# Philosophy of Interdisciplinarity: Problem-Feeding, Conceptual Drift, and Methodological Migration

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One way to bring order into the often muddled picture we have of interdisciplinarity is to sort interdisciplinary projects or aims by the kinds of element that interact in encounters between researchers of the two or more disciplines involved. This is not the usual approach. Since the early seventies and the publication of Erich Jantsch (1972), at least, the level of integration of the disciplines has been the primary focus. For instance, the level of integration is often treated as the distinguishing boundary between multi-, inter-, and trans-disciplinarity.

We identify three kinds of interdisciplinary relation: problem-feeding, conceptual drift, and methodological migration; we focus, in particular, on the first of these. Drawing on examples from the emerging field of Sustainability Science we show that problem-feeding is a common and apparently fruitful way of connecting disparate disciplines. We illustrate some of the roles conceptual drift and methodological migration have in problem-feeding as well as in their own right. Towards the end of the paper we suggest that there is an interesting difference between our approach to interdisciplinarity and the integrative perspective suggested by Jantsch. The interdisciplinarity resulting from problem-feeding between researchers is typically local and temporary; integration is associated with a longer-term, global form of interdisciplinarity.

## Predecessors: Problem shifts and the correlation of terms

Our notion of problem-feeding has a predecessor in the work of Lindley Darden and Nancy Maull on 'interfield theories', i.e. theories relating more than one disciplinary field. More precisely, our notion resembles what Darden and Maull refer to as 'problem shifts'. A brief illustration will suffice for now. Through a series of scientific changes, beginning around 1910, the problem of understanding the physical basis of heritable alterations shifted from genetics to biochemistry, where it was famously solved in the 1950s (see Maull 1977). As we shall see, problem shifts exemplify a special kind of problem-feeding that occurs under certain characteristic conditions.

The conditions under which problem shifts occur are similar to, and overlap with, the preconditions for reductionist accounts of intertheoretic relations. Both concern the correlation of descriptive expressions in two or more disciplinary fields. The historical reason is that Darden and Maull developed their view in response to reductionism, which was then the received view. Maull's (1977) main targets are reductionist analyses of the sort pursued by Ernest Nagel (1961) where one theory deductive-nomologically explains another. In the Nagelian perspective physical optics and Maxwell's electromagnetic theory are intertheoretically related to the extent that Maxwell's theory can be used to derive physical optics; and in general theory T2 is reduced to T1 when all the generalizations in T2 can be logically deduced from generalizations in T1. Normally this requires auxiliaries that correlate descriptive expressions (from now on referred to as 'terms') in T1 and T2. For instance, the light vector of physical optics first had to be identified with the electric force vector of Maxwell's theory. As a result, each term has a primary sense, fixed by its own theory, and a secondary sense, obtained by the correlation.

Derivation is both necessary and sufficient for Nagelian reductionism. Correlation is necessary but not sufficient. It was only to be expected that reduction rather than correlation would become the primary focus for Nagelians. In addition, there is little information in this sort of account as such about the way in which correlations of terms and other interactions between fields come about. A possible exception is Kenneth Schaffner's (1967) modification of Nagel's account. Schaffner proposes that reduction sometimes occurs after a slight modification of T2. Sciences in the process of being reduced display a need for more "careful and corrected redefinition", and as a result of correction reduction is facilitated. However, Schaffner (1974) later warned against deploying this as an explanation of scientific change. Reduction is often too "peripheral" an aim in actual cases to motivate the redefinition of terms in T2.

The development of links between distinct disciplinary fields, Maull claims, is an important but neglected type of scientific change. As has already been hinted above, in order to get to the point where derivational reduction can be considered an option, connections of a more substantial kind already need to be in place:

> Before we can meaningfully ask whether a theory of one field is derivable from a theory of another, that is to say, before the question of reduction can even arise, extensive unification between fields must already have taken place. Connections between terms of the fields must already have been established, explained and warranted by an interlevel theory. (Maull 1977, 161)

Whereas Maull's critical discussion of the explanatory scope of, and preconditions for, reductionism has only a tangential relationship to our account of problem-feeding, her related observation (see also Darden and Maull 1977, 59) about the effects of the correlation of terms is very much to the point:

> This alternative to derivational reduction begins by drawing attention to the way a vocabulary can be 'shared' by different areas of research. Such a 'shared' vocabulary, it turns out, can be used to identify a very special sort of problem, a problem that, although it

> arises within one branch of inquiry, can only be solved with the aid of another science. (Maull 1977, 144)

Central here is the idea that terms are associated with problems. In some cases these are solved within the disciplinary field to which a term has been correlated. On Darden and Maull's account, however, it would be more natural for the problems to be solved within an interfield theory emerging between the original fields.

We have briefly mentioned an example of the latter process. 'Mutation' (heritable alteration of the genotype) was first a proper term in genetics; it was then transformed into 'mutation' (heritable alteration in the base sequence of DNA) of the sort witnessed in biochemistry. This correlation and subsequent transformation made it possible to make progress on previously intractable issues in genetics, including the question, What is the physical nature of the alterable determinants of heredity? In other words, problems concerning the physical nature of the determinants of heredity arose within genetics. Genetics could not solve them. One reason was methodological. Genetics deploys statistical methods and cross-breeding in order to establish regularities in hereditary phenomena. In this sense, the discipline lacked the technical resources to solve the problem which had arisen. However, with the correlation and transformation of the term 'mutation', the problem could shift to biochemistry – where a solution was forthcoming.

To recap: we think it is fair to say that the conditions presupposed by Maull's account of problem shifts are relatively closely related to Schaffner's procedure for correlating terms in two theories by a process of correction and redefinition. Maull is obviously not in favour of reduction, and this introduces a big divide between them, but she points to another effect such correlation may have (at least, when it involves a certain kind of transformation[1]), namely the shifting of problems from one disciplinary field to another.

On Darden and Maull's view problem shifts are made possible through a certain kind of correlation – a *transformation* – of terms. Transformation of terms is thus an ontological how-possibly explanation (Persson 2011) of problem shifts. But an ontological how-possibly explanation of X (i) is only a partial explanation of X and (ii) does not imply uniqueness – other explanations of essentially the same kind of phenomenon may exist. The notion that a full explanation of problem shifts requires more is illustrated by the following observation. Transformation of terms establishes a bridge between disciplinary fields which facilitates the feeding of problems (as well as other elements) from one field to the other. But the existence of the bridge neither tells us the whole story about why problem-feeding occurs nor explains why the bridge appeared in the first place.

[1] Terms do not always have this problem-shifting potential. It seems important that knowledge claims from both fields are associated with the term, and that both fields may contribute by latching further knowledge claims onto it. These are the terms, we think, for which Maull reserves the label 'transformation'. Maull does not mention this, but we take it the knowledge claims already associated with the term can function as a heuristic in the target field and hence be subject to alteration or deletion.

This partial picture cannot be completed by citing the general interdisciplinary aim of transforming terms so that they can function as bridges between fields. A term can be transformed in many different ways, corresponding to the many possible bridges between various disciplinary fields. But transforming the term in one way pre-empts many of the alternative possibilities. The question that arises in Darden and Maull's case is: Why biochemistry?

The task of correlating terms to facilitate shifting of problems will only be worth pursing if there is some promise – or, at least, hope – that the field on the receiving end is in fact suitable. Furthermore, in order for the term to be a proper term in both fields the field assignment needs to be acceptable to both fields. What this implies is that the two fields need to be connected beforehand by some shared hypothesis or research plan.

# Problem-feeding

Let us now leave Darden and Maull's framework and focus instead on problem-feeding more generally. As was seen in the preceding section, problem-feeding is sometimes a secondary phenomenon, in the sense that it is a by-product of more fundamental processes. Our suspicion is that the need to solve problems by first feeding them to another field is sometimes itself the fundamental reason why correlation of terms takes place and the reason why other kinds of bridge between distinct disciplinary fields are created. This is why we think that, among several candidates, problem-feeding is a salient interdisciplinary relation.

A mundane example of what we have in mind is the following. Lawyers have to decide in matters where scientific expertise is *prima facie* relevant. It is not uncommon for scientific experts to be consulted when, for instance, environmental risk management issues are scrutinized – or when the causes of, and responsibility for, a human injury are examined. However, it is far from always the case that attempted problem-feeding from law to science is successful in facilitating the lawyer's decision. Lena Wahlberg (2010) reports the following expert's experience:

> All right, you ask us physicians, 'What do you think?', and we write that in this particular case there are pathological changes; the changes are of such a kind that we do not regard the injury as a consequence of an accident. But the courts have during recent years always ruled in favour of the patient. I don't mind that. *But then why ask us?*

One reason why successful problem-feeding from law to science is so difficult, Wahlberg claims, is that understandings of causation in the two fields differ. The correlation of terms (or rather the ontologies corresponding to 'cause$_{law}$' and 'cause$_{science}$') is only *superficially* in place. Indeed the deployment of the term 'cause' in both fields is liable to give rise to misconceptions about the opportunities for problem-feeding.

The following, more constructive, example is perhaps typical of problem-feeding in sustainability science, where frequently problems are defined by natural sciences and exported to social science. In an influential paper in the *Proceedings of the National Academy of Sciences of the United States of America* (*PNAS*) Timothy Lenton (2008) and his colleagues defined and identified a number of climatic tipping elements (and a few tipping points). Tipping elements are systems which, once pushed across a certain threshold, or tipping point, are likely to exhibit non-linear, disruptive change. The *PNAS* text lists 15 policy-relevant tipping elements, including Arctic summer sea-ice, the Greenland ice sheet, Atlantic thermohaline circulation, and the Indian summer monsoon. These elements, the authors argue, can be pushed by human interaction across a tipping point resulting in Arctic sea-ice loss, the melting of the Greenland ice sheet, Atlantic deep water formation, and Indian monsoon chaotic multi-stability, and so on. Furthermore, all of these elements contribute significantly to human welfare as we know it today. Identification of these tipping points clearly falls, in many cases, within the domain of some natural science discipline or aggregate thereof. On the other hand, addressing them is first and foremost a concern for societies. But the missing piece of the picture is this: in order to work out all relevant facts constraining a viable solution both natural and social sciences will have to contribute substantively. Other problems that Lenton and his colleagues consider are more specific and hinge on the applicability of their own notion of a tipping element. For instance, they are interested in the question whether tipping elements in the social-economic system can be identified.

Another example related to Sustainability Science can be taken from scientific investigation of the $CO_2$ cycle and its role in determining the temperature of the Earth. This offers multiple examples of problem-feeding. Early attempts to understand and model the influence of $CO_2$ were mostly made by and for physicists. Scientists wished to explain why the Earth suddenly got substantially warmer some 10,000 years ago.[2] In the 1820s Joseph Fourier suggested the mechanism that later become known as the 'greenhouse effect.' John Tyndall noted in 1861 that $CO_2$ fitted the specifications, together with other gases such as common water vapour. Physicist Svante Arrhenius (1896) developed the first quantitative model. By laborious computations he managed to come up with the prediction that a doubling of atmospheric $CO_2$ would result in an increase in mean surface temperature of 5-6° C.[3] As Bert Bolin (2007) notes, Arrhenius considered the influx of fossil carbon into the atmosphere caused by human activities a possible source of warming. However, two assumptions that turned out to be incorrect led Arrhenius to dismiss this possibility. The first concerned the increase in fossil fuel consumption, which turned out to be greater than anyone imagined in the final decade of the nineteenth century. The second incorrect assumption concerned the solubility of $CO_2$ in the oceans. Arrhenius overestimated it, as would many after him. By the 1950s approximations of the average time a molecule of $CO_2$ would spend in the atmosphere before it was absorbed by the sea varied from as little as 16 hours to 1,000 years.[4] Hence it must have been fairly clear that there in fact *was* a problem. Nonetheless, in spite of

[2] This, in turn, was suggested by Louis Agassiz in 1840 as an explanation of certain geographical features (Bolin 2007, 3). Agassiz hypothesized that a thick ice sheet must have covered large parts of Europe at one point.
[3] For an overview of the history of this research see Weart (2009) and Bolin (2007).
[4] See Herman Craig (1957) in Weart (1997).

such massive uncertainty, the hypothesis that the process was fast interfered with other significant breakthroughs.[5]

The uncertainty depended on several factors; as Spencer Weart notes the problem was twofold to begin with. The dissolution of $CO_2$ in the oceans depends crucially on (i) the chemistry of oceans and (ii) the turnaround of the oceans. Both of these raised difficult problems in their own right. The solution to the problem would be provided by someone with an intricate knowledge of both issues – the oceanographer Roger Revelle, active at the Scripps Institution of Oceanography in La Jolla, together with chemist Hans Süess.[6] From earlier projects he had been engaged in Revelle knew that different layers of water do not mix very rapidly. In fact they hardly mix at all. Though horizontal dispersion is quite rapid the rate of vertical exchange is low (see Weart 1997). The other problem concerned a complicated set of chemical reactions in the ocean, a so-called 'buffering mechanism'. Such mechanisms maintain acidity in a solution at a constant level. In a short paragraph, Bolin and Erik Eriksson summaries the dynamics conjectured by Revelle and Süess:

> The low buffering capacity of the sea mentioned by Revelle and Süess is due to a change in the dissociation equilibrium between $CO_2$ and $H_2CO_3$ on one hand and $HCO_3$ and $CO_3$ ions on the other. An addition of $CO_2$ to the water will change the pH and thereby decrease the dissociation resulting in a larger portion of $CO_2$ and $H_2CO_3$ molecules. Since the pressure of $CO_2$ in the gas phase being in equilibrium with $CO_2$ dissolved in water is proportional to the number of $CO_2$ and $H_2CO_3$ molecules in the water, an increase of the partial pressure occurs which is much larger (about 12.5 times) than the increase of the *total content* of $CO_2$ in the water. The change of this equilibrium in the sea is almost instantaneous. However, in course of its circulation the ocean water gets in contact with solid $CaCO_3$ on the bottom of the sea whereby a change towards another equilibrium takes place. This latter process is extremely slow and may be disregarded when discussing changes due to fossil fuel combustion. It will, however, be... of major interest when being concerned with processes with a time scale of several thousand years. (Bolin and Eriksson 1959, 131)

The consequence of this mechanism is that even though atmospheric $CO_2$ readily dissolves, as predicted by earlier models, it does not stay dissolved. At the end of the reaction most of it evaporates right back into the atmosphere. Eventually the atmosphere-ocean system settles in a $CO_2$ equilibrium, but that

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[5] It was an obstacle, for instance, to the development of detailed understanding of the radiative properties of CO2 and water vapour – an understanding that would eventually show that the latter did not eclipse former as a greenhouse gas.

[6] Süess had another important role in these revelations; he had experience in carbon-14 dating. This had importance for climate science since establishing the ratio between carbon-14 and carbon-12 isotopes allows one to measure the amount of fossil carbon in the atmosphere. At this time however the technique was in its early phases of development. The role of the biosphere was not well explored, and getting accurate $CO_2$ measurements involved considerable problems.

hinges on the turnaround of the oceans – a process that plays out over millennia. This piece of the puzzle has been of paramount importance in our understanding of the $CO_2$ cycle.[7]

Problems encountered by physicists were solved by chemists and oceanographers. Both the problem and its solution had relevance to *any* student of the larger cycle. At first the pattern may seem like one involving incremental increases of resolution; a process of fine-graining. Importantly, however, as we gain a better understanding of the various sub-mechanisms, our understanding of the system as whole needs to be revised. The fact that the oceans function like a buffer solution radically changes the role they play. It is also noteworthy that this does not straightforwardly entail an increased integration of disciplines. These discoveries do not seem to define the involved disciplinary boundaries and relations to any further degree than they were defined before. Nonetheless the discovery was fruitful and triggered investigation of anthropogenic climate change.

Other cases seem to exhibit a similar dynamic. One concerns the problem of the 'lost' $CO_2$. As the models became more accurate representations of the way $CO_2$ circulates in the atmosphere[8] and dissolves in water scientists started to compare the emissions with $CO_2$ measurements. More than half of the $CO_2$ appeared to have gone missing. Over the decades that followed, this lost $CO_2$ was located, as it were, piece by piece. It turned out that various biological deposits played a crucial role. This meant that biology became important in this field.

More recently social sciences have taken on a greater role – a change illustrated by the Global Carbon Project, housed within the International Human Dimensions Programme on Global Environmental Change (IHDP) (Global Carbon Project 2003). As human emissions have soared over the last century, social drivers have become more and more important. Hence the social sciences have a clearer mandate now than before. One explicit attempt to establish quantitative relations between various social factors and $CO_2$ emissions uses the so-called 'IPAT identity'. This name denotes a formula *I=PAT*, where *I* stands for human impact, *P* for population, *A* for affluence and *T* for technology. The formula is used to identify drivers of environmental change in general, but also – in slightly modified form – $CO_2$ emissions specifically (see Dietz and Rosa 1997). The success of this model is perhaps still to be evaluated, but at any rate it goes to show that sociologists have taken an interest in studies of the $CO_2$ cycle.

*Asymmetries*

At times problem-feeding is mutually beneficial to the fields involved. Darden (1991, 80) notes that "[t]he extremely fruitful interaction between cytology and Mendelism produced new hypotheses and

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[7] It can be noted, as a matter of curiosity perhaps, that Revelle and Süess were primarily concerned with working out this mechanism in order to explain climate change *in the past*, i.e. they were concerned with the same problem Arrhenius was interested in: the coming of the last interglacial period. Bolin and Eriksson, however, are more concerned with the threat of anthropogenic drivers, and towards the end of their paper they suggest an exponential increase in anthropogenic $CO_2$ emissions. Here we seem to be seeing a slow shift in the overarching problem – from explaining the ice-ages to explaining the sort of climate change that concerns most climate scientists today.

[8] Providing accurate measurements of $CO_2$ in the atmosphere took quite some time; it was not possible until the 1960s, when Keeling set up measuring stations (Weart 2009).

predictions for both fields". For example, a crucial aspect of the insight that genes were located on chromosomes was that facts about the latter's spatial interrelations could explain statistical deviations that had been noted in classical genetics. Hence the solution to the problem of the physical location of the gene fed back into genetics, where it explained various other facts that had previously been unaccounted for. There are, however, other – in a sense, weaker – varieties of problem-feeding. There is *unilateral* problem-feeding: problem-feeding, as it were, without solution-feeding. Todd Grantham (2004, 143) seems to have something like this variety in mind when he talks about heuristic dependence, i.e. "theories and/or methods of a field can guide the generation of new hypothesis in a neighboring field".[9] In cases of mere heuristic dependence the solutions produced in the receiving field may have no bearing on any facts in the field of origin. The relevant problem does not arise *in* the field of origin; hence it may not be a problem *for* that field of origin. Grantham offers no examples but they are easy enough to find; one, close to home, is, of course, philosophy of science. Occasionally philosophers of science produce results of relevance to whatever field they study (or even science in general), but for the most part various scientific fields are used to generate *philosophically* interesting problems. In this sense the location and physical nature of the gene was different; here problems in the field of origin were solved. Similarly in the $CO_2$ case sketched above.

For problem-feeding to be mutually beneficial, both of the disciplines or fields involved need to enter into well-defined – though not necessarily very stable – relations: both need to be made complementary, and this complementarity needs to be made explicit. This ordering of fields settles the terms, at least preliminarily, of the cognitive division of labour. Its mutual acceptance ensures that results in the target field are actually valid in the field of origin. These prerequisites should not be taken for granted. It is arguably a mistake to think of fields – disciplinary fields, especially – as particularly well ordered with respect to their domains of enquiry. Considerable overlaps and unclarity about who has explanatory privilege are not uncommon. As Sandra Mitchell (2009) has pointed out, it is often not clear exactly how different theories concerning the same phenomena relate to each other; different accounts, perceived as alternatives, may turn out to be complementary. If there is to be an ordered research process involving many disciplines or fields, we think that at least some of these potential disputes need to be settled. Darden and Maull, in their conception of scientific fields, omit this from consideration. Their 'fields' are to a large extent understood in terms of their methods, tools, and central problems. This puts the emphasis on certain types of boundary and the transactions that go on between them – at the cost of obscuring others. For instance, two fields may already share (as often seems to be the case) a theory, or conception, of the way in which their respective ontologies relate, and of the reach of their respective methods. Such common preconceptions may dictate the terms of engagement, and the perceived validity of the results, and may also settle the question of how to divide the research task as well. Here the notion that fields and disciplines are inter-substitutable may well be problematic. A discipline has something like a 'self-image', and this commonly includes some idea of explanatory scope. Unlike fields, disciplines seem more methodologically heterogeneous and motile. The explanatory claims of disciplines may be rather expansive, shifting, and in an important sense undecided. In intradisciplinary

[9] Grantham also talks of confirmational dependence, where "methods and/or data in one field may be used to confirm hypotheses generated in a neighboring field" (2004, 143).

discourse outer boundaries are perhaps never, or only rarely, disputed. The problems, or types of problem, are a well established part of the discipline, and the methods and problem-solving schemes are largely settled. This, however, is a luxury that cannot be afforded when it comes to relations with other disciplines, where boundaries and reach suddenly become central. The details of this issue lie beyond the scope of this paper.

A brief summary of the main points: what has been argued in this section is (i) that problem-feeding is a kind of interdisciplinary relation; (ii) that problem-feeding is sometimes what initiates relations between researchers of two or more disciplinary fields; but (iii) that this is not the only kind of situation where problem-feeding as an interdisciplinary phenomenon takes place. Problem-feeding is sometimes made possible by other, preceding interdisciplinary relations, to which we now turn.

## Conceptual drift

That correlation of terms plays a crucial role in reduction and the emergence of interfield theories is reason enough to recognize it as an interdisciplinary relation of some consequence. However, we want to underline the centrality of this interdisciplinary relation by discussing the general notion of conceptual drift, of which correlation and transformation of terms can be understood as special cases.

Two examples from research relating to sustainability science introduce the notion of conceptual drift. The first is straightforward.

*Thermodynamics and Ecological Economics*

In the 1970s and 1980s thermodynamic theory played a formative role in the emergence of ecological economics (Røpke 2004; 2005). Following the work of Nicholas Georgescu-Roegen (1971) among others, both ecosystems and certain human (especially economic) activities came to be described as flows of matter and energy. Hence systems that neoclassical economics had treated as distinct were now viewed as interconnected and interdependent. In ecological economics, the economy is thought of as a system that is embedded within other systems and ultimately bounded by hard physical constraints.

The subsuming of economics (and, as it were, ecology) under the umbrella of thermodynamics is not unlike classical projects of scientific integration and involves conceptual drift as a central constituent. One recent example can be found in Alf Hornborg (2011), who utilizes the thermodynamic framework in his discussion of unequal exchange. Here conceptual drift would be a feature of any quantity imported from a disciplinary field other than the one in which the problem of equal/unequal exchange was originally discussed. The occurrence of a thermodynamic term, like entropy, in a discussion of human ecology only highlights the fact that considerable drift has taken place.

One effect this particular drift has is to amplify the *primacy of a physics constraint* (Ladyman and Ross 2007), i.e. the notion that other sciences have to adjust to physics in cases of inconsistency between them and physics. Physics, because it aims to develop accounts that are true everywhere, imposes absolute limits on other sciences. The conceptual drift makes such conflicts and adjustments more probable – as, of course, did the development of ecological economics in relation to the competing 'distinct systems' view of economics.

As we have already demonstrated, conceptual drift improves opportunities for problem-feeding. Subsuming one problem under a concept that has drifted from another discipline, and then exporting the problem to that discipline, would be a typical case of problem-feeding.

Conceptual drift is a kind of interdisciplinary relation. It is sometimes motivated by a desire to integrate two disciplinary fields. Sometimes it is a secondary phenomenon in relation to problem-feeding – at any rate, as long as we think of primary/secondary motivations for interdisciplinarity. (Even then it may well be primary to problem-feeding in the how-possibly sense discussed above.) Conceptual drift from A to B normally results in the possibility that problems in B can be fed into and (sometimes) solved in A.

*Resilience*
Our second example is less clear-cut. It displays several features of conceptual drift. Nevertheless something essential seems to be missing. Resilience theory was developed within theoretical ecology by C. S. Holling (1972) to describe a certain property of ecosystem dynamics. Inspired by Richard Lewontin's (1969) notion of domains of attraction, Holling argued that certain management strategies – in particular, a strategy of maximum sustainable yield – might involve considerably more risk than had previously been recognized. The reason is that the *resilience* of a system, i.e. the margin the system has before (by force of its own dynamics) it departs from its current domain of attraction, might be diminished by such a strategy. Resilience differs from stability. Holling describes stability as "the ability of a system to return to an equilibrium state after a temporary disturbance. The more rapidly it returns, and with the least fluctuation, the more stable it is" (Holling 1972, 17). On the 'stability view' ecosystems typically have a single equilibrium to which the system will return given any initial condition (save for one where one variable is set to nil to begin with). In contrast 'resilience views' usually recognize several local equilibria. The consequence of this for a specified manager of the system (like a community fishing a lake for food) is that naturally occurring, but random, events such as occasional draughts, hurricanes, or diseases which, under normal circumstances, the system would absorb suddenly become more likely to push the system over the brink. Should that happen, the internal dynamics of the system drive its state parameters to zero.[10] Holling derives from this argument prescriptive consequences for a manager of systems that satisfy dynamics of this particular kind – as ecosystems do, for example.[11]

[10] State parameters here are, of course, species population numbers. The models Holling worked with are based on the Lotka-Volterra predator prey models, with some minor auxiliary assumptions to make them more realistic. Though they are still simplistic, involving only two species (one predator, one prey), Holling shows that they nonetheless have several local equilibria of the kind they can move between given some external forcing of one parameter or the other.
[11] It is interesting that Holling, in this early paper, engaged a rather different issue. He discussed the long-standing problem within ecology of how to relate diversity or complexity, on the one hand, with stability, on the other. Elton (1958) and MacArthur (1955) both argued for a relationship where more complex and diverse systems were

The drift of the concept of resilience has happened in the course of its application in other types of system, particularly what have come to be called 'coupled social-ecological systems' (SESs). This application occurs in two ways. First, there is derivative resilience. Here an SES is said to be resilient in virtue of its ecosystem component; hence some aspect of the social component – say, a particular policy or institution – can be *resilient* on condition that it makes or promotes resilience in an ecosystem that the society relies upon. But there is also a non-derivative use of resilience in which both social and ecosystem components of an SES are thought to be resilient in themselves (e.g. see Adger 2000). The latter amounts to a more substantive empirical claim than the former, but both are cases of conceptual drift, as either interpretation will explain how to relate one set of concepts to another set.

*Imperialism*

Arguably the resilience case can be re-cast as a case of *ecologics* imperialism. Writing about economics imperialism, Uskali Mäki and Catherina Marchionni (forthcoming; see also Mäki 2009) suggest a distinction between *domain-only* and *disciplinary* imperialism. In the former one discipline claims explanatory relevance in a domain traditionally associated with another discipline. This is only to be expected, since disciplines are in general not well confined within their domains. Sometimes this takes place with no influence on the disciplines that were previously the only ones to claim the domain infringed upon. No actual interdisciplinarity needs to emerge. Disciplinary imperialism, on the other hand, is more explicit and invasive. Either the imperializing discipline 'takes over' the domain in question; or the methods (models, theories, and/or concepts) of the domain are adopted by the imperialized discipline (or disciplines). We will concern ourselves with the second variety here, for reasons that are to be made explicit.

Without focusing on imperialism as such we can deploy Mäki's and Marchionni's distinction to point up important differences between the two cases presented in this section. Resilience theory is often presented by its proponents as a sustainability framework that may unify social and natural sciences. However, neither their terminology nor their theoretical framework seems to have caught on with the relevant social sciences. Social scientists, who have traditionally been concerned with the domain of enquiry at issue here, have been unaffected by this conceptual drift. In fact, resilience theory, despite having some influence on policy making, seems to be of interest mainly to ecologists with sustainability leanings. Resilience thinking involves a form of conceptual drift that can be characterized as *domain-only*. It differs from the thermodynamics case, which is *disciplinary* (*and hence interdisciplinary*) in our second sense.

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also more stable (see Redfern and Pimm 2000). This relationship has been contested. It seems to clash with other well established intuitions. In engineering it is generally the case that more complexity yields instability. On Holling's account, complexity (or diversity) is related to resilience in this manner, but not to stability. In fact, Holling argues that low (local) stability is not only consistent with high (global) resilience but may be positively related to it (Holling 1972, 18). A system that has many different local equilibria – as a real ecosystem with a vast amount of different species interacting would – could then perhaps be more adaptable by being able, as it were, to explore a larger part of its phase space. The details of this are, to some extent, elaborated in more detail in later works by Holling – in particular, Holling and Gunderson (2001).

One could, perhaps, object at this point that domain-only conceptual drift is irrelevant for interdisciplinarity. However, there are two reasons why a philosopher interested in interdisciplinarity would want to acknowledge domain-only conceptual drift. First, domain-only drift often promises disciplinary drift. The former is not sufficient, but it may nevertheless produce a how-possibly mechanism, eventually generating interdisciplinary relations. Resilience theoreticians provide conceptual structures that might be endorsed by social sciences; they see these structures as a way of unifying dislodged and disconnected domains. We agree up to a point: resilience theory provides mechanisms that might do the job. Second, other factors, like asymmetry of standing, or just plain suspicion, can certainly affect uptake of concepts in the target discipline. True, these other factors are crucial, but unless someone has reason to think the actual conceptual structures in question in fact apply to another domain that set of problems would not even arise.

Not all types of conceptual drift amount to something as substantive as correlations of terms, transformations, or full scale reduction; borrowings may be metaphorical. Stephen Kellert (2008) notes that metaphors may serve an important role in structuring a domain that lacks structure, or re-structuring one that already has structure. This newly imposed structure may invite further enquiries in ways that were not imagined before. It has a heuristic function. When a new metaphor is brought into a discipline for this purpose it is necessary to map the metaphor onto the target domain in some fashion. Hence partial correlation, at least, is already in play, as certain components of the metaphor are attached to certain features of the target domain. Notably, metaphorical use of a concept does not commit the user to very much. The vast majority of domains can be structured in various ways; different structures will suit different ends, and these ends, in themselves, may only be 'active' for limited periods of time. In this sense there is a perfectly admissible way of correlating terms temporally.

Another point worth noting is that the application of conceptual structures to new domains sometimes generates considerable changes in the conceptual structures involved in the drift. Both the original and receiving fields may change. The notion of resilience is notoriously unstable and has been interpreted in countless ways (most of them in Holling's work). Thermodynamic concepts, on the other hand, seem much more stable – probably, for the plain reason that the original context of thermodynamics is independent of what happens when thermodynamic concepts drift into ecological economics and other contexts. Darden and Maull's mutation example is rather distinctive in this respect. Changes and additions appear to have been made to the notion of mutation, but it was stable enough not to split. The long-term reason for that is arguably that the knowledge claims associated with 'mutation' turned out to be correct. In the short term, however, something else needs to be in place to guarantee such stability. We are inclined to think that stability is due to some kind of mutual agreement between the involved fields.

A brief summary: conceptual drift is the sharing, borrowing, or even stealing of concepts and conceptual structures from other disciplines or fields. This requires one or more new concepts to be linked with some other set of concepts. In Nagelian reduction this is achieved wholesale across complete theoretical languages. However, linking can be more localized and limited. Ordinary cross-disciplinary borrowing may involve just a few concepts. The most local variety involves special languages of the sort often thought to be an essential ingredient in getting interdisciplinary projects to work and equally often failing to make it beyond the project – the unofficial dialect of a particular research programme.

# Methodological migration

Conceptual drift is by no means the only way to solve problems with the aid of another disciplinary field. This is easily seen in cases where the appropriate conceptual bridges are already in place. In some such cases there is a choice between actually exporting the problem or building the appropriate competence by borrowing some of the methods deployed in the other field to solve problems of the sort one has previously exported. This may also be possible in cases where conceptual bridges are not relevant.

*Prima facie* methodological migration can occur in several ways. For instance, we could make use of Mäki and Marchionni's distinction again. Methods can be migrated *domain-only* where a method already known within one discipline is deployed to extract information from a domain to which it has not previously been applied. An example of this can perhaps be drawn from the same pool as our resilience example; from within the ranks of ecology there has been a degree of optimism about the power of the methods used there. As before, this does not have to result in actual interdisciplinarity, since the other field operating in the domain need not be influenced. *Disciplinary* methodological migration, on the other hand, involves the taking up, within a discipline, of some methodology from elsewhere.

For the sake of clarity it is perhaps also useful to separate disciplinary methodological migration from what has been called methodological integration; and to point out that only the former is a case of migration. Methodological integration is "the development of particular methods to integrate the bodies of data generated by two fields" Grantham (2004, 144). A *single* method is developed to process data sourced from different fields of enquiry. Methodological integration is clearly interdisciplinary, but it is not what we want to focus on here. It involves joint methodological efforts and problem-solving, but not migration. (What migrates here are data. Data migration could usefully be added to our non-exhaustive list of problem-feeding, conceptual drift, and methodological migration.)

Disciplinary methodological migration can result in the migrating method out-competing the methods that were previously in play. Two observations by Ronald Coase (1978, 204) (see also Mäki 2009) are interesting in this context:

> [I]n the long run it is the subject matter, the kind of question which the practitioners are trying to answer, which tends to be the dominant factor producing the cohesive force that makes a group of scholars a recognizable profession… However, in the short run, the ability of a particular group in handling certain techniques of analysis, or an approach, may give them such advantages that they are able to move successfully into another field or even to dominate it.

Methodological migration, to reframe Coase's view, is a powerful but temporary interdisciplinary relation. This can be contrasted with the position of Margaret Morrison (2000), who claims that unification usually results from the use of similar mathematical techniques.

More often, perhaps, methodological migration generates situations in which methods are somehow used in concert. Migration results in methodological pluralism. 'Mixed methods' research is one possible result. But the label 'methodological pluralism' collects several distinct varieties. Some problem-feeding events are such that several distinct methods are used more or less in sequence to solve various linked problems. At least one non-sequential variety is similar: certain phenomena have multiple causes the investigation of which is carried out using different methods. A third kind of methodological pluralism that might result from methodological migration is illustrated by the use of multiple methods to obtain results that are robust. In this case methodological pluralism involves the use of a set of disparate methods to achieve an epistemic end: for example, solving a particular problem complex (or chain) or checking results for robustness.

Disciplinary methodological migration consorts well with our account of interdisciplinarity, but cases of it do not always fit into that account unproblematically. Simply sharing a certain method is not always sufficient for interdisciplinarity, even if the method has migrated from the one field to the other. For instance, statistical analysis is widespread in both natural and social sciences. However, this does not seem to warrant talk of interdisciplinarity. Why is this? Is it because the migrating method is not sufficiently anchored in the field it from which it drifts? This would be alarming news to those involved in the many unificationist programmes. Philosophers trying to facilitate the unification of A and B would risk finding that the very fact that unification was provided by methods (or concepts, or problems, or some such) suggested by a third party prevents interdisciplinarity between A and B. The risk should not be exaggerated, of course; further developments between A and B would stand a better chance of being truly interdisciplinary.

Howsoever that may be, in our opinion the most interesting interdisciplinary cases of methodological migration typically centre on problem-solving processes. Resilience theorists claim that the methods characteristically pressed into service by to ecologists, like certain types of modelling, are useful even in sustainability contexts. A (rather rough) reconstruction of the reasoning behind this would go something like this: ecosystems have a number of features that are of paramount importance in our understanding of those systems, such as non-linearity, complexity, the possession of many domains of attraction, self-organization, and so on; mathematical models of ecosystems, like the Lotka-Volterra equations, assume little about the systems they describe; so these models may be applicable to a larger class of systems sharing some of these properties and characteristics. It is then claimed that societies do in fact qualify as the relevant type of system, and that the methodological approach can be fruitfully applied in the study of them.

Two points need making here. To begin with, this last analogy claim is a substantial ontological claim ordering two domains in a manner similar to the one we have discussed above. Secondly, and perhaps a little more subtly, it does not follow, simply from this analogy, that the methodology will yield any interesting information. Hence there are two ways this potential methodology migration can go wrong. The analogy may not hold. Alternatively (or additionally) the new phenomena, while belonging to the general class of things appropriately investigated by the method, may also belong to other classes that for some reason are thought more important. Societies might well be complex systems. However, whereas that is an interesting feature of ecosystems, it is perhaps not an interesting feature of societies.

# Discussion

Interdisciplinarity is often construed in terms of integration of some sort, and the much-used multi-, inter-, transdisciplinarity trichotomy is usually set up accordingly. We owe this trichotomy to Jantsch (1972). It has been fleshed out in various ways, but standardly multidisciplinarity is the mere juxtaposition of knowledge; transdisciplinarity involves the sharing of some set of axioms (hence approaching disciplinarity it would seem); and interdisciplianrity is conceived of as the middle ground, being 'both integrative' and 'boundary maintaining'. The term 'integration' is, however, ambiguous. It can be interpreted in different ways, depending on what it is that is supposed to be being integrated. Most often, looking at matters from the point of view of philosophy anyway, we see integration being thought of in terms of theories. As we have pointed out, even when integration is methodological it seems to be executed against some theoretical assumption or other. Theoretical integration can mean a lot of different things. Reduction is often construed as a kind of theoretical integration, but so is the formulation of interfield theories. The motivations may differ accordingly. Epistemic values, such as theoretic parsimony, may warrant eliminative reduction; criteria of a more ontological, or even metaphysical, character may alternatively be employed; our theoretical predicament may tell us more about how the world is after integration than it did before, parsimony notwithstanding. An emphasis upon such ontologically motivated integration makes the integrative process appear cumulative. In particular, it comes to seem that the integrative process will leave the involved parts more integrated after it is completed than they were before it began. Given this measure of interdisciplinary success, it is tempting to think of cross-disciplinary lapses of interaction as failed integrative projects. Their temporary character is owed merely to a false hypothesis. Once the mistake is discovered, the involved disciplines disengage in mutual disappointment.

Certain cases of problem-feeding may serve as an exception to this story about success measured in terms of integration. Problem-feeding involves obvious epistemic goods; one manages to solve problems that would otherwise be impossible, or at least very difficult, to solve. The end result is not that the disciplines involved become more integrated. In the case of the linking of, on the one hand, the buffering mechanism in the oceans and observational results of its slow overturn, and, on the other hand, physical models of the atmosphere, it seems that the important links between chemistry, oceanography, and physics, were in place already. They did not become more closely tied to each other in this process – not in any significant sense, anyway. Nonetheless, an important problem found its solution.

Another interesting aspect of problem-feeding concerns a point quite different from those discussed until now. In modern discussion of interdisciplinarity – which has been ongoing since Sharif and Sharif

(1969) at least – a recurring theme has been a concern that there will be a trade-off between the depth and breadth of knowledge. The ultimate responsibility of interdisciplinary work seems to land on the already burdened shoulders of individual researchers. The worry has been that it is not feasible to train individuals systematically. The individuals involved will simply not be able to digest the amount of information required, and they will therefore be obliged to prioritize breadth over depth. Indeed there are many cases where theoretical constructs have been deployed out of context with questionable results (see Kellert 2008).

In problem-feeding interdisciplinarity, however, it seems that it would be enough for individual researchers to know who they should to consult, i.e. in effect, what discipline is likely to provide a solution. Admittedly, this is not always very clear – as Larry Laudan (1977) has noted; and there will certainly be cases where substantial work needs to be done before problems can be assigned correctly. But in many other cases this is likely to be less problematic. Problem-feeding, for the most part, does not happen in the minds of individual researchers, but in the thinking of groups of them. It is a collective effort, allowing for considerable division of cognitive labour.

An important aspect of problem-feeding (and a possible source of problems for accounts of it) is that it relies on the relative stability of the fields and disciplines between which the problems are supposed to pass. For instance, it was impossible to work out the physical location of the gene within transmission genetics because the methods and tools available there would not allow it. But why did not transmission geneticists simply acquire the tools they needed? Fields and disciplines routinely do this kind of thing. There is a danger here of setting up problems for oneself by making up artificial boundaries between entities. Cognitive boundaries that seem sharp, using Darden and Maull's notion of a scientific *field*, may be substantially blurred by the fact that the actual researchers who are active within the involved fields have extensive contact or are even identical. Moreover, social states of affairs to one side, Darden and Maull's idea of fields allows for substantial theoretical overlaps to occur that may explain how problems get assigned in the first place. In our discussion above it should have become clear that such preliminary connections need to be in place before any further integration or interaction can take place. Such preliminary connections, however, need not always be particularly stable.[12]

# Conclusions

We have distinguished three kinds of interdisciplinary relation. Methodological migration involves the transfer of methods across disciplinary boundaries. Conceptual drift concerns the connecting of terms between disciplines. Our main focus has been on problem-feeding. The transfer of problems between disciplines is interesting for several reasons, but as yet it has not been studied in sufficient detail and depth. This study is preliminary; hence its conclusions remain rather limited.

[12] Furthermore, it is quite clear that modelling disciplines solely on scientific fields in this sense is seriously simplistic. Bechtel (1986; 1993) suggests that we view disciplines as aggregates of fields, both in the sense mentioned above and in Bourdieu's sense, where the emphasis is on social aspects (competition for authority and power). This complicates the picture considerably.

We would nevertheless like to end by making a tentative suggestion with prescriptive consequences. In this paper we have drawn our examples from the fields of climate science and ecological economics, both of which are integral to the formation of the emerging field of Sustainability Science. One of the central problems in Sustainability Science centres on the connections running from certain natural phenomena, like climate change, to certain social and human phenomena, like democracy, economic growth, development, equity, and so on. A paradox seems to loom over explicit endorsement of the pluralism in the field together with the anticipated calls for integration and unification of natural and social sciences – calls often responded to by a search for a common and substantive shared theoretical framework. Resilience theory is one such framework. We think, however, that the focus could be more local and bottom-up. Instead of trying to find an overarching point of reference, one could 'follow the problems.' Perhaps certain assumptions would still have to be shared, as we have indicated, but they would not have to be particularly far-reaching or stable. Carrying problems across disciplinary boundaries and getting answers back is often stability enough.

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