Conceptualizing the (Dis)unity of Science*

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This paper argues that conceptualizing unity as "interconnection" (rather than reduction) provides a more fruitful and versatile framework for the philosophical study of scientific unification. Building on the work of Darden and Maull, Kitcher, and Kincaid, I treat unity as a relationship between *fields*: two fields become more integrated as the number and/or significance of interfield connections grow. Even when reduction fails, two theories or fields can be unified (integrated) in significant ways. I highlight two largely independent dimensions of unification. Fields are *theoretically* unified to the extent that we understand how the ontologies, concepts, and generalizations of these fields are connected. (Reductionism is one form of theoretical unity, but not the only form). Fields are *practically* unified through heuristic connections (e.g., using the heuristics of one field to generate hypotheses in another field) and by the development of methods for integrating the qualitatively distinct bodies of data generated by the two fields. I discuss the relationship between paleontological and neontological systematics to illustrate the utility of conceptualizing unity as interconnection.

1. Introduction. Scientists have often sought, and sometimes achieved, the integration or unification of scientific knowledge. Newton unified mechanics by arguing that the same laws apply to both terrestrial and celestial motion; Maxwell unified the theories of electro-magnetism and optics; Fisher and Wright synthesized Mendelism and Darwinism. Attempts at integration remain prominent on the contemporary scene. Physicists discuss the possibility of a Grand Unified Theory, evolutionary

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psychologists attempt to integrate evolutionary biology and cognitive psychology, and (as we'll see in section 5) some systematists hope to integrate molecular and paleontological systematics. As philosophers of science, it is important to understand the pursuit of unity: What kinds of unification (integration) are sought? What is the epistemic and/or metaphysical significance of unification in science? In order to determine the extent of the unity of science—be it the *localized* unification of neighboring sciences or the *global* unification of the entire domain of sciences—we must first get clear about what it means to say that some area of science is "unified." Consider this analogy. An account of speciation (i.e., the process through which a single species divides to form two descendant species) must be clear about the nature of "species" and the conditions that make two species "distinct." Similarly, philosophers studying the process of scientific unification need to be clear about (1) the entities which enter into the process of unification, and (2) the precise relation(s) which constitute their "unification."

In the history of the philosophy of science, many different forms of unity have been discussed, including the unity of language, unity of method, and the unity of theories (see, e.g., Suppes 1978; Cat et al. 1996; Hacking 1996). Nonetheless, many contemporary discussions focus on what I call the "unity as reduction" (UAR) model. UAR makes two claims: (1) scientific unity is a relationship between *theories,* and (2) theories are unified when a macro-level (or specialized) theory is reduced to a micro-level (or more general) theory. Although philosophers have offered many different analyses of reduction, the claim that the unity of science should be understood in terms of the reduction of theories is assumed by both defenders and critics of unity (e.g., Oppenheim and Putnam 1958; Fodor 1974; Causey 1977; Dupré 1983; Rosenberg 1994). Section 2 argues that UAR is prevalent, though not universal, in philosophical discussions of the unity of science.

This paper argues that conceptualizing unity as "interconnection" (rather than reduction) provides a more fruitful and versatile framework for the philosophical study of scientific unification. According to this alternative conception, *fields* are unified to the extent that they are densely connected. I defend this position by showing that UAR is doubly mistaken. First, the literature on "non-reductive unity" shows that theories can be (partially) unified without reduction (Darden and Maull 1977; Kitcher 1984; Kincaid 1990; Hardcastle 1992; Vance 1996; Wylie 1999; Scerri 2000). I summarize the central themes of this literature in Section 3. (The point of emphasizing non-reductive unity is not to criticize reductionism, but to examine how we conceptualize theoretical unity.) Second, UAR is misleading because unifying *science* involves more than unifying *theories.* Section 4 presents the "unity as interconnection" model and argues that previous discussions of non-reductive unity have overlooked "practical" (non-theoretical) forms of unification. Section 5 explores a recent controversy in systematics to defend the claim that resolving methodological conflicts can increase scientific unity without altering the degree of theoretical unification. Before concluding, I briefly discuss the relationship between the unity as interconnection model and the metaphysical concerns that motivate much of the discussion of UAR (Section 6). Philosophers who aspire to understand *scientists'* attempts to integrate science need something like the unity as interconnection model *as well as* a UAR model tailored to the metaphysical questions.

Despite the fact that many elements of this account have been presented before, my critical synthesis aspires to make two contributions. First, this paper articulates the idea of "unity as interconnection" more fully than previous work. Much of the literature addresses specific models of non-reductionistic unity (e.g., interfield theories and explanatory extensions), leaving the root conception of unity as interconnection implicit and unanalyzed. Once this implicit idea of unity is drawn into the foreground, we can see that reductionistic and non-reductionistic models of unification are instances of a single notion. Unity as interconnection provides a comfortable home for both. Second, philosophers should pay attention to models of inter*field* (as well as inter-theoretic) unity because the former are more versatile: they allow us to recognize the ways in which both theoretical and non-theoretical interfield connections contribute to scientific unity.

2. Unity as Reduction. What does it *mean* to say that some domain of science has been unified? The "unity as reduction" thesis is one influential answer to this question. UAR makes two claims:

> UAR1: unifying *theories* is the crucial aspect of unifying science, and
> UAR2: reduction is the primary form of theoretical unity.

Because this is a thesis about the *meaning* of unity—not a substantive claim about the extent of unity—it has been accepted by both the critics and defenders of unity. Furthermore, UAR can be understood as an account of either the local unification of two neighboring theories or the global unity of all science (i.e., all sciences are, in principle, reducible to physics). This section presents some evidence that the unity as reduction thesis remains prevalent within the philosophy of science.

Consider how the topic of unity is presented in texts that are intended to provide authoritative guidance to advanced undergraduates and beginning graduate students. One influential anthology introduces the topic this way: "reductionism of one form or another has become the model of theoretical unification, a picture of the unity of science" (Trout 1991, 388). Not only is reduction *the* model of theoretical unity (UAR2), but understanding theoretical unification provides a good account of the unity of science (UAR1). This passage may be unusual in its forthrightness but I suspect that both theses are widely (though often tacitly) endorsed. Writing in the Routledge *Encyclopedia of Philosophy,* Jordi Cat concurs: "Since Nagel's influential model of reduction by derivation, most discussions of unity of science have been cast in terms of reductions between concepts and between theories" (1998, 532). The unity as reduction thesis is not limited to textbooks and encyclopedias, however.

Following in Fodor's (1974) footsteps, Dupré (1983) offered a simple modus tollens argument. The unity of science requires reductionism. Reductionism is false. Therefore, we must reject the unity of science. A decade later, Dupré (1993) offered a more complex analysis. The ideal of unity presupposes three metaphysical theses that, together, can be called the metaphysics of an ordered world (essentialism, determinism, and reductionism). However, all three of these theses are at least problematic and probably false. Thus, we should reject the unity of science. Furthermore, Dupré argues (in chapter 10) that non-reductionistic accounts of the unity of science—specifically interfield theories and the unity of method—do not provide significant forms of unity. Thus, Dupré accepts UAR: he is committed to the claim that intertheoretic reduction is necessary for any significant form of scientific unity.

The account of UAR offered in this section is schematic. In particular, it does not explain what counts as a "reduction." This gap is intentional. The aim of this paper is to show that it is more productive to conceptualize unity as "interconnection" than as "reduction." Detailed discussion of how to characterize reduction (or of the extent to which reductions are successful) would draw attention away from my topic: the relationship between these two conceptions of *unity.* (Schaffner 1993 and Sarkar 1998 review the literature on reductionism.)

3. Non-Reductionistic Theoretical Unity. UAR is a common conception of unity but not the only one. It is now generally understood that theoretical integration is possible, even when reductions are not obtainable. This section reviews three familiar models of non-reductive unity (Darden and Maull 1977; Kitcher 1984; Kincaid 1990, 1997) and, based on this review, argues "unity as interconnection" provides a more general conception of unity that accommodates both reductionistic and non-reductionistic models of theoretical unification.

Darden and Maull argue that two fields or branches of science can be unified by a special purpose interfield theory which links them. To understand their model, we need to understand two ideas: fields and interfield theories. A field is

> an area of science consisting of the following elements: a central problem, a domain consisting of items taken to be facts related to that problem, general explanatory factors and goals providing expectations as to how the problem is to be solved, techniques and methods, and, sometimes, but not always, concepts, laws and theories which are related to the problem and which attempt to realize the explanatory goals. (1977, 44)

Darden and Maull offer genetics, biochemistry, and cytology as examples, suggesting that it is more appropriate to call them fields rather than theories. "Field" should not be confused with "discipline." Whereas disciplines are generally understood to have sociological dimensions (e.g., Bechtel 1986), Darden and Maull define and individuate fields on the basis of their conceptual structure. Furthermore, fields are typically much smaller than disciplines. For instance, the discipline of biology is composed of several fields, including genetics, ecology, botany, and entomology. I will use the terms "fields" and "subfields" to refer to conceptually defined entities larger than a single laboratory research group, but smaller than a discipline.

An interfield theory postulates a connection between the entities or processes which are studied by distinct fields. One well-known example is Sutton's hypothesis that genes are parts of chromosomes. This hypothesis provided a conceptual link between cytology and genetics. Once it was accepted, the previously independent fields began to influence one another. For example, given the small number of chromosomes in *Drosophila melanogaster* (a fact known from cytology) and the large number of genes (a fact known from genetics), the theory that genes are part of chromosomes immediately suggested the possibility that many genes are linked on a single chromosome. Furthermore, the law of independent assortment in Mendelian genetics suggested (contrary to cytological theorizing) that the maternal chromosomes are not inherited as a set.

Darden and Maull argue that interfield theories contribute to the unification of the biological sciences without effecting a reduction. Biologists did not attempt to reduce genetics to cytology (or vice-versa). Even after Sutton's hypothesis was accepted, the research programs of these fields remained largely distinct. In fact, trying to view the relationship between genetics and cytology through the lens of reductionism appears conceptually confused. Reduction is a relationship between theories; cytology and genetics are fields (not theories). Furthermore, even if we restrict our attention to the *theories* contained in these fields, there was no attempt to reduce the theories of one field to the theories of the other. Sutton's hypothesis that genes are part of chromosomes led to a more coherent understanding of genetics—in particular, it reduced inter-field conflict and facilitated the flow of ideas between fields—but it id not lead to an intertheoretic reduction.

Kitcher (1984) identifies two important non-reductive intertheoretic relations. First, molecular genetics led to the refinement of Mendelian concepts. Early Mendelians defined "mutation" as a genetic modification that alters the phenotype. The findings of molecular genetics led biologists to reject this definition because mutations can occur without any change in phenotype. Molecular genetics refined the Mendelian concept by providing an account of the physical process of mutation and an improved classification of the different kinds of mutations (e.g., base substitutions, frameshift mutations, inversions, and deletions).[1] Second, molecular genetics provides "explanatory extensions" of Mendelian genetics. An explanatory extension occurs when one theory explains statements that are presupposed by the problem solving patterns of a second theory. For example, Mendelian reasoning patterns assume that mutant genes can replicate. Molecular genetics "extends" Mendelian reasoning by explaining this presupposition of Mendelian argument patterns. Explanatory extensions approach the reductionistic ideal: the lower-level theory explains facts assumed by the higher-level theory. Nonetheless, they fall short of this ideal. Even though *some* specific Mendelian claims can be explained by molecular genetics, the suite of Mendelian reasoning patterns cannot be systematically reduced to molecular reasoning patterns. Mendelian reasoning patterns are not systematically reduced but "extended" by adding premises from molecular genetics. Thus, molecular and Mendelian genetics are closely connected even though molecular genetics cannot do all the explanatory work of Mendelian genetics.

So far, I have described three models of theoretic unification that do not require reduction: interfield theories, conceptual refinement, and explanatory extension. These models forced philosophers to look beyond reductionistic accounts of unity. But without any further development, they leave us with a plurality of different non-reductive relationships and no *general* characterization of theoretical unity. It is, of course, an open question just how far we can go in providing a general characterization of "unity." Morrison (2000) and Hacking (1996) argue that it is difficult, perhaps impossible, to provide a single, coherent account which covers all the different forms of scientific unification. Nonetheless, it is worth trying to articulate one important feature shared by these three models. The intuition that motivates these models has received its clearest expression

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[1] Maull (1977) discussed this example at some length, though with different emphases.

in Harold Kincaid's (1990, 1997) account of integrated inter-level theories, so it is to his account that I now turn.

Within the philosophy of biology, the relationship between Mendelian and molecular genetics has been the touchstone for discussions of inter-theoretic reduction. Kincaid frames the issue differently, asking whether cell biology (the upper-level theory) can be reduced to biochemistry (the lower-level theory).[2] His argument focuses specifically on whether the term "signal sequence" can be reduced. In the 1960s, cell biologists posed an important question: how do protein molecules move from the site of translation (the ribosome) to the site where they function in the cell? During the 1970s, cell biologists proposed a hypothesis which has subsequently been confirmed: translated proteins contain signal sequences—sequences of amino acids that bind to membrane receptors and thereby direct the molecule to the correct place within the cell. Biochemistry systematically reduces cell biology only if it can capture all the explanations of cell biology, including those that refer to signal sequences. Kincaid argues that the language of cell biology (e.g., signal sequence) cannot be captured in purely biochemical terms. Although reduction does not (even in principle) seem possible, the two theories are linked in significant ways. First, the ontologies of the theories are interconnected (e.g., signal sequence tokens are strings of amino acids—comparable to other molecules studied within biochemistry). Second, we may be able to explain how some of the properties of signal sequences supervene on the properties of amino acids. For instance, a study of the biochemical interactions between the signal sequence and receptor sites can explain how the signal sequence binds to a membrane. These two relations both fall comfortably within the reductionistic idea of one-way dependence: the ontology of the higher-level theory depends on the ontology of the lower-level theory. But, in other ways, biochemistry and cell biology are inter-dependent:

> Biochemical accounts often invoke [higher-level] biological truths in their explanations. They also employ information from the cellular level both as a heuristic to suggest new avenues of research and in the construction of experimental design. . .
>
> The dependence, of course, runs the other way as well. Biological assays require all kinds of biochemical information to confirm their results. Explanations of cellular processes in molecular biology constantly invoke biochemical information, and postulating possible biochemical mechanisms often suggests fruitful avenues of research into cellular-level processes. (Kincaid 1997, 67)

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[2] Kincaid actually uses the term "molecular biology." I use the term "cell biology" because it refers to a field that is more clearly distinct from biochemistry.

The central intuition motivating Kincaid's analysis (and my model of unity as interconnection) is that unification is a matter of degree: two theories become more unified as they become increasingly interdependent. Theories can be connected in a variety of ways. Scientists can come to understand how the ontologies, concepts, explanations, generalizations, data, or methods of two theories are related. Even when reduction fails, the concepts and ontologies of the theories may be closely connected and the two theories can be strongly interdependent with regard to their heuristics, methods of confirmation, and explanations. When all of these relations obtain simultaneously, the theories can be said to form an "integrated interlevel theory." Kincaid argues that the relationship between cell biology and biochemistry is an exemplar of this form of non-reductive unity.

I conclude this section by noting two advantages of conceptualizing unity as interconnection. First, conceptualizing unity as interconnection (an idea I will develop more fully in the next section) clarifies the relationship between reductionistic and non reductionistic models of unity. Intertheoretic reduction is a form of interconnection. In standard models of reduction, concepts are connected (via bridge principles) and, in virtue of these conceptual connections, the generalizations of the reduced theory can be derived from the more fundamental theory. As we have seen, however, reduction is not the only way to achieve intertheoretic unification. Thus, I regard unity as interconnection as the more basic (more general) conception of unity; intertheoretic reduction is one special form of this broader notion.[3]

Focusing on interconnection rather than reduction has a second advantage: it makes unity a matter of degree. When unity is understood as reduction, it is not a matter of degree—reduction either obtains or fails to obtain.[4] If reduction is (in principle) possible, then unity is achieved. As a result, conceptualizing unity as reduction generates a problem: whenever reduction is not (in principle) possible, we must embrace the "disunity" of science. (Recall Dupré's modus tollens argument.) The unity as interconnection model avoids this problem by recognizing that unification is

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[3] Hacking (1996) resists the idea that reductionistic models can be incorporated within "interconnection" models. In his view, reductionists conceptualize unity as "one-ness": two theories are unified when a more basic theory subsumes a less basic theory. By contrast, non-reductionistic unity obtains when two theories or fields are interconnected, but retain their distinctive identities. Hacking is right to insist that these are different kinds of connection, but both fit comfortably within the unity as interconnection framework.

[4] This claim applies to the dispute between reductionists and advocates of hierarchy. The set of higher-level generalizations is either completely reduced or it is not. By contrast, the reduction versus replacement dispute has a different dynamic. As Schaffner (1993) and others have noted, the reduced theory (T1) is rarely derivable from the reducing theory (T2). Rather, we derive a modified version of the reduced theory, T1*, from T2. The degree of similarity between T1 and T1* is a matter of degree.

a matter of degree and that two theories can be partially unified (integrated) even when reduction fails. While the examples given in this section focus on theoretical unification, connections between the non-theoretical components of fields can also contribute to scientific unification.

4. Extending Unity as Interconnection: The Practical Unification of Fields. Most philosophical accounts of the unity of science (both reductionistic and non-reductionistic) focus on the unification of *theories.* By contrast I emphasize the unification of *fields*. Attending to interfield relations has an important advantage. Because fields contain *both* theoretical and non-theoretical components (e.g., heuristics and methods), the language of fields provides a richer vocabulary for articulating a wider variety of connections. In particular, focusing on fields allows us to recognize what I call ‘‘practical’’ unification—connections between non theoretical components of two fields. Thus, viewing unity as a relationship between fields provides a more general framework for conceptualizing scientific unification. I am not arguing that accounts of interfield unity should *replace* accounts of intertheoretic unity. It is possible, for example, to have theories that cut across the boundaries of fields. In such cases, focusing on fields may make it harder to clearly perceive relations among theories. (I provide an illustration of this in Section 5.) I do not recommend focusing exclusively on interfield unification but I do maintain that philosophers of science need to explicitly address relations between the non-theoretical components of fields. Even those (few) authors who address the idea of practical unification have had difficulty capturing its significance. A closer examination the models offered by Darden and Maull (1977) and Kincaid (1990) will make this evident.

Darden and Maull’s emphasis on fields makes it easier to identify the non-theoretical components of science. They fail to fully exploit this potential, however. Their account of unity focuses primarily on *theoretical* relationships between fields: ‘‘unity in science is a complex network of relationships between fields effected by interfield *theories*’’ (1977, 61; emphasis added). Although they recognize that new inter-theoretic relationships can have important implications for the practice of science, they argue that the crucial step in achieving unity is developing a theory which links aspects of the intra-field theories. The emphasis on theoretical unification is understandable: practical unification always presupposes some conceptual or theoretical unification. But even if practical unification requires some conceptual unity, practical unification is often an important achievement over and above the initial conceptual connections. At least this is what I will claim in Section 5. Darden and Maull do not acknowledge the ways in which relations between the non-theoretical components of fields can, in their own right, contribute to unification.

(I have similar concerns about Culp and Kitcher's (1989) treatment of "scientific practice.")

Although Darden and Maull identify fields as the entities which are unified, they underestimate the potential importance of practical relations as sources of unification. In contrast, Kincaid has roughly the opposite problem: he explicitly discusses various *practical* relationships between fields, but holds that these practical relations justify the claim that the *theories* are integrated. Kincaid treats unity as a relationship between theories. For example, he treats molecular (cell) biology as "the candidate *theory* for reduction, with biochemistry roughly being the reducing *theory*" (1997, 52; italics added). Like Maull (1977), I find it inappropriate to treat cell biology and biochemistry as "theories." Cell biology and biochemistry contain theoretical hypotheses (and explanatory models) about mechanisms and structures that are not directly observable but it seems more natural to view these as hypotheses *within* the fields, rather than treating biochemistry and cell biology as theories per se. Consider Kincaid's argument that the heuristics and methods of cell biology should not be replaced by strictly biochemical methods:

> Even though the complete sequence of opsin was known [by biochemical methods], it was impossible to distinguish between signal sequences and transmembrane sequences from the biochemical information alone; this fact was only determined from a biological assay. By using a biological assay that copies cell function in the test tube, internal segments of opsin were found to function as signal sequences. It was looking for the *biological function,* not the biochemistry, that allowed this discovery. (1997, 64)

According to Kincaid, interaction between the distinctive methods of these theories led to the discovery that signal sequences can occur in the middle of a translated protein. While the biological assay is part of the *field* of cell biology, this laboratory technique does not seem to be part of any distinctive theory of cell biology. I maintain that it is simpler and more accurate to view this as a case in which inter*field* interaction led to an important insight into the working of signal sequences. The last two paragraphs suggest that even those who have paid the greatest attention to fields and non reductionistic forms of unity have failed to clearly grasp the significance of practical unity.[5] Viewing unity as interconnection provides the resources to address this shortcoming.

The basic structure of my account of unity is borrowed from Kincaid. Two fields can be interconnected or interdependent in the ways emphasized

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[5] I reject Galison's (1997) claim that methodological integration does not contribute to unification. In his view, researchers from fields as diverse as quantum mechanics, industrial

by Kincaid. The principal difference is that I view them as relations between fields. Additionally, I propose to divide the set of interfield relationships into two broad classes. Fields can be *theoretically* unified as the intra-field theories become more densely interconnected. Fields can be *practically* unified insofar as one field comes to rely on the methods, heuristics, or data of a neighboring field. Let's examine these two classes in more detail, beginning with three kinds of theoretical interconnections.

1. *Relations between explanations.* This category includes, but is not limited to, explanatory reductions and explanatory extensions. Kincaid (1990) offers a third possibility: the explanations of cell biology and biochemistry are interdependent because (irreducible) concepts of cell biology are often presupposed in the purportedly lower level biochemical explanations.
2. *Ontological relations.* The ontologies of two theories can be connected in a variety of ways. Darden (1991) suggests four principal ontological inter-relations: part/whole, identity, causal, and structure function. The part/whole relationship between genes and chromosomes provided the basis for an interfield theory linking cytology and genetics. The supervenience of higher level properties on lower level properties is a another possible relation between the ontologies of theories. (Exercise for the reader: are all supervenience relations captured by one of Darden's four relations?)
3. *Other conceptual relationships,* such as conceptual refinement (Kitcher 1984).

Fields can be *practically* connected in at least three ways.

4. *Heuristic dependence*. The theories and/or methods of one field can guide the generation of new hypotheses in a neighboring field. Darden (1991) claims that "using inter-relations" was an important strategy for generating new ideas during the history of genetics.
5. *Confirmational dependence*. The methods and/or data of one field may be used to confirm hypotheses generated in a neighboring field. (Kincaid offers examples of this form.)
6. *Methodological integration*. Methods can be developed to assess an hypothesis in light of the data (often generated by distinct methods) of two fields. This idea should not be confused with "unity of method." The unity of method generally refers to a set of methods used by

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chemistry, and meteorology formed a trading zone within which they discussed the methodology of Monte Carlo simulations. Based on this case study, Galison argues that trading zones do not contribute to the unity of science. I maintain that this is an overgeneralization: even if trading zones do not always make important contributions to unity, methodological integration can sometimes do so (Section 5).

> all sciences (e.g., the logic of hypothesis testing). By contrast, I'm concerned with a more localized phenomenon: the development of particular methods to integrate the bodies of data generated by two fields. Section 5 will offer an example of this form of practical interconnection.

I distinguish the theoretical and practical dimensions by thinking about the *function* of theories. While there has been considerable dispute about the structure of theories, philosophers generally agree that theories are introduced to serve explanatory ends (e.g., the explanation of observed regularities). Thus, I regard the crucial explanatory factors (whatever they may be) as elements of the theory. Presumably these crucial factors will include concepts, laws, and (explanatory) models. Scientific theories are a diverse lot: they vary in generality, strength of confirmation, and structure (e.g., some theories seem to contain empirical laws while others are better viewed as families of models that, by themselves, have no empirical content). Focusing on the function of theories allows me to remain neutral with respect to contentious debates over the structure of theories. Nonetheless, attending to the function of theories is sufficient to divide the components of fields listed by Darden and Maull into two broad categories: the theoretical and practical (non theoretical) dimensions of fields. Theories typically specify an ontology (set of entities, properties, and processes) and explain events in terms of that ontology. Thus, I view relations between the ontologies postulated by theories as theoretical relations. Similarly, and in keeping with the usual practice of regarding intertheoretic reductions and explanatory extensions as forms of theoretical unity, I regard relations between explanations as part of the theoretical dimension (even though explanations per se are not theories). While hypothesis generation, confirmation, and methodological integration are not ''theory free,'' they seem to belong to a separate dimension of science because they are not (directly) oriented toward providing explanations.

Two main factors affect the degree of unification. Fields become more unified either by (1) increasing the number (or variety) of ways in which two fields are connected, or (2) increasing the significance of connections which are already in place. A connection can become more significant if it begins to *transform* the neighboring field. That is, if the introduction of a new concept, generalization, technique, or heuristic leads to considerable change in the absorbing field, then the change is regarded as significant (see Maull 1977).

The practical and theoretical dimensions of unification are largely but not completely independent. I do not see how the distinctive data, heuristics, or methods of one field can come to influence a neighboring field unless we have some (conceptual) understanding of how the two fields are related. By treating theoretical and practical unification as largely independent dimensions, I am suggesting that it is possible to have: (a) considerable theoretical unification even though research practices in the two fields remain largely independent, or (b) considerable practical unification while only having a low degree of theoretical unification. The next section presents an example to defend the claim that practical unification is both scientifically important and largely independent of theoretical unification.

5. An Example of Methodological Integration: The Role of Paleontological Data in Phylogeny Reconstruction. Over the last few years, I have been trying to understand why paleontology is not more fully integrated with evolutionary biology. One useful approach is to contrast paleontology (which studies fossils) and "neontology" (which studies living organisms). While members of both fields study evolution, they typically rely on different methods, presuppose different bodies of background knowledge, and focus on somewhat different questions. I envision "evolutionary paleobiology" as a subfield of paleontology which uses fossil evidence to study long term evolutionary patterns and processes. Evolutionary neontology, on the other hand, emphasizes genetics and population biology, relies more heavily on experiments, and typically studies patterns on shorter time scales. (Carroll 1997 and Kemp 2000 describe the relationship between these fields in more detail.)

Rather than looking at the general relationship between these fields, this section explores one area of interaction: the role of paleontological data in phylogeny reconstruction. In particular, I will focus on two ideas:

(A) Using stratigraphic data to revise molecular clock dates, and
(B) Using stratigraphic data along with character data to assess phylogenetic hypotheses.

Many authors have discussed how the morphology of fossils might contribute to phylogeny reconstruction (e.g., Donoghue et al. 1989; Smith 1998). In contrast, proposals A and B concern "stratigraphic" data—i.e., temporal information inferred from the fact that fossils are found in dated geological strata. My point is not to advocate either proposal. Rather, I simply want to emphasize that fossils and extant taxa provide two different (potentially conflicting) sources of data for reconstructing phylogeny. As a result, systematists need to assess what role, if any, stratigraphic data should play in phylogeny reconstruction. In other words, systematists are trying to assess phylogenetic hypotheses in light of the distinctive data derived from neontological and paleontological methods.

Proposal (A) would use the time of first appearances in the fossil record to check molecular clock estimates of divergence times. A recent study of mammal phylogeny used the techniques of molecular systematics to argue that the placental mammals diverged from other mammals 130 million years ago (Kumar and Hedges 1998). Paleontologists have challenged this claim because fossil evidence points to a divergence around 85 million years ago. Foote and colleagues (1999) argue that it is unlikely that the early placental mammals could have existed 45 million years without being preserved in the fossil record. They reason roughly as follows. Several different lineages of placental mammals are found in the fossil record near the Cretaceous/Tertiary boundary. If the molecular clock estimate is correct, then a number of distinct lineages must have been diversifying for a period of 45 million years, but leaving no fossils. Using various mathematical models of diversification, they estimate the sum of the durations of all the lineages existing during this interval of non-preservation. Based on this estimate (and the assumption that the probability of preservation is similar for placental and non-placental Cretaceous mammals), they determine the probability that *none* of these lineages was fossilized during a 45 million year interval. Foote and colleagues conclude that ‘‘even with the most generous treatment of the hypothesis [of early divergence]. . . the probability of complete non-preservation is only 0 .02’’ (1999, 1311). Thus, it is extremely unlikely that the placental mammals left no fossils for 45 million years. Even after the molecular clock is initially calibrated (a procedure which usually requires input from the fossil record), the fossil record can conflict with specific applications of the molecular clock. It would, therefore, be useful to have techniques for assessing claims about the timing of major evolutionary developments in light of the *total* data set, including both stratigraphic and molecular clock estimates of divergence times.[6]

Paleontological and neontological systematists generally use the same procedures to infer the pattern of branching in a cladogram from *character* data, but paleontologists have additional temporal data that may be relevant to assessing phylogenetic hypotheses. The second proposal would use stratigraphic (temporal) data alongside character data in the reconstruction of phylogeny. A simple thought experiment shows one way in which stratigraphic data might be used in reconstructing a phylogeny. Suppose that a phylogenetic tree requires that we postulate that a species went unobserved for, say, 10 million years.[7] The supposition that the species existed during this interval without being observed in the fossil record is

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[6] Ji et al. (2002) recently announced the discovery of a very early placental mammal—a find that may vindicate the molecular clock estimate of divergence times.

[7] Phylogenetic trees provide a richer representation of phylogeny than cladograms do. Whereas cladograms represent only sister group relationships, trees often represent ancestor-descendant relationships as well as sister-group relationships. Whereas cladograms represent only relative branching times, trees often represent absolute dates of branching events. See Smith (1994) for more on the distinction between trees and cladograms.

an ad hoc hypothesis of non-preservation (Smith 1994). Suppose that an alternative tree handles the morphological character data equally parsimoniously, but does not postulate such a long period of unobserved existence. The second hypothesis fits our total data set (i.e., the character data + stratigraphic data) better. It fits the character data equally well, but provides a better fit with the stratigraphic data because it requires no ad hoc hypothesis of non-preservation. This hypothetical example suggests that, in some instances, it may be reasonable to use stratigraphic data to choose among the different trees that are compatible with a given cladogram. Similar reasoning suggests that stratigraphic data are relevant to determining the pattern of branching in a cladogram. Smith (1998) examined the phylogenetic relationships among six asteroid crown groups. Molecular data support a single most parsimonious (unrooted) tree. However, the molecular data are insufficient to decide between two alternative ways to root the tree: either the spinulosids or the astropectinids was the "root" taxon from which other taxa diverged. While the molecules do not provide a clear answer, stratigraphy may. The fossil record of the spinulosids extends back 200 million years, while the fossil record of the astropectinids goes back only 100 million years. Thus, given that the molecular data do not provide a clear answer and that treating the astropectinids as the root taxon would require postulating a 100 million year gap in the fossil record, we should prefer a cladogram rooted on the spinulosids on stratigraphic grounds.

At present, systematists are deeply divided about how to handle stratigraphic data. Many cladists maintain that we should ignore the stratigraphic record: given the incompleteness of the fossil record, we should not make too much of the fact that some particular taxon is absent from the record. Smith (1994) takes a middle ground position. His method uses stratigraphic data—but only after the parsimony analysis is complete. The result is that considerations of stratigraphic fit are never allowed to over ride parsimony reasoning. Fisher (1994) and Wagner (1998) go further; their methods would accept less parsimonious cladograms if (and only if) they dramatically improve fit with stratigraphic evidence. These issues are complex and undecided, but computer simulation studies have compared the accuracy of traditional cladistic methods to alternative methods that use both character data and stratigraphic data (Fox et al. 1999; Wagner 1998). Methods which use both stratigraphic and character data generally provide more accurate estimates of simulated phylogenies. If these simulation results prove to be robust, they would provide good pragmatic grounds for developing methods that incorporate stratigraphic data in our assessments of phylogenetic hypotheses.

This discussion has not done justice to the complexity and interest of these methodological issues. (See Grantham 2004 for a more detailed discussion.) But the moral of the story does not depend on how these issues are resolved. Rather, I will simply highlight two facts. First, the question of how (or whether) to use stratigraphic data in phylogeny reconstruction is *not* a problem about how the explanations or theories of paleontology and neontology are related. Rather, it is a problem of resolving potential conflicts between the distinctive data generated by two fields. Second, resolving these conflicts should influence our assessment of the degree of interfield integration. Here is one way to see the point. Consider two ways evolutionary biology might develop. In the first scenario, systematists come to believe that stratigraphic data are a crucial component of phylogeny reconstruction. As a result, neontologists and paleontologists not only share data, but also develop methods that assess phylogenetic hypotheses in light of all the relevant data. In the second scenario, the conflict persists: scientists disagree about the relevance of stratigraphic data. Paleontologists and neontologists use methods that lead to conflicting results. I hope you will agree that the extent to which systematics is unified varies in these scenarios. But nothing I have said alters the extent to which the principal explanatory theories held by systematists are (or are not) theoretically unified. Similarly, I have not assumed any changes in the extent to which the explanatory theories of paleontological and neontological evolutionary biology are (or are not) integrated. Since these scenarios differ in the level of unity within systematics, but *not* in the level of theoretical unity, I conclude that methodological developments can contribute to the unification of fields independently of theoretical unification.

This case study involves interactions among several different scientific units (theories, fields, and disciplines). It is worth unpacking this complexity a bit. The scientists in this controversy are aligned with one of two disciplines (geology or biology). Cutting across these disciplines is a large interdisciplinary research cluster centered on the study of evolution. Evolutionary paleobiology lies at the intersection of the research cluster and the field of paleontology. This case study concerns the relationship between neontological and paleontological methods within the field of systematics. Issues of unification can and do arise at different levels within this complex array of partly overlapping units of scientific investigation (see Figure 1).

The prospects for reductively unifying this array of disciplines, fields, and theories are bleak. While a hierarchically expanded version of the synthetic theory provides a unifying framework for the study of evolution, many fields have special purpose theories which are unlikely to be reduced. Consider, for instance, the role of taphonomy within paleontology. Taphonomy studies the processes of fossilization and the extent to which these processes bias the fossil record. The cluster of theories, concepts, and

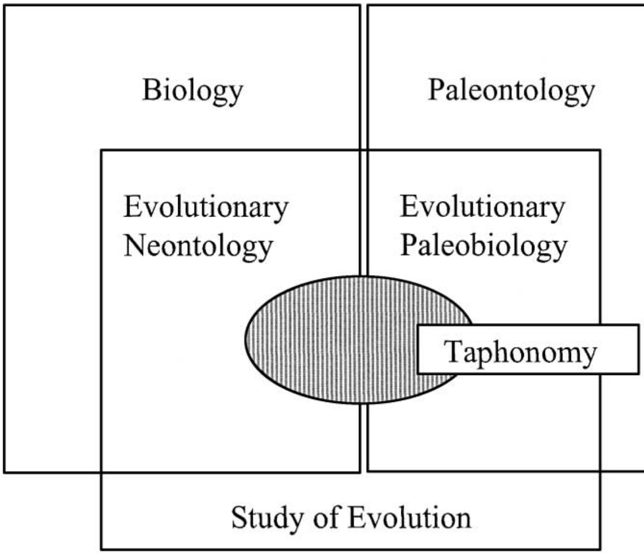

Figure 1. Overlapping scientific units. This case study concerns a number of different scientific units that overlap in complex ways, including disciplines (biology), interdisciplinary research clusters (study of evolution), fields and subfields (paleontology, evolutionary paleobiology), and theories (taphonomy). The shaded area in the center corresponds to systematics.

methods found in taphonomy is so distinctively paleontological that it is hard to identify another theory or field which is even a candidate to “reduce” taphonomy. Taphonomy is, however, central to much work in paleontology and is obviously crucial in determining when stratigraphic data reliable enough to be used in estimating phylogeny.[8]

The reflections of the last two paragraphs underscore two themes of this essay. First, developing a reliable method that uses both character and statigraphic data to estimate phylogeny would increase the unity of systematics (and promote the integration of neontological and paleontological evolutionary biology). This increase in *practical* unification does not, however, require any change in the central explanatory theories used by the various scientific units under discussion (e.g., synthetic theory of evolution shared by evolutionary sciences, theories of speciation used in systematics, or theories of taphonomy found in paleontology). Thus, practical interconnections can enhance scientific unification without altering the degree of theoretical unification. Second, in order to clearly discuss the scientific pursuit of unification (especially in arenas as complex

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[8] Briggs and Crowther’s (2001) encyclopedia of paleobiology devotes 120 (out of 550) pages to taphonomy.

as the one I've been discussing) we need to reflect on the unification of both fields and theories.

6. An Objection.

> Uncovering the *character* of unity and disunity is a philosophical task, one that will contribute to the broader goal of better understanding the practice of science itself. And given the progress and methods of empirical science, there is perhaps nowhere that metaphysics is less helpful. (Morrison 2000, 237)

Much of the philosophical dialogue concerning unity and reductionism focuses on the following argument: Assuming that materialism is true, then all macro-level phenomena (i.e., entities, events, and processes) are just complex combinations of micro level phenomena. If macro-level occurrences are just complex micro-level occurrences, then we should, at least in principle, be able to explain all macro-level occurrences in micro-level terms. This line of argument moves from a metaphysical premise about the relationship between the ontologies of macro and micro levels to epistemological conclusions about the relationship between higher-and lower-level explanations. The force of the argument is that if one accepts materialism (a position few want to deny), then one is forced to accept reductionism and, as a result, the unity of science. Thus, advocates of disunity and anti-reductionism are asked to (1) demonstrate that materialism is false, or (2) show how "emergent" properties can block epistemological reduction without denying materialism. Readers steeped in this philosophical tradition may find the present essay to be peculiar, for it does not engage these issues. I'll argue that even though conceptualizing unity as interconnection does not help to resolve the metaphysical issue, it makes an important contribution to the philosophy of science.

Let me begin with two remarks. First, unity as interconnection is "agnostic" about reduction: it neither presupposes nor denies that intertheoretic reductions occur in science. Unity as interconnection is nonreductionist rather than anti-reductionist. Second, this account is local rather than global. My aim is to develop a conception of unity which allows us to understand the processes through which neighboring fields become more integrated, not to map out a grand vision of how all the various sciences are interrelated. Given these remarks, the account of unity developed in this paper appears to be guilty as charged: it does not address the metaphysical issue which has been central to the unity of science debate. Even though my account of unity as interconnection does not answer the big metaphysical question, it does have something to offer the philosophy of science. In particular, whether or not the metaphysical thesis of reductionism is true, conceptualizing unity as interconnection has important advantages over UAR. Let us consider each possibility in turn.

Suppose that, in some particular domain, reductionism is true. That is, suppose that a lower level theory can (in principle) do all of the explanatory work previously done by the higher-level theory. If reductionism holds and we accept UAR, then the metaphysical discussion is over: the sciences are unified. But even if we were assured that unification is (in principle) possible, the sciences are *not,* in fact, as unified as many scientists would like. Consider, for example, the frequent calls for greater unification within evolutionary biology. Here are two illustrative appeals.

> Combining molecular views of animal phylogeny with the trace fossil record helps evolutionary biologists reconstruct the primitive body plans that gave rise to the living phyla. As the important findings of developmental biology lead to a greater understanding of gene regulation, scientists can begin to reconstruct primitive developmental systems and their pattern of evolution. The synthesis of these fields, which is just beginning, will yield a much more complete picture of the early evolution of animal life. (Erwin, Valentine, and Jablonski 1997, 134)

> Except during the interlude of the New Synthesis, there has been limited communication historically among the disciplines of evolutionary biology, particularly between students of evolutionary history (paleontologists and systematists) and those of the molecular, population, and organismal biology. There has been increasing realization that barriers between these subfields must be overcome if a complete theory of evolution and systematics is to be forged. . . While embracing the diversity of evolutionary biology and systematics, [this Congress] sought to further their integration . (Reaka-Kudla and Colwell 1991, 16)

Would a proof of in-principle reductionism satisfy the scientists who make these appeals for greater unification? Although I do not have the space to fully argue the point here, I am confident that in principle reduction would not satisfy their demands. Even if we were assured that all complex macroevolutionary events (e.g., the emergence of new body plans) could (in principle) be explained in terms of some future theory of evolution, this assurance would not put Erwin and colleagues in possession of the understanding they seek. Furthermore, even if we were actually in possession of a theory that could explain all macroevolutionary phenomena, this *theoretical* understanding would not (by itself) provide methods that would allow us how to handle the kinds of conflicts discussed in section 5.

At least since the 1970s, philosophers have known that the forms of unification pursued by scientists often differ from the philosophical ideal of intertheoretic reduction. (See, e.g., Schaffner (1974) on the peripherality of reduction.) How are we (as philosophers) to make sense of the scientific pursuit of unification? It appears that we have only two options: either the forms of unity sought by scientists (which are often distinct from the philosopher's notion of intertheoretic reduction) *are* epistemically important or they *are not*. Between these options, I prefer the former. As Van der Steen (1993) notes, *some* appeals for greater unification may be misguided; it would be presumptuous, however, to assume that scientific appeals for (non-reductive) unification are uniformly misguided. Unification remains an important regulative ideal (Burian 1993; Kitcher 1999; Wylie 1999). Assuming that something scientifically important is at stake in the pursuit of non-reductive forms of unification, philosophers of science ought to have a conceptual framework for understanding the aims, methods, and accomplishments of these forms of unification. Thus, even if we knew that "in principle" metaphysical reductionism were true, philosophers of science would need a conceptual framework for understanding the pursuit of nonreductive forms of practical and theoretical integration. My aim has been to contribute to the development of such a framework.

What if we begin with the contrary supposition? That is, what if some higher-level theories are not, even in principle, reducible? Accepting both anti-reductionism and UAR entails the "disunity"of science. However, Darden and Maull, Kincaid, and Kitcher argue persuasively that fields can be at least partially integrated without intertheoretic reduction. Similarly, I argue (in Section 5) that methodological developments may help to integrate neontological and paleontological systematics even though the distinctive theories of paleobiology (e.g., taphonomy) are unlikely to be "reduced." Different kinds of (theoretical) unification differ in their metaphysical import (Morrison 2000) and the methodological integration I discuss presumably does not bear on the issues of reductionism or emergence at all. But the metaphysically less significant forms of unification may be scientifically significant. Philosophers who aspire to understand the aims and accomplishments of integration within the sciences need a more versatile concept of unity that recognizes the variety of forms of unification. Whether or not one accepts reductionism, the broader notion of unity as interconnectedness can benefit the philosophy of science.

7. Conclusion. "Unity as reduction" accounts are too narrow. Not only do they focus solely on *theoretical* unity, they only address one form of theoretical unity—intertheoretic reduction. Although the metaphysical issues of reductionism and emergence are philosophically important, treating intertheoretic reduction as a necessary condition for the unity of science leaves the philosophy of science without the resources to understand ongoing attempts to unify scientific knowledge. The unity as reduction paradigm falls short in at least two distinct ways. First, a singleminded focus on theoretical unification ignores some scientific activities which unify fields (practical unification). Section 5 highlighted the problem of developing a method that uses both stratigraphic and character data to estimate phylogeny. Developing tools to integrate the distinctive data generated by paleontologists and neontologists would promote the unification of systematics, but finds no place within accounts of unity that focus solely on the relations among theories. Second, viewing unity as reduction leads us to say that whenever reduction fails, the sciences are in a state of "disunity." But this conclusion does not appear to be reasonable in light of the significant (but non-reductive) interfield relations stressed by Darden, Maull, Kitcher, and Kincaid. Building on their work, I have tried to develop a more adequate account of what it means to unify or integrate two scientific fields.

The unity as interconnection model conceives of unity as a relationship between *fields* rather than theories. Fields are unified to the extent that they are densely interconnected. Two fields can become more unified on both the theoretical dimension (e.g., by connecting the ontologies, explanations, and concepts of two fields) and the practical dimension (e.g., by connecting the heuristics and methods of the fields). This framework provides resources to address the central problems faced by UAR. In particular, unity as interconnection recognizes the importance of practical unification—an aspect of unification that was completely ignored within the unity as reduction framework and not clearly conceptualized even by those sympathetic to the idea of nonreductionistic unity (see Section 3). Furthermore, conceptualizing unity as interconnection provides the resources to make more nuanced assessments of the level of interfield unification whether or not reductions are (in principle or in practice) possible.

It is time to move beyond the philosophical image of unity as reduction in order to see the more complex process through which the many facets of scientific fields become more densely interconnected. There is more to unification than intertheoretic reduction.

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