account in this paper will be carried.

US ecology around mid-century

In 1956 the ecological programmes at the UGa and ORNL occupied peripheral positions within the discipline of ecology, and not only because they were newly built. The UGa was a small land-grant university of modest

[4] G. M. Van Dyne, 'Systems ecology, the state of the art', in *Environmental Sciences Laboratory dedication, February 26 and 27, 1979* (S. I. Auerbach and N. T. Milleman, eds.), Oak Ridge, TN, 1980, pp. 81–104, 82–4. Van Dyne referred to a ORNL publication by Jerry Olson.

[5] R. P. McIntosh, 'Plant ecology, 1947–1972', *Annals of the Missouri Botanical Gardens*, (1974) **61**, pp. 132–65, cited in G. M. Van Dyne, 'Reflections and projections', in *Grasslands, systems analysis and man* (A. I. Breymeyer and Van Dyne, eds.), Cambridge and New York, 1980, pp. 881–921, 885.

[6] *Systems ecology* (H. H. Shugart and R. V. O'Neill, eds.), Stroudsburg PA, 1979. Quote in Introduction, p. 1.

[7] See C. L. Kwa, 'Representations of nature mediating between ecology and science policy: The case of the International Biological Programme', *Social studies of science*, (1987) **17**, pp. 413–42.

reputation and without a long-standing programme in ecology. ORNL was primarily a physics laboratory and its endeavors into other realms were of marginal interest by necessity.

A second shared feature is that they were strongly limited to applied ecological problems, and this seemed enough to prevent them from taking a place in mainstream ecology. Both the UGa and ORNL were entirely or almost entirely dependent on funding by the Atomic Energy Commission, and this draws attention to the extraordinary fact that, prior to 1970, the AEC was the most important patron of American ecology.

Thirdly, there was a sense in which the ecological research at the UGa and ORNL was nevertheless 'mainstream'. The early work was as solidly descriptive as the work being done at the intellectual centers of ecology such as the University of Chicago and Yale University. The effects of radiation on the environment opened, as it were, a new territory for pre-existing ecological approaches and practices. Its practitioners regarded radiation ecology as a subdiscipline of ecology in its own right.

In most American universities, if not all, ecology was (and still is) usually represented by one or more faculty members at Departments of Biology, Zoology, Botany, Forestry, etc. They had widely diverging backgrounds, reflecting the many sources that feed into ecology: zoology and botany, mathematical and field methods, taxonomic and physiological approaches. Furthermore, different foci can be discerned such as the familiar division between terrestrial and aquatic ecology, and, most importantly, the definition of the object of ecology as the population (groups of individuals of any one kind of organism), the community (all the populations occupying a given area) or the 'ecosystem', i.e., the community functioning together with its nonliving environment such as the various climatic factors.[8]

Viewed from the perspective of the protagonists in the account of this paper, this ecological scene was dominated by a few outstanding figures, mostly at elite universities. The University of Chicago was among the most important. Its tradition in ecology dates from the beginning of the century, and includes ecologists such as Henry Cowles, his student Victor Shelford and W. C. Allee. The latter formed the so-called Ecology Group, mostly consisting of his own former graduate students, which functioned until his retirement in 1950.[9] Its theoretical position concerning population ecological problems earned it the designation of 'school'. Apart from Allee, the group

[8] The definitions are taken from E. P. Odum, *Fundamentals of ecology*, 2nd ed., Philadelphia, 1959. The British ecologist Tansley introduced the concept 'ecosystem' in this manner in 1935 (A. G. Tansley, 'The use and abuse of vegetational concepts and terms', *Ecology*, (1935) **16**, pp. 284–307).

[9] See D. Worster, *Nature's economy: A history of ecological ideas.* Cambridge, 1977, pp. 326–31, and Mitman, G., 'From the population to society: The cooperative metaphors of W. C. Allee and A. E. Emerson', *Journal of the history of biology*, (1988), **21**, pp. 173–94.

consisted of Alfred Emerson, Thomas Park, and Karl Schmidt, and also included Orlando Park from nearby Northwestern University. The group at Chicago (the ecologists at the Botany Department not included) was relatively large compared to most universities. Eugene Odum and the Oak Ridge ecologist Stanley Auerbach received at least part of their education from Chicago or Chicago-related ecologists.

At the end of the 1940s, Yale occupied a similar position. G. Evelyn Hutchinson, who was at Yale from 1928 until his retirement in 1971, is perhaps the single most influential American ecologist of this century, through his own work and that of his graduate students. A high proportion of these students rank among the most important ecologists in the US. At the time, three of his former graduate students were on the staff at Yale, too, and they were sometimes designated as the 'Hutchinson School'. According to one of them, Gordon Riley, it was 'a loosely knit, informal group' that later on disbanded.[10] Both at Yale and Chicago, the ecologists were highly committed to research, but though they were designated as 'schools', they did not form research groups that were tied together by a shared research programme. Much of the research was done in the context of education, and both Hutchinson and the Chicago ecologists wrote widely used textbooks. Thus their importance for American ecology far exceeded their numerical size, an importance which can also be inferred from the fact that for instance, the Chicago ecologists Emerson and Park held key positions such as the editorship of *Ecology*.[11]

As indicated by the enumeration of its various sources, ecology is an extremely heterogeneous field, with many subfields developing quite independently of one another. It has been commented recently that this situation probably precludes for ever any theoretical unification.[12] To the extent that the most highly esteemed work in ecology was (and to some extent still is) descriptive, this hardly mattered. The journal *Ecology* refused theoretical papers with few exceptions until 1973, when the Board of Editors changed its policy.[13] Traditional virtues such as a good background in taxonomy thus remained strong prerequisites for academic recognition.

So much for appearances during the 1950s. By the late 1960s the ORNL and the UGa had gained national prominence as centers of ecological research, and the factors that helped them to win that position were the very same factors that initially seemed to constrain them to the marginal position of the peculiar form of applied ecology known as radiation ecology. Seen

[10] G. A. Riley, 'Introduction', *Limnology and oceanography*, (1971) **16**, pp. 177–9, 178 (Issue dedicated to Hutchinson on his retirement).

[11] Mitman, op. cit. (9), p. 177 n.

[12] J. Beatty, 'Ecology and evolutionary biology in the war and post-war years: Questions and comments', *Journal of the history of biology*, (1988) **21**, pp. 245–63.

[13] R. L. Burgess, 'The Ecological Society of America: Historical data and some preliminary analysis', in *History of American ecology* (F. N. Egerton, ed.), pp. 1–24, 17.

retrospectively, the Atomic Energy Commission provided a context in which elements of what was later to emerge as systems ecology could be combined. The problems and perspectives of radiation ecology were broadened in a specific way so as to give rise to systems ecology. In this way, Eugene Odum and Stanley Auerbach, typical naturalists and outdoors ecologists lacking a mathematical background, were the founders of a subdiscipline of ecology that was strongly mathematical.

The role of the AEC and the institutional context it provided to the nascent science of radiation ecology can hardly be overestimated. In various ways, the particular setting of radiation ecology helped to select and shape its research directions. The AEC Division of Biology and Medicine (DBM) was first and foremost interested in the problems radiation posed for *human health*. The importance of the problems of health directed the attention of the AEC-hired ecologists at an early stage to the food chain. Radiation ecology, which was initially developed as research on the direct effects of radiation on organisms and their environment, thus provided important methodologies for the study of the food chain and led ultimately to systems ecology.

Another element pertinent to the development of systems ecology were certain mathematical techniques. Their possibilities and constraints, in particular with respect to computers, shaped some of the essential presuppositions of systems ecology. Computers became available to ecologists during the 1960s. They soon raised high expectations, not least with respect to the possibility of a scientific management of ecosystems through the production of simulation models of ecosystems.

The institutional setting of radiation ecology, within AEC-directed programmes, provided an important impetus to fuse these elements together. 'All of a sudden you were bedfellows with physicists and chemists', as Odum said looking back at the heyday of radiation ecology.[14] Through daily contact with these physicists, the radiation ecologists felt obliged to adopt whatever mathematical and physical methodology was available to win the esteem of their colleagues.

Yet the development from radiation ecology to systems ecology was by no means wholly determined by the institutional context provided by the AEC. Such a view would leave little room for the innovative actions of several of the key actors whose work is described here. Rather we could say, somewhat paradoxically, that this particular institutional context was instrumental in creating an atmosphere in which a confidence to bring about a 'new ecology' could flourish.

What I hope to demonstrate in the following pages is that the practical orientation of radiation ecology was retained in systems ecology on a more

[14] Interview with Eugene Odum (11 October 1985).

abstract level. Radioactive isotopes had caused environmental problems that were addressed by radiation ecology in their specificity. Systems ecology transformed this practical orientation into a generalized managerial approach to the natural environment.

The first programmes of radioecology

After the end of World War II, the Atomic Energy Commission refused during many years to take problems of radioactive waste and fallout seriously.[15] The 1954 Bikini thermonuclear bomb test changed that. The contamination of the Marshall Islands, their inhabitants, and in particular of a Japanese fishing boat, caused world wide concern and it forced the AEC to engage in biological research on a relatively large scale.[16] The AEC Division of Biology and Medicine hired an ecologist, John Wolfe, in 1955, as a consultant. In 1958 he was appointed permanently, and within the DBM an Environmental Sciences Branch was created around him.[17] Wolfe's appointment was instrumental in developing radioecological programmes at many universities.[18] Already by 1954, budgets for ecological research rose sharply at both Oak Ridge and Georgia. By the end of the 1950s, 'radioecology' was being practiced at several National Laboratories and at up to fifty universities, sponsored by the AEC. Almost everywhere, radioecological research was directed to the ecological effects of specific isotopes, or the dose–response relationships between levels of radiation and organisms.

Yet, radioecological studies had preceded Bikini, even though there had been no central policy of engaging in ecological research. At the Hanford Works (now: Pacific Northwest Laboratory), ecological radiation studies started in 1946,[19] and at Oak Ridge National Laboratory in 1950. If ecologists were in effect hired or given an opportunity to do research, it was the

[15] S. J. Heims, *John von Neumann and Norbert Wiener*, Cambridge MA, 1980, p. 283.

[16] J. Newell Stannard, *Radioactivity and health: A history*, Springfield, VA, 1988.

[17] *Radioecology*, op. cit. (2), p. 6. By fiscal year 1970,the budget of the Environmental Sciences Branch was $18.9 million, divided into $6 million for terrestrial and fresh water ecology, $4 million for marine sciences, and $8.9 million for atmospheric radioactivity and fallout.

[18] At Emory University and Colorado State University comparatively large radioecological centers came into being. Both focussed on dose–effect relationships. See Stannard, op. cit. (16).

[19] Research at Hanford originally started in cooperation with the Applied Fisheries Laboratory (later the Laboratory of Radiation Ecology) of the University of Washington. The first radiation studies were done at this university in 1943, on the effects of radiation on salmon and trout. These studies were largely physiological. From 1946 on, environmental surveys were undertaken in the Pacific and Columbia River, now also by scientists hired on the staff of General Electric, the operator of Hanford N.L. See *Radioecology*, op. cit. (2), pp. 4–5, and R. F. Foster, and J. J. Davis, 'The accumulation of radioactive substances in aquatic forms', *Proceedings of the International Conference on the Peaceful Uses of Atomic Energy*, (1956) 13, pp. 364–7, 364.

result of ad hoc decisions by the directors of the individual National Laboratories, sometimes responding only to initiatives taken by lower officials.[20] Most ecological research, if not indeed all of it, originated from problems in the plants and reactors at the sites of the Laboratories themselves. The programmes at Hanford and Oak Ridge both found their origin in the toxic waste problems that the atomic installations had created for themselves: at Hanford significant amounts of radioactive isotopes were being released into the Columbia river; at Oak Ridge a small lake, the White Oak Lake, had been turned into a heavily contaminated waste depository during World War II. It ranked among the most contaminated areas in the world. Equally ad hoc was the decision of the AEC's Biophysics Branch of the DBM to engage in ecological studies at the Savannah River Plant Reservation in South Carolina, where a new nuclear plant was being established in the early 1950s. There was no National Laboratory involved here and the research was commissioned to the University of Georgia.

The ecologists at Hanford achieved the first real breakthrough in radiation ecology. At the International Conference for the Peaceful Uses of Atomic Energy held in Geneva in 1956, two papers reported[21] that several species accumulated radioactive isotopes in surprisingly high quantities.[22]

The plutonium-producing plants at Hanford were cooled directly by water from the Columbia River, which was released to the river again. Even if under normal operating conditions no spills occurred, radioisotopes were formed through neutron activation of solids present in the cooling water. Moreover, the exchange surfaces became activated and radioactive material was released through corrosion. Radioactive levels of the effluent into the Columbia River, however, did not exceed the then permissible levels. But there was a potential environmental problem, since the water of the Columbia River was used as drinking water, and there were also salmon fisheries.[23]

One of the isotopes present, albeit in smaller amounts than a number of

[20] A case in point is Karl Z. Morgan, Director of the Health Physics Division of Oak Ridge National Laboratory (see below). At Hanford, the initial impetus had come from Stafford Warren, medical director of the Medical Engineer District. See Stannard, op. cit. (16), p. 757.

[21] Foster and Davis, op. cit. (19); W. C. Hanson and H. A. Kornberg, 'Radioactivity in terrestrial animals near an atomic energy site', *Proceedings of the International Conference on Peaceful Uses of Atomic Energy*, (1956) 13, pp. 385–8.

[22] The discovery of this concentration phenomenon along the food chain seems to have preceded the discovery of the very similar phenomenon of concentration of pesticides such as DDT and DDD. That DDT was passed on from one level of the food chain to another was noted as early as in 1949. However, the fact that these chemicals were concentrated in, for instance, the fatty tissues of birds, and that this was the cause of bird deaths, was not noted until the late fifties and published in the early sixties. See R. L. Rudd, *Pesticides and the living landscape,* Madison, Wisconsin, 1964, pp. 241–267.

[23] Foster and Davis, op. cit. (19), p. 364; Kornberg, H. A., 'Radiation biology as a supporting function for atomic energy installations', *Proceedings of the Second U.N. International Conference on Peaceful Uses of Atomic Energy*, (1958) 18, pp. 329–35, 330.

others, was phosphorus-32, a biologically important element and fairly long-lived. R. F. Foster and J. J. Davis found high concentration factors of this isotope in various forms of aquatic life, e. g. 150,000 times in a small fish and 350,000 in caddis fly larvae as compared to the P-32 content of the water.[24] W. C. Hanson and H. A. Kornberg, investigating terrestrial animals in the same area, found even higher concentration factors, up to 500,000 times in young swallows and 1,500,000 in the egg yolk of river ducks and geese. Apparently the levels were still such that no effect on the reproductive capability of the waterfowl was noted.[25] But in principle it was established that water containing concentrations of contaminants well within the permissible limits for drinking water could give rise to hazardous levels of radioisotopes in the food organisms of man, as they were to note.[26] But aside from noting the potential relevance of this finding to human health, they pointed out its *ecological importance*.[27] Since it was obvious that the animals obtained the radioactivity from food,[28] the studies that were designed to monitor levels of radioactivity also provided information on nutrient cycles, metabolic rates and ecological relationships.

The idea to use radionuclides in an experimental manner would be taken up by the ecologists at Oak Ridge and at Savannah River. But the team of Hanford and the neighbouring University of Washington at Seattle remained confined to doing environmental surveys. A possible reason is that the Hanford ecologists were financed from operating rather than research funds, the Hanford management (conducted by General Electric for the AEC on a contract basis) apparently unwilling to change this.[29] Union Carbide, which ran the ORNL on a similar contract, did not enforce such constraints, while Odum at the University of Georgia could profit from the much more liberal climate of a university. Thus the Hanford ecologists remained oriented toward the needs of their plant, whereas the ecologists at Oak Ridge increasingly broadened their scope while striving for recognition by the academic ecological community.

Eugene Odum and the University of Georgia

In 1940, when Eugene Odum joined the Department of Zoology of the University of Georgia, he was the only ecologist on the Department's staff.

[24] Foster and Davis, op. cit. (19).

[25] Hanson and Kornberg, op. cit. (21).

[26] J. J. Davis et al., 'Radioactive Materials in Aquatic and Terrestrial Organisms Exposed to Reactor Effluent Water', *Proceedings of the Second U.N. International Conference on Peaceful Uses of Atomic Energy*, (1958) **18**, pp. 423–4.

[27] Foster and Davis, op. cit. (19); J. J. Davis and R. F. Foster, 'Bioaccumulation of radioactive isotopes through aquatic food chains', *Ecology*, (1958) **39**, pp. 530–35.

[28] Direct absorption from water in fish contributed only 1.5% of the radioactivity, Davis and Foster, op. cit. (22).

[29] Stannard, op. cit. (16), p. 770.

At his retirement in 1983, Odum was still at the Department. He was also Director of the Institute of Ecology at the University of Georgia, with about 100 faculty members, 100 graduate students, and 50 support staff. The Institute has a world-wide reputation as a center of ecosystem ecology. Since Odum can be considered its chief architect, it seems justified to start the history of the development of ecology at the University of Georgia with a look at Odum's career.

'One of the best ways to begin the study of ecology is to go out and study a small pond.'[30] Odum's example of an ecosystem is introduced in the style of a natural historian. Eugene Pleasants Odum was born on 17 September 1913. Odum received his undergraduate education at the University of North Carolina at Chapel Hill. He wanted to pursue his graduate studies with Victor Shelford at the University of Illinois at Champaign.[31] Charles Kendeigh, a former student and collaborator of Shelford, became the advisor of his doctoral thesis, on geographical and physiological aspects of the ecology of birds. After he obtained his PhD in 1939, Odum assisted Shelford on his field trips through the United States. He came to know Shelford well, and, along with sharing his naturalist's enthusiasm, sympathised also with Shelford's intellectual project, which was to combine plant and animal ecology in order to understand the functioning of ecosystems (or biomes, as they were called in his terminology). Shelford and Frederic Clements tried to accomplish this goal in their joint publication of 1939, *Bio-ecology*.[32] In the eyes of contemporaries, the book remained largely classificatory, however, and was by many, including Odum, considered a failure.[33]

Odum got his next most important intellectual stimulus from Hutchinson, after he was appointed to a position at the Department of Zoology of the University of Georgia in 1940. Odum's younger brother, Howard Thomas Odum, at the end of the 1940s was a graduate student of Hutchinson's at Yale. Through him Odum was asked to give a seminar. At the time Raymond Lindeman's paper on trophic-dynamics was one of the most talked-about topics. Odum acquainted himself also with Hutchinson's writings, much of which reflected the same interest. Eugene Odum's interests were clearly similar to those of Hutchinson's students who were primarily concerned with the movement of nutrients and biogeochemical cycles.

Odum's *Fundamentals of ecology*, published in 1953, can be seen as an attempt to integrate these various intellectual influences, around the central concept of the ecosystem. The book was written primarily to secure ecology a place among the so-called core curricula of his own Department at the

[30] E. P. Odum, *Fundamentals of ecology*, 1st edn., Philadelphia, 1953, p. 13.

[31] Odum, interview. [32] F. E. Clements and V. Shelford, *Bio-ecology*, New York, 1939.

[33] For instance by G. E. Hutchinson in his review of *Bio-ecology*, *Ecology*, (1940) **21**, 267–268.

University of Georgia.[34] But its instantaneous success was nationwide. The next year, it went through two more printings. It has continued to be reprinted, as well as translated into many languages. A new edition appeared in 1959, followed by a third edition in 1971. For several generations of students, 'the Odum' was *the* textbook.[35]

Odum's 'Fundamentals of ecology'

Fundamentals of ecology is centered around Lindeman's (and Hutchinson's) concept of trophic-dynamics. Trophic-dynamics is essentially a reinterpretation of the feeding relationships (and the cycling of elements such as phosphorus and nitrogen that they involve) existing in a given ecosystem. Food chains, in terms of the species involved, can be extremely complex.[36] Charles Elton, the British ecologist, had found an underlying pattern in the form of trophic levels and expressed them very elegantly by a *pyramid of numbers* (Fig. 1), in which it is shown that the number of animals in small size classes is much greater than the number of animals in larger size classes (that may feed upon them), and these in turn significantly greater than the still bigger animals that may prey upon these, and so on.[37] Lindeman performed a reduction operation on these pyramids of numbers, transforming them into pyramids of biomass corresponding to discrete trophic levels (producers, primary and secondary consumers, and so on). Thus 'the principle of the Eltonian Pyramid (was) redefined in terms of productivity.'[38] As Hutchinson pointed out, the advantage of Lindeman's approach 'lay in reduction of all the interrelated biological events to energetic terms',[39] which could be quantified by measurements of the productivity at the various trophic levels.

Odum dismissed the original Eltonian pyramid of numbers out of hand to entirely embrace the pyramids of biomass and energy (Fig. 2).[40] He con-

[34] E. P. Odum, 'The emergence of ecology as an integrative discipline', *Science*, (1977) **195**, 1289–93, 1289.

[35] R. L. Burgess, 'United States', in *Handbook of contemporary developments in world ecology* (J. Kormondy and J. McCormick, eds.), Westport, CT, 1981, pp. 67–101; G. H. Orians, 'A diversity of textbooks: Ecology comes of age', *Science*, (1973) **181**, pp. 1238–9.

[36] Elton was the first to use the words 'food chain' and 'food cycle', but he himself attributed the concepts to a 1913 publication by Victor Shelford. See D. L. Cox, 'Charles Elton and the emergence of modern ecology', PhD thesis, Washington University, 1979, p. 88.

[37] C. S. Elton, *Animal ecology,* London, 1927. See also Cox, op. cit. (36), pp. 94–9.

[38] R. L. Lindeman, 'The trophic-dynamic aspect of ecology', *Ecology*, (1942) **23**, pp. 399–418, 408. Lindeman attributed this to Hutchinson. See also R. E. Cook, 'Raymond Lindeman and the trophic-dynamic concept in ecology', *Science*, (1977) **198**, pp. 22–6.

[39] G. E. Hutchinson, 'Addendum' [to Lindeman's paper], *Ecology*, (1942) **23**, p. 417.

[40] Odum said notably: 'The pyramid of numbers is not very fundamental or instructive . . . . The pyramid of biomass is of more fundamental interest.' (*Fundamentals*, 1st ed., op. cit.

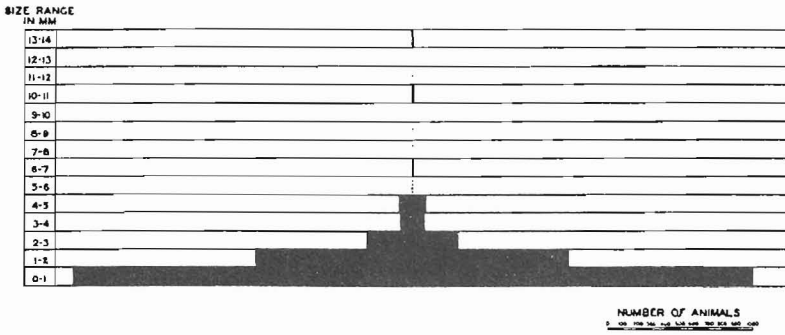


FIG. 1: Eltonian pyramid of numbers according to animal size as drawn by Eliot Williams, 'An ecological study of the floor fauna of the Panama rain forest', *Bulletin of the Chicago Academy of Sciences*, (1941) 6, pp. 63–124, 109.

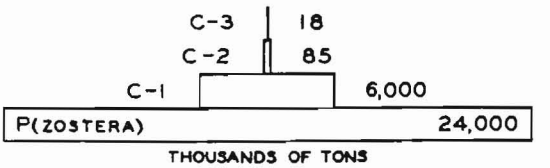


FIG. 2: Pyramid of biomass according to trophic levels as drawn by Odum. P = producers, C = consumers. E. P. Odum, *Fundamentals of ecology*, 1st edn., Philadelphia, 1953, p. 75.

structed some pyramids on the basis of Lindeman's data. Odum reflected Hutchinson's influence in other respects as well. Clements and Shelford had regarded communities or ecosystems as supraorganisms. Odum's thought is informed by the metaphor of the homeostatic machine, by cybernetics.[41]

Hutchinson had most clearly expressed his thoughts about the cybernetic nature of ecosystems in his paper 'Circular causal systems in ecology',[42] where he undertook to demonstrate the validity of Norbert Wiener's cybernetic conceptions for the study of ecosystems. 'It is well known from mathematical theory' (i.e. cybernetics), he stated, 'that circular paths often exist which tend to be self-correcting within certain limits, but which break down, producing violent oscillations, when some variable in the system transgresses limiting values. . . . It is, therefore, usual to find in natural

(p. 74). Elton was understandably not very happy about these remarks, as he made he clear in his review of Odum; see Cox, op. cit. (36), p. 98.

[41] See Kwa, op. cit. (7).

[42] G. E. Hutchinson, 'Circular causal systems in ecology', *Annals of the New York Academy of Sciences*, (1948) 50, pp. 221–46.

circular systems various mechanisms acting to damp oscillations. . . .' These self-correcting circular paths also occurred in ecosystems: 'The resulting stability, which appears to characterize most ecological systems, while it permits their persistence, makes investigation difficult.'[43]

He then identified two different circular causal ecological systems, one of which concerns us here: the biogeochemical cycling of substances through the ecosystem.[44] Hutchinson had already contributed greatly to the elucidation of the cycling of essential biological elements through nature. He now conferred on these cycles the property of being regulatory mechanisms, ensuring the stability of the ecological systems for which they were relevant. H. T. Odum, Hutchinson's student, would complete his PhD thesis in 1950 on the biogeochemical cycling of strontium.[45] Biogeochemical cycles were given an important place in Eugene Odum's *Fundamentals of ecology*.[46]

In several respects *Fundamentals* remains the work of a naturalist, who always looks for the application of abstract principles to concrete ecosystems, and wants to operationalize them in the field. But the book conveyed much more than a naturalist's enthusiasm. We may see this textbook as a careful assimilation of the major new trends in the ecology of the 1940s. As such, it was not only a powerful textbook, but became the manifesto of a movement to modernize research as well. This summarizes its two achievements: (1) the book motivated the research programme at the UGa and also at ORNL to which Odum was asked to be a consultant; and (2) it created a larger audience for the ecosystem approach, ecosystem management and preservation, albeit on a programmatical level. The second edition of 1959 incorporated many of the radio-ecological results, and articulated the systems approach to ecosystems more clearly.

The 'stability principle'

One of the principles of ecosystems that Odum proposed tentatively, and that literally inspired several generations of students of ecology, was the relationship between diversity and stability of ecosystems. The research on food chains at Savannah River, and the tagging of plants and animals that it involved, would also serve to make possible investigations on this relation-

[43] Hutchinson, op. cit. (42), p. 221.

[44] Hutchinson, op. cit. (42), p. 221.

[45] H. T. Odum, 'The biogeochemistry of strontium', PhD thesis, Yale University, 1950; idem, 'The stability of the world strontium cycle', *Science*, (1951) **114**, pp. 407–411; see P. Taylor, 'Technocratic optimism, H. T. Odum and the partial transformation of ecological metaphor after World War II', *Journal of the history of biology*, (1988) **21**, pp. 213–244. H. T. Odum would call the strontium cycling an ecosystem itself, a hypostasis that is apparently lacking with either Hutchinson or Eugene Odum.

[46] Odum, *Fundamentals*, 1st edn, op. cit. (30), pp. 18–23.

ship.[47] Already in the first edition of *Fundamentals* Odum had hinted at the possibility that diverse ecosystems were less likely to feature wildly fluctuating populations, in contrast to poor ecosystems such as tundras which are known for their regular outbreaks of, for instance, lemmings.[48] Following a suggestion by the Spanish ecologist Rámon Márgalef,[49] Odum had proposed the hypothesis that in a diverse ecosystem with many species and many kinds of relations between them, these links would act as checks and balance against disturbances in population numbers, amounts of food minerals and possibly other system parameters as well. In a word: the more species present, the more possible negative feedbacks will be established.[50] The mere presence of species would not, of course, automatically give rise to such balancing relationships. However, by elucidating food webs in all their intricacy, in a quantitative way, one would be able to 'deal with diversity in terms of network variables rather than merely in terms of number of species and individuals present'.[51] Although Odum clearly included in his relationship population interactions such as predation and competition, its outlook was definitively synchronic and functional, rather than evolutionary and long-term.

Odum's relationship between diversity and stability provided a hypothetical mechanism to what he had named 'the stability principle', the idea that, obeying the second law of thermodynamics, every natural system would evolve to a state of stable adjustment. The development of regulating mechanisms would bring successional and to some extent also evolutionary change to a halt.[52]

We may also reconstruct the diversity/stability relationship as Odum's contribution to one of the major theoretical problems of ecology during the 1950s. Odum set out to solve on a 'systems level', a problem that had been posed within the ecology of populations. The central question had been: how are populations regulated?[53] The debate had culminated in the Cold

[47] R. C. Wiegert, E. P. Odum, and J. H. Schnell, 'Forb-arthropod food chains in a one-year experimental field', *Ecology*, (1967) **48**, pp. 75–83.

[48] Odum, *Fundamentals*, 1st edn., op. cit. (30), p. 70.

[49] Odum, personal communication. R. Márgalef's earliest theoretical work on the subject is 'La teoria de la informacion en ecologia', *Memorias de la Real Academia de Ciencias y Artes, Barcelona*, (1957) **32**, pp. 373–449. This reference in Spanish is cited several times by the Odum brothers.

[50] E. P. Odum, *Ecology*, New York, 1963. 'Without much scientific evidence', he added as a qualifier in this textbook (pp. 34, 93). In the 2nd edn of 1975 he expressed his reservation in the relationship even more strongly.

[51] Wiegert et al., op. cit. (47).

[52] *Fundamentals*, 2nd edn, op. cit (8), pp. 45–6.

[53] J. P. Collins, '*Evolutionary ecology* and the use of natural selection in ecological theory', *Journal of the history of biology*, (1986) **19**, pp. 257–88; S. Kingsland, *Modeling nature*, Chicago, 1985, pp. 171–5.

Spring Harbor Symposium of 1957.[54] At the time, two radically opposing answers were given. In one camp, the importance was defended of *density dependent* mechanisms in the regulation of population size, i.e., factors inhibiting or reversing the growth of a population would become more important with increasing population size and vice versa. This would make factors such as competition and predation the most relevant. Others argued for the predominant importance of *density independent* factors, primarily climatic factors that would check population growth and size irrespective of its then prevalent values. Thus the question can also be put as: is the abundance of a certain population of animals checked by depletion of resources and by other animals or by the weather?[55]

The 1957 debate took the form of an empirical controversy: whether interrelated cycles of predator and prey really existed (which would argue for checks by animals), or whether there was a close correspondence between fluctuations in climate and population fluctuations (which would argue for checks by the weather).[56] The former position was the orthodox one, stemming from classical population dynamics. As one of the adherants of the latter position observed, the disciples of the density-dependent position 'vastly outnumber(ed)' those of other theories, which were 'rejected by common consent'.[57] American ecologists were strongly involved in this international controversy, and usually took the 'orthodox' position. Before the controversy really developed, Allee and his collaborators had held it.[58] A couple of years after the Symposium, two of Hutchinson's former graduate students, Frederick E. Smith and L. Slobodkin, in a joint paper with Nelson Hairston, expressed their conviction that 'the hypothesis of control by the weather leads to false or untenable implications', and they upheld the theory

[54] A. J. Nicholson, 'The self-adjustment of populations to change', *Cold Spring Harbor symposia on quantitative biology*, (1957) **22**, pp. 153–73; L. C. Birch, 'The role of weather in determining the distribution and abundance of animals', ibid., pp. 203–18.

[55] As phrased by someone who argued that the terminology of density dependence and independence was misleading: A. Milne, 'Theories of natural control of insect populations', *Cold Spring Harbor symposia*, (1957) **22**, pp. 253–71.

[56] In 1924, the British ecologist Charles Elton advanced the theory that fluctuations in animal numbers found an ultimate cause in climatic variations. He proposed a 10- to 11-year cycle, corresponding to the sunspot cycle. We might infer from this a single causal chain, leading from the sunspots to vegetational growth, and further to the prey species and finally to the predator species. On this account, fluctuations in the number of predators would be caused by fluctuations in the number of prey, but not vice versa (see Cox, op. cit. (36), pp. 54–6). However, when Elton had aquainted himself with the idea of the generation of oscillations through internal causes, he adopted it (see Kingsland, op. cit. (53), pp. 129–30, and Cox, op. cit. (36), p. 62).

[57] Milne, op. cit. (55), p. 254.

[58] Orlando Park wrote notably: 'In general terms, a community at the level of self-maintenance is a self-regulating assemblage in which the populations of plants and animals hold each other in a state of biological equilibrium. . . . the several species populations hold each other in a system of checks and balances. . .' (W. C. Allee et al., *Principles of animal ecology*, Philadelphia, 1949, pp. 507–8).

of resource-limitation in the 'classical density-dependent fashion'.[59] The paper was written in an authoritative fashion. It received much favorable attention and may have contributed to the affirmation of orthodoxy, at least for a time.[60] But that did not settle the controversy. It was argued some time later by ecologist Gordon Orians that this debate revolved not so much about an issue to be decided solely on empirical grounds, but depended rather upon one's perspective: an evolutionary approach would incline more to the density-dependent, a functional one more to the density-independent perspective. At the heart of the controversy were the different concepts of causation implied in these two perspectives.[61] The debate was thus related to a larger question: was there (or should there be) a relationship between ecology and evolutionary theory? The dominance of the 'density-dependent' position could then be interpreted as the dominance of the evolutionary perspective in ecology, with the corresponding relative unimportance of a functionally oriented, synchronic approach.

Eugene Odum put the density-dependent and density-independent mechanisms in conjunction.[62] He assigned the former the function of a steam engine's 'governor'. The latter, he proposed, could 'cause a shifting of upper asymptotic or carrying capacity levels'.[63] If we would recast this in the metaphor of the regulation of the steam engine, the density independent mechanisms, or climatic factors, would determine the *value of the set pressure* which the governor would then maintain. An ecosystem thus appears as a machine, full of stabilizing mechanisms and driven by the climate.

Succession studies at Savannah River

In 1951 it became clear to the AEC that the new atomic weapons plant to be built on the Savannah River in South Carolina would become an even bigger operation than was first thought. Costs for the entire project were estimated at more than a billion dollars. The whole area along the river below Augusta, Georgia, would be transformed.[64]

At the time of the construction of the reactors and laboratories at Oak Ridge, during World War II, there had been no opportunity to study their impact on the environment. The newly established Division of Biology and Medicine of the Atomic Energy Commission did not want this mistake to be

[59] N. G. Hairston, F. E. Smith, and L. B. Slobodkin, 'Community structure, population control, and competition', *American naturalist*, (1960), 94, pp. 421–5, 423.

[60] The paper was so well known in the 1960s that it received the status of an acronym ('HSS'). See McIntosh, op. cit. (1), p. 244.

[61] Orians, G. H., 'Natural Selection and Ecological Theory', *American naturalist*, (1962), 96, pp. 257–63. See also Collins, op. cit. (53).

[62] *Fundamentals*, 2nd edn, op. cit. (8), p. 206.

[63] *Fundamentals*, 2nd edn, op. cit. (8), p. 209.

[64] R. G. Hewlett and F. Duncan, *Atomic shield, 1947/1952*; Volume II of *A history of the United States Atomic Energy Commission*, University Park, PA, 1969, p. 553.

repeated at the Savannah River installations. In the spring of 1951, the Universities of South Carolina and Georgia were invited to submit proposals for 'pre-installation' inventories. As it turned out, the DBM granted not more than $10,000 per year to each university, over three years. Georgia and South Carolina were asked to divide up inventories of terrestrial flora and fauna and the general ecology of the area among themselves.[65] The result was in fact an Environmental Impact Statement long before the practice was legally required.

Odum would congratulate himself ever after for the way he made use of that original offer. The University of South Carolina 'wasted' the grant by using it for the summer salaries of their own staff, and dropped out of the project at the termination of the initial contract period. Odum decided it was solely to be spent on graduate students who could be given research assistantships. Since a research assistant would cost $2000 a year, Odum could afford three of them, and could buy some equipment such as trucks as well. The University of Georgia had approved a PhD programme in biology a few years before. Odum already had one graduate student, and he could thus considerably enlarge the graduate programme through the arrangement with the AEC.[66] In a quick procedure, the DBM approved Odum's grant proposal[67] in late June, and Odum immediately went out to several other universities to interview candidates whom colleagues had recommended.[68]

During 1951 and 1952 approximately 6000 inhabitants were moved out of the area of the Savannah River Plant, abandoning their houses and leaving their fields (approximately one third of an area of 250,000 acres) untilled. Odum established a temporary field laboratory in one of the country houses that were left vacant. The major study that Odum assigned to his students was to follow the development of *old field succession* or *secondary succession*. Since crop fields of various kinds and on various soils were left bare, a gradual but ongoing change of vegetation (with corresponding fauna) would take place — theoretically until a so-called climax stage would be reached. Six hundred fields were designated study areas, and these were exempted from reforestation with pine. To map the successional changes, in vegetation as well as in bird populations, training as a naturalist was a prerequisite. Odum

[65] E. P. Odum, 'Brief history of the University of Georgia's ecological studies at the Savannah River Plant prior to establishment of the On-Site Savannah River Ecology Laboratory, 1951–1962', unpublished manuscript, 1985, 22 p.

[66] Odum, op. cit. (65); interview with Odum.

[67] 'An ecological study of land-use, succession, and indicator invertebrate and warm-blooded vertebrate populations of the Savannah River Operations Area'.

[68] The three graduate students were William H. Cross (MS, Florida State University), Leslie B. Davenport (The College of Charleston) and Edward. J. Kuenzler. Kuenzler graduated at the University of Florida, where H. T. Odum was Assistant Professor. In 1952, a fourth graduate student, Robert Pearson, a bird biologist, was added to this first 'crew'. He had taken his Master's degree with Kendeigh at the University of Illinois.

had selected his students partly on this basis. After three years, they were replaced by a new team of five graduate students, who continued the measurements.[69]

Odum summed up the results of this first line of research at Savannah River in a way true to the intentions set forth in *Fundamentals*. Previous studies of secondary succession had focussed completely on species change in vegetation.[70] In contrast, Odum related these changes to 'functional' aspects of the developing ecosystem under study, i.e. the 'energy flow' through its trophic levels. The results as reported were admittedly incomplete, and were confined to measurements of the productivity of the standing crop through seven consecutive years. Its main finding was that following the abandonment of agriculture, a relatively quick steady state in terms of primary production was achieved, though the process of change in species composition and species dominance had by no means come to an end.[71] The suggestion that the occurrence of steady states had to be looked for at these 'higher levels of integration', or emerging ecosystem properties, was to remain a dominant theme in systems ecology through the years of the International Biological Programme.

Radioecological studies at Savannah River

From 1954 on, the DBM increased the budgets for ecological research at the Savannah River site significantly. Odum hired Robert A. Norris, a former student of his to be a full time 'resident' at the Savannah River research site. The AEC also assigned Odum a more professional laboratory. Support at the AEC headquarters in Washington for ecological research continued to increase. John Wolfe, head of the new Environmental Sciences Branch, was very supportive of the ecological research at Savannah River. He was keen on developing radiation ecology programmes at universities, and, in comparison, seems to have been more supportive of Odum's programme than of Auerbach's at Oak Ridge.[72]

Wolfe backed Odum's proposal for a permanent on-site laboratory at the Savannah River Plant. In 1960, the AEC approved, and the next year the laboratory was made available.[73] The Savannah River Ecology Laboratory thus came into existence, a University of Georgia institution that was completely dependent on 'soft money'. At the time, the SREL had an annual budget of $60,000, but it quickly increased.

[69] E. P. Odum, 'Organic production and turnover in old field succession', *Ecology*, (1960) **41**, pp. 34–49; Odum, op. cit. (67).

[70] An example is the work of H. J. Oosting during the 1940s, cited in Odum, op. cit. (69).

[71] Odum, op. cit (69). See also E. P. Odum, 'The strategy of ecosystem development', *Science*, (1969) **164**, pp. 262–70, 262.

[72] Interviews with Auerbach (16 October 1985) and Odum.

[73] Odum, op. cit. (65).

Already in 1951 Odum had expressed interest, as had the team of South Carolina, in the use of radioactive tracers and in experimenting with radiation effects in their ecological studies. But they had no expertise in these methods, and the first grant did not give them room to develop it.[74] Others had preceded them in the use of radioactive tracers in ecology, notably Hutchinson.[75] But Hutchinson did not have the AEC behind him, a support that would be crucial in this field.

Norris and a graduate student started the first radiation effects studies in 1955. In 1957, the first radioactive tracer experiments were carried out, initiating a second major research line on food chains.[76] Odum took a leave of absence during the academic year of 1957–58, to increase his competence in radiation ecology. He was by then already thoroughly familiar with the Oak Ridge programme, having been asked to serve on its advisory board by Stanley Auerbach. On an NSF fellowship he spent four months at UCLA, studying desert ecology and the effects of radiation near the testing grounds in Nevada. He spent another four months at the Hanford Works.[77] During this period, Odum wrote a new chapter on radiation ecology for the second edition of his *Fundamentals*, which appeared in 1959.[78]

In the early years of radiation ecology, the ecologists at ORNL had been mostly limited to problems (and isotopes) with some direct relevance to the problem of radioactive waste. Odum, in comparison, was much more free to make his own research choices and steered radio-ecological methods in the direction of food chain analysis earlier than Auerbach did. They had to sort out for themselves many problems related to the radioactive tagging of plants and animals, even though other researchers had preceded them. Hutchinson had simply administered a sample of radiophosphorus to a lake by dumping a small quantity into the water, and with the help of Geiger counters and a graduate student traced its uptake by the vegetation and plankton.[79]

Experiments started at Savannah River in the spring of 1957. Tagging techniques proved most useful (by injecting the stems of plants with P-32). Measuring of the uptake of radioisotopes was performed with Geiger counters. By plotting radioactive uptake against time, Odum and his colleagues were able to show the place that various insects occupied in the food chain, such as the predatory activities of certain spiders who fed on

[74] Odum, op. cit (65).

[75] G. E. Hutchinson and V. T. Bowen, 'A direct demonstration of the phosphorus cycle in a small lake', *Proceedings of the National Academy of Sciences*, (1947) 33, pp. 148–53. According to McIntosh, op. cit. (1), p. 126, this is the first paper on the use of radioactive tracers in ecology.

[76] Odum, op. cit. (65), p. 19.

[77] He stayed with Charles Elton in Oxford during the remainder of the year. (Odum, interview).

[78] 'Radiation ecology', ch. 14 of *Fundamentals of ecology*, 2nd edn, op. cit. (8), pp. 452–87.

[79] Hutchinson and Bowen, op. cit. (75).

leafhoppers and beetles (which in turn obtained their radioactivity from plants).[80]

In many respects, the methods were simple and crude, and later would be much improved. The application of radioactive tags to plants was standardized in 1962, and a new method, applicable on a larger scale, was introduced in 1967.[81] In the early years, insects were collected by making ten rapid sweeps through the vegetation with a sweep net (others made fifty sweeps to obtain greater accuracy).[82] In 1967, the use of a commercially made motor driven suction insect collector was reported.[83] The Geiger counter is not a very efficient counting device — it would be replaced by a Tracerlab thin window, gas flow, automatic beta counting system or by a well-type scintillation detector.[84] Still later, stable isotopes could be used with mass spectrometers, while measurements of concentrations of compounds would be performed with gas chromatography.[85]

How did the radiological studies contribute to Odum's stated goal of developing a 'holistic' point of view with respect to the functional characteristics of whole ecosystems? In 1956, on the occasion of the International Conference of Peaceful Uses of Atomic Energy, he had pointed out that studies of the effects of radiation should not be directed merely to individual organisms. The impact on the 'total' system should also be assayed, and tolerance levels for entire ecological systems determined. In order to reach that goal, he distinguished three approaches to finding 'methods for the measurement of total community structure and function': measurement of productivity, trophic structure and species structure.[86]

As we have seen, the research that Odum developed at Savannah river met these demands reasonably well. Occasionally, Odum was rather quick to claim the existence of homeostatic mechanisms.[87] Be this as it may, Odum

[80] E. P. Odum and E. J. Kuenzler, 'Experimental isolation of food chains in an old-field ecosystem with the use of phosphorus-32', in *Radioecology: Proceedings of the first national symposium on radioecology held at Colorado State University, September 10–15, 1961* (V. Schultz and A. W. Klement, eds.), New York, 1963, pp. 113–20.

[81] R. Wiegert and R. G. Lindeborg, 'A "stem well" method of introducing radioisotopes into plants to study food chains', *Ecology*, (1964) 45, pp. 406–10; C. D. Monk, 'Radioisotope tagging through seed soaking', *Bulletin of the Georgian Academy of Sciences*, (1967) **25**, pp. 13–17; G. B. Rose, C. D. Monk and R. G. Wiegert, 'Accumulation and transfer of 45Ca by the biota of a tagged cornfield,', in *Proceedings of the second national symposium on radioecology, May 15–17, 1967* (D. J. Nelson and F. C. Evans, eds.), pp. 672–7.

[82] Odum and Kuenzler, op. cit (80); D. A. Crossley, 'Movement and accumulation of radiostrontium and radiocesium in insects', in *Radioecology*, op. cit. (80), pp. 103–5.

[83] Wiegert, Odum and Schnell, op. cit. (47).

[84] E. P. Odum and F. B. Golley, 'Radioactive tracers as an aid to the measurement of energy flow at the population level in nature', *Radioecology*, op. cit. (80), 403–10.

[85] Odum, interview.

[86] Odum, op. cit. (2).

[87] 'For example, the rate of photosynthesis of a whole forest or a whole corn field may be less variable than that of the individual trees or corn plants, because when one individual or species

did develop testable hypotheses concerning the structure of ecosystems as related to the theoretical properties of ecosystems, such as his general theory on succession, and the famous relationship between diversity and stability of ecosystems.

Yet it is interesting to note that research at the University of Georgia diverged in a number of directions, not least toward physiological studies of individual organisms and populations. In a sense, these originated from an extension of the research on energy transmittal from one trophic level to another. The physiological studies are still being pursued. One relatively early example of these studies, carried out in the late 1950s, was the investigation of excretion rates of various radioactive tracers. The tracers were examined with regard to their appropriateness for the measurement of metabolic rates of animals such as arthropods and fish. It was now possible to quantify the extent to which the energy contained in food consumed by the species in a particular trophic level was 'lost' through processes such as respiration, so that only a limited amount of the energy was assimilated and in turn became available for higher trophic levels. The effects of various environmental factors on these physiological processes were also examined.[88] There was thus no overriding effort to direct as much research as possible to the single level of the ecosystem.

It should be pointed out that Odum had a very liberal attitude with regard to the research that his fellow workers originated. His style of directing the programme consisted, in his own words, in pointing to some opportunities and leaving the rest to the newly hired person.[89] As a result, the programme at Georgia could indeed diverge, to the point that research projects with a 'reductionistic' perspective were welcomed as a natural complement or precursor of the 'holistic' research that Odum had in mind. But Odum more than made up for this divergence by his way of conferring his enthusiasm for discovering the principles on which the functioning of ecosystems were founded. In a way, his style of directing the programme was that of a charismatic role model rather than that of a manager-entrepreneur.

On the institutional level the creation of the Institute of Ecology prevented Odum's activities from falling victim to the vicissitudes of the university's departmental organisation. Odum and his colleagues with faculty positions at the University of Georgia lobbied successfully for an Institute, independent from any department. In 1963, the Board of Regents approved the establishment of the Institute of Radiation Ecology. (The following year,

slows down, another may speed up in a compensatory manner.' Odum, op. cit (50), p. 4. A researcher with a way of thinking different from Odum might have ascribed this phenomenon to the law of great numbers operating in an aggregate, rather than assuming some system property.

[88] Odum and Golley, op. cit. (84).

[89] Odum, interview.

the name was changed to Institute of Ecology.) In 1966 the University provided funds to employ an executive director and secretary. In the same year, the Institute obtained an NIH training grant in ecology, which was used to attract new graduate students and to hire more members of the staff. In 1970, the Graduate School approved the Institute's programme leading to a PhD in ecology.[90]

The Institute of Ecology was a very versatile organisation. Odum and his fellow ecologists could bring in a new person without the necessary approval of one of the departments. If a tenured university position should be desired for someone, it was not just to the Zoology Department they could turn, but to departments such as Entomology or Forestry as well. The ample sources of 'soft money' were used to enhance their academic freedom considerably.

Radioecological studies at Oak Ridge National Laboratory

Oak Ridge National Laboratory started a collaborative project with the Tennessee Valley Authority in 1950. Scientists from the latter agency conducted ecological surveys under the direction of Louis Krumholz, determining the contamination of fish with radionuclides in White Oak Lake.[91] The project was discontinued in 1954, apparently at the instigation of Alvin Weinberg, director of research of ORNL.[92] Krumholz's project had been initiated by the director of the Health Physics Division, Karl Z. Morgan. He and his associate Edward Struxness remained interested in adding ecology to the research performed at their laboratory, biomedical and biochemical investigations of the problems radioactive waste posed to human health. In the same year that Krumholz had to stop his project, ecologist Stanley Auerbach joined the staff of the Health Physics Division.[93]

Stanley Auerbach had received his education from some of the best known American naturalists and animal ecologists. He had taken his Master's in Zoology with Victor Shelford at the University of Illinois in 1947, at the age of 26. He had worked mainly on the taxonomy and ecology of arthropods, and he pursued that interest when, at the suggestion of Shelford, he turned to Orlando Park at Northwestern University to work on his PhD.[94] He completed his dissertation in 1949, and on the basis of it published an extensive description of the centipedes of the Chicago area in

[90] F. B. Golley, 'The Institute of Ecology', *Chemosphere*, (1975) 4, pp. 221–33.

[91] Whicker and Schultz, op. cit. (2), p. 5.

[92] Interview with Stanley Auerbach, 16 October 1985.

[93] Struxness, leader of Waste Disposal Research and Engineering of the Health Physics Division at ORNL, had first asked Orlando Park, Professor of Ecology at Northwestern University, to be consultant to the Division. Park acquainted himself with radiobiological problems and he suggested hiring his latest graduate student, Stanley Auerbach.

[94] Auerbach, interview.

Ecological monographs, while his naturalist inclinations are even more clear from a key to those centipedes.[95]

At Oak Ridge, Auerbach set up an experimental project, in the laboratory and in the field, exposing arthropods to radiation and establishing simple dose–response relationships. In the field studies, radiation sources were brought close to the natural habitat of the arthropods. He made attempts to explain findings that were not in accordance with expected dose–response relationships as a complex combination of reduced predation, parasitism, or competition, thus taking species with whom the centipedes interacted into account. He thus aimed at developing an 'ecosystem' concept of the soil, the habitat of the centipedes.[96] Later, Auerbach would comment that this 'proved to be an intellectual challenge well beyond [their] capabilities'.[97]

In 1956, D. A. (Dac) Crossley, an insect ecologist, was added to the staff and joined Auerbach with his experiments.[98] Auerbach's not yet recognizable empire-in-the-making grew steadily in size, but his institutional environment also became more complex. John Wolfe let Auerbach know that the opinion in Washington was that his arthropod approach was 'too fundamental'. At Oak Ridge, independently of Wolfe, Alvin Weinberg was taking a detailed interest in what all his associates were doing. During laboratory meetings, Weinberg 'grilled' Auerbach with questions concerning the relevance of his research, questions that Auerbach found hard to answer.[99] But Weinberg had reasons of his own not to dismiss an ecology programme at his Laboratory. His policy was one of diversification of research activities,[100] and ecology could conceivably play a role in that, albeit a small one.

Around 1956, the White Oak Lake was drained, producing one of the most contaminated lands in the world, with high amounts of strontium-90 and caesium-137. Auerbach and Crossley concentrated their research on this area. In focussing their attention on the succession of plants and movement of radionuclides through plants and insects, they became in part plant ecologists. In the summer of 1957 they created an experimental agricultural plot where they grew corn, and performed measurements on the uptake and behaviour of strontium-90 and caesium-137 in corn, and subsequent transferral of these radioactive isotopes to insects.[101] Weinberg was very

[95] S. I. Auerbach, 'The centipedes of the Chicago area with special reference to their ecology', *Ecological monographs*, (1951) **21**, pp. 97–124; Auerbach, 'A key to the centipedes of the Chicago area', *Bulletin of the Chicago Academy of Science*, (1951) 9, pp. 108–14.

[96] S. I. Auerbach, 'The Soil Ecosystem and Radioactive Waste Disposal to the Ground', *Ecology*, (1958) **39**, pp. 522–9, 527, 528.

[97] Auerbach, interview.

[98] D. A. Crossley (1927) received his education at Texas Technological College and the University of Kansas. Among his fellow students at Kansas was Paul Ehrlich (Interview with Crossley, 10 October 1985).

[99] Auerbach, interview.

[100] A. W. Teich and W. H. Lambright, 'The redirection of a large national laboratory', *Minerva*, (1976) **14**, pp. 447–74, 452–3.

[101] See S. I. Auerbach and D. A. Crossley, 'Strontium-90 and cesium-137 uptake by vegeta-

happy with the direction that the research had taken, and the ecological programme at Oak Ridge was saved.[102]

Both Auerbach and Crossley were strongly interested in a further development of the ecosystem concept in their research, whatever its meaning would be, even if they did not have the background necessary to do it wholly by themselves. Auerbach's interest in ecosystem concepts dated from his graduate student days. Like his advisor Orlando Park, he had been very impressed by the paper on 'trophic-dynamics' by Raymond Lindeman.[103] Crossley, on his part, sympathized very much with Eugene Odum's ecosystem approach in his *Fundamentals of ecology*, a book that Crossley had greeted with enthusiasm.[104] But they needed help to guide them in their intuitive search for an ecosystem approach. Auerbach would accomplish this for his group thanks to the help of Eugene Odum and Jerry Olson, the former in his capacity of consultant in the early stage of the group, the latter hired in 1958 as a research ecologist.

Odum was nominated to the advisory committee of the Health Physics Division. His nomination also served to enhance the scientific credibility of the ecology programme at the Division in the eyes of Alvin Weinberg.[105] '[Odum] came up every six months or so and told us in great detail what to do and what not to do', Crossley said, looking back twenty years later.[106] Auerbach and Crossley regarded Odum's approach as novel and particularly suited to radio-ecological problems:

> Ecology at that time was in this country very largely physiological ecology. . . . Eugene's approach resurrected the ecosystem concept. It was different and very timely, because we were concerned in 1956 with radioactive fallout, where is it going to go, what is it going to do. We were just beginning to be concerned with pesticides in the environment. These things were problems that could not be approached by the ecology of that time, [which] was evolutionary based ecology.[107]

But if Odum took the lead in establishing ecosystem ecology in the United States, he would not translate its concepts in mathematical terms. In contrast the reputation of Oak Ridge would ultimately rest on the production of simulation models of ecosystems, for which the use of mathematics

tion under natural conditions', *Proceedings of the Second U.N. International Conference on Peaceful Uses of Atomic Energy*, (1958) **18**, pp. 494–9; D. A. Crossley, 'Insect–vegetation relationships in an area contaminated by radioactive wastes', *Ecology*, (1961) **42**, 302–17; idem, 'Movement and accumulation of radiostrontium and radiocesium in insects', in *Radioecology*, op. cit. (80), pp. 103–5; idem, 'Consumption of vegetation by insects', in *Radioecology*, op. cit. (80), pp. 427–30.

[102] Auerbach, interview.

[103] Ibid.

[104] D. A. Crossley, interview (10 October 1985).

[105] E. P. Odum, 'Radiation ecology at Oak Ridge', in *Dedication*, op. cit. (4), pp. 53–7.

[106] Crossley, interview.

[107] Ibid.

in ecology was an essential prerequisite. For this, Jerry Olson, who joined Oak Ridge National Laboratory in 1958, would lay the foundations. The models found their fullest expression during the American contribution to the International Biological Programme (1968–1974). By that time, Oak Ridge was established within the community of academic ecology, a position to which it was led most purposively by Stanley Auerbach.

The institutional development of the Oak Ridge programme

As has been noted above, Stanley Auerbach at Oak Ridge National Laboratories faced a much more constrained situation than Odum in institutional respects. ORNL was not an academic environment, even if an arrangement between ORNL and a group of Tennessee universities, notably the University of Tennessee, brought Auerbach and several of his collaborators an academic status.[108]

In addition to the ORNL's general characteristic of being a mission oriented science laboratory, there was the peculiar circumstance that the ecology group resided in the Health Physics Division, not in the Biology Division. As a consequence, Auerbach, Crossley and the rest were in daily interaction with physicists, not biologists. This circumstance influenced the perspective that Auerbach took in the development of his group's work.[109] Within the Health Physics Division, Auerbach obtained some independence for his group of ecologists with the creation on a Radiation Ecology Section in 1959, of which he became Chief.

It might be said that Auerbach, in building his group, deployed two seemingly conflicting strategies. One was to adapt to the constraints of his environment, in cognitive and institutional respects. This is shown, for instance, by his eagerness to develop a mathematically oriented ecology. The physicists, his superiors and his close colleagues, expected no less.

A second strategy of Auerbach became apparent in his ambition for academic recognition. It was very clearly shown by his desire to play a role in the Ecological Society of America. In 1964, he became Secretary of the ESA (when Eugene Odum became its President-elect); he remained Secretary until 1969. Auerbach had taken the Secretaryship 'for the very deliberate reason that it would provide an entree for (him) into the Ecological Society, and also to get Oak Ridge identified as an ecological center', reasoning that the ESA members would receive mailings seven times a year with Oak Ridge on the letterhead.[110] Being *ex officio* member of ESA committees such as the

[108] The universities involved form the 'Oak Ridge Associated Universities'. Stanley Auerbach received an appointment as Lecturer at the University of Tennessee at Knoxville in 1960, and as Adjunct Research Professor in 1965; D. A. Crossley as Lecturer in 1965 and as Associate Professor in 1966. [109] Auerbach, interview. [110] Ibid.

Public Affairs committee, Auerbach tried to interest Senators and Representatives and their staff in ecology. Several of those contacts proved most useful when the lobbying for the IBP started. Little things helped him to function efficiently. Auerbach could freely make long-distance calls, which most of his academic colleagues at the time could not. During this time, he established a reputation among ecologists in the United States of being a good administrator.

Most important for realizing his ambition of academic recognition was of course the building of a sound research group, visible to the scientific community. Auerbach succeeded, after 1960 turning himself more and more into the administrator of his own expanding program. Around 1966, he had a group of 14 scientists. In 1968 he was designated as Director of the Eastern Deciduous Biome Project of the American part of the International Biological Programme, thus becoming Principal Investigator of a very large NSF-funded research project, in which several ecological academic research centers of high reputation also participated, along with his own group. Auerbach was elected President of the ESA in 1970.

It seems fair to say that Auerbach's success consisted in reconciling the conflicting demands of adapting to the norms and values specific to the ORNL and the Health Physics Division, and realizing academic recognition, the latter most forcefully demonstrated by the fact that a large NSF project was administered through the ORNL. But true to the 'big science' culture of the National Laboratories, Auerbach did not import the more liberal features of a traditional academic work organisation into the ecology group. Hierarchic forms of work organisation became even more apparent when Auerbach's group received a separate status as Ecological Sciences Division[111] within the ORNL in 1970: while Auerbach was thus promoted from Section Chief to Division Director, some of his collaborators could now be promoted to Section Chief. Unlike Odum, he acted as a real scientific director for the whole group, setting out research lines for all of its members. As a result, the Oak Ridge programme had a much more focussed character than the Georgia program. Its focus did evolve, however, in important new directions.

Around 1957 then, we might speak of some convergence between the ecological programmes at Oak Ridge and the work at Savannah River. Both used radioactive tracers to measure the flow of materials through the food chain. Both groups did so in an experimental and quantitative manner, although the Oak Ridge group was constrained with respect to the choice of radioactive isotopes: strontium-90 and caesium-137 were investigated primarily for their relevance to the problems of fallout and radioactive waste. But the Oak Ridge group took the lead in the use of quantitative data for the

[111] The Ecological Sciences Division was renamed Environmental Sciences Division in 1972.

mathematization of ecosystem processes and the construction of ecosystem models.[112]

In 1957 Auerbach attended the annual AIBS meetings in Connecticut, where he listened to a contribution by an as yet little known plant ecologist, Jerry Olson. In his lecture, Olson presented the use of a pair of differential equations to account for the gain and loss of nutrients in an ecosystem. Auerbach discovered a certain similarity between Olson's subject and the research of his own group on the flow of radioactive isotopes through ecosystems, and he felt that Olson's approach might be just the right way to develop the mathematical description of ecosystem flow processes. The following year, Olson was offered a position at the Health Physics Division.[113] Karl Z. Morgan, the director of the Health Physics Division, was also pleased with Olson's equations, discovering a remarkable similarity between them and the kinds that the physicists in his Department employed.[114]

Olson initiated a second stage in the mathematization of ecosystems around 1960. Together with the ecologists Bernard C. Patten,[115] who joined the staff in 1963, and also with George Van Dyne[116] who was hired the following year, Olson developed simple ecosystem models that could be simulated on analog and digital computers.

Jerry Olson (born in 1928) received his education at the University of Chicago. After taking undergraduate degrees in liberal arts and geology, he completed his education in 1951 with a PhD in botany with the plant ecologist Charles Olmsted as supervisor. He interrupted his studies at the University of Chicago for a semester at the University of California at Berkeley, where he studied soil science with Hans Jenny. From 1952 until 1958 he worked at the Connecticut Agricultural Experiment Station in New Haven. Olson did not have any formal connection with Yale University, but he regularly attended the seminars of Hutchinson. Olson regards Olmsted and Jenny, and to a somewhat lesser extent Hutchinson, as his most influential teachers.[117]

The first research line that Olson developed at Oak Ridge was a continuation of his previous empirical research on soil formation and the part played

[112] The use of mathematics for the theoretical investigation of ecological systems was, of course, by no means new in ecology. These previous studies were usually confined to simple systems, such as predator–prey systems, and formed part (as they still do) of population dynamics. See Kingsland, op. cit. (53) and McIntosh, op. cit. (1), pp. 171–92.

[113] Interviews with Jerry Olson (16 October 1985) and Stanley Auerbach. Olson was the fifth person to be hired in the ecology group of the Division. [114] Ibid.

[115] Bernard C. Patten (1928) received his education at Cornell University, the University of Michigan and Rutgers University (PhD in Botany in 1959).

[116] Van Dyne (1922) received his education at Colorado A & M College, South Dakota State University and the University of California at Davis (PhD in nutrition).

[117] Olson, interview.

in it by the breakdown of organic litter. But from now on he had the advantage of having radioactive tracer methods at his disposal.[118]

Secondly, Olson elaborated his theoretical treatment of litter decay. In 1959, Olson gave a paper at the Ecological Society of America Symposium on 'Energy flow in ecosystems', at the University of Pennsylvania. The Symposium was organised by E. P. and H. T. Odum. The paper was published in 1963, in *Ecology*.[119] (The editors of *Current contents* classified the paper as a 'citation classic', noting that it received over 200 citations by 1985.)[120]

In comparison with his earlier work, the article in *Ecology* shows a remarkable shift in vocabulary. Olson discusses the subject of litter decay in terms of *production*, *energy,* and *trophic levels*, none of which were mentioned in previous articles. References to R. Lindeman and E. P. Odum now also appear.[121]

An important new line of theoretical investigation was started around 1960, when Olson turned to the analog computer to simulate ecosystem processes such as litter decay. At first, Olson may have turned to the analog computer as a simple computational device to keep account of net change in radioactivity in terms of gains and losses. But in his earliest publication on the subject, the analog computer is used for much more than that, notably for situations in which the analytic solving of differential equations would be too difficult. Instead of only one 'compartment' (i.e. the soil) of which gains and losses are determined, computations are performed on several coupled compartments at once.[122] Olson was among the first to use an analog computer to investigate ecological problems, and probably the first to use it for simulating ecosystem processes. Shortly before him, H. T. Odum had simulated ecosystem behaviour with an electrical circuit.

In Odum's analogue circuits, the flow of electrons corresponded directly with the flow of carbon through the ecosystem. Branching electrical wires represented different flows of food in a real ecosystem, while variable resistances likewise represented populations of producers and consumers (Figs. 3–4). Milliammeters in each circuit permitted 'rapid visual examination of the electrical flow which represents the flow of carbon . . .'.[123] The manipulation of the analogue (resembling an ecosystem on which Odum had empirical data) enabled him to produce a number of 'hints' as to how

[118] R. E. Shanks and J. S. Olson, 'First-year breakdown of leaf litter in southern Appalachian forests', *Science*, (1961) **134**, pp. 194–5; J. S. Olson and D. A. Crossley, 'Tracer studies of the breakdown of forest litter', *Radioecology*, op. cit. (80), pp. 411–16.

[119] J. S. Olson, 'Energy storage and the balance of producers and decomposers in ecological systems', *Ecology*, (1963) **44**, pp. 322–31. The paper had also been presented at the 9th International Botanical Congress in 1959.

[120] *Current contents* (in press).

[121] Olson, op. cit. (119), pp. 322, 328.

[122] J. S. Olson, 'Analog computer models for movement of nuclides through ecosystems', in *Radioecology*, op. cit. (80), pp. 121–5.

[123] Olson, op. cit. (122), p. 5.

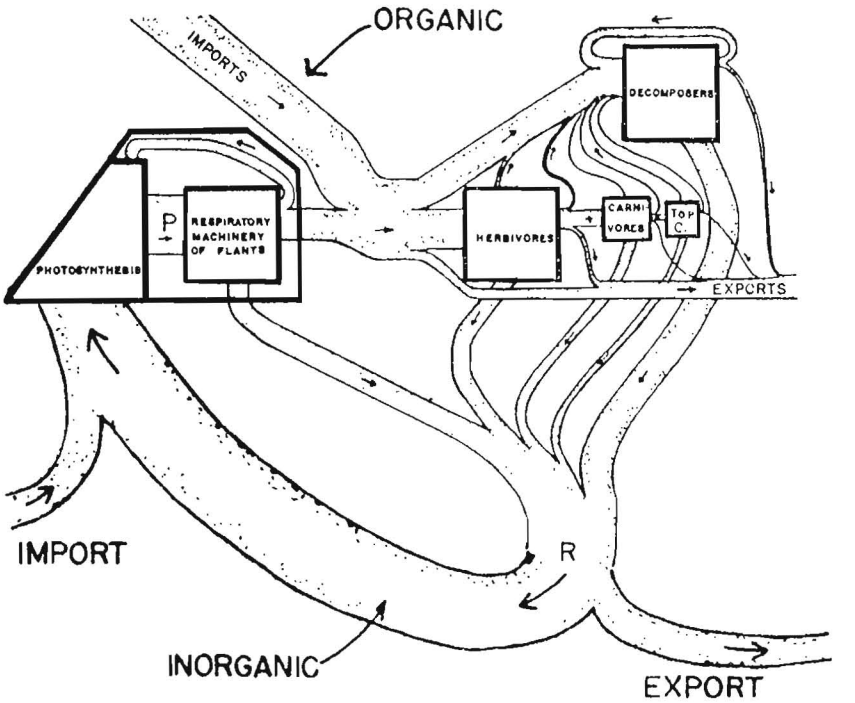


FIG. 3: 'Ecomix cycle for an ecosystem'. H. T. Odum, 'Ecological potential and analogue circuits for the ecosystem', *American scientist*, (1960) 48, pp. 1–8, 2.

ecosystems would work, in qualitative as well as in quantitative ways. Thus the analogue circuits served him as an 'electrical computer'.[124] The perceived analogy between real ecosystems and electrical networks was so important to Odum, that he continued to make use of it.[125]

In comparison, there are important similarities between the way Olson translated a hypothetical scheme of boxes and arrows representing functional relationships in an ecosystem into an analog computer circuit, and Odum's operations leading to electrical circuits. The diagrams resemble each other and can be transformed into each other (Figs. 3–5). The ecosystem diagrams that they represent (consisting of various compartments such as herbivores and decomposers) are essentially a new transformation of Lindeman's pyramids of trophic levels, now drawn as a number of interconnected boxes.[126]

[124] Olson, op. cit. (122), p. 7

[125] H. T. Odum, *Systems ecology*, New York, 1983, p. 10.

[126] The first such diagram may have appeared in H. T. Odum, 'Primary production in flowing waters', *Limnology and oceanography*, (1956) 1, pp. 102–17. It is redrawn in E. P.

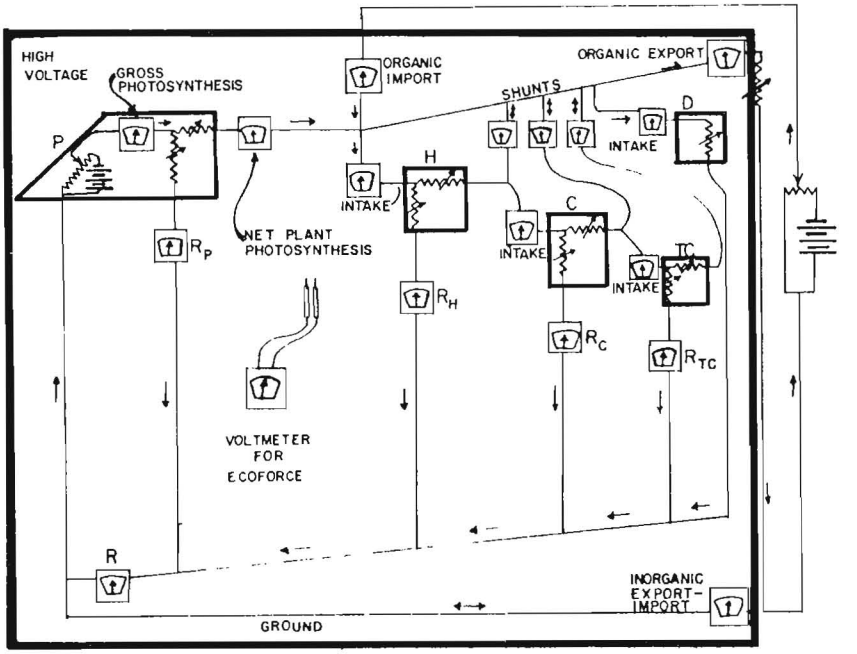


FIG. 4: 'Electrical analogue circuit for a steady state ecosystem like the one in Fig. [3]'. H. T. Odum, 'Ecological potential', 4.

An important similarity between both kinds of models is the fact that they both depend on 'input'. The nature of this input is perhaps most clearly evident in Odum's electrical circuit: the power derived from the charged batteries is analogous to the input of sunlight in the ecosystem.[127] The nature of this 'input' would prove to be of far-reaching consequence for the implicit philosophy of these and later system ecological models. The implication was that the 'driver' of the ecosystem was a non-biotic, climatic factor, and thus put what we may in fact remember as the 'density-independent' factors of the 1950s in ultimate control of ecosystems.

Yet there are differences. One is that Olson introduced positive feedback as well as negative feedback functions in his diagrams, which allowed him to account for situations other than the steady state. Later, Odum would make up for this. The most important difference, however, concerns the nature of the model. Odum measured currents, representing flows. Olson measured voltages, representing flow rates. Rates are expressed by differential equations. In contrast, Odum used the much simpler arithmetic of Ohm's Law.

Odum's 1959 edition of *Fundamentals* (op. cit. (8), p. 47), but does not yet appear in the first (1953) edition.

[127] Olson's 'photosynthetic rate functions' are their equivalent.

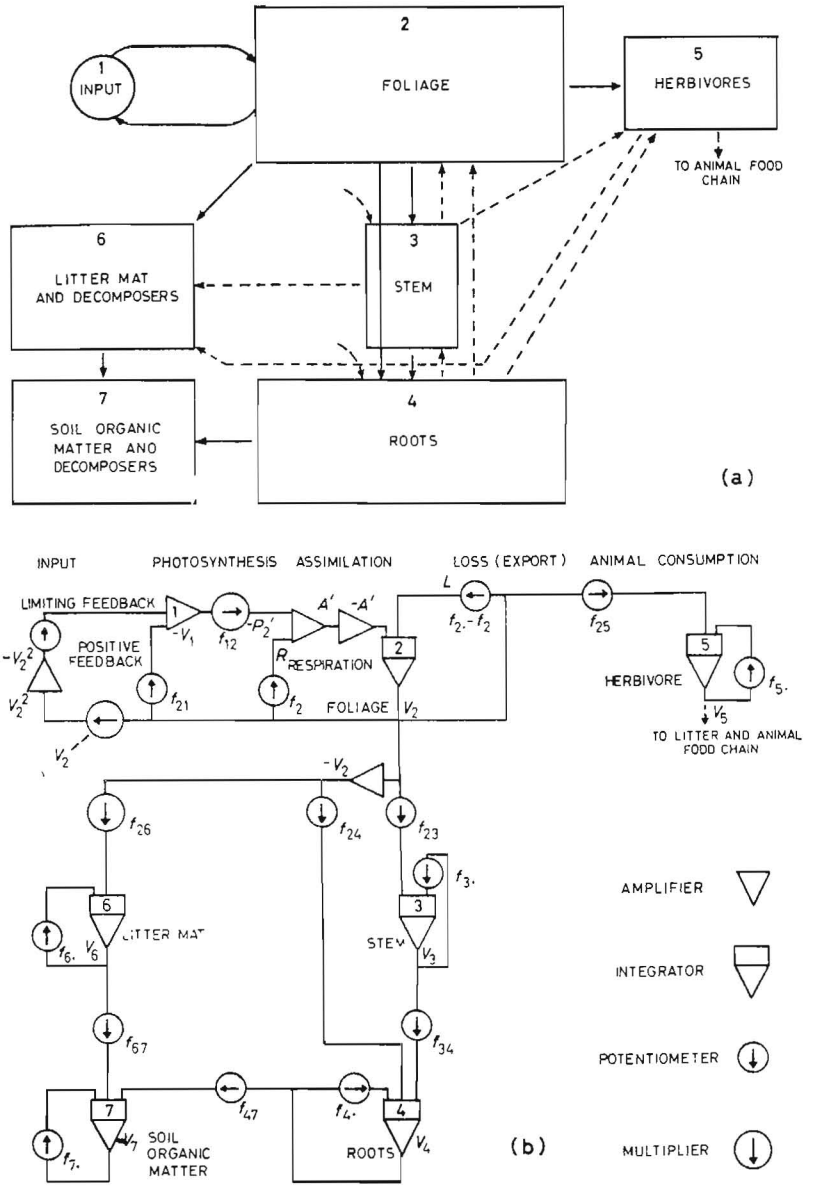


FIG. 5: 'Model ecosystem represented by (a) box diagram and (b) analog computer circuit'. J. S. Olson, 'Gross and net production of terrestrial vegetation', *Journal of ecology*, (1964) **52** (Suppl.), pp. 99–118, 103.

The electrical circuit provided Odum with a direct material analogy, with 'energy' as the common denominator of ecosystem flows and electric currents. In addition, the circuit helped him to make computations. To Olson, like Odum, the analog computer was not primarily a computational device. His diagrams also implied analogies between ecosystems and non-ecological systems. But because these analogies were abstracted in equations, the computational aspect gained more prominence.

Systems ecology at Oak Ridge (1963–1968)

It seemed as if the process of litter decay on which Olson originally worked was too small a problem for the large capacity of the National Laboratory Analog Computer Facility at Oak Ridge. Flows between nine interrelated compartments could be charted, and computations performed on them. These nine compartments represented a partial ecosystem. Olson's original research interest can be deduced from the fact that five of these compartments were soil related, e. g., 'roots' and 'organic litter on top of soil'.[128] Subsequent ecosystem models gradually lost the emphasis on soils and litter decay.[129]

In a paper he gave at the 1963 Jubilee Symposium of the British Ecological Society, while on a Guggenheim fellowship in England, Olson set forth some theoretical questions that he thought could be addressed by ecosystem modeling. He firmly put the steady state of ecosystems in the center of his considerations:

> More important for questions of stability of the community and ecosystem is whether the widespread deflections from the steady state automatically tend to become corrected, and if so, how rapidly . . . . Many models would imply negative feedbacks and a restoring tendency. The further point deducible from models but not from verbal formulation and climax theory is some estimate of time-lags in the response of the ecosystem after a perturbation.[130]

In fact, Olson formulated here what would become the orthodoxy of systems ecology during the International Biological Programme, namely that ecosystems function in, mathematically speaking, a single domain of attraction. If perturbations made ecosystems deflect from their steady state situation, they would self-correct, i.e., move back toward the stable equilibrium point of the domain of attraction.

The new systems orientation also inaugurated an upscaling of the empirical studies and with them, of the use of radioactive nuclides. In 1962,

[128] Olson, op. cit. (122), p. 124.

[129] J. S. Olson, 'Gross and net production of terrestrial vegetation', *Journal of ecology* (1964) **52** (Suppl.), pp. 99–118.

[130] Olson, op. cit (129), p. 112.

Auerbach and Olson took part in an experiment in which they tagged thirty tulip poplars on a forest portion of 20 × 25 m with caesium-137, the results of which were published in *Nature*.[131] The experiment was set up to determine cycles and rates of cycling of caesium in this forest ecosystem and apply the results to Olson's analog computer model of mineral cycling. One of the conclusions was that much more data was needed. This conclusion is not as trivial as it may seem. The expensive IBP projects that started in 1968 were partly conceived as crash programmes to obtain the vast amount of data needed for ecosystem simulation models. In a subsequent publication a validation of the model by comparing model simulations on a digital computer with real data showed that in order to achieve an approximation of the model's behaviour with the real forest system the incorporation of many more refinements, such as seasonal fluctuations would be required.[132] Olson noted with satisfaction that the analogy between the ecological model and physical-chemical and physiological systems seemed to work out.[133]

Systems ecology as it was developing at Oak Ridge obtained a much broader audience when a course — the first of its kind — was offered at the nearby University of Tennessee at Knoxville. The course was given by Olson, Patten and Van Dyne for a number of consecutive years. Their part-time (20%) faculty appointments were paid for by the Ford Foundation.[134] Apart from students who could take the course for credit, the course was also attended by other ORNL ecologists, post-doctoral fellows and visitors, for whom National Science Foundation support was available.[135]

Olson, Patten and Van Dyne contributed in various ways to the course. Olson's method of model construction was followed. The imposition of a strong cybernetic framework on the subject matter might be considered contribution of Patten's.[136] George Van Dyne's contribution to the course may be characterized in part by the development of the general management relevance of the models. Coming from a background in range management,

[131] S. I. Auerbach, J. S. Olson and H. D. Waller, 'Landscape investigations using caesium-137, *Nature*, (1964) **201**, pp. 761–4.

[132] J. S. Olson, 'Equations for cesium transfer in a *Liriodendron* forest', *Health physics*, (1965) **11**, pp. 1385–92.

[133] Olson, op. cit. (132), p. 1392. Measurements of this particular forest tagging experiments were pursued for many years. One follow-up report on this experiment is D. E. Reichle et al., 'Organic matter and 137Cs turnover in forest soil by earthworm populations: Application of bioenergetic models to radionuclide transport', in *Proceedings of the Third National Symposium on Radioecology, May 10–12, 1971, Oak Ridge*, (D. J. Nelson, ed.), Oak Ridge, pp. 240–46.

[134] Jerry Olson already was lecturer at the University of Tennessee since 1960. In 1964 he became Professor of Botany. Patten and Van Dyne became Associate Professors in Botany in 1964.

[135] B. C. Patten, 'Systems ecology: A course sequence in mathematical ecology', *BioScience*, (1966) **16**, pp. 593–8.

[136] See B. C. Patten, 'An introduction to the cybernetics of the ecosystem: The trophic-dynamic aspect', *Ecology*, (1959) **40**, pp. 221–31.

he was strongly interested in the *optimization* of the performances of systems.[137] The course served to create visibility for Oak Ridge National Laboratory as a center for the simulation modeling of ecosystems.

Olson's models became the major influence on the future work of the Oak Ridge ecologists. Crossley, for instance, started learning the mathematics of systems ecology.[138] During the IBP, Crossley would head a separate systems ecological program at the University of Georgia. Patten and Van Dyne after 1965 increasingly used digital computers. Like Crossley, Patten moved to the University of Georgia, Van Dyne to Colorado State University.[139] They all continued ecosystem modeling. During the International Biological Programme, in which the three institutions participated, ecosystem models formed the core of the IBP research projects.[140]

The state of systems ecology around 1967

To what extent then did systems ecology exist in 1967, the year in which Congress decided to endorse an American contribution to the International Biological Programme with the specific intention to promote systems ecological research?[141] A comparison with other system-like ecological activities in the USA at the time will help us to appreciate the extent to which the programs at ORNL and the UGa were forming the beginnings of a new ecological subdiscipline, i.e. systems ecology.

If we disregard for the moment the fact that any computation carried out on a 'system' as simple as for instance two species could be called 'systems ecology', and seek instead to capture the meaning of the term 'systems ecology' assigned to it during and immediately after the IBP, we may come to the following definition: (1) it is ecosystem ecology, i.e., it assumes large entities such as landscapes or biomes to be functionally integrated wholes and it attempts to uncover their structure and behaviour; (2) it attempts to represent such ecosystems by models that lend themselves to simulation and analysis with the help of computational devices, and by doing so to develop or test assumptions concerning the structure and functions of ecosystems; and (3) it attempts to make ecosystems amenable to management, i.e. to put their functioning under the control of man and to optimize its yield or output. Models of the latter kind are called 'total ecosystem models'. By consequence, any model not representing a whole ecosystem is a submodel or a partial model, and, within the context of the systems ecology of the 1960s and 1970s, presupposes such a comprehensive model, even if that

[137] G. M. Van Dyne, *Ecosystems, systems ecology, and systems ecologists*, Oak Ridge, 1966.

[138] 'At the kitchen table, on Sunday afternoons.' (D. A. Crossley, interview).

[139] Van Dyne left Oak Ridge in 1966, Crossley in 1967, and Patten in 1968.

[140] On Van Dyne's project at CSU, see C. L. Kwa, 'The modeling of the grasslands', *Historical studies of the physical and biological sciences*, in press.

[141] See Kwa, op. cit. (7).

model does not in fact exist. The expectation of the early systems ecologists was that the models eventually would mimic the real ecosystem in all its relevant aspects.

In addition, certain basic assumptions about the nature of ecosystems and modeling were shared by the early system ecologists: (1) Ecosystems are stable systems. After a perturbation they return to equilibrium. According to E. P. Odum's thesis, they are the more successful in doing so when they are more diverse, that is when more stabilizing relationships of a cybernetic type can be assumed to exist. (2) True to the legacy of Lindeman, an emphasis is placed on nutrient cycling and the transfer of energy and biomass between compartments of functionally related species rather than on individual species. Diversity and stability are also usually defined in these terms. (3) The actual state of an ecosystem is determined by climatic variables, rather than by the dynamics between species in the ecosystem. The inclusion of these assumptions makes systems ecology a specific research program as much as a subdiscipline. If we take the above definition of systems ecology as a yardstick, systems ecology existed around 1967 as a research project, or even as a research ideal, rather than as an established research practice. It was expected that the IBP would bring this practice about.

What else existed on the American ecological scene that bore relevance to this systems ecology *in statu nascendi*? With respect of the modeling of whole ecosystems, Olson, Patten and Van Dyne (and H. T. Odum) were still pioneers. H. T. Odum's study in Puerto Rico had many important similarities with the Oak Ridge approach. Later, while the IBP was already under way, an attempt to establish a Tropical Forest Biome project for which Odum was to serve as Biome Director, failed to obtain support. Thus Odum was deprived of a way to put his own variant of systems ecology much more to the test.[142]

With regard to comprehensive ecosystem projects, the most important in terms of size and funding was the Hubbard Brook Ecosystem study, which is still being pursued. In 1963, Gene Likens (Yale University) and F. Herbert Bormann (Dartmouth College) set up an experimental project to determine the input–output relations of nutrient elements in a small watershed. But Likens and Bormann never aimed at producing ecosystem models, not even in verbal form.[143] The nutrient budgets that they established represented, in

[142] Several organizational and planning meetings of the Tropical Forest Biome project were held during 1969. The project was still listed as one of the Biome projects in *Research programs constituting U. S. Participation in the International Biological Program*, Report No. 4 of the US National Committee for the International Biological Program (Washington DC, National Academy of Sciences, 1971), pp. 35–36.

[143] On the Hubbard Brook study, see McIntosh, op. cit. (1), pp. 204–8. See also: G. E. Likens et al., 'The calcium, magnesium, potassium and sodium budgets for a small forested ecosystem', *Ecology*, (1967) **48**, pp. 772–85.

the eyes of the IBP ecologists, important but partial system properties of ecosystems.[144] Prior to the IBP, the Hubbard Brook study was probably the biggest of its kind, and certainly the biggest ecological project sponsored by the NSF. Likens and Bormann missed the IBP boat, although the Hubbard Brook study was considered for inclusion in the IBP.[145] Comparing the Hubbard Brook study with the IBP projects carried out at UGa and ORNL, we can note important similarities. For instance, the Hubbard Brook project had a certain precedence in transcending a traditional barrier in ecology between limnological (freshwater) and terrestrial studies, and it also inspired IBP ecologists by its methodology of extensive and continuous measurements.[146] It also had features that the IBP projects had not, among others a strong practical orientation to the problem of erosion of watersheds. Perhaps the project of Likens and Bormann could have been labelled 'systems ecology', but eventually it was not.

In order to understand the significance of the ecological programs at the UGa and ORNL in a national context, we should not merely point out their theoretical importance, but also their sheer size. Research on whole ecosystems as performed at the Oak Ridge National Laboratory and the University of Georgia needed more manpower than was normally available at American universities. The ecology programme at the National Science Foundation was chiefly oriented toward individual research projects at universities, mainly in population dynamics and community ecology. In this context the importance of the Atomic Energy Commission as a patron of 'big ecology' must be understood. But even most of the AEC-funded projects were small and strictly confined to radiological aspects of ecology.[147] In terms of size, the groups at the UGa and ORNL were virtually unique. The IBP enabled them to expand further, and they retained their new size after the IBP.

Systems ecology as a movement and the impact of the AEC

We found a movement of ecologists that wanted to 'revolutionize' ecology in its entirety. This movement wanted to unify the various subdisciplines of ecology under a single perspective. It wanted to point the way toward a new rationalistic approach to the management of nature.

[144] Interview with F. E. Smith (25 September 1985).

[145] See *Report of the working session on deciduous forest ecosystems: Design of research for the comprehensive study (Atlanta, Georgia, January 26–28, 1968)*, for the potential inclusion of the Hubbard Brook study as one of 18 sites (p. 31).

[146] Crossley mentioned the Hubbard Brook study as a source of inspiration for the design of the Coweeta project (Crossley, interview).

[147] In 69 of the 82 research papers read at the First National Symposium on Radioecology in 1961, support from the AEC was acknowledged. Apart from AEC-run laboratories and government agencies, 32 different universities were listed as home addresses of the contributors to this symposium. See *Radioecology*, op. cit. (80).

The starting point was radiation ecology, which originated almost completely in a mission-oriented context. With the exception of Hutchinson's early experiment of dropping a small quantity of radioactive phosphorus in a lake, the monitoring and use of radioactive isotopes was induced by the needs of the AEC.

The first stage of the movement toward systems ecology, 'ecosystem ecology', can be dated at 1953 and is linked with the name of Eugene Odum. This stage marks the beginning of a severance of links with evolutionary biology; a comprehensive functional perspective led to transcendence of traditional barriers in ecology. Several concepts originally developed by Lindeman (and later to become key concepts of systems ecology), may have fallen on fertile ground because of their management applicability and the fact that it appeared to be possible to implement them by radiation ecological methods. They had become entrenched at the UGa and ORNL before systems ecology properly speaking had been developed. Olson started to use its vocabulary only after he had joined the ORNL.

The AEC appears to have constituted a selective environment at least, and perhaps a formative one in some respects. However, the case of Hanford shows that the role of the AEC should not be overestimated. At Hanford, the Laboratory management expressed its needs explicitly, which constrained their ecologists to surveying the environment of the atomic plants. At ORNL and the UGa, the ecosystem ecologists created space for themselves to carry out a programme that apparently was not incompatible with the interests of the AEC. Their programme may indeed have appeared to serve those interests because of its management orientation, but it was a management orientation of a very general scope and not one exclusively designed to solve safety problems with regard to radioactive isotopes. And insofar as the AEC did form a selective environment, systems ecology as it arose in the early 1960s was not its completely contingent product. The ecosystem ecologists did have notions with regard to the direction they wanted to go, however vague they were.

The second stage, or 'systems ecology' phase can be dated at 1959 and is linked with the name of Jerry Olson, among others. The modeling of total ecosystems and simulation on computers was then inaugurated. With 'ecosystem ecology', Odum reached beyond the too narrow constraints of a small university culture with its departmental barriers. With 'systems ecology', the ORNL ecologists reached beyond the too narrow constraints of mission-oriented research. The new concept helped them to transcend the culture of the AEC and the National Laboratories and to reach out to the academic ecology of the universities and the National Science Foundation.

But we would have an incomplete understanding of the radiation/systems ecologists if we were to understand their movement as a way of securing for themselves a niche for a mere new subdiscipline of ecology. First, 'ecosystem

ecology' was not a fail-safe strategy to win them a comfortable position. Even if Eugene Odum had created a sympathetic audience almost singlehandedly, ecosystem ecology risked being too fundamental for the AEC and too applied for academic ecology. Second, the ecosystem/systems ecologists aimed higher than a new subdiscipline. The ecosystem/systems ecologists have consciously sought to put the management of natural resources on a rationalistic footing. The metaphor of the automatic machine, the cybernetic combustion engine, gave a direction to the development of systems ecology concepts and its aims of managerial application in ways that have not always been transparent to its practitioners. The development of systems ecology was a manifestation of great technological optimism with regard to the possibility of the management of natural systems, by no means confined to ecologists.

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The failed promise of human ecology

EUGENE CITTADINO

Responding to an inquiry in 1938 concerning where one could obtain university courses in human ecology, the irrepressible Charles C. Adams – pioneer ecologist, director of the New York State Museum, and champion of the popularization of ecological knowledge – answered that he knew of no such courses.[1] This was a curious response considering that the Department of Sociology at the University of Chicago, Adams's alma mater, had been offering courses in human ecology since 1925 and had acquired a reputation for its ecological perspective. Adams was not unaware of the efforts of the Chicago sociologists; he had cited some of their works in an earlier article relating general and human ecology.[2] Yet he did not advise his inquirer to go to Chicago for courses in human ecology. 1938 was also the year in which sociologist Milla Alihan published her thorough critique of human ecology, focusing, in fact, on the Chicago program. Alihan characterized 'the ecological school' as 'one of the most definite and influential schools in American sociology', and later added: 'Today human ecology is a recognized discipline, its influence permeating many universities in the United States and Canada.'[3] We can only assume from these two assessments that 'human ecology' meant something very different for Adams and for Alihan. A clue to this difference can be found in a paper published in 1967 by ecologist Paul Shepard, who asked, 'Whatever happened to human ecology?' His answer in essence was that human ecology had lost its biology and had become 'that branch of sociology dealing largely with urban geography'.[4] Shepard's judgement may have been a bit harsh, but most of the work that went under the label 'human ecology' had been done by sociologists whose greatest interest was in urban spatial relations. Most of Alihan's 1938 attack was directed at the environmental determinism of this urban school of sociology as well as at what she considered its hopeless confounding of biological and

This paper is based upon work supported by the National Science Foundation under grants no. DIR-8911783 and DIR-9112735.

[1] C. C. Adams, 'A note for social-minded ecologists and geographers', *Ecology*, (1938) 19, p. 501.

[2] Adams, 'The relation of general ecology to human ecology', *Ecology*, (1935) 16, p. 330.

[3] M. A. Alihan, *Social ecology: A critical analysis*, New York, 1938, pp. xi & 2.

[4] P. Shepard, 'Whatever happened to human ecology?' *BioScience*, (1967) 17, p. 891.

sociological terms and concepts, a problem which Shepard found equally disturbing.

Shepard was quite sympathetic toward human ecology. Perhaps more typical of the attitudes of practising ecologists is that expressed by Robert McIntosh in his comprehensive overview of the history of ecological science. McIntosh regarded the recent flurry of activity in human ecology with considerable caution and offered this unflattering summary of the work that had been done thus far: 'The several uses of the word [ecology] were largely window dressing, with little reference to ecologists and their ideas. Geography, sociology, and other disciplines concerned with humans, their cultures, and their relation to the environment sometimes adopted the name but rarely the substance of ecology.'[5] He was referring, of course, to the 'substance' developed by botanists, zoologists, limnologists, and population biologists since the late nineteenth century. If human ecology has ignored much of that substance in the past, in more recent years, owing largely to the awakening of environmental awareness during the last two or three decades, it has come to represent a confusing variety of approaches, methods, and points of view, including within its loosely defined boundaries elements of anthropology, landscape architecture, urban planning, home economics, epidemiology, and public health, not to mention philosophy, political science, and the arts. The only common thread running through all this work is a preoccupation with human groups and their environments; the nature of the human unit and the nature of the environment vary considerably.

Despite the present confusion, which has made the term 'human ecology' practically meaningless, earlier in this century a number of scientists, social scientists, and social planners held out expectations for a robust, full-bodied human ecology that would represent the effective integration of the biological and social sciences. This was certainly Adams's expectation when he lamented the lack of courses in the field. He challenged universities to develop such an integrated program, and he pointed to Lewis Mumford's recent book, *The culture of cities*, as an example of a work of social significance which was sensitive to the biological foundations of human society.[6] In that book, and in an earlier study of American art and architecture in the late nineteenth century, Mumford acknowledged positive signs in some of the work being done in human geography, urban sociology, and regional planning that indicated the emergence of the kind of human ecology that Adams had in mind.[7] In his 1934 autobiography H. G. Wells expressed his belief that a proper education rested on the tripartite foundation of biology, history, and human ecology; and he mentioned that he had intended to title

[5] R. P. McIntosh, *The background of ecology: Concept and theory*, Cambridge, 1985, p. 308.

[6] Adams, op. cit. (1), pp. 500–502.

[7] Mumford, *The culture of cities*, New York, 1938, pp. 301–3; idem., *The brown decades*, New York, 1931, pp. 77–8.

a work on economics and politics 'Outline of human ecology' but had decided against it because the word 'ecology' was not well known.[8] In another work, *The science of life* (a collaborative effort with his son and Julian Huxley), Wells had given considerable attention to ecology and had pointed to its intimate connection with economics, characterizing ecology as economics extended to the whole of life and economics, reciprocally, as the ecology of the human species or 'Human ecology'. He concluded that the dismal science 'might have been a better and brighter science if it had begun biologically'.[9]

Wells's interest in human ecology was not a reflection of a vigorous British movement.[10] There was some isolated work along similar lines, such as that of Mumford's mentor, Scottish botanist and regional planner Patrick Geddes, but human ecology was largely an American enterprise during the first half of the twentieth century. This paper will focus on early efforts in the United States to develop human ecology as a distinct field of study and on the relationship between this new field and the general science of ecology, which was itself in the process of establishing its intellectual and institutional identity. The following discussion should provide a fuller answer to Shepard's question: Whatever happened to human ecology? However, my more modest aim is to identify major problems and themes and to raise questions, in preparation for a more complete study of human ecology.[11] This is a first attempt at addressing this somewhat confusing set of issues, a kind of preliminary sketch of the territory that will nevertheless draw upon occasional unpublished sources. The emphasis will be on the period from the second decade of the twentieth century to the early 1950s, that is, before human ecology became hopelessly intertwined with the new environmentalism.

The title of this paper is a bit misleading, deliberately so. Human ecology certainly did not fail as a cultural phenomenon. Indeed, it is very much with us. It did, however, fail to materialize as a consistent and coherent field of inquiry. The existence of university chairs, departments, journals, and an imposing body of literature has not placed human ecology any closer to fulfilling the expectations held out for it earlier in the century. This situation is not necessarily to be lamented. Despite the hopes and plans of some early

[8] Wells, *Experiment in autobiography*, Boston, 1934, pp. 617–18. Instead of 'Outline of human ecology' he called the 1931 study *The work, wealth and happiness of mankind.*

[9] H. G. Wells, J. S. Huxley, and G. P. Wells, *The science of life*, 2 vols., Garden City, New York, 1931, Vol. II, pp. 961–2.

[10] Wells was apparently 'immensely disconcerted' several years later that his *Work, wealth and happiness of mankind* was still 'the only attempt to bring human ecology into one correlated survey'. Quoted in G. Young (ed.), *Origins of human ecology*, Benchmark papers in ecology, vol. 12, Stroudsberg, PA, 1983, pp. 3–4.

[11] I plan to include an expanded version of this paper as a chapter in a book I am preparing on ecological science between the world wars.

ecologists, human ecology first took hold within sociology and became associated with a particular line of research whose applicability outside the institution and region in which it developed was rather limited. A comparable human ecology movement did not take place within the life sciences, geography, or anthropology. From the 1930s through the 1950s professional ecologists viewed human ecology with increasing scepticism and resisted efforts to find a place for it within the boundaries of their science, whose theoretical and methodological framework was itself not very well developed. In the 1960s and 1970s, when active interest in human ecology seemed to emerge on all fronts at once, a bewildering array of social concerns and ideological agendas was placed right alongside the now more complex, but not necessarily more coherent, theoretical structure of ecological science. The result was not so much the emergence of a new synthetic discipline as, at best, the recognition of the enormous scope and complexity of the enterprise and, at worst, a good deal of conceptual confusion. Although the issues were equally complex earlier in the century, the relatively small number of players and the absence of an active environmental social movement at least make this earlier period more amenable to historical analysis.

A quiet beginning: Huntington, the Ecological Society, and the ideal climate

Despite its early appropriation by sociologists, the term 'human ecology' was first introduced in close association with the initial formal organization of ecological science in Great Britain and the United States. Sociologists usually date the origin of the term to 1921, when it appeared in the influential textbook of Chicago sociologists Robert Park and Ernest Burgess.[12] However, McIntosh contends that the term came up in discussions during the first summer meeting of the British Ecological Society in 1914, wherein the society affirmed its commitment to ecology in the broadest sense 'including human ecology'.[13] Despite this remark, and despite Arthur Tansley's statement in the first issue of the society's *Journal of ecology* regarding the breadth of fields to be considered (although he did not mention human ecology by name), one can search in vain through the early volumes of the journal for articles, notes, or reviews relating to human ecology.[14] This lack of attention may have been the result simply of an editorial policy in favour of the detailed studies of vegetation that dominated

[12] R. E. Park and E. W. Burgess, *Introduction to the science of sociology*, Chicago, 1921, p. 558. This reference is given in virtually every account of the origins of human ecology written by sociologists or historians of sociology. [13] Quoted in McIntosh, op. cit. (5), p. 302.

[14] My examination of the first forty volumes of the *Journal of ecology*, 1913–1952, revealed no articles specifically devoted to human ecology, just an occasional article or review concerning applications of ecological principles to crop plants.

the journal. Nevertheless, human ecology does not seem to have been pursued or acknowledged very widely within the British ecological community during the 1920–1950 period.[15] The situation was somewhat different in the United States. There human ecology was at least a topic of open discussion among ecologists from time to time. As in Britain, the matter of human ecology came up early in the organization of the Ecological Society of America. At the first meeting, in New York, 1916, geographer Ellsworth Huntington, bothered somewhat by the narrow perspective represented by plant and animal ecologists, encouraged other geographers to join the Ecological Society so that human ecology would be well represented.[16] Huntington was elected the society's second president in 1917, and he continued to promote human ecology into the early 1920s. As president, he implored the society's secretary-treasurer Forrest Shreve to consider the publication of a journal and suggested the modification of Shreve's own journal, *Plant world*, for this purpose.[17] Shreve eventually agreed, and the journal first appeared in its new form as *Ecology* in 1920.

Although in the United States, as in Britain, the work of botanists dominated the formal science of ecology in the first decades of the twentieth century, the first article to appear in the new journal was a study by Huntington on the effects of climate on pneumonia and influenza.[18] Considering Huntington's role in establishing the journal, it was only fitting that he should be the author of the premiere article. This study, coming as it did in the wake of the devastating influenza epidemic of 1918–19, represented a prime example of Huntington's approach to human ecology — it was rooted in a broad conception of environment and human habitation, it focused on a particular set of conditions in a particular region (large cities in the northeastern United States), and its results were intended to have immediate application. Huntington's conclusion after studying public health records and

[15] The official history of the Ecological Society, J. Sheail, *Seventy-five years in ecology: The British Ecological Society*, Oxford, 1987, makes no mention of human ecology. Considering the influence that Patrick Geddes had on both British plant ecology and the development of an ecological approach to urban and regional planning, one might have expected more attention to human ecology in Britain. *The sociological review*, which included Geddes and his collaborator Victor Branford on its editorial board from its inception in 1908, occasionally published material which might be construed as human ecology, but the ecological perspective was treated only lightly in this journal and there is certainly no strong sense of an effort to pursue human ecology as a field in itself. The sense that I get from my general reading of more recent literature in the field, as well as from a brief conversation with Dorothy Porter, is that a school of human ecology centred on epidemiology and public health emerged in British medicine around mid century. Personal communication, Dorothy Porter, spring, 1991.

[16] G. F. Martin, *Ellsworth Huntington: His life and thought*, Hamden, CT, 1973, p. 143.

[17] Huntington–Shreve correspondence, January–March, 1917, Ellsworth Huntington Papers, Yale University Archives.

[18] Huntington, 'The control of pneumonia and influenza by the weather', *Ecology*, (1920) 1, pp. 6–23.

climatological data for New York and Boston during the recent epidemic was that the ideal atmospheric conditions for mitigating the effects of pneumonia and influenza were moderate, but variable, temperature and reasonably high humidity.[19] He had reached a similar conclusion in his general study, *Civilization and climate*, for which he gained wide recognition in the years just before America entered the World War. In that work he claimed that the ideal climatic conditions for civilization to flourish were a mean temperature range between 38° and 60°F (3–16°C) and considerable temperature variation over the course of the year and within seasons.[20] Not surprisingly, the climate of western and northern Europe (as well as much of north-eastern North America) matched Huntington's ideal conditions. The book included an appendix ranking 185 different countries or regions according to 'relative civilization'. The countries of western Europe topped the list, those of the Amazon region ranked at the bottom.[21]

Huntington did not shy away from bold generalizations. While working for military intelligence during the war, he wrote a very speculative companion piece to *Civilization and climate* in which he attributed the major steps in evolutionary progress to the stimulating effects of variable climate at the appropriate moments. Following this line of reasoning up through the evolution of human societies, he found a valuable lesson to be learned regarding the recent political situation. While Germany was not the most populous country, its people enjoyed the stimulation of one of the best climates in the world. This favourable climate had made Germany into such a powerful nation, despite its size, that the recognition of this natural cause of German superiority forced other nations into an inevitable policing role:

> Because the Germans are so strong and live in such a wonderfully favorable environment, the Allies should strive the more mightily to help Germany to set her house in order. For the moment this is the chief lesson to be learned from a study of climate and health.[22]

Huntington coupled his environmental determinism with an equally strident biological determinism (he later became a strong proponent of eugenics). In his correspondence with Shreve regarding the nature of a journal for the Ecological Society, he favoured the title '*Environment*', intended deliberately to complement the *Journal of heredity* published by advocates of eugenics. He suggested further that the ideal situation would be for subscribers to one journal to receive the other, the two together representing both sides

[19] Ibid., p. 18.

[20] Huntington, *Civilization and climate*, New Haven, 1915, p. 278.

[21] Ibid., pp. 299–310.

[22] Huntington, *World-power and evolution*, New Haven, 1919, p. 239. Although he submitted this manuscript to the press after the hostilities had ended, Huntington was compelled by his military superiors to modify some of the statements that cast the Germans in such a favourable light. Martin, op. cit. (16), pp. 154–5.

of a complete picture of the scientific study of organisms, including humans. However, neither the journal title nor the alliance with eugenicists was adopted by the Ecological Society.[23]

Huntington's work during the war led to an ambitious plan to promote human ecology. In his desire to join the war effort, he had approached the National Research Council, the agency created to oversee war-related research, with a proposal for a thorough study of the relationship between climate and mental and physical health, to be undertaken at various military training camps. Although some NRC members were impressed by the idea, the council decided in the end that Huntington's services would best be put to use in the Military Intelligence Division of the US Army, writing geographical monographs for regions in which military operations might take place.[24] This he accomplished in such an exemplary fashion that he was able to continue his relationship with the NRC long after the war. In 1921 Huntington persuaded the council to allow him to organize a Committee on Human Ecology. Since the council had already established a Nutrition Committee, which addressed some problems relating humans to their environments, Huntington's committee had to restrict its work to studies of the effects of atmosphere.[25] Even within this limited framework, Huntington soon discovered that the task was too large; a single committee could not possibly do justice to all the issues. The committee was to serve only an advisory capacity at this stage, with an aim toward parcelling out different pieces of the work to smaller committees.[26] Nevertheless, Huntington informed Barrington Moore, the editor of *Ecology*, that the object of the committee was 'to frame plans for the study of all phases of human ecology'.[27] Since Moore had inquired about the nature and composition of the committee, Huntington explained that his choice of committee members reflected his conviction that some of the most valuable material for human ecology is to be found in the records of life insurance companies, hospitals, and health organizations. Consequently, the committee included demographers, statisticians, three insurance company executives, a public health officer, a climatologist, an officer in the Carnegie Corporation, a physiological plant ecologist, and a psychologist.

The physiological plant ecologist, Burton Livingston of Johns Hopkins University, represented the only formal link on the committee to what might be called traditional ecological science. Livingston had been trained at the University of Chicago at the turn of the century under the formidable

[23] Huntington to Shreve, 5 February 1917; Shreve to Huntington, 13 February 1917, Ellsworth Huntington Papers.

[24] Martin, op. cit. (16), ch. 9.

[25] Huntington, 'Preliminary draft of report of Committee on Human Ecology', typescript, Ellsworth Huntington Papers.

[26] Huntington to B. Moore, 27 June 1921, Ellsworth Huntington Papers.

[27] Huntington to Moore, 15 August 1921, Ellsworth Huntington Papers.

duo of Charles R. Barnes and Henry C. Cowles in plant physiology and plant ecology, respectively. Before securing a position at Johns Hopkins, he worked as a researcher at the Carnegie Institution's Desert Botanical Laboratory at Tucson, Arizona, where he met Forrest Shreve and, most likely, Huntington as well.[28] When Huntington was assembling his Committee on Human Ecology, Livingston and Shreve had just completed a massive study of the relationship of plant distribution to climate in the US.[29] Through a combination of field and laboratory work Livingston had become an expert on the physiological effects of moisture and evaporation on plants, and he had invented instruments for recording minute changes in soil moisture and in rates of evaporation. It was likely this familiarity with the minutiae of atmospheric phenomena that led Huntington to invite him to serve on his committee. Perhaps he also thought that the name of a professional ecologist like Livingston might lend scientific respectability to a committee that used the word 'ecology' in its title. However, neither Livingston nor the name 'human ecology' remained with the committee for long.

Huntington's preliminary draft of the committee report conveyed the impression that studies of the effects of atmosphere represented the whole of human ecology. He summarized this view as follows:

> In a word, the great outstanding problem of human ecology is to discover how far the climatic and atmospheric conditions of man's environment influence his health, efficiency, rate of production, and degree of progress; how far they select certain types for preservation; how far they help in giving rise to new types; and how much of man's physical, mental, and social condition at any given time is due to his physical environment as distinguished from the other great factors of inheritance and training.[30]

As he presented it in this report, the theoretical side of human ecology thus represented a reflection of Huntington's personal research program. The report went on to propose, as a practical application of human ecology, the future adoption of large-scale climate-control measures in factories, offices, and homes, in order to provide invigorating indoor atmospheres at all times of the year and to mitigate the deleterious effects on the body and mind of a non-optimum climate. Huntington admitted, however, that human ecology was still at the stage of empirical observation. The next stage toward developing this field into a science would be to compile and analyse statistics regarding mortality, health, and birth rates and to correlate these with cli-

[28] C. A. Shull, 'Burton Edward Livingston, 1875–1948', *Science*, (1948) **107**, pp. 558–60; Livingston, 'Some conversational autobiographical notes on intellectual experiences and development: An auto-obituary', *Ecology*, (1948) **29**, pp. 227–41.

[29] B. E. Livingston and F. Shreve, *The distribution of vegetation in the United States, as related to climatic conditions*, Washington, 1921.

[30] Huntington, op. cit. (25), p. 3.

matic statistics. After making suggestions for carrying on this sort of project in cities and factories throughout the United States, the committee settled on the analysis of mortality statistics for New York City as the most manageable problem it could address in the immediate future.[31] Thus, having begun with the intention of addressing the whole field of human ecology, Huntington's committee first restricted itself to studying only the effects of climate and then restricted itself even further to examining climate and mortality in a single city. The committee remained in existence for nearly ten years but quickly changed its name to Committee on the Atmosphere and Man. In the final report, published in 1930, Huntington made no mention of human ecology.[32]

Although he had been one of the first to use the term, Huntington hardly referred to 'human ecology' at all after the early 1920s. At Moore's invitation he served on the editorial board of *Ecology* from 1920 to 1928, but he seems to have become disenchanted with the increasing narrowness and professionalism of ecology as a discipline and the continued dominance of the field by botanists. In 1937 he voiced this concern to then editor of *Ecology* Alfred Emerson, who had asked him to examine a report regarding the early work of the Ecological Society:

> Another point which I think should be emphasized in any report like this is that originally the Ecological Society was intended to include all phases of ecology, not only botany but zoology and human ecology. In thinking about the future of the Society the comparative neglect of animals and especially of man ought to be taken into account.[33]

Huntington's assessment was not off the mark. Although more attention was being given lately to animal ecology in the journal — Emerson was himself an animal ecologist, and a significant school of animal ecology was developing around his colleagues at the University of Chicago, especially W. C. Allee — human ecology remained a minor theme at most in the agenda of the society and in the pages of the journal. Even with its propensity to create committees on all sorts of topics, the Ecological Society of America did not form a committee on human ecology until 1955, and that initial attempt met with little success.[34]

Huntington had not been alone in his desire to promote human ecology in the early years of the society. Barrington Moore, editor of *Ecology* for the first twelve years of its existence (1920–1931), inaugurated the journal with a plea for a broad view of ecology, including human ecology. He considered

[31] Ibid., pp. 9–10.

[32] Huntington, 'Weather and health: A study of daily mortality in New York City', *Bulletin of the National Research Council, No. 75*, Washington, 1930.

[33] Huntington to Emerson, 15 November 1937, Ellsworth Huntington Papers.

[34] McIntosh, op. cit. (5), pp. 307–8.

ecology a point of view superimposed on the other sciences rather than a branch of any one science, and he argued that a number of fields having to do with aspects of human culture — agriculture, forestry, geography, public health, and even history — were essentially ecological in character.[35] Moore was a forester who had distinguished himself in the world war by leading a large unit of men charged with supplying the Allied troops with lumber from French forests. After resigning his commission, he returned to serve for two consecutive years (1919–1920) as president of the Ecological Society and to assume the editorship of the society's journal.[36] His successor as society president was the polymathic Illinois naturalist-ecologist Stephen A. Forbes. Forbes titled his 1921 presidential address 'The humanizing of ecology', and he made a strong case for ecology as the science which relates the other biological sciences to humankind. As H. G. Wells would do later, Forbes emphasized the close connection between ecology and economics: 'Ecology is, in short, of all the biological sciences, the humanistic science *par excellence*, and if we deprive it of its economic factor we take the economics out of all biology.'[37] Here Forbes, like Moore, was not so much arguing for the establishment of a separate human ecology as he was making the case that the applied (economic) aspects of ecology have a central, not a peripheral, place in the discipline: 'I would humanize ecology, therefore, first by taking the actions and relations of civilized man as fully into account in its definitions, divisions, and coordinations as those of any other kind of organism.'[38]

Despite a fair amount of research undertaken in applied aspects of ecology (especially agriculture, forestry, and range management), professional ecologists generally did not respond to the early pleas of Huntington, Moore, and Forbes to consider humans and their interactions as essential features of a fully integrated ecological science. Ecologists tended to view the activities and institutions of humans as lying outside the realm of 'natural' interrelationships. Human culture was something that interfered with nature. In the US, as elsewhere, there was considerable interest among ecologists in preserving large tracts of pristine nature for scientific study before they became unrecognizably altered by human interference. One of the first committees established by the Ecological Society of America was the Committee on the Preservation of Natural Conditions for Ecological Study under the vigilant leadership of the society's first president Victor Shelford.[39] The view that we

[35] B. Moore, 'The scope of ecology', *Ecology*, (1920) 1, pp. 3–5. This was Moore's presidential address to the Ecological Society, December 1919.

[36] *National cyclopaedia of American biography*, 1922, Vol. xviii, pp. 17–18.

[37] S. A. Forbes, 'The humanizing of ecology', *Ecology*, (1922) 3, p. 90.

[38] Ibid., p. 90.

[39] See the various reports of the committee in *Bulletin of the Ecological Society of America*, (1917) 1, nos. 4, 5, & 9. Perhaps nowhere was this movement stronger and more organized than in the Soviet Union, where huge nature reserves were created in the 1920s for the purpose of ecological study. See D. Weiner, *Models of nature: Ecology, conservation, and cultural revolution in Soviet Russia*, Bloomington, Indiana, 1988, especially ch. 6.

can only know 'nature' by studying it in its undisturbed state has come under criticism in recent years, but in the 1920s and 1930s it was fairly well entrenched among American ecologists.[40]

Another reason for the neglect of the human side of ecology among plant and animal ecologists was that human ecology was emerging as a special branch of the social sciences in the 1920s and 1930s. Considering Huntington's early embracing of the concept, geography might have seemed the likely field to produce a school of human ecology. The tradition of environmental human geography, or anthropo-geography, to which Huntington belonged could trace its roots to the nineteenth century, with the work of, among others, George Perkins Marsh, Élisée Reclus, and Friedrich Ratzel, continuing into the twentieth century with Jean Brunhes and Vidal de la Blache.[41] In the early 1920s, Americans, such as Huntington and the University of Chicago geographer Harlan Barrows, sought consciously to incorporate the new ecological science into this tradition. Moore had identified much of geography with human ecology in his 1919 presidential address to the Ecological Society,[42] and in 1922, as president of the Association of American Geographers, Barrows claimed that geography, as treated in many American universities, had become the subject that dealt exclusively with interactions between man and his environment. 'Thus defined', Barrows stated, 'geography is the science of *human ecology*.'[43] Barrows invited his colleagues to relinquish their association with such traditional subjects as physiography and climatology and to concentrate all their attention on human ecology, but his words may have had quite the opposite effect. In his exhaustive overview of human ecology Gerald Young argues that the often-cited Barrows address led geographers away from human ecology, for fear of being labelled as environmental determinists. This fear, according to Young, persisted into the 1970s and prevented geographers at first from taking advantage of the renewed interest in environmentalism.[44] Whether or not such widespread fear existed, at the University of Chicago, where Barrows was packing classrooms with immensely popular courses on 'The influence of geography on American history' and 'Conservation of natural resources', he certainly had a reputation for emphasizing environmental factors above all others in the understanding of human

[40] For a popular summary of the revisionist view, see D. B. Botkin, *Discordant harmonies: A new ecology for the twenty-first century*, New York, 1990.

[41] G. P. Marsh, *Man and nature*, New York, 1864; E. Reclus, *La terre*, 2 vols., Paris, 1868–69; F. Ratzel, *Anthropo-Geographie*, Stuttgart, 1882; J. Brunhes, *La géographie humaine*, Paris, 1912; P. Vidal de la Blache, *Principes de géographie humaine* (ed. E. de Martonne), Paris, 1921.

[42] Moore, op. cit. (35), p. 4.

[43] H. Barrows, 'Geography as human ecology', *Annals of the Association of American Geographers*, (1923) **13**, p. 3.

[44] G. Young, 'Human ecology as an interdisciplinary concept: A critical inquiry', *Advances in Ecological Research*, (1974) **8**, p. 33.

culture.[45] It is not clear that Barrows's 1922 address deserves all the credit, but human ecology did not become a major feature of academic geography in the United States during the middle third of this century.[46] It was within sociology, not geography, that human ecology first emerged as a significant conceptual framework and research program. That this occurred at Barrows's own institution probably owed little to Barrows or his department, since there was apparently only occasional contact between Chicago sociologists and geographers during this period.[47]

Human ecology as urban sociology

The reference to human ecology in the Park and Burgess sociology textbook occurred in a lengthy chapter on competition, in which the authors explored a number of analogies between human society and the non-human organic world.[48] There is considerably more biology in this chapter, and in this textbook, than one will find in most sociological literature. The Park and Burgess book was clearly the result of a conscious effort to ground a sociology on sound scientific principles without resorting to older evolutionary models or making use of simplistic organic analogies. Park's 1915 article, 'The city', is often cited as a forerunner of the ecological school of urban sociology that emerged at Chicago in the 1920s, but the article made no use of concepts from biological science and simply encouraged students to investigate urban phenomena in their entirety.[49] Park's very specific suggestions in this article no doubt inspired countless doctoral dissertations at Chicago and elsewhere, but his use of the concepts and terms of ecological science begins with the 1921 textbook. The earlier article was written not long after Park had come to Chicago, at the invitation of Albion Small, to lecture in the Sociology Department as a kind of white expert on black culture, since he had been working for several years as Director of Public Relations for Booker T. Washington's Tuskegee Institute.[50] Park, the philosopher turned newspaper reporter turned publicist, found himself at home

[45] C. D. Harris, 'Geography at Chicago in the 1930s and 1940s', *Annals of the Association of American Geographers*, (1979) 69, pp. 23–7; Harris mentions Barrows's popularity, but my assessment is also based on my review of enrolments for courses in 'Examiners' and instructors' reports', 1915–1930, University of Chicago Archives.

[46] A rare exception is the work of Carl Sauer at the University of California, but Sauer was a product of Barrow's department at Chicago. See Sauer, *Land and life* (ed. J. Leighly), Berkeley and Los Angeles, 1967.

[47] H. M. Mayer, 'Urban geography and Chicago in retrospect', *Annals of the Association of American Geographers*, (1979) 69, p. 115.

[48] Park and Burgess, op. cit. (12), ch. 8.

[49] R. Park, 'The city: Suggestions for the investigation of human behavior in the city environment', *American journal of sociology*, (1915) 20, pp. 577–612.

[50] Park, unpublished autobiographical sketch, 1929, Robert Ezra Park Papers, University of Chicago Archives.

in the new setting and began exploring possibilities for research with his students. The 1915 paper was a product of that initial encounter. Having come full circle from the cloistered world of academic philosophy (he studied at Harvard and obtained his PhD at Heidelberg) to the considerably more down-to-earth world of the reporter and the publicity agent and then back again to the academy, he began searching for fitting subjects for research in the vibrant and volatile city that surrounded the university. This was nothing new for Chicago sociologists, but earlier studies of urban phenomena were strongly motivated by the spirit of social reform that swept much of sociology in America before the First World War. Park, not unmoved by the social problems that beset the city, was looking for a more dispassionate, scientific approach to the study of human society. Compelled to share an office with Ernest Burgess, who returned to Chicago in 1916 (he had received his PhD there a few years before), Park spent long hours discussing with Burgess the needs of contemporary sociology. A very positive collaboration developed, one result of which was the 1921 textbook.[51]

It is not clear whether the references to ecology in the text came from Park or from Burgess. One of Park's biographers claims that shortly after Park arrived in Chicago he began talking to students about Ernst Haeckel, the German zoologist who had coined the word 'ecology' in 1866.[52] Park may have been familiar with Haeckel's work from his student days in Germany, but Haeckel contributed very little to the theoretical structure of the science whose name he coined. That structure began to emerge with the work of botanists in the late nineteenth century, and spread to zoologists in the early twentieth century.[53] At the University of Chicago, one of the early centres of the new science, there was an active plant ecology program under the direction of H. C. Cowles, and a burgeoning program in animal ecology. Victor Shelford and Charles Adams were products of Chicago's Zoology Department, as was W. C. Allee, who eventually returned to Chicago to establish an influential program in animal ecology.[54] Curiously, neither Park, nor Burgess, nor anyone else in sociology at Chicago seemed to have had much direct contact with the ecologists in their institution; their

[51] R. Faris, *Chicago sociology, 1920–1932*, San Francisco, 1967, pp. 39–40. There is an extensive literature on Chicago sociology during this period. See the excellent recent discussion in D. Ross, *The origins of American social science*, Cambridge and New York, 1991, chs. 9–10, especially pp. 357–67 and 432–40. Ross focuses on Park and cites all the significant sources.

[52] W. Raushenbush, *Robert E. Park: Biography of a sociologist*, Durham, North Carolina, 1979, p. 163.

[53] See E. Cittadino, 'Ecology and the professionalization of botany in the United States, 1890–1905', *Studies in history of biology*, (1980) 4, pp. 175–98.

[54] The best source for the origins of ecology at Chicago is G. A. Mitman, *The state of nature: Ecology, community, and American social thought, 1900–1950*, Chicago, 1992. Mitman's work focuses on animal ecology; I am preparing a paper on the plant ecology program under H. C. Cowles.

association with scientific work in ecology seems to have come entirely through reading the available literature.[55] The Park–Burgess text cited the work of Adams and William Morton Wheeler in animal ecology and Eugenius Warming and Frederic Clements in plant ecology. For the authors the chief insight which this work gave to sociologists had to do with the relationship between competition and co-operation in the formation of organic communities. Ecological science provided a kind of reinforcement for a view of society as the product of adjustment by competing elements. 'The ecological conception of society', they stated, 'is that of a society created by competitive co-operation.'[56] Ecologists had identified natural communities of plants and animals associated with particular environments as products of struggle and adjustment over time; human communities could be accounted for in similar fashion.

Park and Burgess did very little with this conception until Park began a correspondence with one of his former students, Roderick McKenzie, in the spring of 1924. McKenzie, who had been a colleague of Burgess on the faculty at Ohio State University, was now teaching at the University of Washington. He sent Park a paper on 'The ecological approach to the study of the human community', and asked for his reaction.[57] In three weeks Park sent McKenzie an enthusiastic reply, referring to the paper as 'first rate' and asking McKenzie's permission to publish it in the *American journal of sociology*. Park had only one criticism; he objected to McKenzie's use of the word 'adaptive'. His full comment reveals his attitude regarding the science of ecology and its application to sociology:

> The vine accommodates itself to the tree. Of course, the vine is also a product of adaptation, but what is inherited is the ability to accommodate itself. That is the point about the plant community; it is a product of accommodation and not adaptation, and is therefore not biological in the strict sense of the word; it is sociological. That is the justification for calling it a community.[58]

Insofar as it deals with communities of plants, then, and insofar as these communities can be viewed as products of accommodation, plant ecology is a social science. For Park, the term 'adaptive' implied that characteristics were transmitted by inheritance. He and Burgess had devoted a chapter in their textbook to explaining the distinction between adaptation and accommodation, pointing out that social organization arises by the adjustment of

[55] This is a judgement based upon my examination of the correspondence of Chicago sociologists and plant ecologists and on Gregg Mitman's examination of that of the zoologists. Mitman, op. cit. (54), p. 92; also personal communication.

[56] Park and Burgess, op. cit. (12), p. 558.

[57] McKenzie to Park, 24 May 1924, Robert Park Papers.

[58] Park to McKenzie, 14 June 1924, Robert Park Papers.

conflicting interests in an essentially competitive environment. This process of accommodation is most easily identified in the spatial structure and arrangement of communities, hence the value of comparisons with the work of plant ecologists.[59]

This emphasis on spatial relations became an essential element of human ecology as developed by sociologists. Having made the necessary changes in terminology, McKenzie offered the following definition in the published version of his paper, which appeared in the November issue of the journal:

> In the absence of any precedent let us tentatively define human ecology as a study of the spatial and temporal relations of human beings as affected by the selective, distributive, and accommodative forces of the environment. Human ecology is fundamentally interested in the effect of *position*, in both time and space, upon human institutions and human behavior.[60]

This was no small matter; spatial relations are as fundamental in the human community as in the plant community. 'As these spatial relationships change', McKenzie stated, 'the physical basis of social relations is altered, thereby producing social and political problems.'[61] Although he would later move away from this position, in this early paper McKenzie exploited the analogy with plant ecology for all it was worth:

> The plant ecologist is aware of the effect of struggle for space, food, and light upon the nature of a plant formation, but the sociologist has failed to recognize that the same processes of competition and accommodation are at work determining the size and ecological organization of the human community.[62]

Most of McKenzie's examples concerned changes in the structure and composition of human communities as the result of migration and technological innovation. He saw a clear analogy between these changes in human society and successional change in the natural world, as described so well by plant ecologists, notably Frederic Clements:[63]

> The structural growth of community takes place in successional sequence not unlike the successional stages in the development of the plant

[59] Park and Burgess, op. cit. (12), ch. 10. The authors made very little direct use of plant ecology in this text. Their discussion of accommodation relied heavily on the work of James Mark Baldwin, *Mental development of the child and the race*, New York, 1895, pp. 476–88; and Georg Simmel, *Soziologie*, Leipzig, 1908, pp. 330–36. Another source of the spatial dimension of social interactions was Simmel's concept of *social distance*, which Park and Burgess discuss in ch. 5. See Ross, op. cit. (51), pp. 432–3.

[60] R. D. McKenzie, 'The ecological approach to the study of the human community', *American journal of sociology*, (1924) 30, p. 288. [61] Ibid., p. 288. [62] Ibid., p. 288.

[63] McKenzie and virtually all of the sociological human ecologists made references to F. E.

> formation. Certain specialized forms of utilities and uses do not appear in the human community until a certain stage of development has been attained, just as the beech or pine forest is preceded by successional dominance of other plant species. And just as in plant communities successions are the products of invasion so also in the human community the formations, segregations, and associations that appear, constitute the outcome of a series of invasions.[64]

These 'invasions' involved the movement of people of one economic class, ethnic, or racial type from one area into another, and could be brought about by all sorts of factors, from 'changes in forms and routes of transportation' to 'the introduction of new types of industry' to 'real estate promotion'.[65] Just as in the plant world, the end result of this succession of invasions is the establishment of a stable (climax) community in an area. This might take the form of a similarity in residential structures and in the economic make-up of the residents in a given region of a city. Generalized over the entire city, or a large portion of the city, this process would result in the establishment of a mosaic consisting of many such 'natural areas', just as one finds in the plant world.[66]

Earlier in 1924 Burgess had published an article on 'The growth of the city', making a similar point but with only scant reference to the science of ecology. Using Chicago as his model, Burgess argued that as modern cities expand in area and population, they become differentiated into specific regions roughly equivalent to zones of concentric rings, from the central business and manufacturing districts to the higher class residential districts far removed from the zone of congestion and deterioration surrounding the core. Often there is segregation within a particular zone according to ethnic group. The poorest newcomers 'invade' the city at its core, take the lowest paying jobs, and live in the worst housing. As each new group betters its conditions, it moves away from the centre, 'invading' neighbourhoods in the outer zones and forcing the residents of those zones still further out. The worst social problems in the city — crime, juvenile delinquency, wife desertion, youth gangs — are associated with the zones of greatest mobility.[67] Burgess's somewhat crude treatment of urban zonation was nevertheless a way of making sense of the bewildering array of problems that plagued the nation's cities as America went through the transition from a rural to an urban society. This article, coupled with McKenzie's, formed the theoretical core of the school of human ecology that for a time in the 1920s and 1930s

Clements, *Plant succession*, Washington, 1916, which included numerous examples of the orderly replacement of plant communities over time and an elaborate description of the process.

[64] McKenzie, op. cit. (60), p. 297. [65] Ibid., p. 298. [66] Ibid., p. 300.

[67] E. W. Burgess, 'The growth of the city: An introduction to a research project', *Publications of the American Sociological Society*, (1924) 18, pp. 85–97.

became one of the dominant features of Chicago sociology, and influenced several other institutions mainly through hiring of Chicago graduates.

By the summer of 1924, Park and McKenzie were already discussing plans to co-author a book on human ecology.[68] McKenzie taught a course on the subject that summer at the University of Washington, probably the first such course in the United States.[69] In November Park asked McKenzie to come to Chicago and offer the same course the following summer.[70] Meanwhile, Park included the McKenzie and Burgess papers, along with several of his own, in a book on the city which became a sort of unofficial bible of the Chicago school.[71] Park also set up a 'Division on human ecology' for the December 1925 meeting of the American Sociological Society, with McKenzie in charge. The first session (there were several more such sessions on human ecology at the annual meetings between the mid 1920s and the mid 1930s) included a paper on 'The natural areas of the city' by Harvey Zorbaugh and one by McKenzie on 'The scope of human ecology'.[72] McKenzie's paper repeated some of the themes of his earlier paper on human ecology, but he took pains to demarcate the territory of the new research field, especially to set it off from economics and geography. The human ecologist studies some of the same problems as the economist, he stated, but he does so 'in relation to the processes of human distribution', whereas the economist studies these problems from the point of view of the needs and practices of business. Regarding the relationship between human ecology and geography, McKenzie cited Barrows's address on the identity of the two fields and then proceeded to make a distinction: 'Location as a geographical concept, signifies position on the earth's surface; location as an ecological concept signifies position in a spatial grouping of interacting human beings or of interrelated human institutions.'[73] Although he did not go so far as to state this explicitly, he strongly implied that human ecology is a field to be cultivated properly only by sociologists.

The career of human ecology was launched, or so it seemed. McKenzie taught the first course on human ecology at the University of Chicago, as planned, in the summer of 1925. Park taught the course the following year and twice thereafter before he retired in 1932.[74] Burgess taught human ecology as well and incorporated ecological themes into his courses on urban sociology.[75] He retired in 1951. McKenzie left the University of Washington

[68] McKenzie to Park, 19 July 1924; Park to McKenzie, 1 August 1924, Robert Park Papers.

[69] Raushenbush, op. cit. (55), p. 159.

[70] Park to McKenzie, 21 November 1924, Robert Park Papers.

[71] R. E. Park, E. W. Burgess, and R. D. McKenzie, *The city*, Chicago, 1925.

[72] Zorbaugh, 'The natural areas of the city', *Publications of the American Sociological Society*, (1926) 20, pp. 188–97; McKenzie, 'The scope of human ecology', Ibid., pp. 141–54.

[73] McKenzie, op. cit. (75), p. 142.

[74] Raushenbush, op. cit. (55), p. 160.

[75] My examination of Burgess's course notes for 'Urban sociology' and 'The growth of the city' indicates that Burgess borrowed most of his material on human ecology wholesale from

in 1930 to head the new Department of Sociology at the University of Michigan. Through the courses offered at Chicago, Washington, and Michigan, scores of students were exposed to the concepts and methods of human ecology. Many of these students went on to take positions in sociology at institutions throughout the country. Others, such as James Quinn of the University of Cincinnati and A. B. Hollingshead of Indiana University, were influenced indirectly by the Chicago sociologists.[76] No real leaders emerged from this second generation, with the possible exception of Amos Hawley, one of McKenzie's students at the University of Michigan. McKenzie's new position left him little time to pursue his research, including the book on human ecology that he and Park had planned to write. The two met twice in the 1930s to work on the manuscript, but conflicting interests, problems with the publisher, and McKenzie's advancing illness prevented them from making much headway.[77] When McKenzie died in 1940, the task of writing the book was left to Hawley, who completed his work a decade later. Hawley's book offered a fairly comprehensive summary of sociological work in human ecology over the previous three decades and included an admirable, if somewhat unsophisticated, attempt to review the fundamental background material in general ecology.[78]

By mid century there was an impressive quantity of published work in human ecology. An examination of the references in Hawley's book, along with a bibliography compiled by Quinn a decade earlier, reveals hundreds of papers in the field.[79] The quantity of the work, however, did not reflect an equally impressive quality. Much of it was essentially descriptive in nature, a large quantity of it was simply urban sociology that owed very little to the science of ecology. Most of the studies focused on what McKenzie called 'ecological organization', that is, the division of a region into 'natural areas' representing distinct cultural units, or on ecological succession. Burgess's notion of zonation into concentric rings was treated as an established doctrine and repeated in countless examples. There was some attention also to the concept of dominance, the integration and control of a region by a

McKenzie's published works. Unpublished course notes, Ernest W. Burgess Papers, University of Chicago Archives.

[76] Quinn was trained by E. E. Eubank, who took his doctorate at Chicago in 1915. Hollingshead, who began a correspondence with Park while still a student in Alabama, was invited by Park to write the chapter on 'Human ecology' for his 1939 sociology text. J. A. Quinn, *Human ecology*, New York, 1950, dedication to Eubank, McKenzie, and Park; Raushenbush, op. cit. (52), p. 162; Hollingshead, 'Human Ecology', in *Outline of the principles of sociology* (ed. R. Park), New York, 1939, pp. 63–168.

[77] Raushenbush, op. cit. (52), pp. 161–2.

[78] A. H. Hawley, *Human ecology: A theory of community structure*, New York, 1950.

[79] J. A. Quinn, 'Topical summary of current literature on human ecology', *American journal of sociology*, (1940) **46**, pp. 191–226. Quinn cited 347 separate titles.

dominant centre. McKenzie borrowed this idea not from the plant ecologists, his usual source, but from the work of Chicago physiologist Charles Manning Child. Child had argued that human groups exhibit spatial patterns of organization characterized by regions of dominance and subordination similar to the spatial differentiation and integration of individual cells and organisms.[80] McKenzie first used the concept in a very speculative paper in which he suggested that the old Western method of control of frontier regions by a few imperial centres was being replaced by a multiplicity of regional centres as the Western views penetrated the non-Western world.[81] Thereafter the concept was tossed around a lot by sociologists but seldom employed with fruitful results. Succession received considerable attention by sociologists, but here again, after some ingenious work by McKenzie, the concept was not explored in depth in its application to human society.[82] Urban and regional sociologists simply attached the word 'succession' to any pattern of community change over time.

The first two decades or so of sociological work in human ecology left the field pretty much where it had begun in the mid 1920s. This was the judgement of Hawley, who tried to sort out some of the theoretical issues in a pithy, incisive paper published in 1944. For Hawley, the 'ingenuousness, simplicity, and utility of the early definitive statement' of the field (i.e., in the work of McKenzie and Park) nevertheless did not keep followers from accepting these early studies as dogma rather than suggestions for further investigation. The result was a large outpouring of substandard work:

> Subsequent work in the field, with very few exceptions, was not aimed at exploring the full implication of ecology as applied to man. Instead there was a wholesale application of a little understood point of view and in consequence the theoretical development of the discipline received scarcely any attention. In fact, most so-called ecological studies have been occupied with incidentals and by-products of the approach, and not a few are totally irrelevant to the caption under which they appear in print.[83]

Hawley was particularly bothered by the tendency to attach the label 'human ecology' to any loose treatment of competition in human society or any mapping of human communities. He thought that critics such as Alihan, who focused most of their attention on the misapplication of biological

[80] C. M. Child, *Physiological foundations of behavior*, New York, 1924, ch. 16.

[81] McKenzie, 'The concept of dominance and world organization', *American journal of sociology*, (1927) **33**, pp. 28–42.

[82] See McKenzie, 'Ecological succession in the Puget Sound region', *Publications of the American Sociological Society*, (1929) **23**, pp. 60–80. Although McKenzie revealed here an impressive complexity of temporal changes, his paper made little use of ecological theory.

[83] Hawley, 'Ecology and human ecology', *Social forces*, (1944) **22**, p. 399.

concepts to human society, were missing the point. The problem was that many sociologists were making use only of those aspects of ecology which they believed were distinctly biological in order to give their work scientific respectability. Had they understood from the beginning that ecology is basically a social science, Hawley insisted, they would have exploited the full range of concepts and methods within ecological science.[84] Hawley did not believe that these concepts and methods could be employed without modification in investigations of human social phenomena. Since human culture adds a new dimension that is not found in communities of plants and animals, there is a necessity for a distinctly human ecology apart from general ecology. McKenzie and Park had certainly recognized this point. After initially pushing the analogies between general ecology and human ecology as far as he could, McKenzie stated in 1931 that human ecology 'has but little in common with plant and animal ecology', since humans, as cultural animals, are able to modify their environments to some extent.[85] Neither McKenzie nor Hawley wished to carry this idea too far, however, and to disavow any connection with general ecology. As Park stated the matter, 'The cultural superstructure imposes itself as an instrument of direction and control upon the biotic substructure.'[86] Moving away from McKenzie's narrow focus on spatial relations, Hawley defined human ecology more broadly (if vaguely) as 'the study of the development and the form of communal structure as it occurs in varying environmental contexts'.[87] Since human ecology, thus defined, overlaps with most fields within the social sciences, Hawley added, 'human ecology might well be regarded as the basic social science'.[88]

Basic or not, one might well ask how it is that human ecologists could find a meaningful and productive path through this slippery conceptual turf. This was Alihan's central point in her somewhat overwrought book-length critique of the entire enterprise. She argued that most sociologists who wrote on human ecology were unsure as to whether they were doing biological or social science, and she was troubled by the confusing and inconsistent manner in which they employed concepts like 'community', 'dominance', 'competition', and 'natural area', transferring them back and forth irresponsibly from biology to sociology.[89] Her book seems to have initiated a wave of criticisms and re-evaluations of human ecology from within the sociological community.[90] Some critics simply attacked specific aspects of human eco-

[84] Ibid., pp. 399–400.

[85] McKenzie, 'Ecology, human', *Encyclopaedia of the social sciences* (ed. E. Seligman), New York, 1931, Vol. VII, p. 314.

[86] R. Park, 'Human ecology', *American journal of sociology*, (1936) 42, p. 15.

[87] Hawley, op. cit. (83), p. 404. [88] Ibid., p. 405. [89] Alihan, op. cit. (3).

[90] Young, op. cit. (10), pp. 94–5, claims that the Alihan book may have turned an entire generation of sociologists away from human ecology and back to the Comtian view of sociology sitting atop the epistemological hierarchy.

logy, such as Burgess's model of urban growth or the concept of natural areas.[91] Others, like Hawley in his 1944 paper, attempted to re-examine the conceptual framework of the entire field. There was little agreement. Hollingshead, for example, called for human ecologists to emphasize more strongly the socio-cultural framework of all human activities. Quinn, on the other hand, wanted to limit human ecology to a study of the sub-social elements of human existence.[92] W. E. Gettys of the University of Texas complained about the inconsistent use of terminology and made an observation with which no one would have disagreed: 'So far the human ecologists have provided no integrated, coherent, consistent, and verifiable synthesis.'[93]

Hawley would have been the first to admit that his 1950 text did not provide such a synthesis, although he began the preface by stating, 'This volume attempts to develop a full and coherent theory of human ecology.' In the next paragraph Hawley acknowledged that the book does not achieve this goal, and he referred to human ecology as 'still a fledgling discipline, if indeed it may be called a discipline'.[94] In 1986, with the benefit of the conceptual structure of ecosystem ecology, Hawley attempted another synthesis, but again he admitted that despite having 'speculated on the feasibility of a unified theory of human ecology' for a number of years, his present effort fell far short of that goal.[95] The 1950 text at least represented a synthesis of the McKenzie–Park–Burgess school of human ecology, with all its shortcomings, and Hawley made a very conscientious effort to relate this sociological school to its roots in ecological science.

Human ecology and the ecologists, 1935–55

Hawley's was not the first attempt at a synthesis of human ecology. In 1935 J. W. Bews, a British plant ecologist who had spent most of his career in South Africa, published a very sweeping overview of the entire field. In a single volume he touched on mental testing, eugenics, the ecology of ancient man, epidemiology, Gestalt psychology, the domestication of plants and animals, regional sociology, an ecological explanation for class structure, and much more.[96] Bews was very much influenced by the holistic thinking of General J.C. Smuts, who wrote an introduction for the book, and by Frederic Clements's holistic approach to plant ecology. Ecology for Bews

[91] See, for example, P. Hatt, 'The concept of natural area', *American sociological review*, (1946) 11, pp. 423–7.

[92] A. B. Hollingshead, 'A re-examination of ecological theory', *Sociology and social research*, (1947) 31, pp. 194–204; J. A. Quinn, 'The nature of human ecology — A reexamination and redefinition', *Social forces*, (1939) 18, pp. 161–8.

[93] W. E. Gettys, 'Human ecology and social theory', *Social forces*, (1940) 18, pp. 472–3.

[94] Hawley, op. cit. (78), p. v.

[95] Hawley, *Human ecology: A theoretical essay*, Chicago and London, 1986, p. vii.

[96] J. W. Bews, *Human ecology*, London, 1935.

offered more than a method with which to investigate aspects of human society; it offered a synthetic vision and, as such, a basis for normative judgement:

> As a view-point, human ecology obviously has an important bearing on human conduct, on the kind of question that is vitally interesting to every one of us, how to make the most of our lives and how to live happily. It supplies a working philosophy of life and conduct.[97]

Bews counted among his predecessors in human ecology nineteenth-century French regional sociologist Frédéric Le Play and his Scottish disciple Patrick Geddes. He also acknowledged more recent British demographic and survey work. The only mention in his book of the work of American sociologists came in a few bibliographic references in the final chapter. Throughout the text, however, Bews cited studies by American plant and animal ecologists along with much of the work of Ellsworth Huntington. The book received very little attention. There was an occasional reference to it in American human ecology studies, but for the most part it was ignored by sociologists. Although he had been a frequent contributor to the *Journal of ecology*, the journal chose not to review his book. Across the Atlantic, it was reviewed in *Ecology* by Edward Haskell, who found it 'an encyclopedic compilation of the more elementary systematized knowledge of man–environment interactions interspersed with merely stated problems and unanswered questions . . .'[98] Although he recognized the need for an ecological approach to the study of human problems, Haskell concluded that 'the enormous scope of this medium sized book inevitably precludes the realization of any of its objectives'.[99]

What had become of the nascent interest in human ecology among American plant and animal ecologists? Very little, but the severe economic conditions of the 1930s helped revive discussions. When asked to contribute to the 1935 Festschrift honouring Henry Cowles, Charles Adams responded with a paper on 'The Relation of General Ecology to Human Ecology'. Adams quoted H. G. Wells at length regarding the close relationship between economics and ecology and also the importance of human ecology in education, but what he had to say about the application of ecological science to the human situation was not very illuminating. He offered a cursory listing of examples of social surveys, mentioned briefly the sociological work of McKenzie, Park, and Burgess, and then admitted a basic problem with the entire enterprise: 'Advance in general ecology is now a limiting factor retarding the advance of human ecology.'[100] He called for greater emphasis on the dynamic or process point of view and stated that the central contribution

[97] Ibid., p. 278.
[98] E. F. Haskell, 'Ecology in the study of men', *Ecology*, (1937) 18, p. 442.
[99] Ibid., p. 443.
[100] Adams, op. cit. (2), p. 331.

of ecology to human affairs at the present time is the recognition that the stresses and pressures of human relations are the result of the limitations and unequal distribution of the earth's resources. Human history, then, becomes a record of the responses of human communities to environmentally-induced stresses, paralleling somewhat the responses of communities of plants and animals to environmental changes in the process of ecological succession. Studies of such 'social successions', as he called them, could benefit from a knowledge of ecological succession, as soon as the ecologists refine their own theories. Then the 'neglected borderland between general ecology and human ecology' could be cultivated.[101]

Three years later, when Adams published his brief note in *Ecology* regarding this still neglected borderland, he again stressed the need for the full integration of plant and animal ecology with human ecology.[102] As an example of the sort of study that he had in mind, he referred to a recent publication by Australian geographer Griffith Taylor concerning an environmental theory of racial differentiation and diffusion. Taylor had earlier impressed Ellsworth Huntington, who singled him out as the sort of human ecologist whom the Ecological Society of America ought to invite for membership.[103] In the work referred to by Adams, Taylor had applied to human races a model originally introduced for mammalian evolution in which the more primitive types are driven by the more advanced to the periphery of their zones of distribution. Since the zone of distribution for a given species shifts with environmental changes, these patterns of differentiation and diffusion must be understood against the background of changing environmental conditions, climate being the most important.[104] Adams thought that this model could be applied to examples of succession and diffusion in human ecology and need not be limited to racial differentiation. Griffith Taylor's paper was also mentioned by Walter Taylor in his 1935 presidential address to the Ecological Society of America. The latter Taylor made a strong plea for the utility of ecological science, emphasizing its application to conservation issues, but he thought that the social sciences would also benefit greatly from an ecological perspective. He asked, 'What, after all, is more reasonable than the ecological approach to anthropology, economics, sociology, and ethics?'[105]

Taylor offered few particulars as to how ecology might be incorporated into the social sciences. His central concern was with the application of ecology to such matters as land-use planning, range management, soil erosion

[101] Ibid., pp. 331–2.

[102] Adams, op. cit. (1), p. 502.

[103] Huntington to F. Shreve, 19 January 1917, Ellsworth Huntington Papers.

[104] G. Taylor, 'The ecological basis of anthropology', *Ecology*, (1934), pp. 223–42. The theory also considered the parallel between vertical stratification, representing the historical sequence of occupations in a region, and spatial distribution.

[105] W. Taylor, 'What is ecology and what good is it?', *Ecology*, (1936), p. 343.

studies, resettlement projects, reforestation, and wildlife management. As a member of the US Biological Survey in the Southwest, these were the issues that Taylor saw as most significant. He was not alone of course. In 1935 Paul Sears, a plant ecologist at the University of Oklahoma, published *Deserts on the march*, a general indictment of wasteful and ignorant land-use practices and an urgent appeal for the broad dissemination and use of scientific knowledge regarding conservation.[106] A major concern of Sears, Taylor, and many ecologists in the interior was the prevention of future dust storms of the kind that had wreaked such havoc during the drought years of the mid 1930s. Frederic Clements, who had devoted most of his career to creating a theoretical structure for plant ecology, used the opportunity of the Cowles Festschrift to make a modest contribution to the human side of ecology by offering concrete suggestions regarding the solution of soil erosion problems.[107] There was some question as to whether the broad application of ecological principles to such problems could have prevented the disastrous storms, but there was no serious doubt among ecologists that this was an issue to which their science needed to respond.[108]

Ecologists continued to be of two minds regarding how they would 'humanize' their discipline. At times they talked only of practical applications of ecological knowledge to problems regarding the management of land and resources. At other times they seemed to promote a general ecological perspective as fundamental to social planning of all types. In the closing pages of *Deserts on the march* Sears made the bold suggestion that all sizeable municipalities hire a professional ecologist who could advise local officials in matters of planning and also contribute toward setting policy at the state and national levels.[109] It is not clear how one person would deal with the diversity of problems that even a small city might encounter, but Sears likely thought that one ecological expert is better than none. His suggestion seemed to take both senses of the application of ecology to human affairs into account. In 1939 Sears again addressed the matter of the social function of ecology in a brief popular treatise titled *Life and environment*, written when he took leave of his Oklahoma position to spend two years as a research associate at Teachers College, Columbia University. After listing a number of direct applications of ecology to assessing the water economy of a given region, Sears added another, more general, social function: '. . . to point out the laws of community development and behavior in such way that they may be applied not only within the human community but to the

[106] P. Sears, *Deserts on the march*, Norman, Oklahoma, 1935.

[107] F. E. Clements, 'Experimental ecology in the public service', *Ecology*, (1935) **16**, pp. 342–63.

[108] See D. Worster, *Nature's economy: A history of ecological ideas*, Cambridge, 1985, ch. 12; and R. Tobey, *Saving the prairies: The life cycle of the founding school of American plant ecology, 1895–1955*, Berkeley, 1981, ch. 7.

[109] Sears, op. cit. (106), pp. 228–30.

wider community of living things with which man is integrated, and whose control he has assumed'.[110]

Sociologists who were experiencing the difficulties of applying even the simplest of ecological concepts to the human community must have been disappointed that Sears did not go on to explain these universal laws of development. Sears, at least, was making an effort toward including human social organization within the domain of ecology, even if this effort amounted to nothing more than vague suggestions. In the same year in which that book appeared Adams passed on to Sears a letter he had received from an economist in the Works Progress Administration expressing disappointment that ecologists took such little interest in the human dimension of their field. He asked Adams whether he and Sears were not 'two boys crying in the wilderness of Ecology'.[111] Adams replied that advances are slow in science; one should not expect too much too soon. 'If all the ecologists had turned human ecologists', he said, 'we would not have a general foundation for the whole field today. The human must be built upon the broader foundation.'[112] Even as he wrote this reply, however, Adams was making plans to promote greater discussion of human ecology among his colleagues. The Ecological Society, responding to the growing interest in the application of ecology to human affairs and also to a suggestion from the American Association for the Advancement of Science, had begun arranging joint symposia with the AAAS on topics that brought social scientists and ecologists together in a common forum. Under the leadership of Sears the symposium committee chose as its first three topics wildlife management, land-use planning, and the biological and social effects of fire. The fourth symposium in the series, held in December, 1939, was titled 'On the relation of ecology to human welfare — The human situation', with Adams presiding.

Sears and Adams invited Burgess and McKenzie to participate, but Burgess had conflicting commitments and McKenzie declined the invitation due to illness.[113] The final panel included plant ecologist H. L. Shantz, climatologist C. W. Thornthwaite, Benton Mackaye of the Forest Service, sociologist A. B. Hollingshead, and Eduard Lindeman of the New York School of Social Work. All of the papers were published in an issue of *Ecological monographs* the following year.[114] The range of the discussion indicates the lack of

[110] P. Sears, *Life and environment: The interrelations of living things*, New York, 1939, p. 147.

[111] B. L. Melvin to C. Adams, 25 January 1939, included with Adams's letter to Sears, 14 February 1939, in Paul Bigelow Sears Papers, Manuscripts and Archives, Yale University Library.

[112] Adams to Melvin, 31 January 1939, in Paul Sears Papers.

[113] Adams to Sears, 28 March 1939; R. D. McKenzie to Sears, 14 April 1939, Paul Sears Papers.

[114] 'Symposium on relation of ecology to human welfare — The human situation', *Ecological monographs* (1940) 10, pp. 307–72.

consensus regarding the nature and scope of human ecology. Shantz thought that the main value of plant ecology to human ecology would be to indicate the general characteristics of the plant communities on whose productivity human communities must depend. Beyond that he suggested some very simple analogies between plant and human community structure.[115] Thornthwaite's brief paper stressed the synthetic and derivative nature of both plant and human ecology. 'Plant ecology is not a subdivision of botany', he argued, 'but is a synthetic science consisting of parts of botany, climatology, pedology [soil science], physiography, agronomy, and doubtless other fields of study.' Similarly, 'Human ecology can no more be a subdivision of sociology and anthropology than plant ecology can be a subdivision of botany.' Against Barrows's claim that human ecology is the subject matter of geography, Thornthwaite objected that human ecology is a more fully integrated field, 'since it embraces other social sciences, the subject matter of which has never been claimed by even the most aggressive and ambitious of the geographers'.[116] He failed to mention that the field was integrated only in theory. Mackaye, the most pragmatic of the panellists, steered away from discussions of disciplinary boundaries and simply focused on the nuts and bolts of regional planning, defining the task of human ecology as that of fitting human communities optimally into their natural environments. For Mackaye, the human ecologist was a kind of social engineer.[117] Hollingshead, here as in his other writings, pointed out the difficulties involved with applying general ecological concepts to the human situation and emphasized the predominance of social values over the purely biological. The central difficulty with any ecological approach to understanding human society, he argued, is that the human social order so often encourages practices that unwittingly subvert the ecological order.[118] Lindeman simply offered the suggestion that ecology is the mediating influence between particular scientific specialities and social planning, and he made an appeal for including philosophers to help determine the ends for which the planners furnish the means.[119] Regardless of the problems with the use of the term and its application, all of the panel members seemed to accept the general potential of human ecology as both a useful planning perspective and a synthesis of the natural and human sciences. Only Hollingshead seemed fully to appreciate the complexity involved with that synthesis.

Adams continued to promote discussion of human ecology within the

[115] H. L. Shantz, 'The relation of plant ecology to human welfare', ibid., pp. 311–42.

[116] C. W. Thornthwaite, 'The relation of geography to human ecology', in 'Symposium', op. cit. (114), p. 347.

[117] B. MacKaye, 'Regional planning and ecology', in 'Symposium', op. cit. (114), pp. 349–53.

[118] A. B. Hollingshead, 'Human ecology and human society', in 'Symposium', op. cit. (114), p. 362.

[119] E. C. Lindeman, 'Ecology: An instrument for the integration of science and philosophy', in 'Symposium', op. cit. (114), pp. 367–72.

Ecological Society and to encourage co-operation between the society and Section K, Social and economic sciences, of the American Association for the Advancement of Science. In 1940 he wrote the Secretary of Section K, E. P. Hutchinson, regarding these issues: 'We are endeavoring to get enough members together who are interested in human ecology, to have a strong nucleus in the Ecological Society, and who will keep programs going in that field.'[120] This campaign met with only limited success, to some extent because it was interrupted by the war but also because professional ecologists remained somewhat sceptical of the value of human ecology and its relation to their work. Adams soon retired from his position at the New York State Museum but kept up his correspondence from his home in Albany, on stationery which listed his areas of interest as 'General ecology, human ecology, conservation, and land use', in that order. Perhaps indicative of the attitude of ecologists toward human ecology was the response of botanist Norman Taylor, a former member of the editorial board of *Ecology*. On receiving a letter from Adams on the new stationery, Taylor chided his friend for including human ecology among his interests. He reiterated his low opinion of 'that science' (apparently the two had discussed this matter before), and he told Adams he was glad to see that some public health people had taken it up, thus placing the field 'where it properly belongs, not among a lot of biologists and zoologists'.[121]

Taylor's reference to public health is unclear, but he was not alone in desiring to place human ecology outside the boundaries of ecology proper. In 1948, in response to complaints regarding 'the hypercritical editorial policy' of *Ecology* toward articles on human ecology, the editors conducted a poll of the twelve-member editorial board and the officers of the Ecological Society. The consensus was that 'superior' articles on human ecology should be published, particularly if they concern natural rather than social interrelationships. As one (unnamed) member of the editorial board stated it. 'I should like to see articles in *Ecology* on the effects of Man on the natural community or the effect of the natural community on Man as an animal (not Man as a member of a social organization).'[122] The policy adopted by the editors ran along similar lines:

> Papers dealing mainly with the economic relationships between Man and Man, do not, at present at least, seem to belong in our journal; they should find a more congenial audience in journals of sociology, anthropology, economics, or geography than in our own. As a result of this

[120] Adams to Hutchinson, 1 February 1940, included with Adams's letter to Sears, 1 February 1940, Paul Sears Papers.

[121] N. Taylor to C. Adams, 13 June 1949, Norman Taylor Papers, Library of the New York Botanical Garden, Archives, The Bronx, New York.

[122] Quoted by D. B. Lawrence and T. Park, in 'Report of the editors of *Ecology*', *Bulletin of the Ecological Society of America*, (1949) 30, p. 10.

> advice, the following point of view has been adopted by the editors regarding manuscripts in human ecology: (a) They must make an intellectual contribution to ecology, (b) They must pass all the usual editorial hurdles that confront any manuscript, (c) They must not be propagandistic.[123]

There was no disguising the implication that much of what passed for human ecology was unscientific, non-rigorous, and biased. The editorial policy, as it was stated, seemed to underwrite the notion that the social dimension of human existence had no place in ecological science, perhaps no place in science. It is not clear to what extent the general membership of the Ecological Society shared this view, but it would be safe to say that many professional ecologists shared with other natural scientists the general scepticism regarding the social sciences that led to their exclusion from the National Science Foundation around this same time.[124]

When sociologists Quinn and Hawley published their respective books on human ecology in 1950, they were reviewed in *Ecology* by Yale population ecologist Edward Deevey. The Quinn book was intended as an introductory text, and Deevey quickly dismissed it as narrow in its viewpoint and superficial regarding ecological concepts and principles. He had better things to say for Hawley's book. He praised Hawley for the amount of attention he gave to plant and animal ecology in the early chapters, for his appreciation of the value of demographic studies, and for the general seriousness of his endeavour. Deevey made his views regarding sociology known, when he characterized Hawley's chapter on population balance as 'refreshingly free from the parochialism of U.S. sociology'.[125] However, he found Hawley's treatment of ecological organization in the human community confusing and he thought that his handling of many themes lacked sufficient rigour, particularly, where applicable, mathematical rigour: 'Training for rigorous mathematical thinking is largely being left out of the education of sociologists, if we may judge by Hawley's and Quinn's texts.'[126] In any event, the Hawley text seems to have had little impact on ecologists. In an assessment of the state of human ecology in 1954, Sears, now directing a new conservation program at Yale, thought that most ecologists would dismiss Hawley's book as 'an intricate study of modern institutions. . .'[127] Despite Hawley's desire for completeness and balance, the book was still a sociological treatise

[123] Ibid., p. 10.

[124] See D. Kevles, *The physicists: The history of a scientific community in modern America*, New York, 1979, chs. 21–22. Kevles focuses on Vannevar Bush, but a general sympathy among natural scientists with Bush's disdain for the social sciences certainly helped to promote his position.

[125] E. S. Deevey, Jr, 'Recent textbooks of human ecology', *Ecology*, (1951) 32, p.348.

[126] Ibid., p. 349.

[127] P. Sears, 'Human ecology: A problem in synthesis', *Science*, (1954) 120, p. 961.

written mainly for sociologists. Sears wanted human ecology to be more than that, but he had given up attempting to define the field. 'Human ecology is not so much a specialty', he stated, 'as a scientific activity which must draw upon a wide range of the specialties. What is important is the work to be done rather than the label.'[128] Judging by the remainder of Sears's article 'the work to be done' for human ecology included solving most of the significant social and economic problems of the civilized world. For Sears a fuller incorporation of the ecological perspective into science teaching and into education in general would have far-reaching consequences for contemporary society. Whether it be water pollution, soil erosion, overpopulation, resource limitations, or juvenile delinquency (largely a problem of 'the availability and distribution of space'), ecology can provide the kinds of insights that lead to solutions.[129]

The facile suggestion that the solution to the problem of juvenile delinquency could be found in an increase in the per-capita recreational space in large cities may strike the reader as somewhat naïve, but there was a strong faith, now very familiar to us, behind Sears's remarks. If only ecology received the recognition that it deserved, Sears seemed to be saying, human ecology would surely emerge as the synthetic field that will save us from ourselves. Sears carried this faith with him to the meetings of the Ecological Society's first Committee on Human Ecology, established in 1955. The committee organized a symposium for the following summer, and judging by the abstracts reprinted in the society's *Bulletin*, at least one of the participants shared Sears's faith. Yale geographer Albert Burke summarized his perspective on human ecology as follows: 'Many problems characteristic of present day American culture ranging from a rising incidence of mental illness to failures of policies and programs in international relations are in large part the result of ignorance of the ecological process in human affairs.'[130] The abstract goes on to indict the Judaeo-Christian tradition and the development of modern science as 'a weapon for conquer and control' as the two causes most responsible for our nearly wholesale neglect of ecological knowledge. This sounds very much like the message of the well-known Lynn White article published over a decade later.[131] Such dramatic statements apparently were not sufficient to make human ecology a central concern of the Ecological Society. No further symposia were held. The committee struggled through another year or so and was 'moribund' by the late 1950s, as Robert McIntosh stated it. An attempt to revive it in the mid 1960s was equally short-lived.[132]

[128] Ibid., p. 961.

[129] Ibid., p. 962.

[130] 'Report of the Committee on Human Ecology', *Bulletin of the Ecological Society of America*, (1957) 38, p. 28.

[131] L. White, Jr, 'The historical roots of our ecologic crisis', *Science*, (1967) 155, pp. 1203–7.

[132] McIntosh, op. cit. (5), pp. 307–8.

Whither human ecology?

In 1955, the year in which the Ecological Society formed its human ecology committee, the Wenner-Gren Foundation for Anthropological Research sponsored a symposium at Princeton on 'Man's role in changing the face of the earth'. The symposium was organized by geographer Carl Sauer, social critic and social planner Lewis Mumford, and zoologist-naturalist Marston Bates. Paul Sears was in attendance, as was Burke, along with over seventy others representing some twenty different fields. Geographers comprised the largest group of participants, with botanists and anthropologists next. In addition to Sears, there were several other practising ecologists, including F. Fraser Darling, John T. Curtis, and Frank Egler. Only one sociologist and two economists represented the social sciences (other than anthropology). The symposium was held in honour of George Perkins Marsh, as a kind of mid-twentieth-century reaffirmation of the ideas contained in Marsh's 1864 classic *Man and nature*. Topics ranged from 'The ecology of peasant life in western Europe' to 'Artificially induced precipitation and its potentialities' to 'Sanitation practices and disease control in extending and improving areas for human habitation'. An ecological perspective pervaded the presentations and a few of the participants referred to human ecology by name, but there was little sense of human ecology as a single, unified discipline. The sense conveyed by the published collection of papers and discussions was that of mutual recognition of an interrelational point of view that needed much more attention.[133] Many of the participants shared with Sears the same vague faith in the potential of ecology. Many of the issues raised — overpopulation, pollution, soil erosion, effects of radiation, resource depletion — would be raised again in a few years, but attended then by a good deal more emotion and by a sense of extreme urgency not evident at the 1955 symposium.

In 1967, when Paul Shepard inquired as to the fate of human ecology, ecology in general was beginning to receive some public attention. Shepard's rhetoric reflected the changing mood. On the one hand, he was comfortable, as Sears had been, with the eclectic nature of human ecology: 'It would be impertinent to attempt to define it now so as to exclude its historical forms or its descendent and peripheral disciplines.'[134] Yet for Shepard, as for Sears, its questionable disciplinary status did not undermine the importance of human ecology; it was larger than the sum of its parts:

> The ecology of man has no sacred core to guard against the Philistines. It will be healthiest perhaps when running out in all directions. Its practical significance may be the preservation of the earth and all its inhabitants.[135]

[133] W. L. Thomas, Jr (ed.), *Man's role in changing the face of the earth*, Chicago, 1956.
[134] Shepard, op. cit. (4), p. 894.
[135] Ibid., p. 894.

Such eminent practicality should have been sufficient in itself, but the widespread popular recognition of ecology beginning in the late 1960s served to restore hope for the emergence of a coherent, unified human ecology. In 1969 oceanographer Roger Revelle, then serving as Director of Harvard University's Center for Population Studies, characterized human ecology as 'still rudimentary', but he speculated that with the mutual co-operation of social scientists, natural scientists, and humanists, and with the aid of computers, there was hope for a full-blown, comprehensive theoretical structure to emerge.[136] Ecologist Pierre Dansereau expressed equal optimism in 1970:

> A science of human ecology is in the making. It has not found its bearings yet, for it remains dependent upon sociology and human geography and has not nearly informed itself on the theoretical content of ecology. A new discipline focused on the human habitat will hopefully emerge from a conversation between scientists and humanists trained in a variety of fields and well-grounded in one or more of them, but also, open to the revelations their knowledge may bring to environmental processes.[137]

In 1974 Gerald Young of Washington State University admitted in his lengthy analysis of human ecology that it was 'a fragmented field, far from interdisciplinary', but he expected greater unity and integration in the future, and he offered thirty pages of suggestions for possible unifying concepts and themes from general systems theory, sociology, psychology, and recent ecosystem studies.[138] In 1986, as mentioned earlier, Amos Hawley, armed with some of the new ecological terminology, was still searching for the basis for a grand synthesis.[139]

The search goes on, but rarely under the name of human ecology. Hawley's recent efforts aside, attempts at synthesis are now made somewhat outside the boundaries of the natural and social sciences in the name of 'green politics', 'deep ecology', or 'social ecology', the last term preferred by Murray Bookchin, perhaps the most ambitious and analytical of the newer visionaries.[140] Bookchin's writings, although somewhat discursive, point out what is entailed and what is at stake — epistemologically, morally, politically — in the adoption of the kind of total ecological vision that has become fashionable among many contemporary social critics. This is no casual endeavour. Human ecology, on the other hand, persists as a vague general term attached to any treatment of interactions between humans and their

[136] R. Revelle, 'Human ecology and ethics are inseparable', *New York Times*, 12 January 1969, reprinted in *Man and environment* (ed. A. Hawley), New York, 1975, p. 234.

[137] P. Dansereau (ed.), *Challenge for survival: Land, air, and water for man in Megalopolis*, New York, 1970, p. ix.

[138] Young, op. cit. (44), pp. 58, 59–87.

[139] Hawley, op. cit. (95).

[140] See B. Devall and G. Sessions, *Deep ecology: Living as if nature mattered*, Salt Lake City, 1985; M. Bookchin, 'Social ecology versus deep ecology', *Socialist review* (1988), 3:9–29; also Bookchin, *The philosophy of social ecology: Essays on dialectical naturalism*, Montreal, 1990.

environments, large and small. A source-book of human ecology published in 1983 included roughly a thousand separate entries covering everything from the ecological relations of primitive societies to air and water quality, resource management, birth control, and occupational medicine.[141] The journal *Human ecology*, which has been in existence since the early 1970s, has not provided so much a forum for synthesis as a vehicle for the publication of case studies.[142] Despite the occasional general article and the interdisciplinary aspirations of the editors, the journal's central focus is cultural anthropology, essentially the perspective which anthropologist Julian Steward formalized in the 1950s as 'cultural ecology'.[143] Steward had intended the ecological perspective to serve merely a heuristic function in helping to explain particular instances of cultural adaptation. Cultural ecology was to represent more of a methodology applied to individual case studies than a doctrine or a body of knowledge generalizable to all cultures, and Steward fully expected the term 'cultural ecology' to supersede 'human ecology', since he believed that the latter term had outlived its usefulness.[144] Although its meaning has not been clarified, the term has, of course, persisted, even in the title of the journal which promotes Steward's point of view.

For a time from the 1920s into the 1950s human ecology seemed to hold greater promise as a unifying conceptual framework, and it served additional functions within the disciplinary boundaries of the groups that promoted it. The human ecology movement generated by Chicago sociologists in the 1920s served the dual purpose of consolidating some of the loose theoretical strands of an essentially field-based research program and, according to Robert Faris, providing an alternative to the more extreme eugenic explanations for social ills.[145] For the biological ecologists, on the other hand, human ecology represented a vague hope for the realization of the full potential of a young science whose usefulness was often assumed but seldom demonstrated. Sometimes the rhetoric used by both groups gave the impression that human ecology would provide the theoretical substructure for a new science-based social order. However, a science of human ecology never materialized, although an ecological perspective has certainly made its way into academic and political discourse.

[141] F. Sargent, *Human ecology: A guide to information sources*, Detroit, 1983.

[142] There was an earlier *Journal of human ecology* begun by a group interested in the effects of weather and climate on human behaviour, but it did not survive through three complete volumes (1951–1954). R. W. Dexter, 'The *Journal of human ecology* — Pioneer in the field', *Environmental history newsletter*, (1978) 4, pp. 1–2. I am grateful to R. P. McIntosh for that reference.

[143] J. H. Steward, *Theory of culture change: The methodology of multilinear evolution*, Urbana, Illinois, 1955, especially pp. 30–42.

[144] Ibid., p. 36. See also Steward's discussion of 'Cultural ecology' in *The international encyclopedia of the social sciences* (ed. D. L. Shils), New York, 1968, Vol. IV, pp. 337–44.

[145] Faris, op. cit. (51), ch. 4.

The problem lies as much with the nature of ecology itself as with the efforts of the human ecologists. On the one hand, a separate science of human ecology makes little sense conceptually; ecology, as the science which treats the interrelationships of organisms and their environments, should naturally include human organisms. On the other hand, ecology in practice is not one science but many. It is an elusive, complex, pluralistic, multi-dimensional collection of specialities and points of view held together loosely within a theoretical framework that includes few universal laws and many generalizations of limited applicability. Historically, there have been as many ecologies as there are types of organisms and levels of organization to study — plant and animal ecology, microbial ecology, marine ecology and limnology, etc., and then physiological ecology, population ecology, community ecology, and ecosystem ecology, working up through ever broader levels of organization. Studies of humans and their interactions would not have fit easily into the general theoretical structure of ecology for the simple reason that no such single, agreed-upon structure existed. At the same time, human ecology makes little sense as one among many subspecialties of ecology. Since human culture adds layers of conceptual complexity to organism–environment interactions, human ecology, fully explored, becomes the whole of the social sciences and humanities. Ecologists and social scientists seem to have recognized aspects of these problems from the very beginning, although many held out the hope that with sufficient effort and co-operation a larger synthesis might be achieved. What has been achieved is the gradual (and still very uneven) integration of ecological ideas and environmental concerns into the framework of social, political, and economic theory and practice. Human ecology persists, but simply as a broad category rather than a synthesis or even a point of view.

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Index of names

Abercrombie, Patrick 100
Adam of Bremen 38
Adams, Charles C. 251, 252, 263–4, 272–3, 275, 276–7
Adkisson, Perry 197, 201, 205–7, 208
Aitken, I. 67
Alembert, Jean Le Ronde d', *see* d'Alembert, Jean Le Rond
Alexander the Great 28
Alihan, Milla A. 251, 269, 270
Allee, W. C. 215, 226, 259, 263
Ancaster, Lord 43, 45, 47
Anderson, David 147
Anderson, M. 60
Anstey, Edgar 69, 73
Arago, François 37
Aristotle 30
Arthur, Joseph Charles 159
Ashby, Lord 60
Attiwill, Peter 139–41
Auerbach, Stanley I. 216, 217, 229, 230, 233–8, 244
Augustus 28
Austin, Claude 131, 134
Austin, Mary 153, 173–5, 176

Bach, Paul De, *see* De Bach, Paul
Baird, Thomas 73
Baker, John 102–3, 104–5
Baldwin, Stanley 50–51, 53–4
Balfour, Arthur James 51, 52–4
Barnes, Charles R. 258
Barrows, Harlan 261–2, 267
Bates, Marston 280
Beddington, Reginald 45, 52
Beirne, Bryan P. 190–91
Bernal, John Desmond 102, 104
Bews, J. W. 271–2
Blache, P. Vidal de La, *see* Vidal de La Blache, P.
Blackburn, Jean 136
Blainey, Geoffrey 142
Bolte, Henry 117, 118, 123, 125, 126, 128, 130, 134
Bookchin, Murray 281
Bormann, F. Herbert 246–7
Borthwick, William A. 128, 130–31, 133
Bosch, Robert Van den, *see* Van den Bosch, Robert
Bowers, J. B. 107
Boyd-Orr, John 68
Britton, Nathaniel Lord 155–9, 159, 171, 176
Brockway, Lucile 156
Brongniart, Adolphe Théodore 27
Brongniart, Alexandre 27
Brunhes, Jean 261
Buch, Leopold von 25, 35–7
Buckland, William 27
Buffon, Georges Louis Leclerc de 24, 27, 31
Burbank, Luther 164, 177
Burgess, Ernest W. 254, 262–5, 266–8, 271, 272, 275
Burke, Albert 279, 280
Bush, Vannevar 175
Butcher, Alfred Dunbavin 144, 145
Butcher, R. W. 49

Calder, D. Malcolm 140–41
Calder, Ritchie 68, 74
Candolle, Alphonse Louis Pierre Pyrame de 27
Cannon, William Austin 162, 163, 164
Carnegie, Andrew 156, 158
Carson, Rachel 181, 186, 190, 195, 212
Carter, Jimmy 208–9
Child, Charles Manning 269
Cittadino, Eugene 11, 18, 152, 156
Clausen, Jens 177
Clements, Frederic E. 138, 166–8, 169, 175, 177–8, 221, 223, 264, 265, 271, 274
Collier, John 173
Coville, Frederick Vernon 159–62, 170, 176, 178
Cowles, Henry C. 215, 258, 263, 272, 274
Cox, H. C. 188, 210
Crawford, William 67
Croll, James 20

Crossley, D. A. 234–5, 236, 245
Curtis, John T. 280
Cuvier, Georges Léopold 27

d'Alembert, Jean Le Rond 25
Dansereau, Pierre 281
Darling, F. Fraser 280
Darwin, Charles 6, 10, 17, 20, 158, 170
Davies, Fred 132
Davis, J. J. 220
De Bach, Paul 190–91
de Candolle, Alphonse Louis Pierre Pyrame, *see* Candolle, Alphonse Louis Pierre Pyrame de
de Vries, Hugo 170–71, 177
Deevey, Edward S., Jr 278
den Bosch, Robert Van, *see* Van den Bosch, Robert
Desmond, Adrian 14
Diderot, Denis 25
Diodorus Siculus 30
Diver, Cyril 97, 106
Dower, John 106, 110
Dubos, Jean-Baptiste 28–31, 33, 34, 38
Duffy, L. M. 146
Dyne, George M. Van, *see* Van Dyne, George M.

Ealey, Tim 141
Egler, Frank 280
Elliot, Walter 67–8, 73
Elton, Arthur 69, 73
Elton, Charles S. 91, 93–4, 99, 105, 106, 109, 111–12, 222
Emerson, Alfred 216, 259
Emory, William H. 153
Erlichman, John 201

Falk, Jim 117, 130
Faris, Robert 282
Fawcett, Maisie 138
Forbes, Stephen A. 260
Foster, R. F. 220
Frankenberg, Judith 142
Fraser, Malcolm 134
Fryer, John 110

Gainford, Lord 46, 52
Garnet, J. Ros 134, 145
Geddes, Patrick 253, 272
Gepp, Herbert 135
Gettys, W. E. 271
Gibbon, Edward 29, 30–31, 33, 34
Gilmour, John S. L. 108
Glacken, Clarence 28
Godfrey, Lewis 135
Godwin, Harry 98, 111
Goetzmann, William H. 153
Goodman, David 121
Graham, Edward 99
Greene, Mott T. 7
Gregory, Richard 74
Grierson, John 66–7, 69, 70, 73, 77–8

Haeckel, Ernst 263
Hairston, Nelson G. 226–7
Haldane, J. B. S. 62, 69, 74–7, 78, 79–80, 87
Hall, Harvey Monroe 177
Hamer, Dick 128
Hanson, W. C. 220
Haskell, Edward F. 272
Hawley, Amos H. 268–71, 278–9, 281
Hayden, Ferdinand V. 154
Hays, Samuel P. 149
Heath, Frank 53–4
Hill, Leonard 84
Hobhouse, Arthur 106–7, 110
Hogben, Lancelot 74–5, 77–8
Hollingshead, A. B. 268, 275–6
Hooykaas, Reijer 7
Hopkins, Frederick Gowland 79–80
Horace 29
Hornaday, William T. 163, 168
Huffaker, Carl B. 191, 197, 198–203, 204–6, 208, 210
Humboldt, Alexander von 20, 25, 37, 153
Hume, David 29–31, 33, 34, 35
Huntington, Ellsworth 173, 255–62, 272, 273
Hutchinson, E. P. 277
Hutchinson, G. Evelyn 216, 221, 222–4, 226, 230, 238, 248
Huxley, Julian 74–5, 78, 99, 106, 108, 110, 253

Isaac, Cyril 135

Jee, E. C. 47, 48
Jenny, Hans 238
Jones, William 34
Jordanova, Ludmilla 5, 59–60
Jowitt, William 97

Julius Caesar 28, 31
Julius II, Pope 28
Juvenal 29

Kalm, Peter 37
Kammerer, Paul 172
Kant, Immanuel 24
Kauffer, McKnight 75
Kendeigh, Charles 221
Kennedy, Donald 206–7
Kennedy, John F. 195
King, Clarence 154
Knipling, Edward F. 186, 189, 196–7
Kornberg, H. A. 220
Krumholz, Louis 233

La Blache, P. Vidal de, *see* Vidal de La Blache, P.
Lampadius, August Wilhelm 25
Landy, John 144
Latour, B. 59
Le Play, Frédéric 272
Leigh, Walter 75
Leo X, Pope 28
Leslie, S. C. 72
Likens, Gene E. 246–7
Lindeman, Eduard 275–6
Lindeman, Raymond 221, 222–3, 235, 239, 240, 246, 248
Lines, William J. 120, 149
Linnaeus, Carolus 24
Livingston, Burton E. 163, 166, 167, 257–8
Livingstone, David 9
Louis XIV, King of France 28
Lovelock, James 1, 3, 20
Lyell, Charles 27

MacDonagh, O. 41–2, 44, 46, 50, 52, 54, 55
McDonald, William 117, 118–24, 126–7, 128–9, 132, 134
MacDougal, Daniel Trembly 151–2, 153, 159–60, 161–78
Macfarlane, John M. 160
McGucken, William 76–7, 105
McIntosh, Robert P. 11, 252, 254, 279
Mackaye, Benton 275–6
McKenzie, Roderick D. 264–71, 272, 275
Macleod, R. 47
Mahon, George 201, 203
Márgalef, Rámon 225
Marsh, George Perkins 261, 280
Marshall, A. J. 137–8
Maurice, Henry G. 43, 44–6, 47, 50, 52–3, 54–5
Meinecke, Friedrich 24
Merriam, John C. 175
Messier, Charles 32
Mildmay, Lord 52
Mills, Eric L. 8–9
Milne-Watson, David 72–3
Montesquieu, Charles de Secondat, baron de La Brède et de 26
Moore, Barrington 257, 259–260
Morgan, James Pierpont 156
Morgan, Karl Z. 233, 238
Morrison, Herbert 108–9, 112–13
Mumford, Lewis 252–3, 280
Murchison, Roderick 17
Muskie, Edmund 202, 204

Nash, Roderick 124
Nelkin, Dorothy 141, 142
Newberry, John Strong 155, 158
Newman, George 84
Newsholme, A. 60
Newsom, L. Dale 191–2, 202, 210
Nichols, B. 68
Nicholson, E. Max 57, 106–7, 108, 112–13
Nixon, Richard M. 182, 198, 201, 202
Norris, Robert A. 229, 230

Odum, Eugene P. 213, 216, 217, 220–33, 235–6, 237, 239, 246, 248–9
Odum, Howard Thomas 221, 224, 239–43, 246
Oldroyd, D. R. 24, 30
Olmsted, Charles 238
Olson, Jerry S. 235, 236, 238–45, 246, 248
Orians, Gordon H. 227
Ovid 30

Park, Orlando 216, 233, 235
Park, Robert E. 254, 262–5, 267–8, 269, 270, 271, 272
Park, Thomas 216
Patten, Bernard C. 238, 244–5, 246
Pearsall, William 94–5, 98
Pentelow, F. T. K. 49
Perkins, John H. 181–3, 189, 191, 193, 207, 209
Petronius 30
Pfeffer, Wilhelm 159

Philip II, King of Macedonia 28
Pick, Frank 67
Pimentel, David 211–12
Pinchot, Gifford 160
Play, Frédéric Le, *see* Le Play, Frédéric
Pliny, the Elder 40
Polanyi, Michael 103, 105
Porter, Roy S. 5
Powell, John Wesley 154
Ptolemy, Claudius 26

Quinn, James A. 268, 278

Rabb, Robert L. 188–9, 192, 197, 210
Rainger, Ronald 11
Ratzel, Friedrich 261
Reclus, Élisée 261
Reith, Lord 97
Revelle, Roger 281
Ribicoff, Abraham 196
Riley, Gordon A. 216
Ritter, Karl 153
Robinson, Roy 92
Romanes, George John 169–70
Rotha, Paul 66–7, 68, 69–70, 77–8, 82
Rothschild, Charles 96
Rudwick, Martin J. S. 7, 16
Russell, E. C. 48
Ryan, A. P. 67, 72
Ryle, John A. 79

Sabine, Edward 16
Salisbury, Edward 95, 98
Sauer, Carl 280
Schmidt, Karl 216
Schouw, Joachim von 25, 37–40
Sears, Paul 274–5, 278–9, 280
Shantz, H. L. 275–6
Shapin, Steven 58–9
Sharp, Thomas 100
Sheail, John 96, 110
Shelford, Victor 215, 221, 223, 233, 260, 263
Shepard, Paul 251–2, 253, 280
Shreve, Forrest 163, 166, 167, 171, 175, 177, 255, 256, 258
Shull, George 164
Silkin, Lewis 110
Sloan, Douglas 158
Slobodkin, L. B. 226–7
Small, Albion 262
Smith, Frederick E. 226–7
Smith, Herbert 97, 107
Smith, Ray 201–3, 210
Smuts, Jan Christiaan 271
Spalding, Volney M. 163
Spoehr, Herman Augustus 163, 175, 177–8
Stapledon, R. George 91
Steward, Julian 282
Strabo 35
Stretton, Leonard 135
Struxness, Edward 233
Swann, Paul 66
Sydenham, Thomas 26
Sykes, Godfrey 162, 163–4, 165

Tallents, Stephen 67–8
Tansley, Arthur George 89, 90–95, 97–102, 103–7, 111–12, 114, 137, 254
Taylor, Griffith 273
Taylor, Gwynnyth 126, 134, 145
Taylor, John 66, 75
Taylor, Norman 277
Taylor, Ron 126
Taylor, Walter 273
Theophrastus 24, 40
Thompson, George 126
Thornthwaite, C. W. 275–6
Tizard, Henry T. 52–3
Toaldo, Giuseppe 33
Tobey, Ronald C. 11, 177–8
Torrey, John 155
Tower, William L. 172, 177
Tuan, Yi-Fu 12
Turner, Frederick Jackson 172–3
Turner, John S. 131, 134, 142, 144

Urban, Ignatz 157

Van den Bosch, Robert 202
Van Dyne, George M. 238, 245, 246
Vanderbilt, Cornelius 156
Vidal de La Blache, P. 261
Virgil 34
von Buch, Leopold, *see* Buch, Leopold von
von Humboldt, Alexander, *see* Humboldt, Alexander von
von Schouw, Joachim, *see* Schouw, Joachim von
Vries, Hugo de, *see* de Vries, Hugo

Wallace, Alfred Russell 170
Ward, Basil 75

Warming, Eugenius 264
Warnes, Arthur 62, 69
Washington, Booker T. 262
Watt, Alex 93, 95
Webster, Noah 33–6
Wegener, Alfred 8
Weinberg, Alvin 233, 234–5
Wells, G. P. 253
Wells, H. G. 252–3, 260, 272
Wheeler, William Morton 264
White, Lynn, Jr 279
Whitney, Josiah Dwight 154
Whitten, Jaimie 193–4, 196, 203
Wiener, Norbert 223
Winthrop, John 33
Wolfe, John 218, 229, 234
Woodward, Robert S. 164–5
Worster, Donald 11, 18, 136

Young, Arthur 35
Young, Gerald 261, 281
Young, Robert M. 13

Zorbaugh, Harvey 267