

A Neurathian Conception of the Unity of Science

Angela Potochnik

Abstract

An historically important conception of the unity of science is explanatory reductionism, according to which the unity of science is achieved by explaining all laws of science in terms of their connection to microphysical law. There is, however, a separate tradition that advocates the unity of science. According to that tradition, the unity of science consists of the coordination of diverse fields of science, none of which is taken to have privileged epistemic status. This alternate conception has roots in Otto Neurath's notion of unified science. In this paper, I develop a version of the coordination approach to unity that is inspired by Neurath's views. The resulting conception of the unity of science achieves aims similar to those of explanatory reductionism, but does so in a radically different way. As a result, it is immune to the criticisms facing explanatory reductionism. This conception of unity is also importantly different from the view that science is disunified, and I conclude by demonstrating how it accords better with scientific practice than do conceptions of the disunity of science.

Since the mid-twentieth century, the thesis of the unity of science typically has been taken to consist in explanatory reductionism.¹ On that view, the unity of science is achieved through the vindication of the special sciences by finding their basis in microphysical law. Yet there is another, somewhat neglected tradition with a wholly different conception of the unity of science. According to that tradition, the unity of science is achieved via the coordination of diverse fields of science, none of which is taken to have privileged epistemic

status. This alternate conception has roots in Otto Neurath's notion of unified science. I will call this family of views **coordinate unity**, in contrast to **reductive unity**.

Advocates of the disunity of science focus their criticisms on reductive unity. Dupré (1993), for instance, devotes most of his attention to undermining explanatory reductionism. He quickly dismisses Darden and Maull's (1977) coordinate sense of unity, saying that it only amounts to the claim that "no form of knowledge production can be entirely isolated from all others" (1993, p. 227). Yet the idea that the unity of science consists in the egalitarian coordination of fields should not be dismissed so casually. In this paper, I develop a version of that thesis, and I argue that it is a substantive and promising conception of the unity of science.

My conception of coordinate unity is based on the idea that grappling with the world's complex web of causal influences requires the collaboration of diverse fields of science, despite the various differences among those fields. The resources of separate fields and subfields of science must be regularly brought together in order to facilitate scientific progress, and this constitutes a meaningful sense in which science is a unified whole. In Section 1, I outline Neurath's conception of unified science and sketch the present version of coordinate unity that it inspires. I discuss the methodological hallmarks of this form of unity in Section 2. In Section 3, I show how this coordinate unity achieves aims similar to those of reductive conceptions of the unity of science, and yet avoids the pitfalls of reductive unity. Finally, in Section 4, I argue that this coordinate unity is a substantive and distinctive form of the unity of science, and that it accords better with scientific practice than do prominent conceptions of the disunity of science. Advocates of the disunity of science are largely successful in their criticisms of reductive unity, but it is overly hasty to conclude from that success that there is *no* meaningful sense in which science is a unified whole.

1 Neurath's Unified Science and Causal Complexity

The account of the unity of science that I develop here is inspired by a feature of Neurath's conception of unified science, or *Einheitswissenschaft*. Among the members of the Vienna Circle, Neurath was the most active proponent of the unity of science. Prominent advocates of explanatory reductionism—Oppenheim and Putnam (1958); Nagel (1961); and Hempel (1966)—were directly influenced by the logical empiricism of the Vienna Circle, but their reductive approach to unity bears little resemblance to Neurath's view. Instead, Neurath's unified science is a precursor to the approach to the unity of science that I term coordinate unity.²

Neurath's vision of unified science is, at root, a practical aim: the bringing together of scientists in distinct fields of science in order to facilitate communication and interconnection. Neurath thinks that a prior determination of the forms of interconnection would be impossible. As he puts the point, “instead of aiming at a synthesis of the different sciences on the basis of a prior and independent philosophy, the special sciences will themselves supply their own synthesizing glue” (Neurath, 1937, p. 172). Because there is no preconceived notion of the ways in which the different parts of science will fit together, unified science will not create a complete, all-encompassing system, such as Carnap's *logische Aufbau* was intended to be. Instead, unified science is an ongoing process of systematization without a specific result in mind. This process of systematization is fostered by collaboration amongst diverse scientific endeavors. The result is simply “a preliminary assemblage of knowledge...the totality of scientific matter now at our disposal” (Neurath, 1936a, p. 146).

Neurath outlines several forms of collaboration. To begin with, the unification of scientific language ensures that terminology and symbolism are used in as consistent of a manner as possible among all disciplines. Yet Neurath cautions that this should not be taken so far as to conceal the ambiguity that, in his view, is an essential feature of the commonsense terms that

ground science (Neurath, 1936c; Cartwright et al., 1996). Another form of collaboration is the unification of auxiliary procedures. These are tools like, for example, probability theory that may be called into use repeatedly in different contexts and for different problems. Finally, as far as the subject matter of science is concerned, Neurath writes that “of greatest importance also is the linking of disciplines among themselves by the establishment of ‘cross-connections’ ” (1936a, p. 155). It was to this end that Neurath helped plan the development of the *International Encyclopedia of Unified Science*. The *Encyclopedia* was to be a series of monographs by various philosophers and scientists, and its express aim was to provide a vehicle for progress in the “science of science”. Armed with an appreciation for the diversity of scientific endeavors, scientists and philosophers could work toward developing science as a unified whole (Neurath, 1936b; Reisch, 1994).

In Neurath’s view, the need for these forms of collaboration arises in the process of applying science to particular problems. He writes,

We avoid pseudo-problems of all kinds if, in the analysis of sciences, we set out from predictions, their formulation and their control. But it is precisely this starting point that is little suited for the delimitation of special disciplines. One does not arrive at individual disciplines of stars, stones, plants, animals during the deduction of certain predictions, because time and again the conjunction of statements of different origin become necessary (1936b, p. 132).

In other words, scientific practice forces upon us connections among different parts of science. Predicting and accounting for phenomena in the natural and social world require that distinct scientific disciplines work in conjunction. This aspect of Neurath’s view is, to my mind, the crucial point. It indicates a form of interconnection that is distinctive to science. Simply put, evidential relationships do not respect field boundaries. Accurately treating phenomena of interest regularly requires collaboration among different fields of science, despite divergences in terminology, methodology, and domains of inquiry.

Consider an example from evolutionary biology. Takahashi et al. (2008) investigate the role of mate choice in the evolution of the peacock's colorful train. The researchers observed a population of peafowl for several years; their observations show that the length and elaborateness of trains actually do not affect peacocks' mating success. This approach is typical to field ecology, but the study does not end there. Takahashi et al. also cite findings from phylogeny and endocrinology that support their conclusion. They note that these features of peacock trains have been found to be under estrogen control (Owens and Short, 1995), and that male plumage under estrogen control has been found to be disregarded in mate-choice by peahens. They also cite a molecular phylogeny finding that suggests that all peafowl had bright tail-plumage in the evolutionary past (Kimball et al., 2001). Instead of peacocks evolving a more elaborate train, as is commonly assumed, peahens actually evolved a less ornamental appearance. These findings further undermine the idea that the peacock's colorful train evolved because of peahen mate choice.

Takahashi et al. (2008) define their project not by methodology or by field of research, but by a particular phenomenon of interest: the existence of the peacock's colorful train, with an eye toward sorting out what role, if any, mate choice played in its evolution. Their study incorporates field ecology, phylogeny and endocrinology. Though the focus is the train's influence on mating opportunities, which would be a source of selective advantage, other causal influences on the peacock's train are taken into account. Evidence marshaled from endocrinology provides insight into the developmental causes of the colorful train, and how these developmental causes interact with selection. Phylogenetic evidence provides information about where to look for causal arrows in evolutionary history. It emerges that the proper question is not what caused peacocks to evolve more colorful trains than peahens, but what caused *peahens* to evolve dull coloring and smaller trains from an ancestral state of colorful, long trains.

Takahashi et al.'s work illustrates how transcending field boundaries can help untangle

the evidential interrelationships that result from the interaction of diverse causal factors. This is a common situation in biology. A myriad of causal factors influence the traits of organisms. These include the features of the ecological and social environment that affect trait fitness; direct environmental influence on traits (phenotypic plasticity); genetic influences; the developmental pathways by which these genes exert their influence; epigenetic forms of inheritance; and constraints created by other traits of the organisms, both current and in the evolutionary past. Many other biological phenomena are in a similar boat. Moreover, such causal complexity is not even special to biology. Consider the diverse causal factors involved in climate change, disease epidemics, and even the course of a dollar bill caught in the wind.³ How a dollar travels in the wind even depends on the size, shape and material of the bill, all of which have social causes.

The world is a complicated place, with causal arrows pointing every which way.⁴ This is why evidential interrelationships transcend field boundaries and, consequently, why different fields of science benefit from collaboration. This is the basis for a strong coordinate sense of the unity of science. Because we live in a complex world, with causal relationships that do not abide by field boundaries, the coordination of various fields, subfields and research programs facilitates the success of science.

2 The Coordination of Science

As illustrated by Takahashi et al.'s (2008) research on the peacock's colorful train, evidential relationships that cross field boundaries influence the methodology of science. These methodological effects are the hallmarks of my version of coordinate unity. At least three forms of collaboration among different research programs aid evidence-gathering in the face of causal complexity. First and most obviously, making use of evidence from diverse sources involves sorting out the type and direction of causal interactions. This is true both for

establishing broad patterns of causal influence, as well as for establishing whether particular instances fit into those patterns of influence. Both of these forms of causal analysis occur in Takahashi et al.'s investigation of the influence of mate choice on the evolution of the peacock's colorful train. For instance, the researchers appeal to the finding that male plumage under estrogen control is typically disregarded in peahen mate-choice. This finding regards a general pattern of causal influence, or really non-influence: male plumage under estrogen control tends not to influence peahen mating behavior. The finding that the peacock's train is under estrogen control suggests that the train is likely to accord with that causal pattern; Takahashi et al.'s field study is a first step to confirming this. Establishing the nature of the causal pattern between estrogen-controlled traits and mating behavior in peafowl, as well as establishing the relevance of this to the peacock's train, involves sorting out causal relationships between developmental processes and selective influence. This is so even though these processes are often targets of separate investigations. For that reason, such causal investigation often benefits from collaboration among different labs, different subfields, and sometimes even different fields of science.

A second type of collaboration that can be important to sorting out evidential relationships that cross field boundaries is terminological clarification. The terminology of different fields may diverge, even if their investigations are related, and differences in terminology can block the way to effective evidence-sharing. Fields may even employ the same terms in different ways. Locating overlaps and discrepancies in the terminology of different investigations helps identify the nature of causal interactions that span those investigations and, consequently, helps prevent mistaken inferences. The use of the term "gene" is a clear example of this. The clearest divergence in the concept of a gene occurs between classical genetics and molecular genetics, but some argue that the gene concept varies even within each of these theoretical contexts (Kitcher, 1992; Dupré, 1993; Rosenberg, 1994). Consider how different working definitions of 'gene' manifest in causal claims. For

instance, the relative causal importance of natural selection and genetic drift may depend on whether the gene is understood in the classical sense or the molecular sense. This is one issue at stake in the discussion of the neutral theory of molecular evolution (Kimura, 1983).

Third, a more ambitious step toward understanding evidential relationships that transcend field boundaries is the development of integrated models, viz., models that represent the interplay of causal dynamics that are traditionally treated separately. This sort of model integration is suggested by Roughgarden (2009) for the evolutionary influences and functional influences on behavioral traits. Roughgarden points out that many behaviors, such as a bird's allocation of effort to foraging versus nest-guarding, are the product of selection working on a set of conditional strategies. She consequently suggests integrated models that simultaneously represent selection pressures and the conditional strategies that influence an individual animal's actual behaviors. Integrated models can provide a framework for sorting out causal relationships and evaluating evidential relevance.

However, developing models that systematically integrate diverse causal influences can require overcoming a host of problems. If causes interact in different ways in different specific cases, as is certainly the case in evolution, then integrated models will be highly specific and not widely applicable (Mitchell, 2003). Additionally, modeling approaches may resist integration. For instance, genetic models of evolution represent evolutionary change generation by generation, whereas phenotypic optimality models are equilibrium models that represent only the long-term effects of evolution. This difference is no accident: phenotypic evolutionary change behaves predictably only in the long term, whereas the role of individual genes is most easily discerned in generational change (Eshel and Feldman, 2001; Godfrey-Smith and Wilkins, 2008). It is difficult to imagine how these different approaches would be synthesized (James Griesemer, conversation). Thus, while the integration of models can help sort out evidential relationships that cross field boundaries, integrated models will not always be possible or useful.

I have connected the conception of unity advocated here to a number of other views, which I have grouped under the heading of “coordinate unity.” These include (Darden and Maull, 1977; Bechtel, 1984; Mitchell, 2003; Grantham, 2004), and specifically for neuroscience, (Craver, 2005). I do not want to overstate the differences between my view and what I take to be kindred views, but I should briefly indicate what is distinctive—and advantageous—about my particular version of coordinate unity. My diagnosis of causal complexity and the resultant evidential interconnections as the impetus for unity leads to a focus exclusively on forms of collaboration that result from grappling with that causal complexity. This is a narrower conception of unity than the range of interfield connections that Darden and Maull (1977) and Grantham (2004) advocate, and it is broader than the focus on mechanisms that Bechtel (1984) and Craver (2005) urge. I suspect that that range of interconnections (e.g., part-whole, structure-function, as well as mechanistic relationships) can be productively viewed as versions of causal interconnections.⁵ Additionally, the present view differs from many versions of coordinate unity in not advocating interfield *theories*, but piecemeal integration of the sort Neurath envisions. Finally, though my view has much in common with (Mitchell, 2003), I do not expect unity across fields to furnish explanations (see §3).

In my view, the unity of science consists in the coordination of different fields of science to the end of improving the success of science in the face of widespread causal complexity. This coordination may involve sorting out causal patterns among phenomena investigated in different fields; puzzling through terminological variation among fields; and developing integrated models to simultaneously represent diverse causal influences. There very well may be additional methods that aid in sorting out evidential interrelationships. These methodological features are hallmarks of the unity of science, but they do not constitute it. The unity of science comprises all forms of coordination among fields that result from the shared problem of finding and evaluating evidence in the face of causal complexity, a

problem that is special to science.

3 Coordinate Unity versus Reductive Unity

This formulation of the unity of science has little in common with the classical notion of unity as explanatory reductionism. It is inspired instead by the alternate tradition of coordinate unity and, especially, Neurath's notion of unified science. Despite their fundamental differences, this conception of coordinate unity achieves some of the goals of explanatory reductionism. Yet it approaches those goals in a radically different way, thereby avoiding well-known pitfalls of reductionism.

The basic goal of explanatory reductionism, as articulated by Oppenheim and Putnam (1958), Nagel (1961), and Hempel (1966), was the epistemic vindication of the special sciences by discovering their foundation in microphysics. The hope was that all laws in the special sciences—that is, laws with a limited domain of application—could ultimately be derived from fully general, exceptionless microphysical laws. The project sprang from a commitment to physicalism, understood as the claim that all events are ultimately microphysical events. Oppenheim and Putnam (1958) claim that the rejection of vitalism and similar views leads to explanatory reductionism. The idea is that, if all events are ultimately microphysical events, then all regularities must result from the laws that govern microphysical goings-on and thus should be explainable by those microphysical laws. Without this relationship to the fundamental laws of physics, law-like patterns at higher levels of description would seem to be without explanation. If this is right, then legitimate science posits only regularities that can be vindicated/explained through microphysics. With this last move arrives the hope that explanatory reductionism can provide a way to demarcate science from nonscience.

Many criticisms have been leveled against explanatory reductionism over the past several decades. Fodor (1974) argues that the fact that physical properties multiply realize properties

dealt with in the special sciences impedes reduction; Kitcher (1984) also endorses the idea of multiple realization and maintains that explanatory relations can run in any direction among the distinct, autonomous levels of explanation. Dupré (1993) pushes further in this direction: he argues that distinct fields of inquiry employ noncoincident natural kinds that cross-classify the world, so even multiple realization is unrealistic. Cartwright (1983, 1999) locates the problem in the disparate, strong idealizations made in the process of employing different theoretical models. All of these criticisms share a common feature. Each moves from observations about the complexity of the world to the conclusion that explanatory reductionism is wrong.

My concern here is not to analyze these criticisms of explanatory reductionism, but to show that the complexity of the world poses no problems for my conception of coordinate unity. Indeed, causal complexity is what inspires this conception of the unity of science. Explanatory reductionism relies upon supervenience relationships to connect the subject matter of different fields of science in an explanatory way, and those supervenience relationships and their explanatory value are the target of the criticisms outlined above. In contrast, the present conception of coordinate unity is independent of the metaphysical issue of supervenience. The focal relationships unifying diverse fields of science are *causal* relationships. Complex, interwoven causal processes generate the need for coordination and collaboration among different fields of science, regardless of whether all phenomena supervene on microphysical phenomena. This coordination among fields is also independent of one's views about scientific explanation. The goal is simply to take into account evidential relationships that transcend field boundaries, and this may not furnish explanations.⁶

Shifting the focus to causal relationships yields a very different picture of connections among fields. The posited relationship is not among ontological levels of organization, but among fields of research (cf. Darden and Maull, 1977; Grantham, 2004). Reduction must go in a single direction—downward through levels of organization. In contrast, causal influence

can run in any direction among phenomena dealt with in various fields. Accordingly, this conception of coordinate unity does not privilege a single field of science, nor does it promise systematic relations among fields. A hodgepodge of connections among fields spring up in response to evidential interrelationships that result from causal complexity. This picture is not threatened by antireductionists' observations of multiple realizability, disparities in the classification of kinds, or different forms of idealization. To the contrary, each complicates science such that the methodological steps outlined in §2 are even more essential.

Despite bearing little in common with a reductive approach to the unity of science, this conception of coordinate unity can achieve some similar goals to those of classic explanatory reductionism. First, there is the search for a firmer epistemic grounding for the special sciences. The reductionist approach to this search is impractical at best. Even granting metaphysical supervenience and setting aside multiple realizability and other concerns, current microphysics is ill-equipped to vindicate the special sciences. We might hold on to the hope that an eventual, ultimate physics could have special epistemic bearing on other fields of science, as Oppenheim and Putnam (1958) proposed. But that hope is of little epistemic value to current science, and I see no grounds for expecting an ultimate physics to be any closer at hand than a correspondingly strong epistemic position for any other field of science. Indeed, solid epistemic grounding may prove more forthcoming for these fields of science than for microphysics. Batterman (2002) and Strevens (2006), for instance, suggest alternate sources for higher-level regularities in fields such as optics, thermodynamics, and population ecology.

The conception of coordinate unity suggested here offers a different approach to an improved epistemic position for the sciences. Systems of mutual revision and reinforcement created by the sharing of evidence are incredibly important to the success of science. Different scientific investigations differ in their agendas and their tools. Yet the phenomena they investigate are not isolated events, but different parts of an intricate causal web.

Investigations often can gain valuable data from other research programs, including some that are at first glance unrelated. The coordinate unity of science strengthens the epistemic positioning of all fields of science via the sharing of evidence. This epistemic strengthening of investigations is not unilateral, but multidirectional: evidence can come from any direction, from unanticipated sources, and evidential relationships are often mutually beneficial to the fields involved.

As an illustration of an unlikely collaboration that leads to mutual epistemic gain, consider the emerging area of investigation called biogeophysics. Biogeophysics examines the causal relationships between microorganisms in the Earth and geological features. Recent work by researchers in the fields of microbiology and geophysics has shown that microbial activity influences subsurface geological features, and conversely, that geological properties influence microbial activity (Atekwana et al., 2000). These findings were initially surprising; some were resistant to the idea that such disparate types of phenomena—one tiny and quick-acting and the other enormous and slow to change—could influence one another (Estella Atekwana, conversation). The key to both is conductivity: electron-transport is important to microbes, and conductivity is an important geological influence. The discovery of this causal interrelationship has led to breakthroughs for both microbiology and geophysics. It demonstrates that microbial features must be taken into account when investigating geophysical properties, and that understanding microbial activity involves considering the geophysical properties of the ground that houses the microbes. These discoveries also create new research tools. For instance, employing traditional geophysical techniques creates a new opportunity to explore microbial activity non-invasively and in the field (Atekwana, conversation).

Another traditional goal of explanatory reductionism is the demarcation between science and nonscience. This goal is linked to reductionists' rejection of vitalism, substance dualism, and the like. Demarcating science as that which issues from microphysical law would provide

grounds for discrediting any supposed science that posited an *élan vital*, a nonphysical mind, or other nonphysical substances or events. Yet there are good indications that at least some higher-level regularities do not derive from microphysical law. Multiply realizable and functionally defined properties push in this direction, and the work of Batterman (2002) and Strevens (2006) is suggestive here as well. It may not be microphysics' job to vindicate the regularities observed in the special sciences.

It would be helpful, though, if a conception of the unity of science provided a way to distinguish legitimate science from problematic ventures that sometimes occur in the guise of science, such as creationism and astrology. The present conception of coordinate unity has something to offer here as well. Basing the unity of science on evidential interdependence suggests that legitimate scientific projects include anything that is part of the collaborative venture of science. This suggestion is akin to the idea advocated by Neurath (1938) that "isolated sentences," a term Neurath credits to Reach, cannot be involved in scientific enterprises. In my view, this has both methodological and sociological aspects. Scientific projects aim to untangle causal relationships and patterns, and in the process of doing so, they engage with neighboring projects. The sharing of evidence has implications for methodology (§2), as well as for the social characteristics of science. Accordingly, legitimate scientific projects have a methodology with room for external sources of evidence and a culture of openness to such external influences. Isolated endeavors that are not open to evidence-sharing, or for which evidence-sharing has no meaning, are not science, whatever their other merits. I predict that such pockets of isolated endeavors will become scientifically irrelevant or will fail to establish relevance to begin with.

4 Unity in a Disordered, Dappled World

Advocates of the disunity of science focus their criticisms on a reductive approach to unity. In this section I argue that those criticisms do not threaten the present version of coordinate unity, and yet, that this is a substantive conception of the unity of science, importantly different from the view that science is disunified. This unity of science also accords better with scientific practice than do prominent conceptions of disunity. I focus on Dupré's and Cartwright's versions of disunity, as articulated in *The Disorder of Things* (1993) and *The Dappled World* (1999). The title of this section alludes to those two works.

As mentioned above, Dupré (1993) focuses his criticisms on a reductive approach to the unity of science. His main argument is that scientific kinds employed in various fields cut across one another and, consequently, are irreducible. He points out that kinds are defined functionally, in different ways for different fields, so that different fields of science cross-classify entities in the world. An example is the ways in which the category "gene" is employed: "the genes described structurally by the molecular geneticist are not the same things as those referred to in the models of population genetics or even of classical transmission genetics" (Dupré, 1993, p. 122). Overlapping, irreducibly different kinds interfere with the possibility of reductive unity, for then generalizations about the kinds in one field cannot be explained with reference to the kinds of another field. According to Dupré, this leads to "incommensurable" scientific narratives in different fields (1993, p. 112). Dupré takes this incommensurability seriously; he argues that the field of population genetics is doomed to failure because of it. He claims that cross-cutting kinds plague ecology and genetics. Because population genetics draws from both fields, Dupré pronounces it to be nothing more than "an embodiment of reductionist mythology" (1993, p. 132).

Let us grant Dupré his cross-cutting kinds. This may undermine reductive unity, but it need not lead to the utter incommensurability. Undermining the possibility of tidy

supervenience relationships does not eliminate the possibility of any meaningful relationships among fields. For instance, the three forms of interrelationships that I outlined in Section 2 are possible despite cross-cutting kinds. Indeed, the more convoluted the relationships among fields, the more necessary these steps become. Consider the process of sorting out the relationship between a stretch of DNA (a molecular gene) and a functional unit of inheritance (a classical gene). If these do not bear a neat one-to-one relationship to one another, that just makes sorting out their actual relationship all the more crucial. Observations about molecular genes and observations about classical genes are both observations about the process of genetic transmission. Causal interrelationships do not go away simply because different fields categorize the world in different ways. Cross-cutting kinds may make it more difficult for fields to productively collaborate, but so long as the fields deal with interrelated causal processes, their collaboration is essential. Dupré's declaration of incommensurability is too quick. The subject matter of different fields of science may not be neatly related, but sorting out the complicated relationships is a practical necessity.

The case of population genetics illustrates this point. Dupré deems population genetics a failed enterprise because population genetic models represent evolutionary change in terms of fitness differences among genotypes. In Dupré's estimation, fitness is a property of organisms. As a result, "genetic fitnesses, . . . depending inextricably on both the genetic and the organismic, are misbegotten mongrel concepts, derived from reductionist excesses" (1993, p. 138). Yet surely this wholesale dismissal of a productive field of biology is too hasty. Dupré is right that a genotype's fitness effects can vary depending both on its genetic and ecological contexts, but the same is true for the heritability of phenotypes. Evolution is a complex causal process that is influenced by factors studied in a wide range of fields, as evidenced by the array of interdisciplinary evolutionary investigations (e.g. evolutionary-developmental biology; molecular phylogeny; macroecology and evolution). Population genetics is not an adequate approach to investigating the entire evolutionary process, though it may sometimes

be taken to be. This is demonstrated by Dupré's point about gene fitnesses. Nonetheless, population genetics is a tool for modeling genetic transmission, one important aspect of the evolutionary process. The true lesson seems to be one of caution. How a genotype's fitness-effects vary must be carefully ascertained, taking into account ecological sources of fitness. More generally, evolutionary investigations must carefully tease out the complex causal interactions. They do so by utilizing a variety of approaches, with a variety of emphases, of which population genetics is one.⁷

Coordinate unity also suggests a different picture of science than Cartwright's (1999) disunity of science. At the beginning of *The Dappled World*, Cartwright summarizes her view:

The laws that describe this world are a patchwork, not a pyramid. They do not take after the simple, elegant and abstract structure of a system of axioms and theorems. Rather they look like—and steadfastly stick to looking like—science as we know it: apportioned into disciplines, apparently arbitrarily grown up; governing different sets of properties at different levels of abstraction; pockets of great precision; large parcels of qualitative maxims resisting precise formulation; erratic overlaps; here and there, once in a while, corners that line up, but mostly ragged edges (1999, p. 1).

Cartwright rejects the idea that science is working toward a unified fabric of representation. She maintains instead that there is a plethora of scientific laws, with no systematic relationship among them. She contrasts this with reductionist unity, according to which all laws ultimately trace back to microphysics. Cartwright does not forbid connections among the various projects of science, but nor does she anticipate them—she allows for overlaps, but she expects to find ragged edges.

Neurath is also the hero of Cartwright's *Dappled World*, for he disavowed the notion of a single, complete system of science. Neurath maintained that scientific practice should

govern which connections are drawn among the parts of science. Add to this Cartwright's skepticism about the idea that nature is "well-regulated," and it looks as if the connections in science will be few and far between. As discussed in Section 1, I take a different lesson from Neurath. Recall that Neurath also urges that the nature of actual phenomena leads to collaboration across fields of science: "[setting] out from predictions, their formulation and their control... is little suited for the delimitation of special disciplines" (Neurath, 1936b, p. 132). Scientific practice governs the nature and extent of unity, but scientific success requires connections. Because the structure of science reflects the nature of phenomena themselves, and because phenomena often result from causal processes that are studied in different fields, the development of connections among those fields leads to better science.

Cartwright worries about reductionism's metaphysical commitment to a world well-regulated by laws. She holds that there is no reason to think that nature is "well-regulated," that every event is law-governed. Instead, she thinks it is just as plausible that nature turns out to be "constrained by some specific laws and by a handful of general principles, but it is not determined in detail, even statistically" (1983, p. 49). Notice, though, that coordinate unity does not require the well-orderedness of the world. Laws may not follow from the laws of microphysics; some events may not be law-governed at all. What is required for coordinate unity is only that there is progress to be made in puzzling out complex causal processes, even if they transcend field boundaries. If events are not well-regulated, this simply makes those causal processes more variable, more difficult to tease apart. It does nothing to undermine the value of the goal of collaboration and coordination across fields.

Actuality is always proof of possibility: that successful collaborations exist in science indicate that such collaborations *can* be successful. I have offered examples of the coordination of separate scientific endeavors. Takahashi et al. (2008) combine their own field work with related findings in endocrinology and molecular phylogeny in order to better understand the evolution of the peacock's colorful train. Roughgarden (2009) suggests

the use of integrated models that represent evolutionary influences on behavior as well as direct environmental influences. Biogeophysics systematically investigates the causal interconnections between large-scale, long-term geological phenomena and the behaviors of tiny microbes (Atekwana et al., 2000). And the field of population genetics persists. These and other instances of collaboration support the idea that attempts to coordinate separate fields of scientific investigation are worth the effort. Surely this is preferable, and truer to the spirit of Neurath's unified science, than merely pointing out the difficulties that can plague such attempts of coordination.

In summary, my view of the coordinate unity of science is as follows. A proliferation of complex causal processes that are investigated in multiple fields of science creates the need for collaboration among fields. In particular, research programs benefit from considering the evidential implications of related investigations, even if those investigations occur in different fields, are couched in different terms, or are accomplished with different aims. This makes certain forms of collaboration important to scientific methodology, and it is in virtue of that collaboration, with the goal of establishing evidential implications in the face of causal complexity, that science comprises a unified enterprise. That unity is not in virtue of reduction to a single, fully general science, nor is it in virtue of similarities among fields. Instead, this unity of science emerges from the cross-connections that are established—and continue to be established—among related investigations, in spite of frequent differences in terminology, concepts, and methodology. This is a thesis of the unity of science, but it is compatible with the disunity of the world—the disorderdness and dappledness with which Dupré and Cartwright are concerned. This complexity and lack of well-orderedness does not result in incommensurable, isolated scientific endeavors. A meaningful unity exists in the shared purpose of evidence-gathering in the face of causal complexity, and the collaborative methodology that results. This recalls Neurath's admonishments that we should start out from what is practically necessary for the success of science, and that we should heed actual

scientific practice.

Acknowledgements

Earlier versions of this paper were presented at the ISHPSSB meeting at the University of Guelph (2005), and in colloquia at California State University at Long Beach, Florida State University, Oklahoma State University, Syracuse University, the University of California at Davis, and the University of Western Ontario. I received much helpful feedback from these audiences, as well as from Lanier Anderson, Michael Friedman, Peter Godfrey-Smith, Helen Longino, Elliott Sober, Michael Weisberg, and three anonymous referees.

Notes

¹For example, consider Oppenheim and Putnam (1958); Nagel (1961); Hempel (1966); Fodor (1974); Dupré (1993); Rosenberg (1994).

²Views that I take to be versions of coordinate unity have been developed by Darden and Maull (1977); Bechtel (1984); Mitchell (2003); Grantham (2004); and specifically for neuroscience, Craver (2005). I briefly address the relationship between my view and these other accounts in §2.

³This last example is like one introduced by Neurath (1987) and discussed by Cartwright (1999) for a different purpose.

⁴I do not assume any particular analysis of causation in this paper; several analyses would work equally well for my purposes, including, e.g., the counterfactual, process, and manipulation views. However, I do assume the existence of high-level causal relationships.

⁵To clarify: my claim is not that these types of relationships *are* causal relationships, but that the interconnections that others have placed in these categories can instead be understood in terms of causal connections among fields. For instance, Darden and Maull's (1977) main example of a part-whole relationship is Mendelian genes, which were discovered to be parts of chromosomes. An alternate view is that molecular genetics furnishes information about causal processes involving Mendelian genes, e.g., the molecular causes of mutant alleles and the causal processes that lead to genes' phenotypic effects (cf. Kitcher, 1984).

⁶See (Potochnik, 2010a) for a discussion of how epistemic aims and explanatory aims are often in tension in science.

⁷Potochnik (2010b) defends this conception of the roles of evolutionary ecology and population genetics.

References

- Atekwana, E. A., Sauck, W. A. and Jr., D. D. W. [2000]: ‘Investigations of geoelectrical signatures at a hydrocarbon contaminated site’, *Journal of Applied Geophysics*, **44**(2-3), pp. 167–180.
- Batterman, R. [2002]: *The Devil in the Details*, Oxford, UK: Oxford University Press.
- Bechtel, W. [1984]: ‘Reconceptualizations and interfield connections: The discovery of the link between vitamins and coenzymes’, *Philosophy of Science*, **51**(2), pp. 265–292.
- Cartwright, N. [1983]: *How the Laws of Physics Lie*, Oxford, UK: Oxford University Press.
- Cartwright, N. [1999]: *The Dappled World: A Study of the Boundaries of Science*, Cambridge, UK: Cambridge University Press.
- Cartwright, N., Cat, J., Fleck, L. and Uebel, T. E. [1996]: *Otto Neurath: Philosophy between science and politics*, volume 38 of *Ideas in Context*, Cambridge, UK: Cambridge University Press.
- Craver, C. F. [2005]: ‘Beyond reduction: Mechanisms, multifield integration and the unity of neuroscience’, *Studies in History and Philosophy of Biological and Biomedical Sciences*, **36**(2), pp. 373–395.
- Darden, L. and Maull, N. [1977]: ‘Interfield theories’, *Philosophy of Science*, **44**, pp. 43–64.
- Dupré, J. [1993]: *The Disorder of Things: Metaphysical Foundations of the Disunity of Science*, Cambridge, MA: Harvard University Press.
- Eshel, I. and Feldman, M. W. [2001]: ‘Optimality and evolutionary stability under short-term and long-term selection’, in S. H. Orzack and E. Sober (eds.), *Adaptationism and Optimality*, Cambridge Studies in Philosophy and Biology, chapter 4, Cambridge, UK: Cambridge University Press, pp. 114–160.

- Fodor, J. [1974]: ‘Special sciences: The disunity of science as a working hypothesis’, *Synthese*, **28**, pp. 97–115.
- Godfrey-Smith, P. and Wilkins, J. F. [2008]: ‘Adaptationism’, in S. Sarkar and A. Plutynski (eds.), *A Companion to the Philosophy of Biology*, Wiley-Blackwell.
- Grantham, T. A. [2004]: ‘Conceptualizing the (dis)unity of science’, *Philosophy of Science*, **71**(2), pp. 133–155.
- Hempel, C. [1966]: *Philosophy of Natural Science*, Englewood Cliffs, NJ: Prentice-Hall.
- Kimball, R., Braun, E., Ligon, J., Lucchini, V. and Randi, E. [2001]: ‘A molecular phylogeny of the peacock-pheasants (galliformes: *Polyplectron* spp.) indicates loss and reduction of ornamental traits and display behaviours’, *Biological Journal of the Linnean Society*, **73**, pp. 187–198.
- Kimura, M. [1983]: *The Neutral Theory of Molecular Evolution*, Cambridge, UK: Cambridge University Press.
- Kitcher, P. [1984]: ‘1953 and all that: A tale of two sciences’, *Philosophical Review*, **93**, pp. 335–373.
- Kitcher, P. [1992]: ‘Gene: Current usages’, in E. F. Keller and E. A. Lloyd (eds.), *Keywords in Evolutionary Biology*, Cambridge, MA: Harvard University Press, pp. 128–131.
- Mitchell, S. D. [2003]: *Biological Complexity and Integrative Pluralism*, Cambridge Studies in Philosophy and Biology, Cambridge, UK: Cambridge University Press.
- Nagel, E. [1961]: *The Structure of Science*, London, UK: Routledge and Kegan Paul.
- Neurath, O. [1936a]: ‘Encyclopedia as ‘model’’, in R. S. Cohen and M. Neurath (eds.), *Philosophical Papers 1913-1946*, volume 16 of *Vienna Circle Collection*, Dordrecht, Holland: D. Reidel Publishing Company.
- Neurath, O. [1936b]: ‘Individual sciences, unified science, pseudo-rationalism’, in R. S. Cohen and M. Neurath (eds.), *Philosophical Papers 1913-1946*, volume 16 of *Vienna Circle Collection*, Dordrecht, Holland: D. Reidel Publishing Company.
- Neurath, O. [1936c]: ‘An international encyclopedia of unified science’, in R. S. Cohen and M. Neurath (eds.), *Philosophical Papers 1913-1946*, volume 16 of *Vienna Circle Collection*, Dordrecht, Holland: D. Reidel Publishing Company.

- Neurath, O. [1937]: ‘Unified science and its encyclopedia’, in R. S. Cohen and M. Neurath (eds.), *Philosophical Papers 1913-1946*, volume 16 of *Vienna Circle Collection*, Dordrecht, Holland: D. Reidel Publishing Company.
- Neurath, O. [1938]: ‘Encyclopaedism as a pedagogical aim: A Danish approach’, *Philosophy of Science*, **5**(4), pp. 484–492.
- Neurath, O. [1987]: ‘United science and psychology’, in B. McGuinness (ed.), *Unified Science*, Dordrecht, Holland: D. Reidel Publishing Company.
- Oppenheim, P. and Putnam, H. [1958]: ‘Unity of science as a working hypothesis’, in H. Feigl, M. Scriven and G. Maxwell (eds.), *Minnesota Studies in the Philosophy of Science*, volume 2, Minneapolis: University of Minnesota Press, pp. 3–36.
- Owens, I. and Short, R. [1995]: ‘Hormonal basis of sexual dimorphism in birds: Implications for new theories of sexual selection’, *Trends in Ecology and Evolution*, **10**, pp. 44–47.
- Potochnik, A. [2010a]: ‘Explanatory independence and epistemic interdependence: A case study of the optimality approach’, *The British Journal for the Philosophy of Science*, **61**(1), pp. 213–233.
- Potochnik, A. [2010b]: ‘Levels of explanation reconceived’, *Philosophy of Science*, **77**(1), pp. 59–72.
- Reisch, G. A. [1994]: ‘Planning science: Otto Neurath and the *International Encyclopedia of Unified Science*’, *British Journal of the History of Science*, **27**, pp. 131–153.
- Rosenberg, A. [1994]: *Instrumental Biology or the Disunity of Science*, Chicago: University of Chicago Press.
- Roughgarden, J. [2009]: *The Genial Gene: Deconstructing Darwinian Selfishness*, Berkeley, CA: University of California Press.
- Strevens, M. [2006]: *Bigger than chaos: Understanding complexity through probability*, Cambridge, MA: Harvard University Press.
- Takahashi, M., Arita, H., Hiraiwa-Hasegawa, M. and Hasegawa, T. [2008]: ‘Peahens do not prefer peacocks with more elaborate trains’, *Animal Behavior*, **75**, pp. 1209–1219.