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ABSTRACT The International Geosphere-Biosphere Programme (IGBP), a large international research programme, served to set the research agenda of a number of environmental sciences around the issue of global warming and global change. This paper examines the impact of the interdisciplinary cooperation within the IGBP on ecology and the ecologists' response. Ecology was an integral part of the IGBP from the beginning, yet it was sometimes in uneasy cohabitation with the other sciences involved. The issues of global warming and global change posed opportunities and challenges to ecology. They posed opportunities because an important cause emerged, with promises of exciting new (space) technologies and new funds for the environmental sciences. They posed challenges, because by aligning itself to sciences that study the earth system as a whole, ecology was invited implicitly to bracket its focus on the specificity of local ecosystems, that is, to give up ecology's traditional focus on field studies of plant and animal communities. My aim in this paper is to place the opportunities that global change research offered to ecology in the context of changes within the field that were already underway. Power relationships between disciplines did not give ecology an upper hand vis-à-vis the other earth sciences, but ecologists were able nevertheless to redefine subtly the notion of the global.

Keywords earth sciences, global change, history of ecology, interdisciplinarity, National Aeronautics and Space Administration

Local Ecologies and Global Science:

Discourses and Strategies of the International Geosphere-Biosphere Programme

Chunglin Kwa

Between 1984 and 1990, a broad consensus formed among US scientists, who were from various disciplines and involved in science policy circles, on a plan to investigate the impacts of global warming on the 'Earth system'. Three high-level national committees worked together to develop the plan, while public attention was largely focused on a fourth: an international committee that prepared the International Geosphere-Biosphere Programme (IGBP), and in which US scientists played a key role. The IGBP and its US counterpart, the Global Change Research Program (GCRP), would be interdisciplinary endeavours. The early planning activities of these programmes were in no small part centred on the design of their interdisciplinary configurations.

This paper studies the impact of the interdisciplinary cooperation within the IGBP on one participating science: ecology. As we will see,

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interdisciplinarity meant different things to different sciences. The programming phase of the IGBP (the subject of the first part of this paper) resulted in a configuration of disciplines that assigned to ecology a rather specified functional role. Ecology was relatively late in addressing 'the global', and had less access to funding and to some key technologies (most importantly remote sensing) than, for example, the atmospheric sciences. Yet the other earth sciences recognized the importance of ecology as contributing to the general picture of the earth system. To ecology, this recognition came at some cost, namely the acceptance of the global as a primary research theme and interdisciplinarity as the way to achieve it. With it came the technology of remote sensing. For the ecologists it was an offer they couldn't refuse. As a consequence, ecology was invited implicitly to bracket its focus on the heterogeneity of local ecological systems – that is, to give up ecology's traditional focus on field studies of plant and animal communities.

The second part of the paper discusses the state of ecology before the beginning of the IGBP. Previous efforts to conceptualize very large ecosystems were perceived as having failed within the discipline of ecology, and the study of finer-grained spatial and temporal heterogeneity in landscapes was receiving more attention. But there were early champions of a movement to global ecology. These champions advocated different forms of research, however, and were divided on which remote-sensing techniques would best serve the interests of research on a global scale.

In the third part of the paper, I examine closely US and French ecologists' participation in the IGBP. Power relationships between disciplines did not give ecology an upper hand vis-à-vis the other earth sciences, but IGBP ecologists were able nevertheless to subtly redefine the notion of the global and mitigate pressures for interdisciplinarity. As will be shown, developments in the USA and France were similar. France is, along with the USA, among the few countries in the world with an important space agency (the Centre National d'Études Spatiales). French ecologists were, like their US counterparts, subject to pressures on their research from developers of remote-sensing techniques, but were similarly able to withstand them.

For a programme such as the IGBP, the advantages of interdisciplinarity were taken for granted. The notion of interdisciplinarity served to motivate the scientists and science programmers, and contributed to the IGBP's legitimation in the policy world. That notion, along with another discursive notion, the 'global', helped to sustain the heterogeneous network of IGBP scientists (Heclo, 1978; Hajer, 1995). Interdisciplinarity was materialized in the technology of remote sensing, as well as in the data produced by that technology, which the participating disciplines were supposed to share, and the models to which they were all supposed to contribute. The centrality of these two notions made decisions about the central concerns of global change research seem logical and inevitable.

But expectations of what could be achieved through interdisciplinary cooperation differed among disciplines and within disciplines. Likewise, the 'global' could be understood, visualized or researched in a variety of ways. The various earth sciences that investigate the global environment had different histories of doing so, and each of them constructed the relationship between global and local environments in different ways (Shackley et al., 1998; Jasanoff & Martello, 2004).

The mediating role of models and global satellite data within the interdisciplinary configuration of the earth sciences makes such models and data into typical examples of 'boundary objects'. Boundary objects are shared things, technologies, or diagrams that can, but more often do not, have a similar meaning to the communities of scientists and science policy advisors that use them (Star & Griesemer, 1989). They bind such communities together without necessitating a deeper paradigmatic unity or the existence of an epistemic community (Haas, 1990). In its original formulation, boundary objects mediate between a scientific field on the one hand, and administrators, the public, amateurs, on the other. Later uses of the concept broadened its application to 'communities' in general, including different scientific disciplines (Jasanoff & Wynne, 1998; Edwards, 2001). The concept was also extended to subsume linguistic figures such as metaphors and other rhetorical categories (Shackley & Wynne, 1996; Jasanoff & Wynne, 1998: 5; Halffman, 2003). All conceptions of boundary objects, however, have retained the original idea of their flexibility in different actor worlds. Simon Shackley and Brian Wynne, for instance, showed that different connotations of the concept of 'uncertainty' gave advisory scientists sufficient latitude to separate interpretations used for policy advice on questions about anthropogenic climate change from the richer versions of the meaning of 'uncertainty', which retained their scientific credibility with other scientists. This enabled the science advisors to facilitate interaction and cooperation between science and policy (Shackley & Wynne, 1996: 280).

Boundary objects have different meanings to different participants: this is the core idea of the concept, and it helps to explain several features of the story that follows. I shall argue that, in the IGBP, the interpretative flexibility of boundary objects did not leave all participants at ease, or in agreement. To start with, we shall re-examine the 'interdisciplinarity', to which everyone in the IGBP subscribed. Popular since the early 1970s, the concept of interdisciplinarity carries many positive connotations. By crossing academic boundaries, interdisciplinary research unleashes innovative thinking and practice, and is more adequate in dealing with a number of contemporary problems not foreseen by the established academic disciplines at the time. By designating interdisciplinarity as 'Mode 2 knowledge', Gibbons et al. (1994) have argued for its ascendance. They assert that new loci of knowledge production are arising from sources beyond the universities and academic disciplines. Recently, however, Peter Weingart and others have observed that disciplines have survived very well, while interdisciplinary research centres find themselves on the defensive. Weingart & Stehr (2000), for example, view the identification of 'interdisciplinarity' with 'innovation' as little more than a rhetorical ploy.

Weingart's criticism may or may not withstand empirical scrutiny, but whether or not it does is beyond the scope of this paper (but see Traweek, 2000). However, even as a rhetorical ploy, 'interdisciplinarity' should be taken seriously. Recently, Sharon Traweek (1999) has drawn attention to the fact that during the late 1970s and early 1980s, university bureaucracies started to promote 'interdisciplinarity', at first in the natural sciences, and later in the social sciences and humanities. According to Traweek, such emphases on 'interdisciplinarity' can be understood in the context of resurgent waves of administrative reform, specifically aimed at the entrenched powers of discipline-oriented university departments. Traweek thus understands 'interdisciplinarity' as an expression of a specific bureaucratic strategy at universities, without assuming beforehand a specific goal for the subject matter of the sciences that would be linked together.

Even when boundary objects permit actors to employ different interpretations, actors are likely to be affected by the interpretations of others. Within the IGBP and its related national programmes, a modus operandi was certainly achieved, but it was full of tension. The recurrent calls to become global and interdisciplinary were more than discursive 'glue' that held various sciences together in one programme. They also influenced the participating sciences from within, and therefore can be designated as rhetorical strategies that intervene in the development of sciences. Boundary objects, therefore, are not innocent.[1]

The International Geosphere-Biosphere Programme's Interdisciplinary Configuration

The IGBP became operational in 1990. On the national level, it was complemented by the US GCRP, which was coordinated from the offices of the National Science Foundation (NSF) but included the joint effort of several agencies, most importantly the National Aeronautics and Space Administration (NASA). The IGBP and the GCRP brought (and still bring) together a substantial number of disciplines, including meteorology, solar physics, atmospheric chemistry, oceanography, physical geography and ecology. The official objective of the IGBP is to understand global change, in particular 'to describe and understand the interactive physical, chemical and biological processes that regulate the total Earth system, the unique environment that it provides for life, the changes that are occurring in this system, and the manner in which they are influenced by human activities' (International Geosphere-Biosphere Programme, 1986: 3). In 2000, the IGBP was extended by the International Council of Scientific Unions (ICSU) for another 10 years.

The early planning process of the IGBP, between 1983 and 1986, was full of uncertainty and tension over the direction the programme should take and which disciplines should be included. The eventual plan was the contingent result of many interactions, and perhaps was not foreseen by anyone involved in its production. While the fact that the physical atmospheric and oceanographic sciences dominated the planning process should come as no surprise, more surprising was the fact that one important physical science became more or less excluded from the process: solar–terrestrial physics, which studies the influence of the sun on the Earth. Ecology won a relatively important place, but the social sciences were not included. We will presently see why, and focus in particular on how remote sensing from outer space acquired its dominant status.

At the heart of the IGBP were two different initiatives. The IGBP was first proposed in 1983 by Herbert Friedman, chairman of the US National Academy's Commission on Physical Sciences. He presented it that same year in a 'summer study', a workshop at Woods Hole Oceanographic Institution (MA, USA). Friedman had been an organizer of the International Geophysical Year in 1957–58 and 25 years later he intended to organize another collaborative scientific effort like it. Friedman's first ally in this effort was Thomas Malone, a meteorologist, who was also present at the workshop. He was a former foreign secretary of the US National Academy (1978–82), and before that he was a long-time Chairman of the Academy's Committee on Atmospheric Sciences, and past Treasurer of the ICSU. By 1983, he served on the Board of ICSU. Friedman and Malone agreed that an IGBP needed to be an international endeavour, with ICSU as the required vehicle. But when Malone brought the idea to ICSU, he also built on an ill-conceived proposal by NASA.[2] In 1982, Burton Edelson, the head of NASA's Office of Space Science and Applications, Richard Goody of Harvard who worked with NASA's Jet Propulsion Laboratory, and Michael McElroy, also of Harvard, had conceived a 'Global Habitability' programme, which intended to monitor the earth with satellites and investigate biogeochemical cycles, climate patterns and humankind's influence on the environment. It was formally presented at a UN space conference in Vienna, that same year. Apparently, NASA's arrogance caused general disapproval, notably by representatives of Third World nations (Waldrop, 1984). US domestic reactions were similarly negative. Other agencies were angry not to have been informed beforehand, and officials at the Reagan White House thought the proposal mirrored too closely the thinking of the previous Carter administration. Faced with this overwhelming response, James Beggs, NASA's administrator, instructed his agency to quietly continue working on the idea and to look for support. When the idea for an IGBP was launched, NASA publicly proclaimed its support.

Malone's original proposal to ICSU drew on both Friedman and Goody's ideas, and the tension between those ideas would be played out in the US context. From Goody, Malone took the idea of human influence on the Earth's environment, while Friedman primarily stressed the Earth's physical system. Goody's idea implied a place for the social sciences in the IGBP structure, but Malone chose not to push too hard for the social sciences out of fear that he would lose the interest of some top scientists.[3]

Biology, especially ecology, would play a more important role than originally envisaged by Friedman, but this came at a cost (US National Committee for the International Geosphere-Biosphere Programme, 1988).

John Eddy was chosen as Chair of the US national committee for the IGBP. Like Friedman, he was a solar–terrestrial physicist. Under his guidance, the most important early decision of the US Committee was the ousting of solar–terrestrial physics. Discussions had led to a conclusion that even though work on the solar influence on the earth and its long- and short-term fluctuations would be very important to understanding the global earth system, there was expected to be little data sharing or other forms of interdisciplinary cooperation between that field and other fields such as meteorology and atmospheric chemistry, both of which were to be included in the IGBP. Friedman did not like this conclusion. As Chair of the overseeing National Academy of Sciences Commission on Physical Sciences, he refused to approve the 1986 US IGBP Report, and thus withheld it from publication for several months. Eddy and his committee members had to take some extraordinary steps to obtain its final release.[4]

The configuration of disciplines that was achieved by the IGBP was paralleled, and in fact preceded, by the work of another committee, the NASA-installed Earth System Science committee chaired by Francis Bretherton. There was a fair amount of overlap in membership between the two committees.[5] Representatives of the National Oceanic and Atmospheric Administration (NOAA) and the NSF also participated. The Report of the Earth System Sciences Committee became especially famous because of a diagram that visualized the earth system with a relatively simple physical model. It was a loosely drawn model, fitting on only one sheet of paper, which subsequently became known as the Bretherton diagram (Earth System Sciences Committee, 1988). It was useful because the diagram implicitly identified the sciences that were to be included in a global research programme and assigned a meaningful role to each of them. Even before the committee issued its report, Edelson (1985) at NASA felt confident enough to propose a 'Mission to Planet Earth' in an editorial for *Science* magazine. It was clear by then that an important segment of the scientific community had identified NASA's Mission to Planet Earth, and in particular the Earth Observation System (EOS) programme of satellites, as pivotal to advances in the Earth sciences. The US Congress authorized EOS in 1990.

On the US governmental level, in 1987 an interagency committee was formed, the Committee on Earth Sciences (Kennedy, 1992). Its pro-active members were Shelby Tilford at NASA, Mike Hall at NOAA and Robert Corell at NSF. Corell had only recently joined NSF as Assistant Director for Geosciences, a position for which he had left his faculty position as oceanographer at the University of New Hampshire. With the exception of ecology, Corell's division grouped together all the earth sciences relevant to the US GCRP. Tilford was head of NASA's Mission to Planet Earth programme (MTPE). The single most important achievement of the committee was the production of the second Research Plan on global change for the fiscal year 1991 (Committee on Earth and Environmental Sciences, 1990). This programme delineated the priorities of the US global change research effort[6] and secured the inclusion of the Earth Observation System satellite programme the following year. For much of the 1990s, EOS, and the remote-sensing instruments carried by them, received from one-half to almost two-thirds of the total budget of GCRP – US$1.8 billion by 1995.

What gave EOS such a dominant position in the US GCRP? Was it the railroading power of NASA, keen on selling a new toy in the post-Apollo, post Cold War period? NASA's aim was to develop EOS into a leading-edge remote-sensing system. The first of its satellites (Terra, also designated as AM-1) was projected for launching, first in 1995, then in 1998. It was eventually sent up in December 1999, almost 10 years after the beginning of the implementation phase of the IGBP. Many IGBP scientists thought, and still think, that EOS came too late and was far too costly. To some extent, they even regret the support they provided NASA through the Bretherton committee. They say that the IGBP would have been much better off with a number of smaller satellites, which could have been functioning much earlier. Yet they also recognize that such smaller projects are not what NASA is in the game for. NASA learned its lesson from Landsat. If NASA had merely offered services to other parts of the scientific community, it would end up losing control of its MTPE. Without EOS, NASA would not have been interested in participating in the GCRP. This was accepted by the other agencies, including the NSF. With NASA's imprimatur, funding for global change research would be orders of magnitude higher than without. Even if most of the money would go to NASA itself, it would benefit the other participating sciences.

The Landsat Experience

The history of NASA's Landsat programme makes clear why NASA had no choice but to demand a central role for itself (Mack, 1990, 1998). Landsat had been conceived in the late 1960s to monitor the Earth's resources. In part, its conception anticipated cutbacks in the Apollo project. It would be 'useful' to society, and its immediate beneficiaries would be government agencies such as the US Geological Survey (Department of the Interior), the Department of Agriculture and the Army Corps of Engineers. NASA succeeded in bringing the divergent requirements of the various agencies together, while remaining the principal agency responsible for satellite technology. In itself, this was a major achievement, given the tensions between NASA and the other agencies. But NASA was not able to gather enough political support. The Bureau of the Budget (BoB) cut the Landsat budget from US$41.5 to 10.0 million in 1969, at the height of the Vietnam War. Two years later, just before the launch of the first Landsat satellite, NASA had to concede under pressure of the BoB that once its technological feasibility had been demonstrated, Landsat would be commercialized and hence given away to the private sector (Mack, 1990).

Landsat was effectively privatized in 1984, an outcome that all the US administrations, including Carter's, had pushed. The decision was ill fated, however. The French Satellite Probatoire pour l'Observation de la Terre (SPOT) was launched in 1986 and quickly outcompeted Landsat on the commercial market with sensors that were capable of producing finer-resolution pictures of the Earth than Landsat could provide.[7] SPOT has retained the commercial edge to this day. Landsat's corporate adventure was over with its return to NASA in 1992, following the endorsement of the Land Remote Sensing Policy Act.

The Landsat experience shows that NASA, a research agency from its very inception, did not have a magic touch in heading a big scientific project with practical benefits. As Mack has demonstrated, once Landsat had been set on the track of being 'useful', NASA was apparently not able to redefine Landsat's remote-sensing data as a public good and to gain political support for that view (Mack, 1998). But precisely on this point, NASA succeeded with EOS. For EOS, NASA needed the support of 'fundamental science', which it received from the Bretherton commission and from the US National Committee for the IGBP. And although the EOS programme also ran into funding problems, and as a result had to relinquish part of its technological innovativeness, it was by then solidly anchored within NASA programmes.

NASA's push for dominance within the US GCRP contributed strongly to the configuration of disciplines that emerged around the planning of the IGBP. The IGBP was organized around technologies: remote sensing from Earth-orbiting satellites and computer modelling. These did have their appeal. The use of technology would bring scientists from various disciplines to use the same dataset and, accordingly, to deploy the same scales in the analysis of planetary phenomena. Technology, models, data and scale were at the heart of the strategy to make scientists 'speak the same language', an often-used metaphor. Computer modelling was thought to provide 'a common lexicon if not a single language for the sciences' (International Geosphere-Biosphere Programme, 1986: 9). 'Speaking the same language' was also sometimes used more literally, as in the case of atmospheric and stratospheric scientists, who recently learned to their surprise that west winds and westerlies designate different natural phenomena.[8] According to this idea, in order for data to become a shared language, scientists must use the same data across disciplines, or at the very least data that are calibrated on the same spatial and temporal scales. The best way to ensure that this will be the case is to use data that have been gleaned from a shared perspective: outer space. Remote sensing from spacecraft is available to earth science scientists, and they would be well advised to use it, considering EOS's share of the US GCRP budget. So strong is the emphasis on remote sensing that we might think of it as a case of 'technology push': the availability of technology taking precedence over the user's demand. As Bruno Latour (1986: 28) has noted in another context, this is a way of achieving interdisciplinarity 'through the back door' of measurements and data acquisition (see also Fujimura, 1988; Beaulieu, 2001: 668).

Interdisciplinarity: An Invitation to Become Global

IGBP's challenge to ecology was to become 'global', and the 'localness' of ecology was questioned. A process toward getting meteorology and ecology to work together in the IGBP was started early. In 1985, a workshop was held at Boulder (CO, USA) with the University Corporation for Atmospheric Research (UCAR) as the host. The workshop organizers were John Eddy and ecologist Harold Mooney at Stanford University – Mooney would become Eddy's successor as chair of the US IGBP committee (1986–88). The organizers noted straight away that, in the past, collaboration between the atmospheric and the ecological sciences had been hindered by the 'perception of one field in the service of the other' (University Corporation for Atmospheric Research, 1985: 2). The ecologists' primary challenge was one of scale. As the workshop organizers put it: 'A fundamental problem in linking the endeavours of atmospheric and ecological scientists is differences in temporal and spatial scales' (University Corporation for Atmospheric Research, 1985: 13). The IGBP Planning Group echoed a year later: 'The task of identifying mutually acceptable scales in space and time for meaningful study of the Earth as a system is at present a significant obstacle in integrating studies of the atmosphere, land, oceans, and biota' (International Geosphere-Biosphere Programme, 1986: 9). Global change implies a global scale. Climatologists and oceanographers need ecological data at the scale of either the entire globe or very large portions of the earth. The only truly viable method to build this data collection, as they perceived it, is remote sensing. Commenting on ecologists, Robert Corell uses the simile of the measuring chord on board oceanographic vessels. 'There is but one chord, and everybody has to use it. As a result, oceanographers are quite used to working together.' Corell sees the satellites as the 'integrators'.[9] Now it is the ecologists' turn to go through the same process, as if they were on board a space ship with oceanographers and atmospheric scientists. Ecologist Hank Shugart (University of Virginia) found in recent years that NASA, through its Earth System Sciences programme, was only willing to give him access to its grants if he actually made use of remote-sensing data.[10] Shugart did in fact develop working relationships with scientists at the Goddard Space Flight Center, with whom he co-authored papers on research which conformed to the NASA requirements.

But scale and method are not the same. High-resolution images of 10 × 10 m can be very useful for more traditional ecological analyses of local ecosystems, and many ecologists would be keen to use them. In current parlance, ecologists, and the ecological programme within the IGBP, are termed a 'user group' with respect to remote-sensing data, and as such they are among several other user groups. Around the year 2000, the leadership of Global Change and Terrestrial Ecosystems (GCTE), the ecological programme within the IGBP, started advocating a 1 km grid of data instead of the 4 and 8 km grids then in use.[11]

How have ecologists reacted to the pressures exerted on them by other segments of the scientific and policy networks involved in global change research? In 1999, after an initial ten years of global change research, Corell voiced pessimism about the degree of success in enticing ecologists to gear their work to the needs of global change research: 'Ecologists are having a hard time with this. [Ecology] has come out of small processes, the analysis of small plots of land, even Odum's work. And suddenly they ask: "How does this scale up?"'[12]

Corell's mention of small plots of land may be a rhetorical exaggeration, but the point is clear. Ecology usually has been concerned with the ecology of particular regions, often defined on the basis of a particular type of vegetation, for example grasslands in the moderate zones, chaparral or deciduous forests. Scaling up from a forest to a continent involves a process of abstraction, which may lead to the loss of the very content of ecology. Yet, investigating at this scale is necessary to obtain the data needed by various sciences involved in global change research. Does Corell's comment imply that the goal of bringing ecology to a global scale was not achieved?

We should note first that ecology did have champions of global change research in the years before the IGBP was conceptualized. Daniel Botkin was among the first of these champions. Botkin believed that ecology was in need of reform, and should concern itself more with measurements on a global scale of important ecological parameters. Many years before EOS, NASA provided Botkin with a forum to pursue his ideas.[13] Harold Mooney also positioned himself as a strong advocate of the global move within ecology. According to him, without the IGBP ecology by itself would never have ventured as far in global research, and he credited the atmospheric scientists with arguing persuasively that ecology should embrace the global.[14] Mooney's position, as Past President of the Ecological Society of America, carried weight in the ecological community. In addition, in his roles as Chairman of the US IGBP committee and member of the Scientific Steering Committee of the IGBP GCTE Project from 1990 to 1996, he did what he could to engage his fellow ecologists in global change research.

At this point we should note that US ecologists could in principle ignore the pressures to conduct global research, since the ecological programmes of the NSF are not in geosciences, much to the regret of Corell.[15] Mary Clutter, NSF's Assistant Director of Biological Sciences, was not enthusiastic about the theme of global change.[16] But a clear-cut divide did not occur between a traditional ecology orienting itself to local ecosystems and a global ecology orienting itself to the needs of the atmospheric science. Botkin, Mooney, and also Shugart, seized the opportunity of remote sensing, and set out to mould it toward the needs of mesoscale research in ecology. Their efforts achieved variable degrees of success, as we will see in the following sections. Their efforts were undertaken in the face of an alternative scenario for global ecology that aimed to lead ecology on a different path than the one Botkin, Mooney and Shugart supported.

Ecology at Dawn of the Era of Remote Sensing

Ecology's previous big programme, the International Biological Program (IBP), had formally come to an end in the US in 1976. Its results for the discipline were mixed. On the one hand, the IBP had solidly anchored ecosystem ecology within the institutional structures of the NSF. On the other hand, several negative lessons had been learned from the IBP experience. These lessons especially concerned the big comprehensive ecosystem models that had been developed by the various US Biome projects. A specific ideal had been implemented in ecosystem research, namely the development of comprehensive, synthetic models. Initially, it was hoped that they would accurately mimic the behaviour of biome-wide ecosystems. The models were built on the further assumption that the state of an ecosystem would be determined by climatic factors (the so-called non-biotic factors), such as solar radiation, rainfall, and so forth. Already during the final years of the IBP, the ecological community judged the IBP models to be too big and clumsy, containing unnecessary detail, and with deficient dynamic behaviour. They did not account for spatial differentiation, did not account for critical differences between functionally similar species, and they assumed (wrongly, the critics said) the ideal existence of a single stable state in any given ecosystem (Kwa, 1993).

In the wake of the IBP there was a twofold development. Among the ecosystem modellers, a style prevailed which focused on analytic, rather than synthetic models. To begin with, models usually included a much smaller number of variables, thereby sacrificing comprehensiveness. Second, most models focused on specific aspects of ecosystem functioning. During the days of the IBP, they were called 'partial models', referring to a big synthetic model, even if that model did not yet exist. The 'ontological' corollary of relinquishing comprehensiveness was that modellers henceforth would not aim to build physical analogies of ecosystems. Instead, they upheld pragmatic criteria for predicting specific aspects of an ecosystem's behaviour.

This development was even more pronounced among ecologists who did not identify themselves as ecosystem ecologists. Evolutionary ecologists saw their criticism of IBP models vindicated, namely, their criticisms that the IBP had focused too much on non-biotic factors as the ultimate driving variables of ecosystems. Consistent with their own backgrounds, evolutionary ecologists focused on inter-species interactions, and on contingent developments in ecosystems. These ecologists claimed that the IBP systems ecologists had not appreciated either the productive role of disturbances in the functioning of ecological systems. It is telling that none of the more outspoken leaders of the IBP Biome projects were invited to join the IGBP planning activities. One of them made bitter comments on how the bad press the IBP received (which he thought was undeserved) had relegated ecosystem ecologists, and ecology as a discipline, to a position of second rank within the IGBP.[17] The irony was that the IBP approach to the steering of ecosystems was closer to what physical earth scientists expected ecology to contribute to the IGBP.

The increased status of the evolutionary ecologists was reflected in their dominance of the scientific steering committee of the GCTE, the ecological core project of the IGBP. Brian Walker was invited to create the committee, at the invitation of Thomas Malone and James McCarthy.[18] This was remarkable. In view of the expectations by the meteorologists and oceanographers about the role of ecological factors in the Earth system, it could not be assumed that evolutionary ecologists would best serve the interests of the other disciplines within the IGBP. McCarthy apparently gave precedence to the current status of evolutionary ecology over the programmatic demands made by other disciplines.

Two of the steering committee members, Harold Mooney and Hank Shugart, had been involved with the IBP, but Mooney had been the principal investigator of a comparatively small project, which was brought into the IBP as a sort of counterweight to the Biome projects. Mooney's project had a very different design than the other projects. Hank Shugart, a member of the core modelling team of the Eastern Deciduous Forest Biome project, had made a noteworthy transition to the evolutionary ecology point of view soon after the IBP ended.[19] It thus seemed that within the IGBP ecology would be given a chance to turn the global agenda toward the more 'traditional' focus of ecology on local ecosystems – specific plant and animal communities associated with specific landscapes.

However, at the same time, some extremely simplified representations of vegetation had been developed, primarily through the invention of the so-called vegetation index. These representations strongly influenced the GCTE, as we shall see. They were also more successful than Daniel Botkin's initial attempt to develop a mesoscale 'space ecology'.

A Pre-International Geosphere-Biosphere Programme Encounter Between Space Science and Ecology

In the late 1970s, Botkin received a grant from NASA to work on a project that aimed to develop an ecologically sustainable life-support system in space. His part of the project centred on questions about the circulation of oxygen and carbon dioxide. He was joined by the mathematician Berrien Moore, among others. They received funding for five years. Botkin also organized, at NASA's Division of Life Sciences, a series of workshops on the question of whether there is indeed such a thing as a global ecology.

Subsequently, Botkin took up questions about whether or not remote-sensing techniques were an effective means for estimating the productivity of the world's agricultural and natural ecosystems. This investigation was the beginning of the end of Botkin's relationship with NASA. At that time, he teamed with Robert McDonald, a scientist at the Johnson Space Center, and they advocated using the Landsat satellite, with its 30 m resolution for remote sensing, rather than the Advanced Very High Resolution Radiometer (AVHRR) on NOAA's TIROS-N weather satellite, which had a resolution of between 1 and 4 km. Landsat would provide comparatively detailed samples of the Earth's vegetation, which Botkin wanted to use to arrive at a general picture of the Earth through statistical sampling. AVHRR turned out much less detailed, but it could provide complete 'wall to wall' pictures of the Earth, in particular through the efforts of Jim Tucker at the Goddard (Calder, 1990; Mooney, 1998). Based on the so-called vegetation index (see later), such pictures were eminently suitable for reproduction in the *National Geographic* centennial edition, and NASA decided to go ahead with the AVHRR technique. Botkin finally lost the argument when NASA transferred responsibility for Earth vegetation mapping from Johnson to Goddard. Goddard's director had supported Tucker since 1982 (Calder, 1990). According to Botkin, the episode proved that, for NASA, raising public support, in this case through *National Geographic*, prevailed over scientific interests.[20] However, financial considerations may also have played a part. Landsat was privatized at the time of NASA's decision in favour of AVHRR, and EOSAT, the company that owned Landsat, charged up to US$4000 for a single picture. The AVHRR pictures were obtained free of charge (that is, already covered by government funding).[21] Botkin was asked to join the Bretherton committee, which importantly defined the future of the earth system sciences, and his name remains on the list of its members. However, feeling he no longer had a role to play, he did not show up for its meetings.

The Discovery of 'The Big Leaf'

A chance discovery by Jim Tucker marked the beginning of a style of global ecology that became successful if not dominant in the US GCRP and the IGBP. A scientist at Colorado State University, Tucker worked on the radiative properties of plant leaves. He found that green plants look remarkably different when photographed in (visible) red light rather than infrared light. On the basis of this discovery, Tucker developed the 'vegetation index'.[22] When Tucker transferred to NASA's Goddard, he found out that AVHRR on NOAA's satellite suited him better than NASA's own Landsat. Among his first accomplishments were year-round pictures of the African continent, which showed clear-cut differences in seasonal patterns of different types of vegetation, such as rain forests and savannahs. But what exactly the vegetation index measured was not yet known. At first, scientists hoped that the vegetation index would be a measure of standing biomass, but this proved not to be the case.

The meaning of the vegetation index was elucidated with the help of Piers Sellars, a British ecologist then working at the University of Maryland. Through cooperation with Tucker, starting in 1983, Sellars developed the Simple Biosphere Model (SiB), which he presented in 1986. The SiB showed, or suggested, that among other things the vegetation index is a universal measure of plant *growth*, or plant productivity. Differences between the leaves of deciduous trees, pine needles and cacti are not important. In cooperation with carbon dioxide expert C. David Keeling, it was further established that there was a good relationship between the vegetation index and carbon dioxide uptake. A recent ecology textbook states this as follows: '(The index is) *believed* to be related to photosynthetic activity' (Shugart, 1998: 329, my emphasis). Assuming that plant growth entails evaporation, the vegetation index also measures another important link between vegetation masses and the atmosphere: water. The SiB modelled these interactions, and hence suggested itself as a necessary addition to General Circulation Models, which would thus become more 'realistic'.

Experiments to confirm the relationship between the vegetation index, as measured by remote sensing, with measurements of vegetational processes in a Kansas prairie field site were first undertaken in 1987 and 1989, with support from NASA (Calder, 1990; Mooney, 1998: 54). It was found that the prairie works as one very big leaf in its chemical exchanges with the atmosphere.

It may seem strange that a measurement technique with so many uncertainties in its interpretation caught on so rapidly. The fact that it could be *visualized* so effectively by technological means explains it, and this is especially remarkable because the vegetation index is a complex signal, which as such cannot be seen (since it is based on a calculation on the basis of two signals, only one of which is visible to the human eye). Yet Tucker's approach of mapping the earth with low-resolution images did more than excite the editorial board and readership of *National Geographic*. Harold Mooney recounts how his interest in global change research was captured by whole-Earth maps, combined into a 'movie' that showed the vegetation index changing over the seasons (Mooney, 1998: 51). Stunning presentations of this kind can now be made on an ordinary personal computer.

The International Geosphere-Biosphere Programme's Ecological Core Project

The GCTE project was the stage on which the possible tensions were played out between IGBP interests and the theoretical concerns and scientific styles of the ecologists. The GCTE was accepted as a core project in 1990, and became 'operational' two years later, when a number of research projects were admitted into its Core Research Programme. The objectives of GCTE were twofold: to predict the effects of climate change, atmospheric composition and land use on terrestrial ecosystems, and to determine the feedback of these effects on the atmosphere. Within GCTE were three foci: one on ecosystem physiology, led by Mooney, a second on 'change in ecosystem structure', led by Ian Noble, and a third on agriculture and forestry (International Geosphere-Biosphere Programme, 1992). By 1996, about 400 scientists and technicians from 39 countries were active in the project, with a total annual expenditure of more than US$20 million. GCTE was run from a Core Project Office in Canberra, Australia, until its completion in December 2003 (Walker & Steffen, 1996; International Group of Funding Agencies, 1998: 20).

Brian Walker (Australia) was GCTE's first Chairperson, a position that he held during two tenures at the GCTE Scientific Steering Committee, from 1990 to 1996. Walker had been active previously in an international ecological research programme of the International Union of Biological Science (IUBS): Responses of Savannahs to Stress and Disturbances (RSSB). This programme ran throughout the 1980s, which the UN designated as the 'Decade of the Tropics'. The formal end of the RSSB in 1990 coincided with the beginning of the operational phase of the IGBP, which was also when planning for the GCTE started. Together with Walker, two more scientists who had been active in the RSSB became members of the GCTE steering committee: Ian Noble, also from Australia, and Jean-Claude Menaut, of the École Normale Supérieure in Paris. Among the other members of the Steering Committee were Mooney, Herman (Hank) Shugart in the USA, and Jan Goudriaan in the Netherlands. Mooney was GCTE's vice-chair. Jerry Melillo was involved as ex officio member, and was an advisor to the steering committee.

The members of the steering committee represented diverse scientific specialties and interests. The inclusion of ecosystem physiology as a subject was dictated by the needs of the IGBP as a whole, and yet the members typically represent post-IBP ecology, which had evolved toward a certain emphasis on local dynamics in spatially differentiated ecosystems. Ian Noble is a case in point.[23] The scientific planning of the GCTE reflected this orientation. Hank Shugart does not recall that any scientific agenda was imposed on the steering committee, or that the IGBP bureau in Stockholm told them which problems to address, and how.[24]

At a different level, the steering committee had to grapple with the significance of remote sensing from satellites. They did so for the GCTE programme in particular and the field of ecology in general. No one had to impose this agenda on the steering committee: it was there from the outset. Shugart, for instance, was already a member of NASA committees, advising on the development of remote-sensing techniques.[25] The steering committee saw itself faced with a flood of proposals for the GCTE from individual scientists, which reflected the popularity of the vegetation index and the appeal of models that extrapolate from very local physiological (plant leaf) characteristics to biome-wide or global models (Mooney, 2000: 24). At a planning meeting in Canberra, Canada, Jean-Claude Menaut spoke out against what he termed 'big leaf' models.[26]

'Big leaf' models hardly suit the concerns of ecologists investigating mesoscale ecological dynamics. A case in point is the so-called Terrestrial Ecosystem Model (TEM), which was published in 1993 by Jerry Melillo and others (Melillo et al., 1993). Described as a 'large-scale model' – TEM encompasses the whole earth – it is in fact, from a dynamic point of view, a very simple model in which all vegetation is represented by just one compartment, and soil by another. Models such as TEM usually focus on material transfers between atmosphere, soil and vegetation. The occurrence of transfers is almost exclusively located in the physiological processes at the leaf level, which implies that such models cannot even envision changes in species composition, and so they would be unsuitable for developing a new ecological model of landscapes subject to climatic change. Another example is BIOME2 by I.C. Prentice and others (Prentice et al., 1993). In 1995, it was part of a comparative analysis of vegetational changes resulting from an increase in atmospheric carbon dioxide. Not included in this comparison were several other models, which likewise focus on so-called 'abiotic forcings' on terrestrial ecosystems, among which was the Dynamic Global Vegetation Model, developed by Wolfgang Cramer in Potsdam, Germany. The new global orientation, with large-scale, 'low-resolution' images, was also expressed at the annual conferences of the Ecological Society of America.[27]

Apparently there is a substantial effort underway with regard to this style of modelling in the context of the IGBP. This makes it all the more noteworthy that the leadership of the GCTE core project had a very different theoretical orientation to the question of how useful ecology could be for global climate change research. As we will see, Menaut and Shugart, among others, remain critical of homogeneous landscape models such as TEM.

By examining the work and careers of some of the leading ecologists involved in the GCTE, I hope to deepen our insight into how diverse, and sometimes contradictory, points of departure coexisted in one programme, and on how ecologists reconciled a global focus with their perception of ecology's mesoscale research agenda. Three were members of the first scientific steering committee of the GCTE while the fourth (Saugier) was instrumental in setting up the ecology part of the French contribution to the IGBP.

Ecosystem Physiology in the USA: Harold Mooney

Harold Mooney sets high standards for ecological research in order for it to be relevant for global climate change research. According to his standards, critical species differences at the ecophysiological level and complex biotic interactions at the community or ecosystem level should be accounted for, even if this will make it difficult to deliver the required generalizations on continental or global levels. 'Simple extrapolations from short-term physiological measurements to predictions of long-term whole system responses [are] untenable', wrote Mooney (1996: 17). At the very least Mooney's approach implies that work on global scales in ecology should be checked against mesoscale work.

Mooney (b. 1932) received his primary botanical and ecological education at the University of California at Santa Barbara, where he was taught to analyse plant associations on the basis of the ideas developed by Henry Gleason. Gleason had explained the distribution of plants in the environment by reference to their individual requirements, rather than through some cooperative mechanism between plants, or by reference to a higher organismic 'whole', as Frederic Clements had taught. Even though Mooney would not deviate much from Gleason's assumptions throughout his career, he also developed interest in Clements' ideas (Mooney, 2000: 5). Subsequently, Mooney moved to Duke University, where he was a graduate student of Dwight Billings. Billings' work was oriented toward physiological ecology, the adaptation of plants to their local environments.

The physiological limitations of plants' abilities to exploit their environments and the effects of such limitations on their spatial distribution continued to be Mooney's research theme during his first job, at the University of California at Los Angeles, and also when he led an IBP project on convergent evolution, comparing the Mediterranean climates of Chile and California from 1967 onwards. Extending his research activities to extreme environments, Mooney's interest shifted to focusing on the role of disturbances (such as fire) and stress on ecosystems. The importance of fires in natural ecosystems is now generally acknowledged in nature conservation. At that time, he also developed an interest in the effects of biotic interaction between plants, something that the Gleasonian approach usually overlooked, but which was central to the evolutionary explanations in the field of population dynamics (Mooney, 2000: 14).

From within the planning activities of the GCTE, Mooney concerned himself with one basic scientific question: what is the effect of the increase of carbon dioxide in the atmosphere on biological systems? Mooney proposed that this should be studied on the 'ecosystem' level, as he criticized the fact that the effect of increased carbon dioxide was mainly studied at the level of the individual plant. He further proposed that larger-scale field experimentation should be promoted, most importantly free-air carbon dioxide enrichment. Mooney also directed part of his own research to this type of work. He and his co-workers showed that enrichment with carbon dioxide of an annual grassland led to, among other things, species changes (Mooney, 2000: 24–25).

Will climate change facilitate the invasion of ecosystems by alien species? As Mooney sees it, the species make-up of a given region cannot be regarded as the passive reflection of a certain state of the climate. Chance effects play an important role, and, according to Mooney, a temporary change of the climate may lead to the permanent settling of a new species, with many potential consequences for ecosystems as a whole. Current concerns about biodiversity are at the centre of Mooney's research, and he promotes it also on the programmatic level, both within and outside the GRCP. He chaired an ad hoc group of ICSU that designed the operational plan for Diversitas, an international biodiversity research programme (Mooney, 2000: 28).

Diversitas and the GCTE project promote the following basic idea: global change effects such as global warming, together with changes in human activity, may lead to the extinction of species in an ecosystem, or to the introduction of alien species which may, in turn, lead to changes in the ecological community structure, including loss of other species. A second assumption, demonstrated in some field studies, is that new species might utilize available food resources very differently, which would have consequences for a number of other species. Such changes in community structure and resource utilization might, in turn, feed back on the atmosphere and thus have further global change implications. One simulation study showed that diversity itself may have such consequences: a deciduous forest consisting of a community of nine different tree species showed 30% greater photosynthetic uptake of atmospheric carbon dioxide than a forest consisting of just one tree species. A conclusion drawn from this is that ecosystem models that fail to account for diversity in the physiological features of plant species may produce deficient representions of the response of the earth's biota to global environmental change (Chapin et al., 1998).

Ecosystem Physiology in France: Bernard Saugier

When the French endorsed a national programme for the IGBP, jointly prepared by the government and the Academy of Sciences, it showed a certain bias towards the physical sciences. In reaction to this bias, Bernard Saugier (b. 1943), an ecologist at the Université de Paris at Orsay (Paris-Sud), organized a subcommittee on ecology under the Academy committee. Among the other members was Jean-Claude Menaut, a CNRS researcher at the Ecology Laboratory of the Ecole Normale Supérieure in Paris. The committee persuaded the government to earmark IGBP funds for ecological research, and as result the programme 'Ecosystems' was set up at the Ministry in 1989, presided over by Saugier. The programme received the modest sum of FF 3 million per year, divided over five projects, but it was enough to buy some equipment for the various projects.[28]

Saugier's ecophysiological work at Paris-Sud is comparatively straightforward (Saugier, 1995). Saugier emphasizes technical innovation in experimentation, and he mostly steers away from theoretical positions within ecology. The scientific aim of his group is to predict the effect on forests of the increase of atmospheric carbon dioxide resulting from global climate change. The group's work fits the ecophysiological focus of the GCTE well, with its emphasis on experimental carbon dioxide enrichment of forests. The treatment of individual trees (young beech in particular) is accompanied by a modelling effort to enable the team to extrapolate the results to the forest as a whole. In addition to trees, the group also studies *Arabidopsis thaliana* (a herb) to see whether increased atmospheric carbon dioxide has an effect on its genotypic equilibrium.[29] The group has working relationships with the Centre d'Etudes Spatiales de la Biosphère

(CESBIO) in Toulouse, where the French remote-sensing effort is located. In Orsay, remote-sensing data are compared with field data in order to make better biological sense of the former.[30] Saugier's research is an example of work with strong affinity with global models that deploy measures of the vegetation taken as a single entity. However, it can be argued that Saugier takes advantage of such models for his primary concern, which is experimental ecophysiology.

Saugier was educated as a physicist at the Ecole Normale Supérieure, rue d'Ulm. After receiving his *licence* degree, he obtained a Centre National de la Recherche Scientifique (CNRS) appointment, which he served at Montpellier, in 1966. Saugier went to work with an ecophysiologist who studied fluxes of matter between plants and the environment in controlled climate rooms. This work was conducted at the Centre d'Etudes Phytosociologiques et Ecologiques (CEPE), a CNRS laboratory created in 1961.

At the time, Montpellier was the seat of another famous French 'phytosociological' research station: Josias Braun-Blanquet's private institute, the Station Internationale de Géobotanique Méditerranéenne et Alpine (SIGMA). SIGMA had many ties with CEPE. Such ties were indicated by the term *phytosociologie* (sociology of plants) in CEPE's name, which refers to the approach in plant ecology founded by Braun-Blanquet in the early 20th century, known as the Zürich–Montpellier school. The school's basic assumption is that plants form 'associations', which exploit their environment as if they were table companions.

In the 1950s, the US ecologist R.H. Whittaker applied Henry Gleason's critique of holistic approaches in ecology to the Zürich–Montpellier approach, while retaining some of its more technical analyses.[31] Yet for quite some time, on the European continent, the Braun-Blanquet school remained relatively immune to criticism. It continued to enjoy an important place at the universities and in nature conservation programmes in France, the Netherlands, Germany and Switzerland. In France, the turnaround came in about 1975, when the CNRS began to withdraw support from the school. In 1988, in a somewhat belated but still highly symbolic event, CEPE saw its name changed to the Centre d'Ecologie Fonctionnelle et Evolutive.

Saugier himself had not built an allegiance to the Braun-Blanquet school, and his more physics-oriented work did not necessitate such an allegiance. However, Saugier also remained aloof from the other theoretical approaches prevalent in ecology.[32] For example, he expressed disappointment with the lack of interest for functional points of view among the adherents of the Zürich–Montpellier school.

This changed in 1969, when Saugier went abroad. He joined Bob Coopland's ecosystem modelling group at the University of Saskatchewan, and was immediately impressed by George Van Dyne, the leader of the IBP Grassland Biome Project. Van Dyne frequently joined the group and taught about ecosystem modelling and how it provided a synthetic view of ecosystems. Saugier developed a model for the microclimate of the grasslands. He did so with the help of a computing team, which analysed the vast amount of data he had gathered in the grasslands. In 1971, when Saugier returned to France to continue his work with Eckhardt, he introduced ecosystem modelling in Montpellier.

Yet Saugier did not buy Van Dyne's modelling approach wholesale. He thought that the comprehensive model of the Grassland Biome Project was too simple for his own purpose – which was to develop a model of soil-water relationships – and he did not contribute to ELM either. From a practical point of view, the ecosystem approach made sense to him, with its emphasis on material and energy fluxes, and he was acquainted with the work of Lindeman, Odum and Duvigneau. But he did not think of himself as contributing to a 'theory' of ecosystems.[33]

When Saugier was offered a full professorship at the University of Paris-Sud at Orsay, he was able to attract several CNRS researchers to his team.[34] In 1985, he responded to an initiative by Marianne Mousseau at the CNRS to work on carbon dioxide fixation in various ecosystems. Saugier contributed his own expertise on photosynthesis in forests. Out of this formed a European network concerned with carbon dioxide fixation. He therefore was well prepared in 1988, when he heard about the IGBP.

Meanwhile, from 1982 on, Saugier had started to take part in a project to make remote-sensing data available for ecological research, in cooperation with the Laboratoire d'Etudes de Recherches Télédection Spatiales (LERTS) in Toulouse, an institute that would later merge into CESBIO. At first, this was a difficult relationship. Gilbert Saint, the Director of the institute, was responsible for the development of VÉGÉTATION, a remote-sensing instrument aboard SPOT and operational since 1998. It is similar to the US AVHRR, making low-resolution images, repeated at short time-intervals. Seasonal growth could be closely monitored with this instrument. Saint, a *polytechnicien*, was a space engineer. The insights of the ecologists did not automatically impress him. In defence of Saint, it might be said that his first priority was to get the physics of measurement in good order – this at least is the view of Nicolas Viovy, who worked at LERTS until 1993.[35] But decisions as to what kind of detecting equipment should be used are not innocent, as we have seen in the case of the opposing views of Daniel Botkin and Jim Tucker.

Regional Studies in the USA: Hank Shugart

Herman H. Shugart (b. 1944) is Professor of Environmental Sciences at the University of Virginia. A particular focus in his work is on gap models, which describe patterns of development in forests and landscapes from the vantage point of the life histories of individual trees.

Shugart completed his master's thesis at the University of Arkansas and went to the University of Georgia to obtain his doctoral degree. He had been attracted by the reputation of Eugene Odum, the Director of the Institute of Ecology at that university. There he worked with Bernard

Patten, by then well known for his systems ecological approach. At first, however, Shugart continued to pursue research he had begun with his master's thesis, which employed statistical (multivariate) analysis of habitat selection by bird populations. That methodology was virtually the opposite of the systems approach in ecology, and was influenced by Gleason's individualistic outlook, as developed by R.H. Whittaker.

Shugart was drawn to systems analysis, and his first job was at the Oak Ridge National Laboratory (ORNL; TN, USA). Whereas the University of Georgia was primarily known for Odum's holistic vision of ecosystems, research on ecosystems at ORNL was more formally mathematized within the framework of systems analysis. Patten had worked there before he moved to Georgia. In an arrangement with the University of Tennessee, Shugart taught the systems ecology course that had previously been taught by Patten, Jerry Olson and George Van Dyne. Shugart was assigned to ORNL's modelling team, led by R.V. O'Neill, which coordinated the various modelling efforts of the Eastern Deciduous Forest Biome of the IBP. As Shugart recounted:

> We would go on the road, build a model with a local scientist, and figure what we had done. We did that for a year and a half. One day we talked about fish, another day about decomposition. We discussed constantly what is to go into a model and what is to be left out.[36]

But Shugart had by then partly lost interest in the systems approach, and he returned to his point of origin where he encountered a novel approach to modelling ecosystems: the so-called 'gap models'. Gaps are open spaces in forests, and gap models study their formation (usually associated with the death of a large tree) and consequences for the forest ecosystem. Gap models were initially developed by Daniel Botkin, and are now seen as belonging to a class of models called individual-based models, which has roots in population dynamics (Botkin et al., 1972; Botkin, 1990: 118). When Botkin developed the first gap model, he was at the IBM Watson laboratory, where his model existed only in an internal IBM memo, dated 1970, which was passed on to Shugart by Botkin's colleagues J.F. Janak and J.R. Wallis. Shugart contacted Botkin, and this exchange was the start of a volume on forest succession that they co-edited (West et al., 1981). To Shugart, one advantage of the gap models was that they brought him back to his interest in natural history and the life cycle of plant species. They also promised to be a key to understanding the patchiness of a landscape, its spatial heterogeneity.

His exposure to the work of C.S. Holling in 1972 further strengthened Shugart's orientation to individual-based modelling. Holling advocated a view on ecosystems that stressed non-equilibrium-seeking behaviour, and he developed mathematical tools for rendering the shift of an ecosystem from one stable state to another. The possibility of developing mathematical models also appealed to Shugart. Others at ORNL, notably D.L. DeAngelis, also started working on individual-based modelling. Shugart remained at ORNL until 1984, when he transferred to the University of

Virginia. Berrien Moore, who was interested in Shugart's work on landscapes, drew him into serving on NASA committees. Moore had continued on Botkin's line of grants when the latter stopped working with NASA.

In his continuing work on the consequences of global climatic change for ecological systems, Shugart uses gap models and related individual-based models. Central to his concern are differences in spatial scale of ecological responses to, for instance, a doubling of carbon dioxide in the atmosphere. Starting from the observation that the world's landscapes typically are fragmented, mosaic-like, Shugart is interested in how changes at the micro-level of individual plants express themselves on the meso-level of landscapes. Shugart's model experiments suggest that relatively simple changes at the species level can have important and complex consequences at the landscape level. For instance, a small differential in the growth rates of trees may lead to significant changes of the species composition over large areas of forest, and also alter the pattern of landscape fragmentation, pushing boundaries between various areas in one direction or the other. Yet Shugart cautions that a circularity may underlie such predictions, since they are derived from retrospective 'tests', based on reconstructed paleo-ecological communities, while the paleoclimate data that are fed into the model are themselves inferred from palynological (pollen) evidence (Shugart, 1998: 372).

While small changes at the species level may lead to big changes at the landscape level, the important questions concern the consequences of climate change at a continental or global scale. It remains to be seen whether individual-based modelling will lead to significantly different predictions than models based on homogeneous landscapes. For the latter models, scaling up from leaf to biome is achieved less problematically. Recently, it has been argued that individual-based models cannot be developed in a 'bottom-up' manner to make systems-level predictions, as required for biome-wide or continental-scale problems (Grimm, 1999). However, A.M. Solomon did manage to extend the applicability of gap models from individual sites to an important part of the North American continent. His model made 'physiognomically' realistic predictions of the effects of global warming on existing landscapes. Moreover, his model predicted that these landscapes would undergo changes in species composition for hundreds of years after the simulated atmospheric changes had ceased (Shugart, 1998: 373).

Critiquing simpler and more homogeneous models such as TEM, Shugart argues that one of their basic procedures – scaling up local ecological processes to biome-wide or even global scales – is usually not possible (Shugart, 1998: 414). Shugart (1998: 415) also does not share another of the basic assumptions of homogeneous models, which is that ecological processes seek equilibrium. Moreover, according to his analysis, the changes in vegetation structure that different models of global climatic change predict are in most if not all cases a logical consequence of their basic assumptions about the controlling (climatic) factors (Shugart, 1998: 440).

Regional Studies in France: Jean-Claude Menaut

Jean-Claude Menaut is an ecologist at the Ecole Normale Supérieure (ENS) in Paris, appointed by the CNRS. Trained as a field naturalist, in 1999 he became director of CESBIO, the spatial research laboratory in Toulouse that formed that year from a fusion of LERTS and another institute for space research. Both institutes had employed ecologists on their staff, but joint projects had not got off the ground – a fact that the CNRS and the CNES recognized. As its name indicates, CESBIO's mission is to understand the functioning of the biosphere in relation to the climate and human activities, largely on the basis of satellite data. Menaut's appointment is all the more remarkable when we recall that the previous director of LERTS had been a space engineer.

In 1990, during his tenure with the steering committee of the GCTE, Menaut criticized what he scathingly called the 'big leaf approach': the scaling up, with the help of models, from a very small plot of land to a continental or global level. According to Menaut, this was simplistic biogeography, inferior to work done by biogeographers 20 years earlier. Menaut extended this criticism to (low-resolution) remote sensing, which was used at the time to measure the vegetation index. Remote sensing was used to discriminate among biomes, and to make crude estimates of primary production, which Menaut considered simplistic and uninteresting.

Menaut advocated the alternative approach of taking mesoscales – landscapes – as the point of departure. In his view, the landscape is the appropriate 'local unit' that is constrained by the global climate changes, and this local unit, in turn, influences the global processes represented in GCM. Landscapes, as he defined them, consist of an amalgam of different ecosystems, with a particular spatial organization. Spatial organization is crucial: Menaut argues that there are critical differences between a hill with a forest on top and with a wheat field below, and vice versa, even when they occupy equivalent amounts of surface area. Menaut speaks of the 'emergent property' of the landscape, which is lost in low-resolution vegetation mapping through remote sensing. Instead, very-high-resolution data are needed from various sources (including remote sensing). These data may be scaled up, but not by mere summation. Only then can they inform the interpretation of low-resolution data. When, conversely, low-resolution data are assembled, they have to be complemented by knowledge of the local terrain.

There is an additional advantage to models of a landscape, and of a region (a slightly higher-order landscape). They are able to attract a political constituency, and a regional political constituency is what CESBIO supports in south-western France (the région Midi-Pyrénées). A region is, therefore, both an ecological and political–administrative unit.

When global change is redefined as *regional change*, the IGBP becomes politically concrete and meaningful, and Menaut's model clearly taps into this politico-ecological unity. One consequence is that the CESBIO's work is meant to be relevant to *users*, and not exclusively for scientific communities.

In 2000, a new IGBP core project was initiated: Regional Aspects of Global Change and Interactive Processes at the Land–Atmosphere Interface (RAGC). Menaut was active in its early conceptualization, and through this project his concept of the regional may emerge as a strong alternative to the global. As a rhetorical category, regional models may also serve to rally members of the atmospheric research community, by appealing to the original mission of meteorologists to forecast the weather, along with geographers and other parts of the global change community.

Conclusion

Mooney, Shugart and Menaut acted as champions of global research and interdisciplinarity, and they demanded reform in their own discipline. Yet these same ecologists did not give up ecology's focus on mesoscale field research. On the contrary, global change provided them with a new context to investigate dynamic processes in plant and animal communities. They resisted the reduction of ecological research to a scaling-up of physiological processes, and as members of the GCTE steering committee they had the power to follow through. At the same time, they acknowledged the importance of physiological ecology to global change research, and they contributed to its planning. Saugier is an example of an ecophysiologist who pursued his original research programme with new techniques (remote sensing), rather than making his research subservient to other disciplines.

We might be led to conclude that appeals to 'interdisciplinarity' and the 'global', along with the technology of remote sensing and shared data sets, were boundary objects enabling cooperation between ecology and other earth sciences, which permitted those disciplines to pursue their own research agendas. Viewed with hindsight, this might seem to be an unproblematic outcome of the story of ecology in the IGBP. But such a view would ignore pressures and tensions that were apparent during the initial phases of the programme, and which had substantial influence on later developments in global-scale ecology. 'Interdisciplinarity' would better be viewed as a boundary object *and at the same time* a strategy to intervene in the development of ecology.

Ecology was profoundly affected by interdisciplinary cooperation in the context of global change research. Remote sensing, and in particular the vegetation index based on that imaging technique, might have placed a sizable part of ecology on a different footing. It required scaling up the 'object' of ecological research to a degree that far surpassed its disciplinary traditions. The technique imposed itself at a time when ecology was heading in another direction – away from the grandiose biome models developed in the early 1970s, at the time of the IBP. Such models held genuine appeal to a number of individual ecologists, but, as we have seen, such appeal was mitigated by other developments in the discipline as a whole.

There was no strategist or mastermind who can be credited with the overall design and implementation of the interdisciplinary approach in the earth sciences, though, of course, we can name a few key individuals. In the USA these would be, in the order of their appearance in the story, but not necessarily in order of importance: Thomas Malone, John Eddy, Francis Bretherton, James McCarthy, Harold Mooney, Robert Corell, Mike Hall and Shelby Tilford.[37] NASA could be considered an actor in itself: a very big player reaping large benefit from the adoption of remote sensing as the leading technology of the US GRCP and the IGBP. However, NASA was not in control of the overall development of these programmes. Nor for that matter was government 'policy' or 'the physical earth sciences' in control. In one sense, the Earth System science research programmes were the contingent outcomes of interactions among these and other individuals; in another they were outcomes of a strategies embodied in the discursive notions of 'interdisciplinarity' and 'the global', and in remote-sensing technology.

If a strategy is at work, we might also ask whether there is resistance. Ecologists have turned the technology of remote sensing itself into a battlefield, sometimes to their own advantage, and to the advantage of ecological 'local knowledge' itself.

Notes

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1. This aspect of boundary objects brings it closer to the 'standardized packages' discussed by Joan Fujimura (1992).
2. Author's interview with Thomas Malone, West Hartford, CN, USA, May 2000.
3. Malone, interview.
4. Author's interview with James McCarthy, Cambridge, MA, USA, November 1999.
5. Francis Bretherton, James McCarthy, Berrien Moore, D. James Baker and Kevin Burke were members of both the US National Committee for the IGBP (1988) and the Earth System Sciences Committee (1988).
6. Nicknamed 'the seven tombstones' (referring to their graphic appearances in the Report). Solar–terrestrial physics ended up last.
7. At the origin of this problem was the fear of US intelligence services for non-classified high-resolution images. When President Reagan lifted the ban on high-resolution images to enable Landsat to compete with SPOT, it was too late. See Lambright (1994).
8. Author's interview with Marie-Lise Chanin, Verrières-le-Buisson, February 2000.
9. Author's interview with Robert Corell, Washington DC, USA, November 1999.

10. Author's interview with H.H. Shugart, Charlottesville, VA, USA, November 1999.
11. Author's interview with Ian Noble, Amsterdam, The Netherlands, July 2001.
12. Corell, interview.
13. Author's interview with Daniel Botkin, Washington, DC, USA, May 2000.
14. Author's interview with Harold Mooney, Stanford, CA, USA, November 1999.
15. The degree to which ecology as a field is orienting itself towards global change research is in need of investigation. According to Mooney, there is an appreciable effect, which to date has not yet been quantified.
16. Shugart, interview.
17. Author's interview with Orie Loucks, Indianapolis, IN, USA, October 1985.
18. McCarthy, personal communication, September 2000.
19. Shugart, interview.
20. Botkin, interview, and Botkin's letter to author, 13 May 2000.
21. Author's interview with Tony Janetos, Amsterdam, The Netherlands, July 2001.
22. In full: Normalized Difference Vegetation Index (NDVI). The NDVI is the difference between infrared and visible signals divided by the sum of these values.
23. Noble, interview.
24. Shugart, interview.
25. As a member of NASA's Earth Observing Satellite – Synthetic Aperture Radar Team (from 1988 to 1994).
26. Interviews with Noble and Menaut.
27. Interview with Mooney.
28. Author's interview with Jean Labrousse, at the time *responsible* at the Ministry for Research and Technology, February 2000.
29. See Saugier (1995).
30. Author's interview with Bernard Saugier, Orsay, March 2000. I also benefited from a discussion with Valérie Le Dantec, Orsay, France, March 2000.
31. In 1926, US ecologist Henry Gleason held, contrary to his famous countryman Frederic Clements, that there are no grounds to suppose that many species constitute a super-organism on the landscape or biome level.
32. The Zürich–Montpellier school is an interesting case of a 'local' approach to ecology, in the sense that it is culturally bound to a few European countries. Internationalization in the sciences has been instrumental in making national styles in ecology disappear.
33. Saugier, interview.
34. CNRS researchers have 'tenure' with CNRS itself, and may choose to locate at a CNRS laboratory or a university institute.
35. Author's interview with Nicolas Viovy, Orsay, March 2000.
36. Shugart, interview.
37. With disciplinary backgrounds that attest to the interdisciplinarity of the IGBP and the GCRP: atmospheric sciences (Malone, Bretherton, Hall), oceanography (McCarthy, Corell), engineering (Tilford), ecology and biology (Mooney, McCarthy), earth sciences (Tilford).

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