

## Why integrative pluralism?\*

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This article is an exposition and defense of a perspective I call 'integrative pluralism'. I will argue that integrative pluralism is the best description of the relationship of scientific theories, models, and explanations of complex biological phenomena. Complexity is endemic in biology, and various features of multicomponent, multilevel, evolved systems constitute it. The types of scientific representations and the very methods we use to study biological systems must reflect both that complexity and variety. Developing models of single causal components, such as the effects of genetic variation, or of single-level interactions, such as the operation of selection on individuals, give valuable, if partial, accounts. These explanations need to be integrated in order to understand what historical, proximal, and interactive processes generate the array of biological phenomena we observe. Both the ontology and the representation of complex systems recommend adopting a stance of integrative pluralism, not only in biology, but in general.

Clearly, the way the world is dictates what we can say about it. The way our representations are structured also plays a significant role in the scientific accounts we develop. Theories and models are idealized, partial descriptions, couched in the conceptual frameworks of the day, framed in a language that carries meanings from the broader social context. The suggestion that our current best theories of the nature of nature exactly capture the world in all its details is hubris. The idealized and partial character of our representations suggests that there will never be a single account that can do all the work of describing and explaining complex phenomena. Different degrees of abstraction, attention to different components of a system, are appropriate to our varying pragmatic goals and conceptual and computational abilities.

If different models are perceived as partial solutions to a question, then one might argue that a theory of division of labor in social insects, for example, would be one that correctly unified the partial accounts. However, while integration of the partial accounts is indeed required for explaining a concrete particular, unification at the theoretical level is unlikely to be very robust. This is due to the nature of the complexity

characterizing the domain of phenomena studied. It is the diversity of the 'solutions' to adaptive problems and the historical contingencies influencing those variable paths that preclude global, theoretical unification. Ants, for example, exhibit division of labor behavior similar to that of bees. Individual task performance varies with age, learning, and in response to internal and external contexts. Ant colonies, however, contain negligible genetic diversity compared with bee colonies. Thus, the theoretical constituents that would be integrated in the explanation of division of labor in ants would not be the same set as those required for the explanation of the 'same' phenomenon in bees. For ants, a self-organization model based on genetic diversity would often not apply. Even that claim is contingent, since Boomsma, *et al.* (1999) has detected in one species of ant nearly as much genetic diversity as in honeybees. Thus, what in fact explains the existence and characteristics of division of labor in social insects, what appeared at first sight to be the 'same' phenomenon requiring a single explanation, will be itself contingent on the particular features and pathways that occur in each case. At the same time, there is only one 'true' integrated explanation for honeybees and one 'true' integrated explanation for leaf-cutting ants, and hence competition among explanations in specific cases will, and does, occur. Nevertheless, compatible pluralism will remain for the models of potentially contributing causes, if not in the application of the models to explain a specific, concrete event.

A promising model of pluralism can be forged from understanding that causal models are abstractions that will always remain idealizations. By making simplifying assumptions regarding the noninterference of other potential causes, causal models describe only what would be expected in idealized circumstances (Levins, 1968). This conception of theories helps to explain how it can be that models of different causal factors *qua* models do not directly conflict. However, even if the models may be jointly consistent, in the application of models to the explanation of a concrete case, conflict can arise. In actual cases, multiple causes are likely to be present and interact, and other local elements may also contribute to a specific causal history. Thus in explanation, models of variant possible contributing factors must be integrated to yield the correct description of the actual constellation of causes and conditions that brought about the event to be explained.

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“Our choice of models, and to some extent our choice of words to describe them, is important because it affects how we think about the world... [O]ur choice of model decides what phenomena we regard as readily explicable, and which need further investigation.” (Maynard Smith, 1987: 120)

Pluralism in scientific theory and practice has certainly been studied and advocated before. As a result of historical investigations of scientific methods, Feyerabend was led to defend *epistemological anarchism* (1975, 1981). He endorsed any method as being as good as every other in generating acceptable scientific results. Like Feyerabend's, the most recent defenses of pluralism in science have launched their accounts from an epistemological perspective. Feminist stance theory, for example, grounds pluralism in a *perspectivalism* based on individual and group social experience (Harding, 1986; Longino, 1990). Other disunity supporters argue from the partial character of descriptions or diverging areas of interest of the researchers (Cartwright, 1980, 1982, 1989, 1994; Dupré, 1983, 1996). In contrast, a few have linked pluralism with ontological views about natural kinds (Dupré, 1983, 1993; Hacking, 1996). While my approach draws on many of the insights of these philosophers, it differs from them in grounding pluralism jointly on metaphysical and epistemological arguments, and appealing to complexity as a critical tool for understanding the nature and limits of diversity in scientific methods and representations.

It has long been argued that biology has no laws (Beatty, 1995; Smart, 1968). Yet biologists speak of 'laws' in their writings. One of Mendel's 'laws' claims that with respect to each pair of alleles at a locus on the chromosome of a sexual organism, 50 percent of the organism's gametes will carry one representative of that pair, and 50 percent will carry the other representative of the pair. Recently, a number of biological scaling 'laws' have been discovered. These include Kleiber's law that metabolism increases in proportion to body mass raised to the  $3/4$  power, and the scaling law that respiratory rate is inversely proportional to body mass raised to the  $1/4$  power (Pool, 1997; West, *et al.* 1997).

Why do some philosophers fail to count these results of biological investigation as laws? How are they different from Proust's law of definite proportion, or Galileo's law of free fall or the conservation of mass-energy law? Those who argue that there are no laws in biology point to the historical contingency of biological structures and the particularity of the referents in biological generalizations as grounds for excluding the law designation. In considering the problem of the existence of biological laws I was led to a general reflection on laws in science. My conclusion is that we need to think about scientific laws in a very different way:

to recognize a multidimensional framework in which knowledge claims may be located and to use this more complex framework to explore the variety of epistemic practices that constitute science.

### Why reductionism is compelling

A compelling argument can be made for reductionism. We live in one world, not many worlds. Further, the material from which all the entities in the world are built is ultimately one kind of 'stuff', that is, matter. It is the job of scientists to represent the features of that one world in a way that allows us to explain the patterns of phenomena we observe, to predict what will occur in future situations, and to permit us to intervene in ways that allow us to accomplish our practical goals. The next step in this argument is to add the assumption that the representations scientists come to accept as true stand in some sort of strong mapping relation with the actual features or structures of the world. The strongest form of this argument would assume that the representations most widely accepted by the scientific community are direct mirrors of the world's structure.

If this were the case, then we would expect generally accepted, but diverse, representations of a given domain - for example, the different accounts of division of labor in social insects - to stand in some strong mapping relation to each other. What is that relation? Minimally, we would expect the representations to be consistent with each other. If the world is one, and scientific claims accurately describe it, then two contradictory statements cannot both be true. But reductionists argue for a stronger relation than this. They argue for intertranslatability, or derivability. Ultimately, there is one fundamental, maximally accurate description from which the others can be derived. These are the earmarks of a reductionist view of science. While intertranslatability is symmetric, derivation is not. There is a preferred ordering to the direction of such a reduction, and that ordering is generally provided by another metaphysical assumption, namely, compositional materialism.

The material composition assumption that every object is made up of one type of substance, namely, matter, suggests that there is some basic level of description of the material building blocks (Moser & Trout, 1995). The privileged level of description is taken to be the most fundamental, and one can come to understand the more complex objects by knowing the properties of the simple components and the composition functions. So if atoms make up molecules, and molecules constitute chemical elements, and elements make up different types of material objects, and material objects are the parts of cells, and cells make up organisms, and organisms make up societies, then if

we understand atoms (or quarks or whatever we take as fundamental) and how these combine to form less fundamental objects, that should suffice. Scientific truths of biology then could be restated as truths about chemistry, those in chemistry in terms of physics. So the reasoning goes.

Indeed, if we believe that a description at the most fundamental level - physical or material description - is sufficient to describe the causal interactions responsible for all changes of state, that is, if we endorse the doctrine of causal completeness, then while descriptions, explanations, and predictions in a language of biology might be convenient (and true, if translatable or reducible to the fundamental level of representations), they are not necessary. We could, in principle, reduce all the diversity of the representations of contemporary science to statements describing the fundamental elements (see Causey, 1977).

### **Yet reductionism doesn't capture the realities of scientific inquiry**

**G**rounds for rejecting reductionism for all cases are found in a more comprehensive analysis of the nature of scientific representations. The required simple mirroring relationship between theory and world does not hold. Since Kant, most philosophers accept that every representation will be shaped, in part, by the concepts that humans bring to the task of describing the world. While compositional materialism may be correct that all entities are made up of matter, the inference to a logical 'composition' relationship between the 'entities' in our scientific theories is not immediate.

Scientific representations are abstractions or idealizations (Cartwright, 1980, 1982, 1989, 1994; Dupré, 1983, 1993, 1996; Wimsatt, 1987). They can represent only partial features of individuals rather than the individuals themselves as complex causal agents. For example, parasite and host theories explain population-level interactions and identify abstract individuals in terms of their roles in those interactions. The theories are then used to explain actual interactions between concrete individuals such as particular malaria viruses and humans. Yet the abstract individual identified by its functional role in the population models clearly does not exhaustively describe the individual as a causal agent (Dupré, 1993). An individual human being is truly described in different theories at the same time as a host to a parasite, a consumer in an ecosystem, and a phenotypic expression of a set of genotypes, as well as a mammalian organism, a homeostatic endotherm, an organization of multiple cell types, and so on.

Actual, complex events or concrete individuals - the constituents of the one-world ontology - are, at

the same time, instances of objects of multiple abstract theories concerned with different compositional levels. Reductionism requires replacing the higher-level abstractions by lower-level ones. Yet the abstractions, which constitute theoretical objects at the different levels, do not constitute identical representations across levels. That is, even if the descriptions at the various levels are all accurate, by being partial they may not be representing the same features of nature and hence would not stand in any straightforward derivability or intertranslatability relation, nor form a neat, nested hierarchy. Dupré defends this type of picture. He is a materialist but denies that "there is any interesting sense in which ontological priority must be accorded to the allegedly homogenous stuff out of which bigger things are made" (1993: 89). Thus, Dupré endorses a weak, compositional materialism, that is, the view that "whatever kinds of things there may be, they are all made of physical entities" (p. 92) that permits anti-reductionism. The material from which entities are composed does not carry all the explanatory weight. However, it is the association of matter with cause and the further alignment of cause with explanation that makes materialism and antireduction difficult bedfellows. In considering these issues in general, Dupré addresses the argument that runs from the requirement that explanations at different structural levels must at least be consistent with one another. That such consistency is ensured by reduction is obvious, but Dupré points out that the only way to reach such consistency entails that reduction rests on a particular view about causality, namely, the assumption of *causal completeness*.

Thus, reductionists present an even stronger argument that higher-level descriptions are dispensable. Causal closure is a doctrine about sufficient cause. Dupré considers this argument and rejects it. Dupré's reconstruction of the reductionist's argument is as follows: Consider an event at the microphysical level. Dupré uses the example of the motion of an electron in one's index finger. As a person moves one's finger to type the letter 'b' on a keyboard, the electron has to move. If the microlevel is causally complete, then there is a set of events at that level, prior to the motion of the electron, that is sufficient to cause it to so move. Nevertheless, an explanation might be given at a macro level, that is, by appealing to one's intention to type the letter 'b' to explain the motion of the finger that, of course, entails the movement of the electron. Since what is going on at the micro level, given the assumption of causal completeness, is sufficient to bring about the motion of the electron, the macro story must be at least consistent with the micro causal story. Furthermore, since causal completeness presumes sufficiency at the micro level, nothing more at the macro level is necessary to bring about the movement of the electron.

Hence, macro-level events appear to be causally inert. Since on this account, explanation is in terms of causes and all one needs for the causal story is microlevel events, all explanation could be run at the micro level. But this is just to restate the doctrine of in-principle reductionism.

This is a powerful argument, and one that sits behind many contemporary defenses of some form of reduction. In the face of it, many have promoted what Dupré correctly identifies as a weak form of reductionism, namely, *supervenience*. Others have bitten the bullet on causal explanations being the domain of the micro level and have opted for a defense of some other kind of explanation as appropriate to macro-level science. But Dupré wants to defend a much more robust pluralism and so claims that "... a central purpose of the ontological pluralism [he has] been defending is to imply that there are genuinely causal entities at many different levels of organization. And this is enough to show that causal completeness at one particular level is wholly incredible" (1993: 101). That may well be his purpose, but we have to look at whether or not he can get there with the arguments he provides. Dupré attempts to invert the reductionist *modus ponens* (causal completeness requires reductionism) into an antireductionist *modus tollens* (the failure of reductionism implies the falsity of causal completeness). While Dupré's account of the relationship between the two theses of causal completeness and reduction is correct on a purely ontological interpretation, his arguments do not accomplish the inversion he desires.

What Dupré uses as evidence that reductionism fails is the implausibility of it succeeding in any of a variety of the cases in biology and psychology that he considers. But all this shows is that reduction is unavailable in fact in these cases. It does not show that it is impossible in principle. It is the second, stronger claim that is needed to overturn causal completeness, for it is in principle reduction that figures as the conclusion of the reductionist argument.

I suggest a different argument that can be launched against reductionism. The reductionist argues that if the world is composed of physical matter and if all composite entities are made up of just physical matter, then if there is an account of the complete cause of events at the physical level, nothing more causally, and hence explanatorily, is required by higher-level descriptions. But how do we understand that? There are two ways.

First, metaphysically, it is the view that there is at the physical level a single, unique process of physical interactions that bring about a physical result. Unless one adopts a position of uncaused events, this does

seem inescapable. But what about representing this process in physics? One can believe that all events are caused and at the same time argue convincingly that all the factors contributing to the complete cause cannot be represented by any single theory (and the representational mechanism that instantiates it) in physics. The local, contingent constituents of every causal process just, are not included in the scope of physical theory, as Cartwright has argued (1994), and these will always be part of the complete cause. So there may well be causal closure at the level of physical entities, while there is always incompleteness, or causal openness, in the representation or theorizing about those processes, the representations that make up the physics entities.

There is a further source for disarming causal completeness as the ground for reduction. That is the view that there are two aspects to composite, complex objects or events: the material and the manner in which the material is arranged, that is, matter and form, or material and structure. Some patterns in the world that we identify as causal processes may depend as much on the structural characteristics of complex objects as on the material characteristics. In fact, as Bechtel and Richardson (1993) have suggested, there may well be a continuum of contributions from matter and structure such that actual causal processes occupy many different locations.

How does this bear on causal closure? Well, if the physical level is construed only materially, then structure is a level up and causally significant. Hence causal closure is false. If, however, structure is included in the physical level, then macro objects are physical and closure applies to a collection of micro and macro objects and events. Either way, reduction to a purely material physical level is thwarted.

By breaking the connection between physical material and physics representations, the arguments for causal completeness and theory reduction are no longer as closely linked, and Dupré's reverse inference from nonreduction to noncompleteness does not go through. Nevertheless, there are conceptual and empirical arguments against the inference from causal closure to reduction that I believe make a strong case for the rejection of reductionism.

There are recent, seemingly new, arguments for reductionism, especially in the 'new wave' reduction of psychology to neurophysiology (Bickle, 1997; Churchland, 1986). These permit appropriate modification of terms and claims of the two sciences to accommodate the elimination of false claims by reduction to lower-level true claims, thus recognizing that the mapping between terms need not be direct. There are specific objections, concerning the translatability,

eliminability, and causal efficacy of functional properties that challenge the newer versions of reduction (Schouten & De Jong, 1999). Each new wrinkle in the reductionist position elicits new responses from anti-reductionists. Rather than counter every new twist in the reductionist argument, I will instead attempt to articulate and explore an alternative view of the relations among scientific claims.

### Alternatives to reductionism

'Anything goes' pluralism is associated with Paul Feyerabend's epistemological anarchism (Feyerabend, 1975, 1981). In the absence of foundational standards of justification, Feyerabend argues that any and all forms of argument are acceptable in science. Some versions of social constructivism (see Collins, 1982, 1985; Latour, 1988; Woolgar, 1988) can be taken to promote 'anything goes' pluralism, at least as far as truth is concerned. Perhaps it would be more aptly identified as 'anything that scientists accept goes'. This shifts the criteria by which different scientific claims are granted authority from their representational adequacy to their social authority. Thus, one might argue that as long as some group of self-identified scientists agree to accept a claim about the world, then it 'goes' and the constraints on the ways in which scientific groups are formed may be loose enough to condone an interpretation of 'anything goes'.

I find both the advocacy of retaining all, possibly inconsistent, theories that emerge from a community of investigators and the insistence that any collection of analyses of the same phenomena must be reduced to a single theory equally unacceptable. The challenge is to define clearly the middle ground: How can a diverse, well-confirmed, but irreducible set of theories be used collectively to achieve a more complete understanding than any of the theories taken in isolation?

A *prima facie* problem for reductionism is the apparent diversity of theories in a science such as ecology. It has been argued that some of the compatibility of diverse accounts can be explained by the divergence of questions and interests that the scientist brings to the table. Mayr, Tinbergen, and Sherman have all defended a 'levels of analysis' account of the plurality of theories and explanations that is almost dogma within the biological community (Mayr, 1961, 1982; Sherman, 1988; Tinbergen, 1963). Mayr distinguished between proximate 'how' and ultimate 'why' questions in the face of the possible encroachment of genetics into whole organism biology (Beatty, 1994). Tinbergen elaborated four different kinds of questions posed in the study of ethology. A recent revival of this model by Sherman further subdivides the four 'levels' of questions that partition biological research. These

are: evolutionary origins, functional consequences (the two 'why' questions), ontogenetic processes, and mechanisms (the two 'how' questions). Sherman's addendum divides questions of mechanisms into those that target physiology and those that target cognition (Sherman, 1988).

For example, biologists explaining division of labor in social insects might approach this phenomenon with four different questions. Those concerned with evolutionary origins would investigate why this behavior arose when it did multiple times in the lineages of social insects; those with functional concerns would ask what current consequences the trait has on colony ergonomic efficiency and hence fitness; those with queries about ontogenetic processes would aim to explain how the various behaviors come to be expressed in the individual insects over time; and those interested in mechanisms would detail the environmental triggers and hormonal or cognitive mechanisms that then issue in the behaviors. Different questions invoke different explanatory schemata. Sherman claims, "Every hypothesis in biology is subsumed within this framework; competition between alternatives appropriately occurs within and not among levels" (Sherman, 1988: 616). Thus, answers to questions at the different levels represent compatible components of a pluralistic, multidimensional body of knowledge. On this view, there is no need to account for intertheory relations among the levels.

Even if we granted that this describes current scientific practice, the question remains as to what relations should connect these autonomous enterprises. One reading of this type of compatibilism leads to an isolationist stance with respect to the separate analyses. If there is no competition between levels, there need be no interaction among scientists working at different levels either. The problem with the isolationist picture of compatible pluralism is that it presupposes explanatory closure within each 'level of analysis' and a narrowness in scope of scientific investigation that precludes the type of fruitful interactions between disciplines and subdisciplines that has characterized much of the history of science (see Darden & Maull, 1977). Even when the unlikely situation occurs that a scientist is narrowly concerned with only one level of analysis, it is a mistake to think that the answers to other questions have no bearing on the investigation at that level. There may be causal dependence or causal interaction between processes described by the different analyses. If so, remaining within a single level will fail to provide understanding for the questions addressed at that very level (Bechtel & Richardson, 1993). Thus, the division of levels of analyses, different questions with different answers, could be mistakenly interpreted as a justification for unproductive heuristics for scientific

investigation.

The answers to different questions appeal to distinct abstract models of causal processes as well as specify their application to concrete instances. Fundamental scientific laws used in explanations of concrete phenomena have *ceteris paribus* clauses (Cartwright, 1980). In other words, they describe what is to be expected in idealized situations, when only one of a set of potential causal factors is operating, that is, when nothing else is interfering. All simple models also suffer by design from this type of unrealism. If we accept that multiple causal factors can, and often do, interact in the production and maintenance of the phenomena that the real cases are complex, without the *ceteris paribus* proviso, the laws would be literally false. With the proviso, however, such models do not themselves directly account for many, if any, real cases. That is, while abstract models describe the effects of the operation of a single causal process, our world does not normally approximate the ideal world that the models directly represent. The concrete explanatory situations on which we bring the abstract models to bear are messy, perhaps unique products of historical contingencies and interacting, multiple causal factors. In this sense, the simplified, abstract, theoretical models developed in science are literally false (cf. Wimsatt, 1987). This does not make them undesirable or useless (cf. Richerson & Boyd, 1987). Indeed, simplification allows models to be mathematically and empirically tractable for increasing precision of assumptions, crisper testability, and allowing counterintuitive results to be generated. The robust convergence of results of a variety of simple models is evidence that the result does capture a feature found in the complex world.

Nevertheless, the pragmatic virtue of simplicity is most frequently bought at the cost of realism in explanation (Levins, 1968). Though, of course, it is possible that a single causal process may completely determine a particular event, given the complexity and diversity of the phenomena, simple models are more likely to capture partial causes. In this situation, individually more complete models may be developed, if there are means for replacing the false assumptions in the model by more realistic descriptions (say, in models of language acquisition or measures of genetic fitness). At the most concrete level in generating an explanation, a model may introduce all of the relevant features that uniquely characterize a given event (say, the ecological conditions of a particular lake). However, the cost here is that of generality. A full-scale map of a town would express the greatest realism; however, it would be as useless for finding city hall on a map as one that represented the town as a single point. So, too, all that is true in a concrete model of Clear Lake will likely not apply directly to a neighboring lake. It is by distinguishing

the idealized models from their applications that we can identify the location and scope of integration.

In short, *isolationist pluralism* employs a levels-of-analysis framework to endorse a strategy of limiting interactions between various theories offering explanations in a given domain. While some scientists may restrict their interests to a specific level only, this is not necessarily the case. Pluralism better describes the causal models that, by modeling the contribution of individual causes, necessarily abstract away the operation of other compounding factors. By so doing, they can make no incompatible claim about the operation of the ignored causes. Once this structure of causal models is recognized, one may understand why competitive interactions arise within a level (e.g., how best to measure fitness in determining the current function of a cultural trait). However, the model must be distinguished from its application. In application, one can immediately see that causal models that provide answers at different levels are indeed related. Thus, although pluralism is to be defended, it is not the pluralism of questions and the consequent independence of answers, but rather a pluralism of models of causal processes that may describe contributing factors in a given explanatory situation. This is not to recommend an 'anything goes' pluralism. Not all explanations are equally good. Hence, to defend a strategy of pluralism for causal models and criticism of explanatory applications of those models requires a further account of how idealized models are to be integrated into explaining concrete, nonideal cases.

### Integration

I have argued elsewhere that the dual complexity of the phenomena studied by scientists and the diverse interests and pragmatic constraints on the representations scientists devise to explain the phenomena conspire against simple pictures of scientific knowledge (Mitchell, *et al.*, 1997; Mitchell, 2000). Correspondingly, the strategy for integrating diverse theories and explanations will not be algorithmic. This is evident from even a superficial investigation of how genetics and population biology are jointly modeled, or of current models of how the biochemistry of hormone production in a developing organism affects and is affected by the external environmental conditions in which the organism finds itself. The genetics of a population will constrain the variation on which natural selection can operate, and the operation of natural selection can change the genetic constitution of the population. Complex systems, like those studied by biology, are going to harbor multiple, interacting forces at different scales, with variable temporal orders operating in diverse combinations in different particular situations. Integration of theories and models in such cases will not be as simple or global as in the case of, say, vector addition of electromagnetic and gravitational

forces in physics.

The work of Michael Friedman (1974) and Philip Kitcher (1981) seems to acknowledge the persistence of multiple potential descriptions of individual phenomena - the pluralism side of the story - but they marry it to a unificationist goal rather than an integrationist goal. Indeed, they argue that the unification of ever more phenomena under one theoretical schema is central to scientific explanation. This can be seen in Kitcher's criteria for identifying scientific progress with the acceptance of theories embodying the fewest explanatory schemata for the widest phenomenal coverage. This type of explanatory unification is certainly evident in some of the best-known cases in the history of science, including, famously, Newton's 'unification' of terrestrial and celestial mechanics, Lyell's 'unification' of the historically distant and current geological forces of change, and Darwin's 'unification' of explanations of diversity and distribution of phenotypic features of humans and nonhumans by appeal to a single schematic principle of natural selection on heritable variation. Subsumption of diverse phenomena by appeal to increased generality and abstraction is indeed one way in which a plurality of accounts can be related. This constitutes a type of theoretical integration (see also Morrison, 2000). However, I do not think it is the only mode of relationship. Therefore, identifying theoretical unification as the only means of doing good science is a mistake that removes the impetus to understand the value of diverse integrative strategies in scientific inquiry.

Darwin's great unifying insight in *The Origin of Species* (1859) was to see that the very different situations of the diversity of morphology of the thirteen species of finches he observed in the Galapagos Islands, the variety of domesticated pigeons in England, and the precise construction of the orchids he investigated were all explained by a single schema: natural selection operating gradually over slight heritable variation in individual members of a population.

While selection characterized at this level of abstraction unifies quite a significant number of phenomena, there are reasons to go both more abstract and less abstract with corresponding increases and decreases in generality. Within biology, hierarchical selection theory expands the biological targets from Darwin's individual organisms in a population both up to kin groups, trait groups, unrelated groups, and possibly species levels and down to gametes and genes. Darden and Cain (1989) move up the scale and beyond biology. "Natural selection, clonal selection for antibody production, and selective theories of higher brain function are examples of selection type theories. Selection theories solve adaptation problems by specifying a

process through which one thing comes to be adapted to another thing" (p. 106). (See also Skipper, 1999.)

Within biology, even individual or organismic selection is broken down into *r* and *k* selection. *r* selection characterizes the processes occurring in populations that are not near the carrying capacity of their environment, and hence selection occurs by increasing fecundity, while *k* selection operates in populations at their quantitative limit, and hence the mechanism is instantiated by increased survival of offspring rather than number of offspring. The consequences of these different kinds of organismic selection are not always in concert. A number of studies both in the lab and in the field show mixed results of the relationship between fecundity and survival: out of 22 lab studies, a negative relationship was found in seven (and positive in five); out of 41 field studies, a negative relationship was found in 23 (and positive in four); the remaining cases were nonsignificant (Stearns, 1992). Indeed, the more concretely described individual processes that constitute organismic selection may operate antagonistically, additively, or synergistically. Hence, collapsing the different processes constituent of natural selection operating on organismic variation in a population into a single representation of the process of selection can obscure what may be important differences. The point is that different levels of abstraction are required for different tasks (see also Mitchell, 2000 for a version of this argument applied to chemistry).

Specific theoretical unifications, while being one form of integrating diverse causal models, must be justified by appeal to more than the fewer-the-better argument. That is, one must at least specify what it is 'better' for.

### Integrative pluralism

Integration, the alternative to both reduction and isolation, occurs at many levels of abstraction and is driven by a variety of pragmatic interests. Establishing the philosophical arguments for the need for some form of integrative pluralism is clearly only the first step toward a better understanding of science. Indeed, the arguments I have given for expecting pluralism imply that the types of integration within science will also be varied and diverse. No single theoretical framework, no simple algorithm, will suffice. This is also evident from the type of case study-driven work that has already considered questions of integration. Darden and Maull (1977) outlined several kinds of 'interfield' relations that might characterize integration across two theoretical boundaries: physical localization and part-whole relation, physical description of an entity described in another theory, identifying a structure underlying a function, and identifying a cause in one field of an effect described in another (see also Darden,

1986, 1991). Bechtel (1986) identified other 'cross-disciplinary' patterns that are nonreductive: conceptual links between disciplines to induce modification of perspective in one or the other, recognition of new levels of organization to solve unsolved problems in existing fields, using research techniques from one field to develop theoretical models in another, extending and applying a theoretical framework from one field to another, and "developing a new theoretical framework that will reconceptualize research in now separate domains as it tries to integrate them" (p. 47). Bechtel and Richardson (1993) have further explored integrative strategies in their more recent work, especially with respect to crossing different levels of organization. These studies call for further cases in order to catalog what appears to be a wide variety of ways in which integration proceeds.

My own investigations of interdisciplinary work between developmental and evolutionary models (Mitchell, *et al.*, 1992) and also between biology, writ large, and cultural models (Mitchell, *et al.*, 1997) have suggested a set of working hypotheses. In that work I proposed three types of integration: (1) mechanical rules, (2) local theoretical unification, and (3) explanatory, concrete integration.

*Mechanical rules* can be used to quantitatively determine the joint effects of independent additive causal processes explained by different theories. Vector addition on the contributions of electromagnetic and gravitational forces to resultant motion is an example. The integration of theories is simply a demonstration that they are simultaneously applicable in a linear way. Sewell-Wright attempted to do the same for the effects of mutation and selection on gene frequencies (see Sober, 1987). *Prima facie*, this type of integration seems appropriate for causes that are additive and operate on the same entities for comparable time periods. However, some biological phenomena do not seem to be amenable to mechanical rule integration. Think of the slime mold. When there is sufficient food in the environment, slime mold exists as independent amoebae. They move, feed on bacteria, and reproduce by cell division. When food becomes scarce, a chemical signal stops cell division and they move toward each other to form a multicellular 'pseudoplasmodium' of tens of thousands of cells. This new association of cells then differentiates into a stalk supporting a fruiting body that produces spores. The spores are launched to spread to a new environment (with better chances of food), and life as individual amoebae begins again (Keller & Segel, 1970). This type of 'emergent' effect of the interaction of individual components of a complex system is typically nonlinear, and thus the individual component causes are nonadditive. Therefore, theories explaining the causes of emergent phenomena requires

consideration of interactions.

The second model of integration, *local theoretical unification*, aims to develop models in which a number of features of a complex process are jointly modeled. This is similar to the explanatory unification counseled by Friedman and Kitcher. However, as I argued above, the appropriate scope of the unity and corresponding degree of abstraction will be settled by a combination of pragmatic and ontological constraints. The problem of scale in ecology illustrates this. In discussing the trade-off of detail for generality in modeling evolution on different classes of entity and ecological relationships among populations, Levin comments, "Here ... the problem is not to choose the correct scale of description, but rather to recognize that change is taking place on many scales at the same time, and that it is the interaction among phenomena on different scales that must occupy our attention" (1992: 1947). Furthermore, "In general, one must recognize that different processes are likely to be important on different scales, and find ways to achieve their integration" (p. 1950). A more concrete example of this is found in the discussion of 'top-down' and 'bottom-up' theories of the regulation of trophic structure and species composition in an ecosystem. Liebold, *et al.* (1997) argue that these "two artificially distinct perspectives" (p. 468) that model the effects on trophic structure of predation and resources, respectively, are both supported by empirical evidence. They propose a 'synthesized' model that takes both forces into account and thereby links community and ecosystem approaches. At the same time, they recognize that their model may still represent only "one subsystem in the more complex array of food web interactions," a local unification in my terminology, or, alternatively, may predict "the cumulative behavior of many subsystems that act in parallel but roughly additive ways" (p. 483), thus constituting something like a mechanical rule.

The third type of integration, *explanatory, concrete integration*, appears to occur in cases of high complexity and pressing pragmatic goals (see Oreskes, *et al.*, 1994). That is, when a large number of at most partially independent factors participate in structuring a biological process, and where those factors span time and dimension scales as well as standard scientific disciplines, even modest theoretical unification will be elusive. Think of the changes of state of a complex ecosystem such as that of Lake Erie. There is an ongoing modeling project to consider the lake-wide effects of the invasion of zebra mussels, declining phosphorus loading, continuing toxic contamination, and fish harvesting on the structure of the fish community of that lake (see Culver, 1999; Koonce & Locci, 1999). The different factors contributing to these effects are large and diverse, including the chemicals silica,

ammonia, nitrate, phosphate, total phosphorus, and phosphorus in sediments; five taxa of phytoplankton, six taxa of herbivorous zooplankton (including zebra mussel veligers), and three taxa of predatory crustacean zooplankton; zebra mussels and four taxa of other macrobenthos; and eleven taxa of planktivorous fish and six taxa of piscivorous fish. The model also represents seasonal and spatial variation in solar radiation. "Phytoplankton, zooplankton, and planktivorous fish are sensitive to light, temperature, and nutrient concentrations they experience in nonlinear ways. Hence one cannot adequately model the functions of the pelagic zone using the basin-wide averages of state variables" (Culver, 1999). Features of the method of integration of these multiple factors for a single lake may be local to Lake Erie or may be symptomatic of a class of situations, but are unlikely to be global and algorithmic. As such, I suggest, it may be better described as explanatory, concrete integration.

## Conclusion

I take this project to be similar in spirit to Carnap's analysis of the acceptance of different linguistic forms within science. He concludes his investigation of the relative worth of using thing language, abstract language, or not speaking at all:

*"The acceptance or rejection of abstract linguistic forms, just as the acceptance or rejection of any other linguistic forms in any branch of science, will finally be decided by their efficiency as instruments, the ratio of the results achieved to the amount and complexity of the efforts required. To decree dogmatic prohibitions of certain linguistic forms instead of testing them by their success or failure in practical use is worse than futile; it is positively harmful because it may obstruct scientific progress. The history of science shows examples of such prohibitions based on prejudices deriving from religious, mythological, metaphysical, or other irrational sources, which slowed up the developments for shorter or longer periods of time. Let us learn from the lessons of history. Let us grant to those who work in any special field of investigation the freedom to use any form of expression which seems useful to them; the work in the field will sooner or later lead to the elimination of those forms which have no useful function. Let us be cautious in making assertions and critical in examining them, but tolerant in permitting linguistic forms" (Carnap, 1950).*

I endorse both Carnap's pragmatic standard and his plea for toleration. However, I believe it should be applied not only to linguistic expressions within science, but also to philosophical expressions about science.

In defending integrative pluralism, an image of science that makes room for compatible pluralism, I have attempted to steer clear of two undesirable methodological pitfalls. The first is an isolationist stance that partitions scientific investigations into discrete levels of questions and their corresponding answers in a way that precludes the satisfactory investigation of any of the levels. The second is an uncritical anarchism that endorses all and any propositions. Neither of these positions correctly locates where and when competition in fact occurs between theories and explanations in biology. I have appealed to the idealized structure of scientific models and emphasized the distinction between the model and its application to a concrete situation. While the idealized and abstract character of models allows compatibility at the theoretical level, the realistic and concrete nature of explanation entails integration and resolution. Given the multiplicity of causal paths and historical contingency of biological phenomena, the type of integration that can occur in the application of models, that is, their use in explanations, will itself be piecemeal and local. The result is that pluralism with respect to models can and should coexist with integration in the generation of explanations of complex and varied biological phenomena. By extension, integrative pluralism is a fruitful approach to the generation of explanations of most complex phenomena.

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