THE IMPACT OF 'SUSTAINABILITY' ON THE FIELD OF ENVIRONMENTAL SCIENCE

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INTRODUCTION

This paper is about the part the concept of sustainability plays in environmental science. More specifically, I will argue that the present upsurge in its use is transforming environmental science into an interdisciplinary field while up to now it was best characterized as multidisciplinary.

If indeed such a transformation is taking place, I think it is a matter of importance to recognize it for what it is. For one, environmental scientists, particularly in the Netherlands, have spent considerable time and effort discussing the question of whether environmental science at present is multi- or interdisciplinary.[1] One could ignore this discussion and look upon it as a local oddity if it weren't for the existence of particular benefits that an interdisciplinary status brings in its wake. I will not discuss them in any detail but, quite obviously, some of them concern such matters as an improved position within academia, better access to governmental and other funds, more impact on societal decision making, etc. I will return to these issues in my concluding remarks. But quite apart from any such utilitarian concerns, there is another, equally valid reason for raising the issue of interdisciplinarity with respect to environmental science: Issues like this are a matter of intrinsic philosophical interest; they help us understand how science develops.

To be able to distinguish between multidisciplinarity and interdisciplinarity I will make extensive use of ideas first developed by Lindley Darden and Nancy Maull, first in the late seventies. Their papers point out various ways in which branches of science may become unified other than through reduction.[2] Of course, the move from multidisciplinarity to interdisciplinarity largely is an integrative one, a step towards unification. It is for this reason that the analytical machinery employed by Maull and Darden is very useful for my present purposes. Particularly their characterization of the notion of a *field* proves to be very helpful. According to them,

A field can be specified by reference to a *focal problem*, a *domain* consisting of "facts" related to that problem, *explanatory goals* providing expectations as to how the problem is to be solved, special *methods* and *techniques*, and sometimes, but not always, laws and *theories* (italics added),[3]

or,

... a field has: a *central problem*; a *domain* to be explained; *techniques* and *methods* (unique to it or shared with other fields); concepts, laws, and *theories*; *special vocabulary*; and more general assumptions and *goals* more or less shared by those scientists using the techniques in trying to solve the central problem (italics added).[4]

Fields are akin such entities as disciplines and research programs. An apt example of a field is ecology, its focal problem being the question of how it is possible for populations of organism belonging to different species to jointly inhabit a particular slice of the biosphere in roughly the numbers in which they do coexist. Explanations in ecology have to be phrased in terms of relations between organisms, both of the same and different species, and relations between organisms and the physical environment. Relevant ecological facts, of course, come in great multitudes, but existing distribution and dispersal patterns of species certainly belong to them. Fields are related to theories, but differ from them in important ways. Although each theory belongs to some single field, fields may feature none, one, or multiple theories. One of the theories of ecology is the theory that explains the dynamics of predator-prey relations; another one would be the one that explains the way biomass is constrained at different trophic levels: the higher the level, the less the biomass. Fields, finally, possess a special or specific vocabulary, a set of terms that are specific if not unique to a particular field. With respect to ecology the term `trophic level' provides an example, and so do `niche', and `limiting similarity'.

What about multidisciplinary and interdisciplinary fields? Most likely, there exists no hard and fast boundary between them. We are probably dealing here with concepts that cannot be defined or even distinguished from each other through a set of jointly sufficient and separately necessary characteristics. Hence, I will not waste any time in attempting to do so. Nevertheless, I feel one may confidently say that the concepts form the extremes of a continuum. At the one side one finds multidisciplinary fields, characterized by the lack of such things as a single focal problem, uniform explanatory goals, methods and theories specific for the entire field, and a single specific vocabulary. Multidisciplinary fields really are multiple but related disciplines. At the other extreme of the continuum true interdisciplinarity reigns, marked by the presence of a focal problem, uniform field-specific explanatory goals, methods and theories, and a specific vocabulary. Of course, the fields that are part of the multidiscipline do exhibit these characteristics unless they are themselves of a multidisciplinary nature

My investigation of the role of the concept of sustainability in the transition of environmental science from multidisciplinarity to interdisciplinarity will take the following form. First I will look at environmental science as it has been practiced up to now. I will investigate:

i) What focal problem does it have if any?
ii) What are its explanatory goals?
iii) Are there any special methods?
iv) Are there special theories and is there a specific vocabulary?

It will quickly become clear that environmental science as traditionally practiced follows a multidisciplinary approach. However, I will subsequently show that roughly at the time of publication of the United Nations' report *Our Common Future* in 1987, things have taken a different turn. Although from then on the solution of environmental problems still takes input from various disciplines, the introduction of the notion of sustainability has altered environmental science altogether and transformed it into an interdisciplinary field.

TRADITIONAL ENVIRONMENTAL SCIENCE AS A MULTIDISCIPLINARY FIELD

Clearly, environmental science is about the solution of environmental problems. But does traditional environmental science exhibit a central or focal problem in the way that for instance ecology does? There does not seem to be any specific problem that all environmental scientists address. Let us, by way of initial approach to the issue, try to frame a definition of what the class of environmental problems roughly looks like. This definition is not an attempt prescribe once and for all what is to count as an environmental problem and what isn't. Rather, it is an effort to come up with some criteria that according to most would be part of the notion of an environmental problem as it is commonly used.

Environmental problems

As a first criterion, any definition should restrict environmental problems to changes in the physical environment, that is, rule out changes in our social environment (obviously, the physical environment should comprise both biotic (plants, animals) and abiotic (climate, landscape, water and air quality, soil type, etc.) components). This may seem a relatively uncontroversial restriction, but it is nonetheless crucial. Not many would argue, I take it, that the hindrance caused by, say, drug dealing in some neighborhood should count as an environmental problem; nor would many insists that the nuisance of having to live under a totalitarian regime is to be counted an environmental problem. Yet both are human interferences with our environment. The point that disqualifies them as environmental problems is that they are changes *solely* of our social environment. If, on the other hand, we look at something like the depletion of the stratospheric ozone layer, commonly seen as a genuine environmental problem, we are dealing with a change of the physical environment which, admittedly, has consequences for our social environment. A pertinent example would be the dramatically altered attitude of Australians and New Zealanders towards sun bathing. However, since it is the change in the physical environment that has prompted these responses, we are allowed to describe the ozone problem as an environmental problem. So we may provisionally conclude that:

An environmental problem is a change of state of the physical environment.

But the characterization is insufficiently specific. Not all changes of the physical environment are environmental problems. What about natural disasters? A case in point would be the damage believed to be done to the ozone layer by the ashes shot into the air in the June 1991 eruption of Mount Pinatubo in the Philippines.[5] Clearly, this is a change of the physical environment, but should it count as environmental problem?

Consider the case of the scientists who argue that the depletion of the ozone layer is part of a natural process, one that is not linked to the human caused release of chlorofluorocarbons or CFCs as they are often called for short. Or alternatively, the case of those who argue that the current increase in the average temperature of the atmosphere is due to a natural cycle rather than to its increased carbon dioxide content. The very possibility of discussing these matters requires us to distinguish between the `natural' and the `human caused'. Such a distinction is also absolutely necessary in order to be able to adjudicate liabilities. A plant using large quantities of ozone destroying CFCs can be held accountable for damaging the ozone layer. However, Mount Pinatubo isn't answerable to the charge of obstructing the stratospheric ozone production, nor are the inhabitants of the Philippines for that matter. So allowing natural disasters, and other non-humanly caused changes in the environment to count as environmental problems would require us to subsequently distinguish between natural and non-natural ones. In view of these complications I believe, the categories of human-caused problems and natural disasters had better be kept apart right away. This leads to the following definition:

An environmental problem is a change of state of the physical environment which
i) is brought about by human interference with the physical environment.

In this definition a crucial element is still lacking. According to most environmental scientists, environmental science carries an ineliminable normative load. Environmental problems are changes in the environment the consequences of which are somehow deemed unacceptable. Thus it is not the depletion of the ozone itself that one finds unacceptable but rather its effects, increased incidence of skin cancer, crop damage, etc.[6] One finds these consequences unacceptable because, if anything, we should have less skin cancer, larger harvests, etc. Apparently, we are invoking *norms* - with respect to our personal of economic well-being - in calling a particular change of the physical environment an environmental problem. Any definition of the notion of an environmental problem, therefore, should reflect this normative aspect. So we have to add a second clause to the effect that only those human-induced changes of the physical environment are environmental problems that are considered to have (morally) unacceptable consequences. A serious problem, however, is lurking in the back, as an example makes clear.

Suppose a soccer ball lands in my yard kicked their by my neighbor's kids. This constitutes a particular kind of change of state of the physical environment caused by human beings and I really find it an unacceptable infringement of my privacy as the ball knocks down the dill that I have grown so patiently. Although the event obeys all the relevant criteria one would be hard-pressed to call this an environmental problem. Intuitively, a fallen football may be a nuisance, it certainly isn't an environmental problem. Calling it one meets with at least two objections.

First, if the straying ball is a problem at all, it is so on account of my private beliefs about proper child behavior. I may think, for instance, that as a moral rule, children should not pester their neighbors. The event is unacceptable for me because of particular norms I happen to have. These norms need not be shared by others and most probably aren't. Other people will no doubt believe that children should be allowed to play and an occasional ball ending up into someone's yard is no big deal. What this seems to show is that exclusively personal norms - norms that are not shared by others - cannot justify the `unacceptable' in the delineation of environmental problems. Would shared norms do? As it happens, however, the juxtaposition of personal and shared does not get us to the heart of the matter either. To show why this is so, I'll slightly change the example.

Suppose this time my neighbor, the kids' father, decides to cut down a row of trees that fences off his yard from mine. Again we are dealing with a human-induced change of the physical environment. I find it an unacceptable change - for esthetic reasons, I happen to like the trees - but my neighbor obviously doesn't. He wants to replace the trees with a plastic fence which needs no care and thus is much more convenient. The matter of norms being privately owned or commonly shared is not exactly at issue here. For the sake of argument we could assume that over issues like this the population is split in half exactly. At issue is the difference between the norms to which I and my neighbor seek recourse to justify our opinions. I happen to think that trees are beautiful or that they should be saved whenever and wherever possible, that plastic is ugly or that one should use as little of it as possible. He does not care about trees at all (Getting rid of all those leaves in the fall, what a chore!), is oblivious to their beauty, and downplays the effects of extensive plastic use (What difference does one fence make anyway). In order to decide whether we are dealing with an environmental problem, however, we have to make up our minds about who is right and who is wrong here. For that, we need to evaluate the norms in which my neighbor and I ground our respective decisions. Mine are esthetic values, his is a particular kind of opportunism. Obviously, though, what constitutes an environmental problem has a lot to do with the *kind* of norms one uses to justify what is and what is not to count as an acceptable infringement of the environment, not so much with their degree of public support. This lands us squarely into a discussion on environmental ethics, which I will not get into as it would take me too far afield. [7] But this much can be said: If something constitutes an environmental problem it always is the problem of some individual or group of individuals. And, when discussing *the* ozone problem, one is only justified to use this parlance because the ozone problem is a problem with respect to norms shared by almost everybody.

Summarizing the discussion, I arrive at the following definition:

An environmental problem is a change of state of the physical environment which
i) is brought about by human interference with the physical environment, and
ii) has effects which are unacceptable with respect to a particular set of privately owned or commonly shared norms.

Taking stock, although the definition reveals that environmental science has a restricted range of problems it addresses, and thus perhaps may be said to have a restricted domain, a single focal problem seems to be lacking.

Explanatory goals

In the previous section, I characterized environmental science as a problem solving field. Generally speaking, this is not a highly controversial position to defend. Many philosophers have explicitly declared that one should view science as a problem solving enterprise. Karl Popper and Larry Laudan perhaps have been most outspoken in this respect. Laudan even has made an extensive analysis of the ramifications of such a position, something I will turn to shortly. Furthermore, many of those who do not explicitly subscribe to a problem solving stance implicitly endorse it. Darden and Maull are examples of this breed.

How are environmental problems to be looked upon, philosophically? The classification that Larry Laudan has introduced may help us here.[8] He distinguishes between empirical problems and conceptual problems. *Empirical* problems are first order problems, `oddities about the natural world in need of explanation'; they are substantive questions about the objects which constitute the domain of a given science. *Conceptual* problems, however, are second order problems, which means that they arise only with respect to a theory. Laudan discerns two kinds, internal and external conceptual problems. The *internal* kind is about a theory's methodological status: is it coherent, are its basic categories of analysis sufficiently specific, etc.? *External* conceptual problems arise out of a conflict between the theory exhibiting the problem and some other belief about the world, be it a scientific theory or doctrine of a broader scope. Do environmental problems belong to any of these categories?

Ample reflection reveals that environmental problems belong to neither. They are not conceptual problems, as, quite clearly, they are about the empirical world. What else could `a change of state of the physical environment' as mentioned in the definition be taken to mean? In spite of this empirical character environmental problems are not empirical problems *sensu* Laudan either. They are not oddities requiring *explanation*, if anything, they are oddities requiring *remedy*. They are not substantive questions about the objects in the domain of environmental science, if anything, they are practical questions posed by society to science, questions external to science, that is. It thus seems that environmental problems belong to an altogether different category, one that, for want of a better term, one might denote as *practical* problems. I will not pursue the question of how this kind of problems could be related to Laudan's classificatory scheme. I shall rest content by noting that practical problems seem to have the following characteristics: i) they are external to science in that they originate in society ate large, not necessarily in the narrow confines of academia, ii) they are considered solved when their occurrence can be controlled rather than their origins understood. The upshot of these considerations is that Laudan's analysis isn't very helpful, at least at this stage of inquiry.[9] More important for our present purposes, it also implies that the solution of an environmental problem takes a form different than foreseen in Maull and Darden's general scheme.

According to Maull and Darden a problem will be solved by *explaining* it, and there are particular explanatory goals that constrain the set of appropriate explanations. In the case of practical problems *remedies* rather than explanations are what one is after, and a problem is dealt with satisfactorily if its occurrence is prevented or its effects mitigated. So environmental science also falls short of being a true field in this respect. However, in a weak sense, the criterion is fulfilled. As much as Darden and Maull want to constrain proper solutions for (empirical) problems, solutions for practical problems are also constrained by expectations. These expectations are particular norms that allow one to distinguish better from worse solutions. And these norms, of course, directly derive from the norms that allow one to recognize a practical problem in the first place (*cf.* the definition of environmental problems).[10] For example, the depletion of the ozone layer is an environmental problem because, among other things, it poses health risks. Solutions to the ozone problem can then be ranked according to the extent to which they diminish the health risk.

What about the other criteria? In order to investigate their fate we need to take a more detailed look at the practice of environmental problem solving. Let us do so by further examining the ozone example.

Intermezzo: Acting upon environmental problems

Ideally, solving environmental problems can be portrayed as a three-staged process.[11] One may distinguish the phases of the problem's *description*, *causal analysis*, and *solution analysis* or *scenario building*. In the description phase a problem is recognized to be an environmental problem and described in a terminology that makes it fit for further analysis. Since a problem is by definition a change in the natural environment, the domain of the natural sciences, they advance the appropriate descriptive terminology. The problem's causal analysis, phase two, consists of mapping out the causal network that connects the initial human interference with its problematic effects. Somewhere in this web, a node representing the environmental problem under scrutiny is located. Once we have this map we may enter the third phase. In this phase various scenarios are analyzed. In these scenarios interventions that may lead to the problem's disappearance or at least abatement, feature large. The interventions again are human interferences, but this time of a kind different than the ones that gave rise to the problem in the first place.

I will now take a more detailed look at all phases, particularly phases two and three as they reveal most about the character of present-day environmental science.

Phase 1: descriptions and their adequacy

Suppose we are faced with a particular environmental problem, say, the depletion of the stratospheric ozone layer. This is a genuine environmental problem, at least to the extent that we can ignore or discount the effects of natural causes such as the effects of Mount Pinatubo's eruption mentioned earlier. I will not discuss precisely how this could be done. Clearly, this differs on a case by case basis. The general rule is that it is sensible to keep in mind that an analysis of the causal background of some purported environmental problem might reveal that the humanly induced effects are negligible (or even non-existent) as compared to the effects from natural causes. In such cases, we have to revise our original opinion as there is no environmental problem to speak off. Of course, action might be needed to cater for its ill-effects. But then the rubric under which this action is to be carried out should change. We are not in the business of solving an environmental problem, rather, we are trying to alleviate human suffering by offering humanitarian help. But suppose, as is generally agreed, that the ozone problem is a genuine environmental problem.

Phase 2: analyzing the problem's causal network

There is general agreement that the release in the atmosphere of CFCs is by far the main culprit of the thinning of the ozone layer. CFCs are widely used, for instance as propellant in aerosol cans, as blowing agent for the creation of foams (such as Styrofoam), as coolant in air conditioners and refrigerators, and also as cleansing agent for electronic circuitry. They are cheap and chemically almost inert, two factors that explain their wide use. However, in the upper stratosphere the story changes. Up there lies a thin layer of ozone, a highly reactive form of oxygen. It is formed through the action of solar radiation upon ordinary oxygen molecules. In the troposphere, the lowest stratum of the atmosphere, where ozone is also formed, it reacts instantly with other molecules and hence disappears quickly. In the stratosphere, however, there aren't many molecules to react with, which has resulted in the built-up of a thin layer of ozone. The importance of this layer for life on earth is that it absorbs almost all harmful solar radiation, so called UV-B radiation, from reaching the earth.

The CFCs introduced by us in the lower atmosphere have slowly leaked into the upper stratosphere because of their long life time (due to their chemical inertness). There they have affected the ozone layer's protective potential dramatically. In spite of their unwillingness to engage in chemical reactions, solar radiation is able to split off a chlorine-radical (a highly reactive form of chlorine) of the CFCs. This radical reacts with ozone and changes it back into ordinary oxygen, which is a much less effective absorbent of UV-B radiation. To make things worse, the chlorine-radical acts as a catalyst, that is, after having reacted with an ozone molecule it becomes freshly available to engage in a reaction with another ozone molecule. The net effect of all this is that the thickness of the ozone layer (concentration of ozone molecules) will be reduced for years to come, not in the least because it takes about 15 years for CFCs released into the atmosphere to reach the upper stratosphere. [12]

So we now have established a connection between a human interference - release of CFCs in the atmosphere - and an environmental problem - increased intensity of UV-B radiation. If needed, the entire story could be phrased in more exact terms. A numerical relation could be established between, for example, the amount of CFC released per annum for some year, the average ozone concentration some number of years later, and the average intensity of the UV-B radiation reaching the earth. Such a rephrasing ideally takes the form of a mathematical model which, preferably, contains the purported mechanism behind the increase in UV-B radiation and allows us to extrapolate from known values of variables to unknown ones. Slightly getting ahead of my story, in this particular case there is no doubt that one would be highly interested in the effects of a lowered CFC release on atmospheric UV-B radiation levels.

More generally and abstracting away from the example at hand, one could say that the object of the analysis phase has been the construction of a *model*, preferably a mathematical one, that describes the causal network connecting interference with problem. Ideally this description is not a pure description but contains enough insight into the mechanisms connecting interference and effect to allow us to speak of an explanatory model. [13]

Phase 3: scenario building

In the case of the ozone layer problem, the world community has acted amazingly promptly to the threat of increased intensities of UV-B radiation. In 1987, at a conference in Montreal a protocol was signed with the intent of taking measures to protect the ozone layer. The protocol demanded levels of the major CFCs to be kept at 1986 levels until 1993. After 1993 a gradual reduction was to take place. However, it soon turned out that the levels of CFC-emission admissible according to the Montreal protocol would not sufficiently protect the ozone layer. In fact, calculations showed that chlorine concentrations would keep rising as a consequence of which the ozone layer would probably end up being depleted entirely. Under the guidance of UNEP, the United Nations Environmental Program, within a year after the Montreal Protocol a new, more stringent agreement was signed in London. Under this agreement, chlorine levels should start falling around the year 2000.

Little reflection reveals that the story just told has all the marks of a *decision making* process. And indeed decision processes are the warp and woof of the third phase, the scenario building phase. In environmental decision processes, as in any decision process, four elements play a part. First, there are obviously the *actions* one may undertake. Actions are human interferences that are meant to solve a given problem. In the context of the CFC case, they are the various measures to which the signatories to the Montreal and London agreement have committed themselves. Second, actions are taken with the aim of producing a particular result or *outcome*, the solution to the environmental problem. In the example, outcomes are the degree to which the destruction of the ozone layer is cut back.

Third, what the outcome will be not only depends on the action but also on the *system* acted upon. This is where the analysis of the causal network and the model that resulted from it pay off. The model captures, or so it should, the important characteristics of the system under consideration, such as the rate at which CFCs migrate from the troposphere to the upper stratosphere, the chemical reactions that take place, the rates at which they progress. The model allows one to trace (compute) the consequences of actions. As models are always *models of* some natural system, they harbor uncertainties. A model may be structurally wrong - a risk to be minimized by working with various models in a decision process. A model may rest on little or biased data - something to be countered by including measures of the reliability of the data into a model's predictions.[14] In spite of all precautions, science is fallible and so its predictions may fail to come true.

Finally, in order to be able to choose among the actions considered, outcomes are valued with respect to their *utility*. The utility of an action should reflect the extent to which the problem at hand is solved. In the final analysis, an outcome's utility determines whether the corresponding action is chosen or not. In the ozone case, a solution that reduces CFC-emission considerably has a higher utility than one that doesn't, etc. As the London protocol does just this it has a higher utility than the Montreal protocol.

Methods, theories and a special vocabulary

The description given above of how environmental science goes about solving its problems discloses a characteristic of the field, even though it isn't a characteristic unique to environmental science: it deploys models in a decision theoretical context. It seems, this method suits the goal of practical problem solving particularly well. Indeed, here we may have a method that is characteristic of the field at large. But since it does not characterize the field uniquely, it is a rather weak reason for calling environmental science a true field.

The above description reveals something more. As we saw, the analysis of the ozone problem took inputs from environmental chemistry, in particular atmospheric chemistry and photochemistry. Also, although we didn't explicitly discuss this, the knowledge contributed by meteorologists about radiation intensities, temperatures and transportation mechanisms at various altitudes is required for a proper analysis of the ozone problem.

What prompts us to act upon the depletion of the ozone layer are the effects of the increased intensity of UV-B radiation. The incidence of skin cancer, for example, is expected to rise. Other effects on the human body such as cataracts in the eye's lens, retinal damage and suppression of the immune system may also result. Other organisms, both animals and plants, both higher and lower, will be similarly affected through mutations or damage to their physiological systems. If crop plants are affected - and they probably would - effects on yields are to be expected. If plankton is affected - and because of the small size of the organisms involved serious damage is likely - marine food chains may change as these organisms are at the bottom of such chains. This in turn may have consequences for entire marine ecosystems, including amounts of fish harvested from the seas. Finally, climatic changes may also result, as the thinner ozone layer lets more solar radiation pass through, heating up the lower parts of the atmosphere. Whatever the climatological effects exactly will be remains to be seen but they might well include effects on crop yields, which in turn may cause people to move, etc. (There are obvious parallels with the green house effect.) So the consequences of the emission of CFCs may range far and wide and their understanding may concern a number of theories from various scientific disciplines, ranging from medicine, via physiology, ecology, agronomy, and fishery biology to climatology.

The number of disciplines involved becomes even larger when we also take into account the analysis of scenarios that could lead to the problem's solution. A non-systematic survey lists the following specialists. Chemists, particularly of a technological bend, might devise alternative molecules that mimic the CFCs in their functions without having their drawbacks. And indeed, CFCs have been replaced with different propellants, non-CFC fire retardents have been developed, foams are being blown in different ways, etc. Another technological solution would be to change the process under consideration such that there is no need for a CFC or a replacement. To some extent the electronics industry which uses CFCs as a cleansing agent has been successful in reducing the need for such agents; in some cases styrene foam trays have been replaced by carton ones; refrigerators are under development that operate on an entirely different principle, using ultrasound rather than refrigerants and a compressor. Yet another possibility would be to reduce the societal want for particular a product. Do we really need aerosol cans? If not, we don't need the propellants that go in them either. Also, do we really need air conditioners in so many cars? If not, we would save on refrigerants. Examining such questions would be a matter for politicians and social scientist of various persuasions. Economic analyses, for example, will have to accompany almost any proposed solution. Finally, if a government wants to impose particular measures legal action is necessary, which requires inputs from lawyers.

The above survey shows that, although the description of an environmental problem may be considered the privilege of the natural sciences, the knowledge used in the analysis of the causes of and solutions for environmental problems is derived from a much wider variety of disciplines ranging from the social sciences to the natural sciences. However usually, the models used are not so comprehensive as to span the entire breadth of disciplines mentioned. They are rather limited as is evidenced by the following description of a photochemical model:

We are now in a position to see how kinetic models of atmospheric reactions can be formulated. In principle, all that is required is the writing of a [reaction] equation ... for each component, *i*, of the atmosphere, taking into account transport processes, and recognizing that in such equations the production rates and loss rates will, in general, be complex functions involving photochemical rate coefficient and rate constants multiplied by concentrations.[15]

Basically, this model restricts itself to chemical reaction equations and the only transgression into another discipline might be the incorporation of transport processes, belonging to the realm of the physics of the atmosphere.

In order to calculate the increased skin cancer risk as a function of UV-B exposure, other models, particularly epidemiological models, would be required. Such models exist and allow the calculation of risks incurred at projected ozone levels. Similarly, ecological models would be needed to estimate ecosystem damage and crop reduction as a consequence of lowered ozone concentrations. At present, such models are only available in rudimentary form.

The above survey shows that, first, a number of theories from disparate fields are involved in formulating solutions for the ozone problem. Although I haven't analyzed other examples, this conclusion appears to apply generally. Second in order to solve the problem techniques and methods are employed from various disciplines. Both chemical analysis techniques and epidemiological census techniques are required. This too, I would submit, applies quite generally. Third, also at the level of the theories employed one draws from multiple (sub)fields. The photochemist calculates the ozone concentration as a function of past and current emission levels; the epidemiologist computes the risks one runs at particular ozone concentrations; the ecologist and agronomist try to evaluate ecosystem consequences in terms of diversity changes and crop size reductions as a function of the same ozone concentrations. What in the case at hand binds the various (sub)fields together is the existence of the ozone problem. This applies quite generally, I would submit: it is the problem at hand that dictates what combination of subdisciplines will be involved. All in all, traditional environmental science appears to draw heavily upon existing scientific disciplines and there is no method nor theory unique to environmental science.

There is a fourth feature of environmental science that deserves our attention. As I mentioned, according to Maull and Darden, individual fields can each be characterized by their so-called special vocabulary. In the ozone case we saw that the models used are largely monodisciplinary. This means that special vocabularies are kept apart. This, I hold, also is a general feature of traditional environmental science. It may happen that, in discussing solution scenarios, vocabularies are mixed. However, mixing is all that happens, the vocabularies do not interact with each other so that, for instance meaning shifts occur. Chemists still talk chemistry, ecologists ecology, etc.

Wrapping up our discussion so far, then, environmental science as practiced traditionally is a genuine multidisciplinary field. It may indeed possess a set of similar problems, a truly focal problem is missing though. Second, although practical goals can be discerned, explanatory goals are conspicuously lacking. Third, the special vocabularies, methods, techniques and theories used all prove to belong to sub-fields of environmental science rather than to a unified field called environmental science. So the picture that emerges is that of quite a heterogeneous field, one consisting of multiple subfields each with its own methods, techniques and theories. As I will now argue, the introduction of the notion of *sustainability* has changed all this.

ENVIRONMENTAL SCIENCE AS AN INTERDISCIPLINE

To be precise, I will argue that the notion of sustainability has introduced into environmental science both a focal problem and an associated special vocabulary; that, besides practical goals there now are explanatory goals too; that methods and techniques have evolved not belonging to specific subfields; and, finally, that there are even indications for the evolution of specific theoretical insights. All this is evidence for the emergence of an interdiscipline or interfield of environmental science. I will now discuss each piece of evidence in some detail.

Specific vocabulary

Just now, I posited that the special vocabulary of environmental science is a mere mixture of the special vocabularies of a number of disciplines referred to as environmental sub-disciplines. To a large extent the current situation is no different. However, there are good reasons to regard sustainability as a new notion, unique to a new interdisciplinary field (interfield) called environmental science.

In 1987 the World Commission on Environment and Development published a report called *Our Common Future*[16]. It is the written result of a two year study after issues of environment and development conducted by a prestigious group of people led by Norwegian prime minister Gro Harlem Brundtland. The report, commonly known as the Brundtland report, marks a milestone in our thinking in environment and development.[17] It showed the intricate links between environmental concerns and development, particularly on non-industrialized countries. More important for our present purposes, it defined and popularized the notion of sustainability. According to the report,

Sustainable development is development that meets the needs of the present without compromising the ability of future generations to meet their own needs.[18]

Note that this definition has a clear-cut normative import, it forbids development that would compromise the needs of future generations.

Focal problems

This new notion not only can be regarded as part of an incipient special vocabulary, it also provides the newly emerging interfield with a focal problem. In line with the practical nature of environmental science discussed above, the central question would be `How does one achieve sustainability?' The economist Herman Daly has addressed this question and, in an attempt to answer it, operationalized the notion of sustainability.[19] Using the reformulation that Meadows et al. have given of Daly's ideas:

* For a *renewable resource* -- soil, water, forest, fish -- the sustainable rate of use can be no greater than the rate of regeneration. ...
* For a *non-renewable resource* -- fossil fuel, high grade mineral ore, fossil ground water -- the sustainable rate of use can be no greater than the rate at which a renewable resource, used sustainably, can be substituted for it ...
* For a *pollutant* the sustainable rate of emission can be no greater than the rate at which that pollutant can be recycled, absorbed, or rendered harmless by the environment. ...[20]

This operationalization is illuminating for a number of reasons. First, it leads to an enlargement of the interfield's special vocabulary: renewable resource and non-renewable resource are cases in point; Daly's term `quasi-sustainable' which he uses to characterize sustainable use of non-renewables, which, of course, strictly speaking is impossible, falls under the same rubric.[21] Second, the operationalization points to a new method for achieving sustainability, unique to the interfield.

Methods and techniques

There even is a candidate for a field specific method. Although the method is mentioned by Daly nor Meadows, their followers have developed it and now it belongs to the standard tool kit of the environmental scientist.[22] I am referring to Life Cycle Analysis. Put briefly, Life Cycle Analysis seeks to track down the entire life cycle of a product, from the mining of its constituents through its manufacturing to its ultimate discarding; it seeks to look at this life cycle from the point of view of sustainability. To what extent can waste be recycled, what pollutants result, is energy used sustainably, etc. are questions addressed. Quite obviously this method thanks its very existence to the introduction of the notion of sustainability. In this sense it supports the contention that an interfield called environmental science exists.

A third piece of evidence also regards methods. Environmental problems are still solved using the three-staged process described earlier. As sustainability has permeated the specific vocabulary of environmental science, one may expect that it has affected both a problem's description and its causal analysis in terms of a model. And indeed, in the Dutch National Environmental Outlook 2 these phenomena can be witnessed to occur, for instance with respect to the ozone case discussed earlier. A telling phrase is: `Given the complexity and great uncertainties it is not possible to draw up a strict standard for sustainability', implying that the entire modeling effort is centered around the issue of sustainability.[23] And indeed, one goes on by saying that

An effects-based approach could be to reduce the concentration of chlorine and bromine in the atmosphere to 1.5 to 2 ppbv chlorine equivalent ... The reasoning behind this is that if this level, measured in 1975 just before the hole in the ozone layer was discovered, is exceeded it is likely that the ozone layer above the Antarctic will not recover.[24]

This quote points also to another aspect. Recall that in environmental decision making, the utility of outcomes had to be judged so as to make a decision possible. As the quotation reveals, in the ozone case the utility ordering really is a sustainability ordering, that is, to what extent an outcome is to be preferred to another depends on its contribution to a sustainable solution. Emission levels above 2 ppbv have lower utility than levels below it.

Theories

As a final piece of evidence, consider the following quote from Daly's paper:

The Brundtland Commission Report ... has made a great contribution by emphasizing the importance of sustainable development and in effect forcing it to the top of the agenda of the United Nations and the multilateral development banks. To achieve this remarkable consensus, the Commission had to be less than rigorous in avoiding self-contradiction. One hoped that the *glaring contradiction* of a world economy growing by a factor 5 or10 and at the same time respecting ecological limits, which was present but subdued in the Report, would be resolved in future discussion. In fact, however, Mrs. Brundtland has subsequently urged economic growth by a factor 5 or 10 as a necessary part of sustainable development.[25]

What the quote makes clear is that there is a conflict about what exactly sustainable development means. One may construe this conflict in one of two ways, as a conflict about methodological norms (self-contradiction) or as a conflict about economic and ecological possibilities. Either way, we are dealing with a conceptual problem *sensu* Laudan, be it internal or external. The interesting point is that sustainability comes out of this as a theoretical notion; after all, conceptual problems are exhibited by theories only. From this one may conclude, admittedly somewhat prematurely, that we are witnessing the evolution of a *theory* of sustainable development, a theory belonging to the new interfield of environmental science.

In summary, from the evidence adduced one may conclude at this stage that the multidisciplinary field of environmental science is on its way of becoming a true interdiscipline or interfield, even though there still is a long way to go.

CONCLUSION

In the introduction to this paper I raised an issue: why bother at all about the question of whether environmental science is an interdiscipline or a mere multidiscipline. I then referred to particular benefits that come with interdisciplinary status such as better access to funds, etc. At this junction, I can be a bit more specific about the issue of benefits. There is one benefit that remained unmentioned. It has to do with the efficiency with which environmental problems may be solved.[26]

Quite in general and omitting all details, one might say that traditional, multidisciplinary environmental problem solving is less efficient than interdisciplinary environmental problem solving. Traditionally, a problem is partitioned, split up in non-overlapping sub-problems that, ideally, jointly cover the entire breadth of the original problem. The splitting up is done in such a fashion that established disciplines such as chemistry, biology, economics - usually in the guise of their applied cousins environmental chemistry, environmental biology, and environmental economy - can have a go at the problem. This, at least, is what the ozone case very much suggested. The approach seems to be a perfectly valid and valuable one in that at least it guarantees input from various disciplines thus enriching and strengthening the analysis carried out. Also, at times it may be the best one can hope for. However, it also suffers from short-comings.

To continue the use of the mathematical metaphor, the partitioning might well be less than perfect. Because sub-problems do not fit the existing disciplinary subdivision they may be left out thus making the analysis incomplete. Alternatively, the same sub-problem may be analyzed from two different disciplinary angles - ecological damage assessed by ecologists and agronomists would be a case in point - at best making the analysis redundant, at worst incoherent. What is needed to prevent this from happening is a kind of bird's eye view, some way of overseeing the problem in its entire complexity, with all its ramifications.

Traditionally, this was achieved through what somewhat cynically might be called `problem management': a group of people tries to solve a problem by partitioning the relevant questions optimally. More often than not, these groups are existing bodies, part of a full-fledged social structure, they may government departments, research institutes, NGOs, or political pressure groups. In their turn these groups invoke the help of other such groups and so on. The complicated web that thus is spun has its own social dynamics which, in terms of the efficiency of problem management, leaves to be desired for. To mention but one example, the negotiating table seems hardly the best place to sort out the answers to scientific problems. And, indeed, the history of the US attitude towards the question of global warming, the British attitude towards the question of maintaining biodiversity, or the Dutch government's policy with respect to the animal manure surplus betray political wheeling and dealing, revealing that forces other than a wish to arrive at environmentally sound solutions are operative. The upshot of all this is that the success of a multidisciplinary approach for the greater part depends on the organization of the group of researchers involved and its dynamics; only to a lesser extent the `quality' of the knowledge, of the theories and models involved matters.[27]

In an interdisciplinary setting much of this may perhaps be avoided. Problems that are overlooked because of a non-optimal splitting of the original problem may be avoided now that a focal problem is available. Group dynamics are internalized and hence easier to control. What used to be an ad hoc network of people now has become an internal network.

These speculations may sound overly optimistic and perhaps they are. However, quite irrespective of their fate, environmental science does seem to be heading into the direction of interdisciplinarity. Time will tell us what ultimately the outcomes of this trend will be.

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Footnotes

[1] *Cf.* Zandvoort (1986) and Udo de Haes (1991) who explicitly discuss the issue. I am not aware of any similar discussions outside of the Dutch context, but I would be highly surprised were they to be confined to the Netherlands only.

[2] cf. Maull, 1977; Darden and Maull, 1977; Darden, 1991. Similar ideas have been voiced by many others and various accounts exist of how interdisciplinarity comes about. See for instance Mitchell et al. for an analysis of how the biological and social sciences might fruitfully co-operate.

[3] Darden and Maull, 1977.

[4] Darden, 1991, p.19.

[5] See Vincent Kiernan, (1993) p.8. See also G. Pitari and V. Rizi, Geophysical Research Letters, **18** (1991), 833 who predicted a reduction as a consequence of Mount Pinatubo's eruption.

[6] This will be discussed in more detail later on.

[7] See the contributions by Kristin Shrader-Frechette, Michael Ruse and Gunnar Skirbekk in the volume this paper is published in.

[8] Laudan, 1977

[9] I should mention that at least one group of philosophers has been explicitly concerned about these matters. Usually they are referred to as the Starnbergers (Böhme et al., 1978). In the seventies, they have performed a number of studies which all attempted to show how it was that particular scientific theories were applied, that is, used to solve practical problems. Their analysis, however, is hardly useful as their focus of attention is theories which also have to be mature. In my view this is too narrow an approach to be useful here. See also Zandvoort, *op. cit.* and Boersema, 1991.

[10] My claims are made with respect to practical problems in general. However, my analysis pertains to environmental problems only. Although I do not adduce evidence in support of the more general claim, I do believe that, first, *all* practical problems ultimately rest on norms, second, *all* solutions to practical problems can be ordered on an ordinal scale on the basis of the same norms.

[11] See, for instance, Udo de Haes, 1991.

[12] This story is described in many papers and books. A good source for a technical treatment is to be found in Phillips (1988) and Sherwood Rowland (1988), and RIVM (1992). A more accessible but nonetheless comprehensive treatment can be found in chapter 5 of Meadows et al., 1993.

[13] Mechanisms, admittedly, is a somewhat vague notion, but the idea I want to get across here is that the model contains genuine insights into the system's internal functioning and not merely establishes the strength of statistical correlations between variables. Although purely statistical and descriptive models allow extrapolation to values not observed this is a risky business. The assumption is made, but not independently checked, that a system under observation does not changes its behavior in any structural sense. The occurrence of threshold-like phenomena, for instance, would be disastrous. Explanatory models containing behavior mechanism are not sensitive to this kind phenomena, that is, in so far as the mechanism takes thresholds into account. Of course, a model cannot foresee what it wasn't designed to foresee.

[14] See for an introductory treatment of the role of models Doucet and Sloep (1992), particularly chapters 13 and 14.

[15] Phillips, 1988, p. 136

[17] Meadows et al. literally say: `the two primary contributions of this [the Brundtland] study were the definition and popularization of the idea of sustainability and the strong linkage of the issues of environment and development.' (Meadows, 1992, p.273)

[18] United Nations World Commission of Environment, 1987.

[19] Daly, 1990.

[20] Meadows et al. 1992, p.46.

[21] Daly, 1990, p.4

[22] Cf. RIVM, 1992 *passim* and Meester, 1992.

[23] RIVM, 1992, p.131.

[24] *ibid.*

[25] Daly, 1990, pp. 1 - 2.; emphasis added

[26] That this is an issue of some importance is evidenced by a letter sent to the New Scientist in 1991. The letter was sent by Newby, a social scientist. He argued that cooperation between scientists of various persuasions is an absolute necessity. At the same time complained about the sluggishness with which such cooperation is established.

[27] *Cf.* Glasbergen (1993) for evidence and a rather cynical view. He wonders whether environmental conflicts could perhaps better be fought out in the political arena alone. There, he argues, the entire decision process is taking place anyway, without much of an eye for scientific arguments. So why not skip the science entirely and immeditely?

References

Boersema, J.J. (1991). *Theorievorming en verklarende theorieën in de milieukunde*. In: Boersema, J.J., J.W. Copius Peereboom, W.T. de Groot (eds.), Basisboek milieukunde, vierde, geheel herziene druk, Boom, Meppel, pp.35 - 45.

Böhme, G., W. van den Daele, R.Hohfeld, W. Krohn, W. Schäfer, T. Spengler (eds.), (1978). *Starnberg Studïen I. Die Gesellschaftlichen Orientierung des Wissenschaftlichen Fortschritts*, Suhrkamp Verlag, Frankfurt a. M.

Daly, H.E. (1990). *Commentary: Toward Some Operational Principles of Sustainable Development*, Ecological Economics, **2**, 1 - 6.

Darden, L. (1991). *Theory Change in Science; Strategies from Mendelian Genetics*, Oxford University Press, New York, Oxford.

Darden, L., N. Maull (1977). *Interfield theories*, Philosophy of Science,**44**, 43 - 64.

Doucet, P. and P.B. Sloep (1992). *Mathematical Modeling in the Life Sciences*, Ellis Horwood & Simon and Schuster, New York, London.

Glasbergen, P. (1993). *Managing Environmental Conflict in an International context*. In: Sloep, P.B. (ed.), International Issues in Environmental Sciences, Open universiteit, Heerlen, pp. 7 - 15.

Kiernan, V. (1993) *Atmospheric ozone hits a new low*, New Scientist,**138**, 1871

Laudan, L. (1977). *Progress and Its Problems*, U. California Press, Berkeley, Ca.

Maull, N. (1977) *Unifying Science without Reduction*, Studies in History and Philosophy of Science, **8**, 143 - 162.

Meadows, D.H., D.L. Meadows, J. Randers (1992). *Beyond the Limits. Global Collapse or Sustainable Future*, Earthscan Publications Ltd., London.

Meester, M.A.M. (ed.) (1992). *Milieu en technologie*, Open universiteit, Heerlen.

Mitchell, S.D., L. Daston, G. Gigerenzer, N. Sesardic, P.B. Sloep, The Why's and How's of Interdisciplinarity. In: Weingart et al. *Human by Nature, Between Biology and the Social Sciences*, Lawrence Erlbaum, London

Newby, H., 1991, *Talking Point: Opening the Door to Social Scientists*, New Scientist, **127**, 26.

Phillips, D. (1988). *Kinetics of reactions of stratospheric importance*.In: Coyle, J.D., R.R.Hill, D.R.Roberts (eds.), Light, chemical change and life: a source book in photochemistry, The Open University Press, Milton Keynes, pp. 128 - 141.

RIVM, National Institute of Public Health and Environmental Protection (1992). *National Environmental Outlook 2, 1990-2010*, RIVM, Bilthoven.

Sherwood Rowland, F. (1988). *Chlorofluorocarbons and stratospheric ozone*, In: Coyle, J.D., R.R.Hill and D.R. Roberts (eds.), Light, chemical change and life: a source book in photochemistry, The Open University Press, Milton Keynes, pp. 142 - 161.

Udo de Haes, H.A. (1991). *Algemene aspecten van de milieukunde*. In: Boersema, J.J., J.W. Copius Peereboom, W.T de Groot (eds.), Basisboek milieukunde, vierde, geheel herziene druk, Boom, Meppel, pp. 21 - 34.

United Nations World Commission on Environment and Development, (1987). *Our Common Future (`The Brundtland Report')* Oxford U.P., Oxford.

Zandvoort, H., (1986). *Milieukunde en interdisciplinariteit*, Kennis en Methode, **3**, 230 - 251.