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## **Reduction, Integration, and the Unity of Science: Natural, Behavioral, and Social Sciences and the Humanities<sup>1</sup>**

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1. A Historical Look at Unity
2. Field Guide to Modern Concepts of Reduction and Unity
3. Kitcher's Revisionist Account of Unification
4. Critics of Unity
5. Integration Instead of Unity
6. Reduction via Mechanisms
7. Case Studies in Reduction and Unification across the Disciplines
8. Conclusions

### **1. 0. A Historical Look at Unity**

The notion that science is unified in one way or another dates back at least to Aristotle, though unity claims since then have been diverse and variously motivated. By way of introduction to the modern discussion of unity, disunity, and integration, in this first section we examine five historical attempts to unify knowledge: Aristotle's metaphysical and hierarchical unity; the Enlightenment project of the French Encyclopedists; the systematic unity of *Naturphilosophen* Lorenz Oken; the methodological unity of the Vienna School's *Encyclopedia of Unified Science*; and finally, the organizational unity of cybernetics and general systems theory. We treat these unification projects not only as context, but also because, as we shall see, something of their momentum carries over into the modern discussion.

#### *1.1. Aristotle's Metaphysical and Hierarchical Unity*

Aristotle arranged the 'sciences' into three divisions: the theoretical sciences (metaphysics, mathematics, and physics); the practical sciences (e.g., ethics and politics), and the productive sciences (e.g., poetry and rhetoric). That is, he divided sciences according to their purposes. Theoretical sciences are concerned with knowledge alone and for its own sake, practical sciences

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are for doing, and productive sciences are for making. Despite these divisions, however, Aristotle's image of the sciences was one of a unified hierarchy. In the *Metaphysics*, he made clear that the theoretical sciences—most particularly metaphysics or 'theology'—are at the top of the hierarchy. These are the sciences that investigate first causes, and the people who know them know universally and in the highest degree, as well as "understand...all the underlying subjects" (*Metaphysics A.2*).

Aristotle argued that the theoretical sciences are the most basic. It is by virtue of theoretical knowledge that one has true command of practical and productive matters. Without theory, one merely has experience. With theory, one has art (*techné*). Consider Aristotle's example of the physician who treats Callias. Medicine for Aristotle is a practical science, but its practice is enhanced by a grasp of theory. The better physician will not be one who knows only how to treat Callias, or men of a certain age, or those with the specific ailment afflicting Callias. Rather, the better physician will be one who understands disease *qua* disease, according to its principles and causes, and understands people *qua* people.

This consequence of a science's rank in the hierarchy applies even within the theoretical sciences. It is by virtue of doing metaphysics, the highest theoretical science, that one truly grasps the lesser two theoretical sciences. That is, the better physicist or mathematician is one who understands metaphysics. As Aristotle makes clear in the middle books of the *Metaphysics*, he thinks there are causes and substances that are beyond the reach of physics. For him, physics is the science of sensible substances and their causes, but there is a more fundamental substance (*ousia*) as well as a more fundamental source of motion. The study of this substance and of the first motion inform physics rather than the other way around. Mathematics has the same relationship with metaphysics as physics: the study of surfaces and quantity depends upon and is informed by the more universal questions of metaphysics (*Metaphysics. M and N*): Are there mathematical objects? Do numbers exist? Are numbers causes? Are they substances? Aristotle does not take up these questions by asking what we know about mathematics, but rather by asking what we know universally.

## 1.2. French Encyclopedists

When we think of comprehensive accounts of knowledge today, we often think of encyclopedias. These modern works have their origin in the period after the scientific revolution, when the integration of knowledge achieved by Aristotle and maintained by the Scholastics was fundamentally undercut. Historically the most famous encyclopedia was *Encyclopédie, ou dictionnaire raisonné des sciences, des arts et des métiers* (Encyclopedia, or Reasoned Dictionary of the Sciences, Arts, and Trades). Its 17 volumes (plus 11 volumes of illustrations) were produced over the period 1751–72 under the editorship of Denis Diderot along with the mathematician Jean Le Rond d'Alembert. The project had its origins in a French translation undertaken by John Mills in 1743–5 of Ephraim Chambers's *Cyclopaedia, or Universal Dictionary of Arts and Sciences*. The French publisher wrested control from Mills and, intending speedy publication, engaged two editors in succession who instead expanded the project's contours. The second, Diderot, undertook a monumental effort to outline the present state of knowledge in the sciences, arts, and practical crafts and to make this knowledge widely accessible. Originally each topic was to be covered by a scholar or craftsman expert in it, and

contributors included such prominent Enlightenment figures as Voltaire and Rousseau. In the end, though, Diderot and d'Alembert wrote many of the 71,818 entries themselves.

Although clearly embracing a philosophical perspective, the *Encyclopédie* served more to bring together different domains of knowledge than to unify or even systematize them. To the extent that there was a unifying theme, it lay in the Enlightenment's reliance on reason and empirical observation to provide knowledge. Even religion was presented as an object of human reason, not as a source of knowledge via revelation. The *Encyclopédie* thus stood in opposition to the scholastic tradition, which maintained Aristotle's legacy but subordinated it to Christian theology. The entry on philosophy emphasizes the role of reason:

Reason is to the *philosopher* what grace is to the Christian. . . . Other men are carried away by their passions, without their actions being preceded by reflection: these are men who walk in the shadows; whereas the *philosopher*, even in his passions, acts only after reflection; he walks in the night, but he is preceded by a torch. The *philosopher* forms his principles on the basis of an infinite number of discrete observations. . . . He certainly does not confuse it with probability; he takes as true that which is true, as false that which is false, as doubtful that which is doubtful, and as probable that which is only probable. He goes further – & here is a great perfection of the *philosopher* – when he has no proper motive for judging, he remains undecided. (Translation by Dena Goodman from *The Encyclopedia of Diderot and d'Alembert Collaborative Translation Project*, <http://www.hti.umich.edu/d/did/>)

Not surprisingly, this emphasis on reason and empirical knowledge and criticism of claims for revealed truth ran afoul of the Church, so after the first seven volumes were published in Paris under a royal privilege, the remainder were published under the false imprint of Samuel Faulche, Neuchâtel (in fact they were published in Paris).

Reflecting the great diversity of human pursuits that involve acquisition of knowledge, the *Encyclopédie* represents a compilation of knowledge rather than an integration of it. In many respects, this reflects our contemporary situation. But in the wake of the enlightenment, other theorists resumed the pursuit of systematic unity.

### 1.3. Oken's Systematic Unity

Lorenz Oken (1779-1851) was an anatomist and a leader of the *Naturphilosophie* movement in Germany. A student and follower of Friedrich Schelling, Oken applied the precepts of *Naturphilosophie* to his thinking about biological systematics. The metaphysics he learned from Schelling—a Pantheistic view by which everything in nature could be deduced from a first principle, namely God—led him not only to treat the biological world as a part of God, but also to articulate a hierarchical classification of everything (Oken, 1809, 1831; Ghiselin and Breidbach, 2002). Oken treated the organization of the world as a divine code that could be read by understanding the systematic relations between each thing and everything else.

Oken's approach to systematics was essentially that of the *scala natura*. His *Lehrbuch der Naturphilosophie* offers an account in which his philosophical, theological, numerological, and biological assumptions were all tied together to produce a single, unified 'anatomy' of the world.

There was first an argument that God is nothing, since all comes from nothing. This is just to say, of course, that God is (the source of) everything (Ghiselin, 2004). After this theological argument was some numerological reasoning relating the four basic elements of the world (fire, air, water, and earth) to processes like electricity and crystallization. The book culminated in an argument that war-making is the highest art.

The thoroughgoing unity of Oken's classification is well illustrated by his theory of color.<sup>2</sup> For numerological, theological, alchemical, and scientific reasons, red corresponds to fire, then to love, and then to God the Father. Blue, as we might expect, corresponds to air, then to faith, and then to God the Holy Spirit. Yellow corresponds to earth, vice, and Satan. The colors of natural entities fit into, and are regarded as explained by, this overarching system. For example, animals are predominantly red because they correspond to fire (and the cosmos). Plants have green leaves because they correspond to water (and the planets). Flowers get a three-way classification: those of lower plants are most often yellow, the intermediate ones blue, and the highest ones red.

#### 1.4. *Encyclopedia of Unified Science*

Whereas Oken attempted to build unity in terms of conceptual (semantic) ideas, other approaches to systematizing knowledge appealed to logic (syntax) for the bridges between bodies of knowledge. Logical positivism, later known as logical empiricism, developed in the 1920s in Austria (Verein Ernst Mach in Wien, commonly known as *the Vienna Circle*), Germany (Gesellschaft für Wissenschaftliche Philosophie Berlin, commonly known as *the Berlin Circle*), and Poland. The term and basic doctrine of *positivism* originated with August Comte, an early 19<sup>th</sup> century French philosopher who was skeptical of philosophical systems and metaphysics generally and emphasized positive knowledge—that is, knowledge grounded on observation and experimentation. A more immediate influence was the positivism of Ernst Mach, a professor of physics in Prague and Vienna until his retirement in 1901. He adopted a radical empiricism in which the only source of knowledge was sensory experience, and scientific laws were instrumental, serving to describe and predict phenomena available to the senses. Most of the early logical positivists adopted Mach's emphasis on the experiential grounding of knowledge, although most did not share his extreme instrumentalism. The adjective *logical* identifies the chief resource to which the logical positivists appealed in advancing beyond individual observations to generalized scientific claims. The logic to which they appealed was not traditional Aristotelian logic, but rather the modern mathematical logic developed in the late 19<sup>th</sup> and early 20<sup>th</sup> centuries by Frege, Peano, Russell, Whitehead, and others. Many of the logical positivists were themselves scientists who were concerned about clarifying the foundations of science, especially in light of major developments in physics and other sciences contemporaneous with the rise of mathematical logic.

Although many of the logical positivists focused on physics, their emphasis was on providing a general account of knowledge, which they equated with scientific knowledge. They also, as discussed in more detail below, articulated a vision of how different sciences could be unified into a theoretical whole through theory reduction. One motivation was to counter a view, widespread at the time, that psychology addressed an inner world that was discontinuous with the outer world studied by the other sciences. Initially Carnap (1928) proposed to overcome this

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<sup>2</sup> This example is due to Michael Ghiselin, and is spelled out in more detail in Ghiselin (2004).

discontinuity by treating all science as grounded on private experience, from which the world was *constructed*. This project, however, was unsuccessful. An alternative proposal for unification was offered by Moritz Schlick, who distinguished the content of experience (specific sensations) from its structure (relations between experiences). He maintained that the structure of experience was objective and could be investigated empirically. These and other attempts to provide a common account of the methodology of all sciences and link them into a common theoretical edifice gave rise to the *International Encyclopedia of Unified Science*, edited jointly by Otto Neurath, Rudolf Carnap, and Charles Morris.<sup>3</sup>

Neurath envisaged that the encyclopedia would grow to hundreds of volume, with one entry issued each month in a subscription series. In the end only 20 entries were published in two volumes, the first under the original title and the second under the more modest title *Foundations of the unity of science; toward an international encyclopedia of unified science*. The goal, according to Neurath (1938, p. 24), was “to integrate the scientific disciplines, so to unify them, so to dovetail them together, that advances in one will bring about advances in the others.” The main tool for such dovetailing of different sciences was logical analysis, which would serve to relate the concepts and ultimately the theoretical claims of various sciences. Although the editors envisioned an axiomatized integration of the great body of knowledge provided by the various sciences, they adopted a piecemeal strategy. They fully expected this procedure would uncover inconsistencies whose eventual resolution would improve each science as well as the prospects for their integration.

Since the account of unity advanced by the logical positivists has been the chief focus of philosophical accounts of the unity of science ever since, we will return in greater detail to this account in part 2 of this chapter. First, however, we consider one last proposal for unity which, although receiving less attention in philosophy, has and continues to have considerable influence in the sciences themselves.

### 1.5. Cybernetics and General Systems Theory

Beginning in the 1940s cybernetics and general systems theory advanced a very different conception of how to unify science that focused primarily on the organization found in phenomena the sciences seek to explain, especially the biological and social sciences. The term *cybernetics* was coined by mathematician Norbert Wiener from the Greek word for ‘helmsperson’, and was applied to systems that could steer themselves (Wiener, 1948). Working during World War II, Wiener initially focused on a practical problem: developing a system for improving the accuracy of anti-aircraft guns. His desired solution invoked feedback control; that is, the accuracy of previous shots would be used to adjust gun controls before taking the next shot. Challenges he faced in getting the idea to work led him to collaborate with an engineer, Julian Bigelow, and a physiologist, Arturo Rosenblueth. In a paper in *Philosophy of Science* (Rosenblueth, Wiener, & Bigelow, 1943), the three developed the idea that feedback enabled both biological and artificial systems to be goal-directed. They regarded this as resuscitating a notion that was anathema to the positivists: that of *teleology*. Subsequently Wiener organized a

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<sup>3</sup> The term *unified science* was first invoked in 1938 when *Erkenntnis*, which had been the house organ of the Vienna Circle since 1930, was moved to the Hague and renamed the *Journal of Unified Science*. Just two years later, however, it ceased publication.

multi-year conference series. He initially called it Conference on Circular Causal and Feedback Mechanisms in Biological and Social Systems but, beginning in 1949, the conference adopted Wiener's term *cybernetics* for its name. As the initial name suggests, the participants regarded the idea of feedback organization as having the potential to unify biological and social systems.

Around the same time, biologist Ludwig von Bertalanffy (1951) advanced General Systems Theory as an antireductionist yet unifying perspective. Rather than focusing on the particular components out of which different things were made, systems theory emphasized the organization of parts into wholes and maintained that the same principles of organization, such as negative feedback, would be found applicable in physics, chemistry, biology, the social sciences, and technology.

Although there is an International Society for Systems Sciences that is still active and runs large international meetings, cybernetics and general systems theory have declined into niche specializations. Today the strongest influence of these approaches is indirect, funneled through successors with new ways to identify general principles of organization and use them towards unifying science. The new work goes under such rubrics as the sciences of complexity, complexity theory, and self-organizing systems and emphasizes systems with non-linear interactions. Tools for describing such systems were first developed in physics by Poincaré and others in the late 19<sup>th</sup> century, giving rise to Dynamical Systems Theory (DST) in the 20<sup>th</sup> century. DST was initially applied to physical phenomena such as eddies in a stream (Landau, 1944), but also was used to elucidate phenomena in biology (see Kaufmann, 1993) and then psychology. The earliest psychological accounts focused on motor coordination (Kelso, 1995) and its development (Thelen & Smith, 1994), but gradually DST has expanded to other domains. Indeed, some proponents have presented DST as a revolutionary, overarching alternative to other approaches to cognition (Port & van Gelder, 1995; Keijzer, 2001).

One particularly interesting offshoot of complex systems research has been the introduction of a number of important ideas about the structure of networks and how they can be used to characterize phenomena in the world. Most traditional investigations of networks focused either on regular lattices, in which only neighboring units are connected, or on random networks (the focus of pioneering investigations by Erdős and Rényi). Such organization is very different from the "small world" networks first articulated by Stanley Milgram (1967), who discovered empirically that while individual humans are primarily connected to those around them (as in regular lattices; this feature is known as *high clustering*), they are indirectly connected to a vast number of others via relatively short paths of direct connections through people who know each other (as in random networks; this is known as *short path length* and provided the premise for the play and movie *Six Degrees of Separation*). Duncan Watts and Steven Strogatz (1998) showed that minimal changes to a regular lattice can transform it into a small-world network and explored real-world phenomena exhibiting this form of organization—including collaborations between actors in feature films, the electrical power grid of the western U.S., and the neural network in a nematode. Moreover, Albert-László Barabási and Réka Albert (1999) discovered that many networks in the real world are *scale free*, in that connections exhibit a power-law distribution (the majority of units are connected to only a few others, but a few are connected to a *very* large number of others). (The term *scale free* is used to reflect the fact that power-law distributions lack any intrinsic scale. Barabási and his collaborators have attempted to account

for the occurrence of scale-free networks as a result of historically earlier nodes having a longer time to attract attachments and to new nodes preferentially attaching to already highly connected nodes (Albert & Barabási, 2002). More recently Cees van Leeuwen and his collaborators have shown how scale-free small-world networks can evolve through coupling of chaotic oscillators (Gong & van Leeuwen, 2003). These developments potentially provide a powerful set of tools for analyzing organization in a wide variety of natural and social systems.

## 2.0. Field Guide to Modern Concepts of Reduction and Unity

In the 20<sup>th</sup> century, claims about unity of science were commonly tied to claims about theory reduction. In particular, the strategy was to reduce the theories of higher-level sciences such as biology to the laws and theories of lower-level sciences such as physics and chemistry. (Spelling out the notion of levels is challenging and we will return to this issue at several junctures below.) Claims about reduction were, in turn, treated as claims about deductive relations between theories. Recently, strong dissent has been raised on both scores, with some philosophers rejecting both reduction (see below, 2.3) and unity of science (see below, 4.0). Other philosophers, more sanguine about unity, have advanced alternative conceptions that emphasize integration more than unity and detach these issues from questions of theory reduction. In addition, accounts of reduction that do not tie it to deductive relations between theories have been advanced. Although the more recent alternative treatments of both integration and reduction offer much promise for providing more adequate accounts of both of these notions, we will start by laying out the traditional accounts of both positions.

### 2.1. *The Theory-Reduction Model*

The logical positivists advanced the theory-reduction model as part of their effort to provide an account of science that avoided entanglement with metaphysical issues. To accomplish this they focused on the knowledge claims of science and emphasized the role of logical relations between these. A crucial move was to represent two kinds of knowledge claims in the same format, yielding sets of propositions encompassing both observation statements (reports of empirical observations such as “The marble is rolling down the incline”) and theoretical statements like Newton's law of universal gravitation, which says that the attractive force between any two bodies is equal to the product of their masses divided by the square of the distance between them. Nagel identified an intermediate category of *experimental laws*, which provide an empirical summary of the phenomena observed. Galileo’s law that the distance a falling object travels is proportional to the square of the time it is in motion is an example. These experimental laws are contrasted with *theoretical laws*, such as Newton’s, which go beyond the observed phenomena by positing theoretical entities like forces and masses to account for the experimental laws. The power of laws or theories to explain observations could then be rooted in the ability to derive new observation statements—predictions—from laws. This is the well-known deductive-nomological (D-N) or covering-law model of explanation (Hempel & Oppenheim, 1948; Hempel, 1965). To account for the relations between the laws or theories of different sciences, the logical empiricists proposed simply generalizing this account, and argued that it should be possible to derive the laws or theories of one discipline or science from those of another

(Oppenheim & Putnam, 1958; Nagel, 1961; see also Woodger, 1952; Quine, 1964; Kuipers, 2001, chapter 3).<sup>4</sup>

Two fundamental challenges arose in developing this generalization of the D-N model. First, the laws in the different sciences are typically presented in different vocabularies.<sup>5</sup> Laws in physics, for example, might employ terms such as *mass* and *attractive force*, whereas those in chemistry would invoke names of elements and molecules and types of chemical bonds. But logical inferences are only possible between statements using the same vocabulary, in much the same way as certain algebraic problems can be solved only when the units of time, length, or weight are expressed using the same measure. To address this issue, advocates of the theory reduction model appealed to *bridge principles* (Nagel called them *rules of correspondence*) that equated vocabulary in the two laws. Sklar emphasized that these correspondence claims are really identity claims: “Light waves are not correlated with electromagnetic waves, for they *are* electromagnetic waves” (Sklar, 1967, p. 120). Applied to the context of relating psychology to neuroscience, this contention that the terms in the different theories picked out the same entity became the foundation of the celebrated mind-brain identity theory (Place, 1956; Feigl, 1958/1967; Smart, 1959). Although such bridge principles might seem unproblematic, we will see that they are the target of one of the more powerful objections to unification through reduction.

The second challenge confronting advocates of the theory reduction model is the fact that the regularities captured in higher-level laws arise only under a particular range of conditions. To accommodate this, they proposed that reduction also required statements of boundary conditions. With these components in place, a reduction was then conceived to have the form of the following deduction:

Lower-level laws (in the basic, reducing science)	
Bridge principles	
Boundary conditions	
∴ Higher-level laws (in the secondary, reduced science)	

An oft-cited example is the derivation of the Boyle-Charles’ law from the kinetic theory of gases, as part of an overall reduction of classical thermodynamics to the newer and more basic science of statistical mechanics (Nagel, 1961, pp.338-366). This law states that the temperature ( $T$ ) of an ideal gas in a container is proportional to the pressure ( $P$ ) of the gas and volume ( $V$ ) of the container. Because the term *temperature* does not appear in statistical mechanics, to achieve the

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<sup>4</sup> Kemeny and Oppenheim (1956) advanced an alternative account of reduction that did not derive the reduced theory from the reducing theory but only required generating identical observable predictions from the reducing theory as the reduced theory. This account of reduction is far more liberal, since it allows for the reduction of what are regarded as false theories (e.g., phlogiston chemistry) from what are taken to be true theories (e.g., Lavoisier’s oxygen-based chemistry) as long as the predictions made by the reducing theory include all those made by the reduced theory. Yet another alternative was put forward by Patrick Suppes, who required an isomorphism between any model (in the model-theoretic sense) of one theory and a model of the reduced theory: “To show in a sharp sense that thermodynamics may be reduced to statistical mechanics, we would need to axiomatize both disciplines by defining appropriate set theoretical predicates, and then show that given any model  $T$  of thermodynamics we may find a model of statistical mechanics on the basis of which we may construct a model isomorphic to  $T$ ” (Suppes, 1957, p. 271).

<sup>5</sup> Nagel did consider cases in which the same vocabulary was employed in the reducing and reduced theories. He referred to such reductions as *homogeneous*.

reduction a linkage to a term in that science is required. This is expressed in a bridge principle (rule of correspondence) stating that the temperature of a gas is proportional to the mean kinetic energy ( $E$ ) of its molecules. A number of boundary conditions also must be specified, such as those limiting the deduction to monotonic gases in a temperature range far from liquefaction. With the appropriate bridge principles and boundary conditions included as premises, the Boyle-Charles' law can be derived from laws of statistical mechanics. Here is a key part of the full derivation:

Laws of statistical mechanics (including the theorem  $PV = 2E/3$ )  
 Bridge principles ( $2E/3 = kT$ )  
Boundary conditions (monotonic gas;  $T$  in specified range)  
 $\therefore$  Boyle-Charles' law ( $PV = kT$ )

Notice that behind the unity-as-reduction conception is a view of the natural world as comprised of levels, often referred to as 'levels of organization'. Given their reluctance to engage ontological issues, the logical empiricists tended to construe levels in terms of the disciplines that investigate them. On this view there are levels of organization in the world that correspond to such disciplines as physics, chemistry, biology, psychology, and sociology. Unification consists of reducing the theories of each higher discipline to theories of a lower discipline. Some philosophers regard this as thereby achieving a reduction between the disciplines themselves. So, for instance, if biological theory were reduced to physical and chemical theory, the science of biology would also thereby be reduced to the sciences of physics and chemistry, and biology would no longer be an autonomous science.

While embracing many features of the logical empiricists' account of reduction, Robert Causey (1977) advanced a more ontologically committed interpretation of levels wherein higher levels resulted from the structuring of lower-level entities. On this view, theories at the lower level primarily describe the operation of parts of the structured wholes, while those at the higher level focus on the behavior of the structured wholes themselves. For a reduction to be possible, the lower-level theory must itself have the resources to describe the structured wholes and their behavior. (Although this is quite problematic, assume for a moment that it is possible.) We then have two descriptions of the higher-level entity, one as a whole unit in the vocabulary of the higher-level science, and one as an entity structured out of lower-level components. For Causey, reduction then requires bridge principles that relate terms in the higher-level theory referring to the wholes to those terms in the lower-level theory that characterize them as composed, structured wholes. Assuming that the lower-level theory has laws that describe the behavior of the structured wholes, one can try to derive the upper-level theory from the lower-level one.

An important feature of the theory-reduction model is that it requires the lower-level theory (or science) to have all the resources required to derive the upper-level theory (when bridge principles and boundary conditions are supplied). Below we will consider whether this is plausible. But noting this feature of the model allows us to consider what many practitioners of higher-level sciences find problematic in philosophical accounts of reduction: successful reduction apparently obviates any need for any laws or theories specific to the higher-level sciences. At least in a hypothetical final picture of science, higher-level sciences would be expendable or redundant: by supplying the appropriate boundary conditions, any higher level regularity could be derived directly from the lower-level theory. In practice, at a given stage in

the development of science, appeals to the higher-level sciences may be required because the reduction base may not yet have been developed. Higher-level sciences may even play a heuristic role in the development of the lower-level sciences; for example, they may reveal regularities (laws) in the behavior of the structured wholes that must be accounted for. In this respect, there may even be a co-evolution of higher- and lower-level sciences (Churchland, 1986). In the end, however, the theories of the lower-level science will be complete, and the only reason for invoking the vocabulary and laws of the higher-level science will be that they provide a convenient shorthand for referring to what, in the lower-level theory, may be unmanageably complex statements.

## 2.2. Revisionist Accounts of Theory Reduction

Among the early challenges to the theory-reduction model, one of the most influential focused on the possibility of establishing the appropriate bridge principles. Paul Feyerabend (1962; 1970), as a result of adopting an account that characterized the meaning of scientific vocabulary in terms of the theory in which they were used, argued that words in different theories, even if they have the same form, are *incommensurable* with one another. In classical thermodynamics, for example, *temperature* can be defined in terms of Carnot cycles and its behavior described by the non-statistical version of the second law of thermodynamics. But in statistical thermodynamics, temperature is characterized in statistical terms. Given the important differences in the surrounding theory and hence the different entailments of the meanings 'temperature', it would seem impossible to construct bridge principles that would adequately relate 'temperature' as used in these two theories. At the same time, Thomas Kuhn (1962/1970) focused on other examples of putative reduction, such as Newtonian to Einsteinian mechanics, and maintained that words like 'mass', were used incommensurably in the two theories. On the basis of such examples, Kuhn challenged the account of progress implicitly assumed by the logical empiricists, in which sciences progress towards better theories through a process of continual extension and refinement. Kuhn argued instead that the history of science is a history of revolutions in which new theories replace, rather than build upon older, incommensurable theories.

One specific context in which Feyerabend maintained that reduction would fail involved attempts to relate psychological theories presented in mentalistic vocabulary to accounts of brain function in neuroscience. Although Feyerabend later came to champion the position that incompatible theories ought both to be maintained (Feyerabend, 1975), in his early writing on mind-brain relations he advanced a position known as *eliminative materialism* (Feyerabend, 1963). The key claim of Feyerabend and subsequent eliminativists (Rorty, 1970; Churchland, 1981; Churchland, 1986) is that instead of reducing the old (folk) psychological theory to the new neuroscientific theory, the old psychological theory is *replaced* by the new theory and *eliminated* from the corpus of science. The old theory accounted for the observed motions of the planets by assuming that they moved on epicycles whose centers themselves orbit around the earth (the epicycles explained the apparent retrograde motion of the planets when viewed from earth). Copernican astronomy, as in Figure 1b, explains the same phenomenon by assuming that the earth and other planets orbit the sun. Since the two astronomical accounts are inconsistent and we assume that the Copernican account is fundamentally correct, eliminativists conclude that Ptolemy's account is wrong and should be discarded and replaced. Such replacement befell not

only historical theories such as Ptolemaic astronomy, the impetus theory of motion, and phlogiston chemistry but also, on this view, awaits folk psychology and other mentalistic accounts.<sup>6</sup>

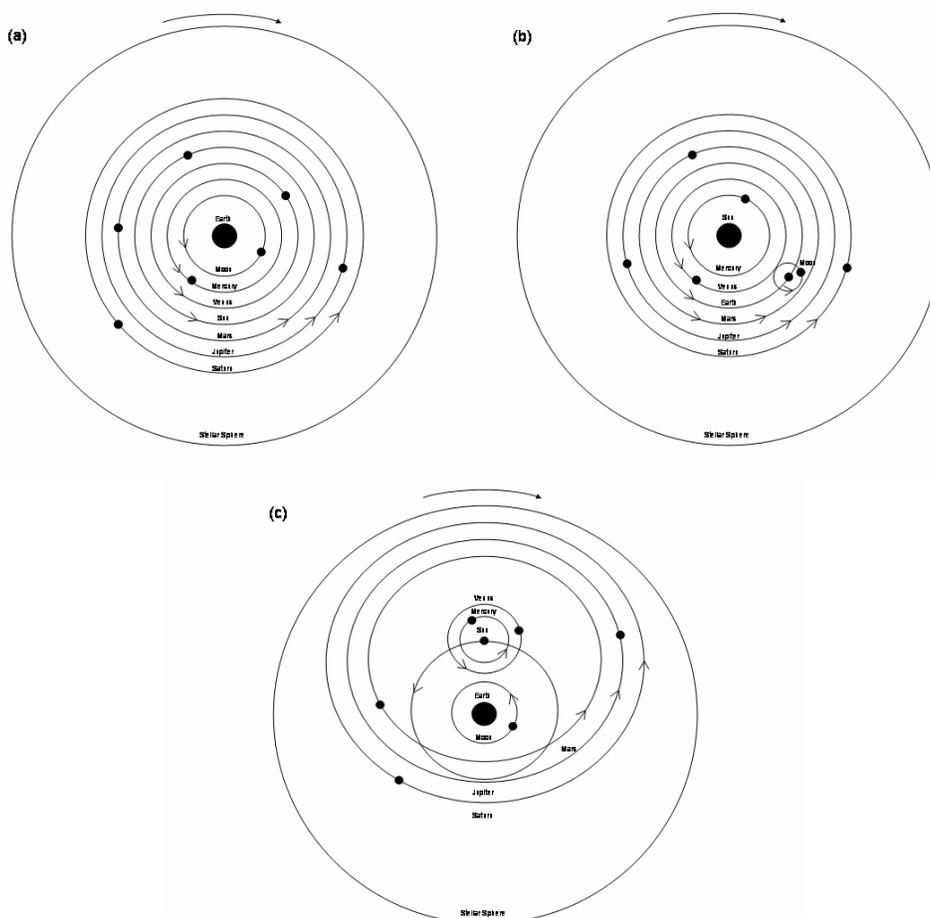


Figure 1: The orbits of the planets according to (a) Ptolemy, (b) Copernicus, and Tycho Brahe. In all cases the planets orbit counter-clockwise while the stellar sphere moves clockwise. In Brahe's account, the earth is at the center of the solar system, as it was for Ptolemy, but the planets other than the moon revolve around the sun, as in Copernicus's account.

Although Feyerabend and Kuhn viewed themselves as opposing reduction, and eliminativists such as the Churchlands held that elimination awaits when reduction fails, other philosophers such as Kenneth Schaffner treated Feyerabend and Kuhn as advancing an alternative account of reduction. On this alternative, even when deduction fails (as it must when the reducing theory is true and the reduced theory is false), one can still relate the old theory to the new one. In the late 16<sup>th</sup> century, for example, Tycho Brahe developed a way to map the Copernican model of the solar system onto the Ptolemaic one (Figure 1c). This showed that all the empirical observations that had supported Ptolemy's model also fit Copernicus's and offered support to it. Thus,

<sup>6</sup> Although most commonly the Churchlands have targeted folk psychology for their eliminativist claims, they also on occasion target contemporary cognitive psychology: "There is a tendency to assume that the capacities at the cognitive level are well defined . . . As we see in the case of memory and learning, however, the categorial definition is far from optimal, and remembering stands to go the way of impetus" (Churchland, 1986, p. 373).

one reason for exploring such relations between an old theory and its replacement is to enable the replacing theory to claim much of the empirical support that had been developed for the old theory.

After construing the discovery of such similarities as a kind of reduction that differs from the traditional model in interesting ways, Schaffner (1967) suggested that these two kinds of reduction need not be regarded as competitors. Instead, he proposed a comprehensive account in which reduction by deduction and reduction by replacement each play a role. In particular, a frequent consequence of a new lower-level theory ( $T_1$ ) is that an old upper-level theory ( $T_2$ ) gives way to a revised one ( $T_2^*$ ).  $T_2^*$  should be deducible from  $T_1$ , just as envisaged in the standard theory-reduction model, but Schaffner thought its relation to  $T_2$  should also be recognized. He suggested that the  $T_2$ - $T_2^*$  relation was one of analogy:

$T_2^*$  corrects  $T_2$  in the sense of providing more accurate experimentally verifiable predictions than  $T_2$  in almost all cases (identical results cannot be ruled out however), and should also indicate why  $T_2$  was incorrect (e.g., crucial variable ignored), and why it worked as well as it did. . . . The relations between  $T_2$  and  $T_2^*$  should be one of strong analogy—that is (in current jargon) they possess a large “positive analogy” (144).

Subsequently Schaffner (1969) amended his model to incorporate revision of an existing lower level theory ( $T_1$ ) to obtain a corrected lower-level theory ( $T_1^*$ ) in addition to the revision of old higher-level  $T_2$  into  $T_2^*$  (see Figure 2).

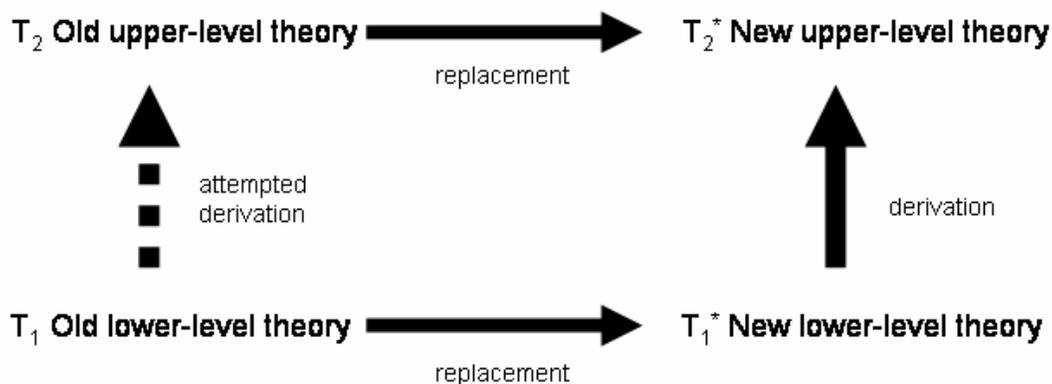


Figure 2. Schaffner's (1969) model of reduction, in which a new upper-level theory ( $T_2^*$ ) is derived from a new lower-level theory ( $T_1^*$ ) and each new theory replaces an older theory at the same level.

Schaffner provided little guidance as to what counted as a strong analogy. Thomas Nickles (1973) argued that in many instances these analogies could be understood mathematically as limit relations. At specific limit values for variables in the new theory, he argued, the new theory will yield the older theory, nearly enough. Nickles give the example of Einstein's formula for momentum reducing to the Newtonian formula by taking the limit as velocity approaches zero. Such limit relations enable researchers to appreciate why the older theory worked as well as it did—most velocities Newtonian scientists considered were sufficiently small that the actual momentum differed only minutely from that predicted by the Newtonian formula.



In his development of Nickles' position, Wimsatt offered a novel reading of when new theories eliminate older ones. In cases for which there is a close pattern match between the old theory and the new one, the older theory might well continue to be employed because it is simpler or easier to use. But reductions between successively introduced theories are unlikely to be transitive. Rather, "*intralevel reductions should be intransitive— . . . a number of intralevel reductions could 'add up' to an intralevel replacement. . . . Relativistic [Einsteinian] mechanics may reduce to classical mechanics (etc.) but it clearly replaces (rather than reduces to) Aristotelian physics*" (Wimsatt, 1976a, pp. 217-219).

Wimsatt's distinction between interlevel and intralevel reductions reveals interesting consequences for the eliminativist argument as applied to the relation between neuroscience and psychology. Whereas "eliminative materialism seems . . . to derive its inspiration from intralevel reduction," Wimsatt contended, "the proper model for the mind-body problem is interlevel reduction" (Wimsatt, 1976a, p. 215). This critique was further developed by McCauley (1986; 1996), who showed that historical cases exemplifying the replacement and elimination of an old theory have all involved a revised theory that is at the same level as the old theory. McCauley suggested that the same would be true in the case of a psychological theory: elimination would be expected only when it was superseded by a replacement theory that lay at the same level— i.e., another psychological theory rather than a neural one). As for interlevel reductions, McCauley distinguished cases in which there is a tight fit between upper- and lower-level theories and cases in which there is not. Loose fit may result from the very nature of theorizing at the upper and lower level. In some cases, the finer grain of an account at the lower level may enable it to explain what appear to be deviations at the higher level. But the advantage is not always with the lower level. In other cases,

the upper-level theory lays out regularities about a subset of the phenomena that the lower-level theory encompasses but for which it has neither the resources nor the motivation to highlight. That is the price of the lower-level theory's generality and finer grain (McCauley, 1996, p. 31).

McCauley thus advocates a pluralistic approach that would allow theorists a fair degree of autonomy. Theories at higher and lower levels could be developed independently, with no immediate need to force the levels to relate in a reductionistic manner.

### 2.3. *Criticism of Theory Reduction*

Revisionists presented the difficulty of providing bridge principles as arising principally with cases involving successive theories at the same level (Wimsatt, as we will see below, is an exception), leading them to invoke a different account of intralevel and interlevel relations. Some influential critics, however, see the problem as arising even in the interlevel case and as providing the death-knell for the theory reduction account of interlevel relations. Similar arguments were advanced independently by two such critics, David Hull regarding biology and Jerry Fodor regarding psychology. The strategy in both cases was to maintain that the same term used in the laws of the higher-level theory must be related on different occasions with different terms and fall under different laws at the lower level. As it is sometimes expressed, one type of entity as characterized in the higher-level theory is *realized* by multiple different types of lower level entities on different occasions.

Hull (1972; 1974) focuses on the notion of *gene* as it figures in both Mendelian genetics and molecular genetics. One challenge to providing a reductive account in this case is that genes in Mendelian accounts are characterized in terms of phenotypic traits for which they code (e.g., a pea plant is tall, not short). Genes in molecular genetics are characterized genotypically in terms of their molecular constitution. Any one of a number of distinct molecular mechanisms could produce the same phenotypic trait (this is often referred to as *multiple realizability*, to which we will return below). Although the complicated nature of the phenotype-genotype map makes developing the reduction difficult, it does not necessarily block it. To achieve a reduction, what is needed is “to discover one or more molecular mechanisms which correspond to the various predicate terms of Mendelian genetics, such that the resulting classification of traits into types corresponds fairly well with the classification of these traits according to the principles of Mendelian genetics” (Hull, 1972, p. 497).<sup>7</sup> Hull went on to point out that even this modest goal cannot be reached; instead, scientists have found that “the same molecular mechanism can produce different phenotypic effects.” This is just the reverse of multiple realizability, as it involves multiple different effects produced by the same mechanism. The reason is not a mystery: other conditions vary. Which conditions and combinations of conditions produce different phenotypic effects can be determined empirically by researchers, if desired. However, to bring such detailed findings into molecular genetics, so an adequate reduction of Mendelian genetics could be accomplished, would result in a radical expansion in scope: “We are no longer correlating Mendelian predicate terms with molecular mechanisms but with the entire molecular milieu” (p. 498). One possible conclusion is that reduction fails in the case of Mendelian genetics, but Hull pointed the blame instead at the account of reduction offered by philosophers:

If the logical empiricist analysis of reduction is correct, then Mendelian genetics cannot be reduced to molecular genetics. The long-awaited reduction of a biological theory to physics and chemistry turns out not to be a case of “reduction” after all but an example of replacement. But given our pre-analytic intuitions about reduction, it *is* a case of reduction, a paradigm case (Hull, 1974, p. 44).

If a paradigm case of reduction fails to go through on the theory-reduction model, Hull reasoned, the philosophical framework would seem to have failed. However, some philosophers of biology drew a different conclusion from the difficulties identified by Hull: they treat the failure as pointing to fundamental deficiencies in biology. In particular, Alexander Rosenberg (1994) argued that because natural selection selects for function rather than structure, the relations between Mendelian genetics (phenotypic features characterized functionally) and molecular genetics (genotypes characterized structurally) are so complex that any attempt to construct bridge laws between them will yield disjunctions too long to be useful for creatures of our mental capacity. Focusing just on the multiple realizability of traits, not the reverse relation, Rosenberg observes that given an environmental ‘problem’ to solve, selection can achieve the same phenotypic function by any number of molecular pathways. The phenotypic or functional features ‘tallness’ and ‘roundness’ are, in other words, multiply realizable from the point of view of molecular genetics. Offering bridge laws, then, will amount to making a list of all the possible pathways. This process, Rosenberg argues, leads to intractably long lists rather than a better understanding (which is what a true science would provide) of the molecular underpinnings of

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<sup>7</sup> As Richardson (1979) noted, Nagel actually allowed for such multiple realizations of the same higher-level property as long as it was possible to explain why the different lower-level properties realized the same higher-level one. Differences in context may determine whether a particular lower-level property realizes a higher-level one.

Mendelian genetics or of the operation of natural selection. Since the theories of functional biology are not reducible to molecular foundations, they provide only problematic access to the biological world.

The other critic of theory reduction, Fodor (1974), focused on psychological predicates and argues that they cannot be linked via bridge principles to neuroscientific ones. Invoking an analogy with finance, he noted that money does not correspond to any natural kind of physical stuff. In the right circumstances, pieces of paper, gold, silver, bronze, or even patterns of electrons can each serve as money; hence, money is multiply realizable. The example nicely draws out Fodor's primary point that the factors that determine kinds in behavioral and societal realms, such as finance, are very different from those determining kinds in the physical realm. In particular, Fodor, as well as Hilary Putnam (1978), maintained that psychological kinds should be identified functionally in terms of how they interact in the generation of behavior. For example, hunger will interact with cognitive states, such as beliefs, in generating particular food-seeking behaviors. Given the differences in their nervous systems, a functional state such as hunger will arise as a result of different neural processes in species such as octopi and humans, although in both cases the state will result in food-seeking behavior (this example is due to Putnam). Accordingly, both Fodor and Putnam reject the project of reducing psychology to neuroscience, instead advocating the autonomy of what Fodor refers to as the *special sciences*.<sup>8</sup>

A second response is advocated by Causey and Hooker. They recommend acknowledging multiple realizability and accepting that a different reduction will be needed for different lower-level realizations of a given higher-level law. Far from promoting unity, this response may actually result in greater disunity when phenomena that appear very similar in high-level terms turn out to be reduced to very different lower-level theories. Pylyshyn (1984), for example, argued that folk psychology successfully groups diverse behaviors under the same regularities, enabling us to predict behavior effectively, but that virtue would be lost if one tried to reduce it to the diverse behaviors that realized the regularity. For example, in our folk idiom we make generalizations about people's propensity to answer the phone, yet on different occasions that activity can involve different motor systems (e.g., picking up a handpiece and talking; sending a text message). By treating each instance separately, we lose the generality provided in the folk idiom "answering the phone."

Although many philosophers have assumed that multiple realizability is rampant and undermines the prospects of relating higher-level kinds to those of the more basic sciences, drawing such connections has been a key strategy in biological investigation. While recognizing that the

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<sup>8</sup> Fodor also maintains that in developing their taxonomies and relating states, special sciences will commonly appeal to very different principles than those that are typical in more basic sciences. For example, in seeking a psychological account of human decision making, we will prefer one that renders people and their decisions as rational; whereas this is not an objective in developing neuroscientific accounts. Charles Taylor (1967, p. 206) made essentially the same argument: ". . . if human behavior exhibits lawlike regularity, on the physiological level, of the sort which enables prediction and control, and a rougher regularity of a less all-embracing kind on the psychological level, it does not follow that we can discover one-one or even one-many correspondences between the terms which figure in the first regularities and those which figure in the second. For we can talk usefully about a given set of phenomena in concepts of different ranges, belonging to different modes of classification, between which there may be no exact correspondence, without denying that one range yields laws which are far richer in explanatory force than the others."

mechanisms underlying physiological and psychological processes in different species do differ, investigators nonetheless draw extensively on what they have learned in one taxon to understand others. For example, much of what is now known about mechanisms of visual processing in humans was secured through research on other mammalian species, especially the cat and monkey (Bechtel, 2001). Although neuroscientists fully realize that there are differences between brains of different organisms, especially of organisms from different taxa, they also expect and have found extensive commonality. This should not be surprising—it has long been known that biological mechanisms at all levels are often highly conserved, attributed in part to the high cost in fitness for large changes. Also—and this point has not received sufficient attention in the philosophical literature—biologists generalize from mechanisms, processes, and features identified in one taxon, to others by means of what might be called *phylogenetic reasoning*. Where such mechanisms, processes, and features can be shown to be carried through lineages, investigators expect fundamental similarities (Hennig, 1966). Accordingly, the underlying mechanisms are not likely to be as radically different as advocates of multiple realizability assume. Researchers also expect differences between taxa and seek these out, but these will often be variations on a common structure: in the language of cladistics systematics, these similarities (and dissimilarities) will be both *shared* because of membership in a common lineage and *derived* due to the differential influences of evolution.<sup>9</sup> Given the conservative nature of evolution, we should not be surprised that human brains retain much of what is found in cat and monkey brains (and indeed, even the brains of invertebrates).

Those who view multiple realizability as an obstacle for reduction often neglect a further factor—just as there are neural differences between organisms and especially between species, there are psychological differences as well. The behavior of a hungry octopus is very different from that of a hungry human. Putnam ignores these differences when he applies the same psychological predicate to both. But these differences often matter as well in developing psychological theory. In both psychology and neuroscience, researchers can select a coarse-grained analysis, lumping together instances that differ in many respects, or a fine-grained analysis, splitting similar instances into different kinds. For different purposes, they may select one or the other. Putative examples of multiple realizability, however, often trade on invoking coarse-grained analyses of psychological kinds and fine-grained analyses of neural kinds. When the same grain is employed in lumping brains in the same category as is employed in lumping mental states into the same category, the alleged problems induced by multiple realizability for reduction seem to vanish (Bechtel & Mundale, 1999).

Leaving behind these worries about multiple realizability, a critical feature of theory reduction accounts, either in their original or revisionist versions, is the assumption that the lower-level theories have sufficient resources from which to derive all the laws of the higher-level science. This assumption is radically implausible. A first objection is that the lower-level theories to which higher-level ones could be successfully reduced would have to be rather different from those currently under development in the lower-level sciences. We can appreciate this by

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<sup>9</sup> There are, of course, examples of convergent evolution in which similar adaptations arise in different lineages (e.g., wings in bats, birds, and pterodactyls). But these are typically readily distinguishable functionally in a variety of ways (e.g., the amount of weight that can be supported or response to turbulence in the case of wings) and so typically do not provide good examples of the *same* function being multiply realized. For further criticisms of the assumption of multiple realizability, see Bickle (2003), Polger (2004), and Shapere (2004)

returning to Causey's version of the theory-reduction model. In his discussion, although not in his formal treatment, Causey suggests that researchers will study the behavior of the components of structured wholes when they are not part of the whole (his *non-bound condition*) and then derive their behavior when part of the structured whole from this information plus specification of the boundary conditions prevailing when they are bound. Yet, in real science, researchers frequently find that what they know about the behavior of entities in their non-bound condition fails to reveal how they will behave in various complex environments. The behavior of atoms as they behave independently reveals little of how they will behave when bound into molecules; likewise, the behavior of amino acid strings reveals little of how they will behave when folded into proteins. Instead, how such entities will behave in bound situations has to be determined empirically. (One indication of this is that when research teams include scientists from both lower-level and higher-level disciplines, the relationship is not one in which the lower-level scientist provide general theories and the higher-level scientist derives the consequences. Rather, all recognize they must discover new information and that what the lower-level scientist often has to offer are techniques that can help reveal how the component parts are behaving in the more complex environment.)

An alternative strategy is simply to incorporate into the lower-level theory everything that is learned about lower-level entities as they are bound into various structured wholes. Clifford Hooker adopts this view:

First, the mathematical development of statistical mechanics has been heavily influenced precisely by the attempt to construct a basis for the corresponding thermodynamical properties and laws. For example, it was the discrepancies between the Boltzmann entropy and thermodynamical entropy that led to the development of the Gibbs entropies, and the attempt to match mean statistical quantities to thermodynamical equilibrium values which led to the development of ergodic theory. Conversely, however, thermodynamics is itself undergoing a process of enrichment through the injection "back" into it of statistical mechanical constructs, e.g., the various entropies can be injected "back" into thermodynamics, the differences among them forming a basis for the solution of the Gibbs paradox (Hooker, 1981, p. 49).

The idea that lower-level theories need to be enriched to account for what is learned at the higher level leads to a view that reduced and reducing theories co-evolve, a view that Patricia Churchland (1986) espouses for the relation between psychology and neuroscience. The difficulty with this approach is that lower-level accounts of the behavior of entities when they are bound in complex structures may share little with accounts of how they behave in isolation. The resulting lower-level theory may be so complex and its various claims sufficiently unrelated to one another that little unity will have been achieved.

Before leaving criticisms of the theory-reduction account, we should note one feature of the account not often discussed—the role played by boundary conditions. It is only under specific boundary conditions that, on this account, higher-level laws can be derived from lower-level ones. But where do these boundary conditions come from? They are not themselves derived from the lower-level laws. Rather, they must be determined empirically as investigators try to develop the reduction. This has significant consequences for the claims that reduction unifies all higher-

level laws in terms of basic-level ones. In fact, the higher-level laws are derived from lower-level theories *plus* bridge principles and boundary conditions. Even if the rest of the theory reduction account proved adequate, it would not promote as much unity between the various sciences as is often suggested.

### 3.0. Kitcher's Revisionist Account of Unification

Pursuing a line of argument first formulated by Michael Friedman (1974), Philip Kitcher has argued for more than two decades that we should be interested in the unity of science because of the tight connection between unification and explanation. Kitcher (1981) defends this view as a means to offering an account of explanation that both builds on the work of some of the logical empiricists (particularly Hempel and Feigl) and overcomes some shortcomings of the covering-law (D-N) model of explanation (and by extension, the theory reduction model). Three of these inadequacies are of chief importance. First, according to Kitcher (1981, p. 508), the covering-law model does not make clear just how it is that scientific explanation advances understanding. Second, the covering-law model does not offer a means to weigh the explanatory power of some theory, or of some theory as against another one. Third, the quality of the covering-law model depends on there being a good way to distinguish between laws and accidental generalizations, but this distinction has been famously problematic since Goodman (1955).

Kitcher's emphasis on unification is meant to be a way to retain the logical empiricists' commitment to explanation as derivation. Kitcher is able to avoid the problems discussed above by arguing that successful explanations are part of a "system" or "store" of explanations, such that no putative explanation can be evaluated individually, but rather must be assessed (at least partly) by reference to the rest of the explanations science accepts at a time.

Science supplies us with explanations whose worth cannot be appreciated by considering them one-by-one but only by seeing how they form part of a systematic picture of the order of nature (Kitcher, 1989, p. 430).

The central move here is to accept, with the logical empiricists, that explanations are derivations, but to deny that such derivations can be assessed in a piecemeal fashion. Rather, they must be part of the best systematization of the set of statements accepted by the scientific community at a given time. "Best systematization" here means, roughly, the set of derivations that minimizes the number of argument patterns while maximizing the number of conclusions. The number of argument patterns can be obtained by giving a classification of argument patterns based on inferential characteristics.

The change from individual derivations to a best system of derivations circumvents the three problems noted above by making no use of the law-accidental generalization dichotomy, by providing a means of assessment for the explanatory power of a candidate explanation (a better explanation is one that leads to more conclusions while adding the least number of argument patterns), and finally, by showing how explanations lead to understanding. The unification approach accomplishes the latter by "showing us how to derive descriptions of many phenomena using the same patterns of derivation...and it teaches us to reduce the number of types of facts we have to accept as ultimate (or brute)" (Kitcher, 1989, p. 432). On this view, unificatory power is a criterion by which new explanations can be evaluated against old ones, and a means to force

explanations to advance our understanding by making them cumulative parts of an over-arching system.

Prompted by critics of unity (see below), Kitcher seems to have softened his view in recent years to one that he calls “modest unificationism” (Kitcher, 1999). The essential scheme—“finding as much unity as we can by discovering perspectives from which we can fit a large number of apparently disparate empirical results into a small number of schemata” (Kitcher, 1999, p. 339)—is the same, but Kitcher now acknowledges that the world may indeed be a messy place and that we may have to “employ concepts that cannot be neatly integrated” into a single best system. Still, Kitcher is not willing to abandon unification entirely, as he thinks that explanatory unification functions well as a “regulative ideal.”

#### **4.0. Critics of Unity**

In the late 1970s and on through the early- and mid-1980s, the idea that science is or can be unified even in Kitcher’s revisionist sense met with powerful criticisms from a group of philosophers centered around Stanford University. In “The Plurality of Science,” Patrick Suppes (1981) offers a short argument to the effect that unity of science theses as conceived by philosophers and scientists down the ages have been poorly supported by theory and practice. The several forms of reductionism upon which these theses rely, Suppes claims, are untenable. What is left is a kind of pluralism of scientific language, practice, and subject matter. These, Suppes argues, are diverging rather than converging, and this is as it should be.

At about the same time as Suppes published his piece on pluralism, Nancy Cartwright was developing her view that the empirical success of our best physical theories argues against, rather than for, the universality of our theories and the unity of science (Cartwright, 1980, 1983, 1999). John Dupré also (Dupré, 1983, 1993) mounted an attack on the unity of science that was motivated by his understanding of biological science, particularly regarding how natural kinds are identified and differentiated.

Cartwright’s opposition to the unity of science works by turning the observations that fund views like the one voiced by Oppenheim and Putnam and Nagel on their heads. Cartwright grants that science can often provide predictions of impressive accuracy and can be used to manipulate certain systems very precisely. She argues that in order to do so, however, the laboratory scientist or mathematical modeler must abstract in crucial ways from the world as we usually encounter it. The charge, at base, is that scientists often describe and model systems that are constituted as much by human engineering as they are by the world. Research systems such as a sealed beaker in a laboratory incubator, or an insulated housing to be sent aloft in a spacecraft, are highly circumscribed and shielded from intrusions. But outside the beaker or box, in the universe at large, the models may very well fail to apply. Cartwright emphasizes that the world is a good deal messier than our theoretical descriptions of carefully and artificially isolated systems in it would lead us to believe.

According to Cartwright, the more restricted relevance of theoretical models suggested by this view should not be cause for concern. We do not usually try to apply models outside their domain of applicability, so this view is not really asking us to give up anything with respect to

our use of models for prediction, manipulation, and control. Our models of the mechanics of falling objects do not offer good counsel on what, exactly, will happen even to fairly solid, relatively heavy, though oddly shaped objects dropped from the Golden Gate Bridge into the water below. It's possible for a person to jump or fall from the bridge and be retrieved just beneath it very much alive, as happened to a real estate agent in 1988. More often, one does not survive the fall, as happened to the same real estate agent in 2003. Neither models of mechanics nor of biology will tell us exactly which outcome will result—even for the very same 'object'—because there is no good model that includes all there relevant forces. In this case, mechanical and biological models apply only partially at best.

What Cartwright does ask us to give up is what she takes to be the unsupported assumption that there *could be* such a model—that mechanics can *in principle* be universalized to be useful in those cases where it is currently of limited applicability. In order for models of (for instance) falling objects to be universalized, it must be the case that all instances of falling are relevantly similar. Whether some real case is enough like the model case, Cartwright argues, will have to be worked out for each new application. On this view it is anything but clear that we can build a model to fit every real or imagined situation. This is not a claim about our cognitive limits—Cartwright is not claiming that we cannot build models of some systems because their dynamics are too complex for us to measure or describe. She is arguing, rather, that we ought to consider in such cases whether what we have is a system that is genuinely and relevantly different than the ones we know how to deal with. Where this is so, we should not expect there to be any one small set of theories or models that will come to include all others. The best we can hope for is a patchwork of theories and models that will sometimes be compatible and sometimes will not.

By contrast to Cartwright's focus on models and their applicability, John Dupré's opposition to unity of science arguments focuses on the concepts used in different disciplines of science and is motivated by his view that essentialism about kinds is indefensible and thus that kind-membership is a much messier affair than we usually allow. He argues that most things objectively belong to more than one kind. Moreover, he thinks that privileging one kind-membership claim over another for the same individual is always unprincipled. Take a chicken (or all chickens), for example. Chickens are noticed by both biological taxonomists and cooks, but are chickens more fundamentally members of the taxonomic class 'Aves', or of the kind 'gustatory objects'? Both kinds, Dupré says, are objective, and there is no principled way to prefer one taxonomy to the other or to take one to be more basic. It will do no good, of course, to retreat to the position that one of these kinds is scientific while the other is not: we have neither a principle of demarcation nor reasons to think that science is more basic than cuisine.

For Dupré, though kind-membership is objective, it is also context relative. Is the thing I now have before me a common and domesticated instance of the taxonomic class 'Aves', or the sort of thing that a lot of people like to eat when it has been sautéed with mushrooms and port wine? One answer to this question that Dupré will endorse is 'yes'. Another is that arriving at a 'correct' or unambiguous division of objects into kinds requires one to specify one's underlying intent or theoretical perspective in carrying out the classification.

The upshot of Dupré's ontology for the unity of science debate is that the kind of hierarchical ordering that some unity theses rely upon is essentialist or idealist by his lights, and is therefore

not to be found in the world. Sometimes one will get nice orderings, but only for a particular purpose, and the very same objects will often belong to some non-hierarchical ordering as well. Dupré points out that the parts of an automobile are hierarchically ordered only so long as we are interested in them *qua* parts of a car. Old pistons with their rings and wrist pins removed very often end up on the desks of autoshop managers and serve as instances of the kind ‘ashtray’ and ‘paperweight’. When they do, they seem not to be part of a hierarchical ordering of parts.

Those unity of science theses that rely on seeing in past and present science some progress toward identifying the most basic kinds—the few microkinds in terms of which many or all macrokinds can or will be described, derived, or explained—will be frustrated if Dupré’s ontology is accepted. On Dupré’s picture of the world, identifying some kind of thing as most basic for some pursuit will not make it the most basic for all pursuits or even for all scientific pursuits. Put simply, Dupré’s anti-unity thesis is that the world itself is radically disordered. We should not, then, expect any science that accurately describes the world to be itself so ordered as to be unified.

### 5.0. Integration Instead of Unity

The underlying idea of both the theory-reduction model and Kitcher’s revisionist account is that science will be unified through deductive relations. But a variety of scientific enterprises involve constructing bridges between theories without either one being reduced to the other. Lindley Darden and Nancy Maull saw the importance of integration without reduction and incorporated this characteristic when they advanced the notion of an *interfield theory*. Foundational to their account is the notion of a *field*, which they characterized in terms of the following elements:

a central problem, a domain consisting of items taken to be facts related to that problem, general explanatory facts 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, p. 144).

By downplaying concepts, laws, and theories while emphasizing expectations, techniques, and methods, Darden and Maull departed significantly from traditional philosophical accounts. Their starting point was a field (this notion was first developed by Dudley Shapere, 1974) and its diverse characteristics, not theories that may or may not be part of what the field has to offer.<sup>10</sup> In examining cases in which two different fields became integrated, they arrived at the further

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<sup>10</sup> Darden and Maull’s notion of a field focused primarily on cognitive features: “a central problem, a domain consisting of items taken to be facts related to that problem, general explanatory facts 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, p. 144). But, as sociologists of science have emphasized, fields are also characterized by social structures—laboratories, departments, funding agencies, journals, and professional societies. There are also various informal networks, such as Derek de Solla Price sought to characterize with the notion of *invisible colleges* (1961; see also Crane, 1972; Chubin, 1982). Recently techniques such as analysis of citation and co-authorship have been used to identify such networks (Wasserman & Faust, 1994). These aspects of fields are shaped in part by social considerations but often play an important role in determining, for example, what problems are taken serious or what methods are accepted for addressing them. As a result, interfield connections involve more than just interfield theories but interfield communities, which often end up transforming the fields from which they originated.

notion of an *interfield theory*, “a different type of theory... which sets out and explains the relations between fields.” They identified several types of interfield relations: (a) structure-function, e.g., physical chemistry targets the structure of molecules while biochemistry describes their function; (b) physical location of a postulated entity or process, e.g., the chromosomes identified in cells by cytologists provide the physical location of the genes postulated by geneticists (a case that also exemplifies structure-function and part-whole relations); (c) physical nature of a postulated entity or process, e.g., biochemistry specifies the physical realization of entities postulated by the operon theory in genetics; (d) cause-effect; e.g., biochemical interactions are a cause of heritable patterns of gene expression.<sup>11</sup>

Such relations between different fields are not always obvious or straightforward to develop, since fields may conceptualize the phenomena they investigate in very different terms. Consider the construction of the interfield theory of vitamins, which successfully integrated research on nutritional requirements with the biochemistry of metabolism. Most B vitamins are either coenzymes or precursors of coenzymes that serve to transport hydrogen or phosphate groups from one macromolecule to another. But prior to the 1930s, neither nutrition researchers nor biochemists could recognize this function. For nutrition researchers, vitamins were a puzzle because they were required in the diet, but only in minute quantities. The working conception of nutrition from the mid-19<sup>th</sup> century was that nutrients were either burned to liberate energy or recruited into the structure of the animal’s body (this was especially true of proteins, but also of fats). The minute quantity of vitamins required in a diet, however, would not provide for generating much energy or building much structure. Moreover, the only known components involved in metabolic reactions were carbohydrates, fats, and proteins and the enzymes that broke them down (catabolized them) into a succession of smaller molecules including pyruvate and succinate. With the rise of biochemical laboratory methods early in the 20<sup>th</sup> century, researchers learned that such reactions could be maintained in extracts of cells in the laboratory, but only if the substances that became known as coenzymes were provided. No one knew why until it was discovered in the 1930s that the energy released in catabolic metabolic reactions was harvested and stored by reversible reactions in active chemical groups of the coenzymes. For example, carrying hydrogen involved a reduction reaction (picking up hydrogen from a donor) followed by oxidation (handing off the hydrogen to a recipient). Since each active chemical group could reduce and oxidize repeatedly, it made sense that a great deal of work could be done under conditions of minimal replenishment. With this reconceptualization of biochemistry, an interfield theory relating nutrition and metabolism could be developed which helped guide further research in each field. For example, vitamin B<sub>2</sub> was a major component of the flavin nucleotide coenzymes and, in particular, contributed the active group that played such an essential role in harvesting energy. (For further discussion of this case see Bechtel, 1984.)

Interfield theories sometimes serve simply to bridge existing disciplines, allowing practitioners in each discipline to utilize techniques developed and knowledge procured in the other. In the most interesting cases, however, constructing a bridge between fields or disciplines results in the construction of a new discipline. For example (see Bechtel, 2006), cell biology emerged after World War II from what had been a *terra incognita* between biochemistry and classical cytology. Its visionary pioneers developed techniques for using new instruments to tackle new

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<sup>11</sup> See Darden (1986) for an extension of this account to the multidisciplinary integration achieved by the synthetic theory of evolution in the 1930s.

problems. For instance, the electron microscope was used to identify cell components at a much smaller scale than previously possible and the ultracentrifuge was used to localize particular biochemical reactions in the newly discovered components. The methodological and theoretical bridges constructed between cytology and biochemistry gave rise to cell biology as a new discipline. Not all cases of successful interfield interaction result in new disciplines, however. If the existing disciplines are well-established and there is no uncharted territory requiring new instruments, interdisciplinary clusters such as cognitive science are more likely to result (Bechtel, 1986).

## 6.0. Reduction via Mechanisms

Although philosophers have generally construed reduction as theory reduction, this notion fits poorly with what is scientists typically call ‘reduction’. As Wimsatt (1976b) put it: “At least in biology, most scientists see their work as explaining types of phenomena by discovering mechanisms, rather than explaining theories by deriving them or reducing them to other theories, and *this* is seen as reduction, or as integrally tied to it.”<sup>12</sup> To appreciate Wimsatt’s claim, it is necessary to understand what is meant by a mechanism and by mechanistic explanation. These notions have been pursued since the late 1980s by an emerging school of philosophers of science focusing on biology rather than physics (Bechtel & Richardson, 1993; Glennan, 1996, 2002; Machamer, Darden, & Craver, 2000). The following provides a basic conception of mechanism:

A mechanism is a structure performing a function in virtue of its components parts, component operations, and their organization. The orchestrated functioning of the mechanism is responsible for one or more phenomena (Bechtel & Abrahamsen, 2005).

A central feature of mechanistic explanations, and the one that makes them reductive, is that they involve decomposing the system responsible for a phenomenon into component parts and component operations. Given that parts and their operations are at a lower level of organization than the mechanism as a whole, mechanistic explanations appeal to a lower level than the phenomenon being explained. For most scientists and non-philosophers, such appeals to lower levels are the hallmark of reduction. As we will see, though, lower-level components of a mechanism do not work in isolation and do not individually account for the phenomenon. Rather, they must be properly organized in order to generate the phenomenon. The most important feature of mechanistic explanation to bear in mind is that it seeks to explain why a mechanism as a whole behaves in a particular fashion under specific conditions. This strategy in no way undermines the reality of the phenomenon being explained; rather, it begins by treating the phenomenon as something that really occurs when the mechanism operates in a particular set of environments.

It is most convenient to introduce the mechanistic perspective on reduction by considering an example. One of the major activities of cells is the manufacture and export of proteins. Beginning around the middle of the 20<sup>th</sup> century, cell biologists together with biochemists and molecular biologists set out to explain how cells carry out this activity. Philosophers examining this case have focused especially on how DNA is transcribed into RNA, which then *codes* for the

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<sup>12</sup> For Wimsatt, the complexity of mappings between lower- and upper-level entities establishes both the failure of translation as required in bridge principles and of reduction as a relation between theories (Wimsatt, 1975, p. 221).

sequence of amino acids that comprise a protein. Even this part of the mechanism is extremely complex. For example, three types of RNA are involved. The sequence information is transcribed (by a complicated set of operations) into the sequence of base pairs comprising messenger RNA (mRNA). But to synthesize proteins, these must be *read* by ribosomes, which are complex structures composed of ribosomal RNA (rRNA) and proteins. They temporarily attach to mRNA strands and move along them. A third kind of RNA, transfer RNA (tRNA) forms bonds with particular free amino acids and transports them to the ribosome. There the ribosome creates peptide bonds between the last added amino acid and this new one before moving down the mRNA and repeating the process. (For an account of the discovery of this mechanism, see Darden & Craver, 2002.) But this is only part of the mechanism. When proteins are synthesized for export from the cell, the ribosomes are attached to the membrane of the endoplasmic reticulum. The emerging strands are pushed across the membrane into the inner space of the endoplasmic reticulum and then transported to the Golgi apparatus. There they are encapsulated in another membrane and transported across a series of sacs (the saccules of the Golgi stack). There carbohydrates are combined with the proteins to create secretory particles, which are then excreted from the cell through the process of exocytosis (Whaley, 1975; Bechtel, 2006).

One important point to note from this example is that the components of the mechanism do different things than what the mechanism as a whole does. Individual lower-level components do not explain the overall performance of the mechanism. Individual enzymes, for example, catalyze particular reactions. They do not perform whole physiological activities such as protein synthesis. Only the mechanism as a whole is capable of generating the phenomenon, and then only under appropriate conditions. Herein lies the explanation for the need for bridge principles in the theory-reduction account—different vocabulary is needed to describe what the parts of a mechanism do than is required to describe what the mechanism as a whole does. The appropriate bridge in this case, however, is not a set of translation rules, but an account of how the operations of the parts of the mechanism are organized so as to yield the behavior of the whole mechanism.

One consequence of taking apart a mechanism that depends on organization to generate the phenomenon is that the investigator destroys the phenomenon itself. A not uncommon situation in science is that after investigators decompose a system they find they cannot readily put it back together again. Sometimes this is because they have neglected some important component. But more frequently it is because they have failed to recognize the specific mode of organization that was involved in the functioning mechanism. The simplest mode of organization is to relate the operations of different parts in a linear series. Understanding more than this simplest mode of organization has presented a serious challenge to humans (Bechtel & Richardson, 1993).

A simple but extremely powerful organizational principle is a negative feedback loop in which the product of an operation feeds back into an earlier operation, allowing for its regulation. (Recall that negative feedback was the central principle advanced by the cyberneticists and general systems theorists in their proposals to unify science.) We are all familiar with this kind of organization from mechanical systems in the home. In the heating system for example, a thermostat monitors the output of an operation (the heating of the air) and, when the desired temperature has been reached, sends back a signal that stops the furnace from generating more heat. As familiar as negative feedback is today, it was a very difficult concept for engineers and

scientists to acquire. It was reinvented numerous times, each in a specific application (for a discussion of the history of re-discovery of negative feedback, see Mayr, 1970). Ancient water clocks, for example, required that the water-supply tank be maintained at a constant level; in approximately 270 BCE, Ktesibios invented a feedback control system for such clocks. Windmills need to be pointed into the wind, and British blacksmith E. Lee developed the fantail as a feedback system to keep the windmill properly oriented. A temperature regulator for furnaces was developed by Cornelis Drebbel around 1624. Finally, James Watts' invention of a governor for his steam engine helped establish the principle as a general one for use in engineering. This was in large part a result of the mathematical analysis of such control systems in terms of differential equations developed by James Clerk Maxwell.

Recognizing negative feedback control in biological systems was equally difficult. Vitalists in the 19<sup>th</sup> century objected to mechanist accounts in physiology on the grounds that they could not conceive how a mechanism could behave in the manner biological organisms were known to behave.<sup>13</sup> In particular, organisms maintain themselves in the face of various assaults of their environment. Claude Bernard (1865) developed a framework for answering such objections by distinguishing between an *inner environment* in which the organs of an organism function and the *outer environment* in which the organism lives. He proposed that each organ in the body was designed to respond to specific changes in the internal environment so as to help maintain the *constancy of the internal environment*. As a result of the actions of the various organs, the inner environment provided a buffer against conditions in the external environment. Bernard, however, was not able to characterize in any detail how the organs each helped to maintain the constancy of the internal environment. Walter Cannon (1929) picked up this thread from Bernard and introduced the term 'homeostasis' (from the Greek words for 'same' and 'state') for the capacity of living systems to maintain a relatively constant internal environment. He also sketched a taxonomy of strategies through which animals are capable of maintaining homeostasis. The simplest involve storing surplus supplies in time of plenty, either by simple accumulation in selected tissues (e.g., water in muscle or skin), or by conversion to a different form (e.g., glucose into glycogen) from which reconversion in time of need is possible. Cannon noted that in most cases such conversions are under neural control. A second means of maintaining homeostasis is through negative feedback—measuring the effects of a continuous process and using that to alter the rate of its performance (e.g., measuring internal temperature and when it is too high or too low increasing or decreased the rate of blood flow by modifying the size of peripheral blood vesicles).

Negative feedback is frequently realized in biological systems as a result of cyclic organization in which the products of several successive chemical operations ultimately combine with some new input to produce an earlier intermediate. The citric acid cycle, first advanced by Krebs and Johnson (1937), provides an illustrative example (see figure 4). The ultimate function of the citric acid cycle is to enable synthesis of ATP, the macromolecule in which energy is stored in animal cells for use in such activities as muscle contraction. Specifically, energy is stored in a high-energy bond created by adding a phosphate group to ADP. A small amount of ATP is generated within the citric acid cycle itself (substrate-level phosphorylation), and a larger amount

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<sup>13</sup> Bichat (1805), provides some of the most compelling arguments of such a type for vitalism. He focused, for example, on the apparent indeterminism in the responses of organisms to external stimuli and the tendency of organisms to behave in ways that resisted external forces that would kill them.

using the energy that is released by oxidative reactions in the cycle and transported, in the form of NADH or FADH, to another mechanism (oxidative phosphorylation). There is no point in performing the oxidations in the citric acid cycle at a rate that exceeds the system's capacity to synthesize ATP from ADP. Hence, when this happens, NADH and FADH build up and there is no NAD or FAD available to support further oxidations in the citric acid cycle. Thus, the rate of the citric acid cycle is regulated by means of negative feedback. The less ADP available, the less NAD and FAD is available, and therefore the less oxaloacetic acid is available to react with acetyl-CoA, the substrate that typically enters the cycle from other metabolic processes.

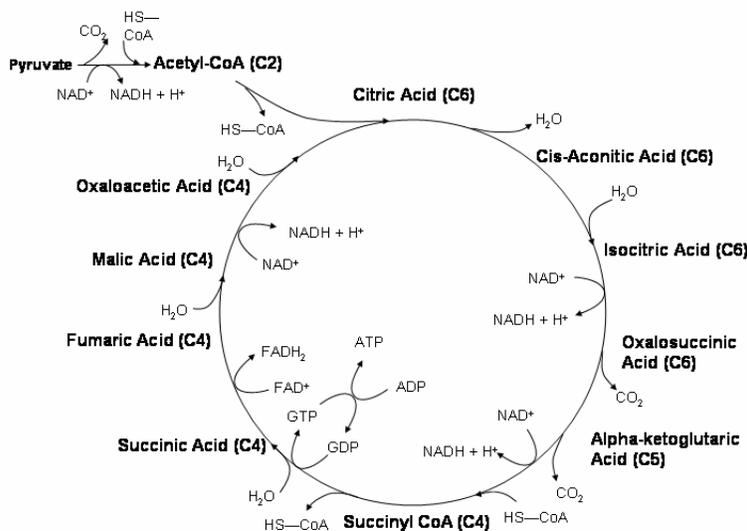


Figure 4. The citric acid cycle, a central biochemical reaction in cell metabolism. The crucial oxidation reactions are shown in the interior. When energy demands are low, there is no ADP available, which in turn means there is no  $\text{NAD}^+$  or  $\text{FAD}^+$  available (all supplied being taken up in NADH or  $\text{FADH}_2$ ). This will result in no accumulation of oxaloacetic acid to react with acetyl-CoA, thereby bringing the reactions in the cycle to a halt. Trough such feedback, critical metabolites are conserved until they are needed to synthesize new ATP from ADP.

Although once the citric acid cycle was discovered its functional significance became apparent, the work leading to its discovery had other motivations. The spur to develop this and other cycles was the realization that the initially conceptualized linear pathway of reactions resulted in a product that, lacking hydrogen, could not be further oxidized directly. Recombination with something else was an expedient to overcome this obstacle. In short order biochemists discovered a number of cycles, such as the citric acid cycle, and began to appreciate cyclic organization as a common design principle in living organisms. But this was a hard-won battle since the focus remained on the overall production of the end product from the input, not the organization in between.

As difficult as it was to understand the significance of negative feedback, the importance of positive feedback was even more difficult to appreciate. At first positive feedback seemed not to be very functional since it appeared to lead to run-away mechanisms. That is, if the product of a mechanism spurred the mechanism to produce yet more of it, the process would continue until all supplies were exhausted. Yet, there are constrained contexts in which positive feedback is

desirable. Particularly important are sets of reactions that function autocatalytically, with one reaction producing a catalyst for a second reaction, and it in turn producing a catalyst for the first reaction (Kaufmann, 1993; Maturana & Varela, 1980). Theorists interested in the origins of life have been the leaders in exploring these ideas (see, for example, the intriguing models of Gánti, 1975, 2003), but they have yet to achieve major uptake in the broader scientific community.

It is easiest to recognize the role of organization in generating higher levels by considering the perspective of an engineer who has been asked to organize existing components in a new way to accomplish some task. When she has finished, she has built something new, perhaps something for which she could secure a patent. We would not expect the patent office to deny her a patent because all of the components were already known to her—they were also known to the others who failed to have the insight needed to develop the new mechanism. Thus, invention of a new organization alone is noteworthy. (In real life, an engineer would more often invent some of the components as well as their organization. However, at some level of decomposition the invented components would themselves be built from existing ones.)

Beyond organization, the environment is often key to understanding how a mechanism works. Mechanisms are not isolated systems, but depend on conditions in their environment. This is particularly the case for biological mechanisms as against physical machines that may be engineered to perform in an identical fashion over a wide range of conditions. With biological mechanisms evolved to operate in a specific range of environments, features of the environment may be co-opted into the mechanism's operation. Evolution is an opportunist, and if something can be relied upon in the mechanism's environment, then it does not have to be generated by the mechanism. Vitamins provide just one well-known example. Because our ancestors could generally count on the availability of vitamins in their foods, there was no evolutionary pressure for us to retain the ability to synthesize them. Nonetheless, insofar as such environmental factors are necessary for the functioning of the mechanism, mechanistic explanations need to focus on the mechanism's context, not just its internal configuration.

With this account of mechanisms and mechanistic explanation in place, we can consider further how they offer a fresh perspective. Unlike theory-reduction accounts, mechanistic reductionism neither denies the importance of context or of higher levels of organization nor appeals exclusively to the components of a mechanism in explaining what the mechanism does. The appeal to components, in fact, serves a very restricted purpose of explaining how, in a given context, the mechanism is able to produce a particular phenomenon. There are other differences as well. Whereas theory reduction is often treated as transitive, with higher-level theories ultimately being reduced to those at the lowest level, mechanistic reductions often proceed for only one or two iterations. Once investigators understand the operations performed by the parts and how the organization orchestrates their operation to produce the phenomenon, they generally have neither the desire nor the tools to pursue a further round of decomposition into subparts and suboperations. Moreover, it is not the case that detailed knowledge of how the component parts or subparts operate will already be available in lower-level disciplines, since, as we discussed above, these parts will be operating in specialized contexts not typically studied by practitioners of the lower-level science. While the study of mechanisms is reductionistic and can promote integration of knowledge from various disciplines, it does not promote a grand unificationist vision.

### 6.1. *Rethinking Levels*

The notion of levels plays a central role in all accounts of reduction, but it has not been fully explicated in any of them. In the early accounts of theory reduction, levels were associated with broad scientific disciplines, so that one sees reference to the physical level, the chemical level, etc. But just why the objects of physics, which range in size from the sub-atomic to the universe, comprise a level is left unspecified. Although still committed to the theory reduction framework, philosophers such as Causey approached levels from a more ontological perspective, emphasizing that lower levels deal with the parts of wholes studied at higher levels. Wimsatt develops this mereological perspective, making part-whole relations fundamental in distinguishing levels:

By level of organization, I will mean here compositional levels—hierarchical divisions of stuff (paradigmatically but not necessarily material stuff) organized by part-whole relations, in which wholes at one level function as parts at the next (and at all higher) levels (Wimsatt, 1976a).

One limitation of compositional relations from Wimsatt's perspective is that they do not permit ordering of entities not part of the same part-whole hierarchy. Accordingly, Wimsatt also appeals to interactions between entities in identifying levels—entities interact principally with others at their own level and with entities at lower levels in terms of the complexes of which they are part. People, for example, interact primarily with other people, animals, plants, computers, furniture, etc., not the cells of other people or the chips of computer. Accordingly, Wimsatt comments: “*Levels of organization can be thought of as local maxima of regularity and predictability in the phase space of alternative modes of organization of matter*” (Wimsatt, 1994).

Wimsatt notes that the neat layering of levels breaks down at higher levels—Individual humans do engage in relations with entities several times larger or smaller than themselves. Accordingly, he introduces the notions of *perspectives* and *causal thickets* for cases in which neat layering into levels breaks down. But the problems go deeper and calls into question the general project of conceiving of the natural world as layered in terms of levels. In biology it is routine for things of very different size-scales to interact. The transfer of energy released in basic metabolism to ATP, for example, is mediated by the transport of protons across the inner mitochondrial membrane, and its diffusion back. Yet the very membrane that is maintaining the proton gradient is also composed in part of protons. Protons are thus part of the very structure through which the protons are being transported. Thinking in terms of the operation of the mechanism, it is correct to say that the protons in the membrane are at lower level than those being transported across it.

Thinking in terms of mechanisms allows one to articulate a more limited but less problematic conception of levels. From the point of view of a given mechanism performing a particular function, the component parts into which a researcher decomposes it constitutes a lower level. If researchers decomposed these parts, they reach yet a lower level. This account allows for the denizens of a level to be of different sizes as long as they are working parts of the same mechanism. Moreover, it is compatible with viewing two structurally identical entities as at different levels if one performs its operations in a sub-mechanism of another—an proton that is being pumped across a membrane is at a higher level than one that is part of the membrane. But

an important feature of this account of levels is that they are limited to the scope of the original mechanism.

One advantage of construing and limiting the notion of levels to levels of organization in mechanisms is that it permits a coherent account of the important idea that lies behind the problematic notion of downward causation (Campbell, 1974). The important idea behind appeals to downward causation is that causal effects of interactions of higher-level entities have consequences for their component parts. Your DNA is a passenger on all your travels and some of your neurons are altered every time you learn something new. The notion of downward causation is problematic, though, since it seems to result in a problem of causal overdetermination—if we assume that there is a comprehensive account of causal interactions of entities at a lower level, then the effect is already determined regardless of any putative top-down effect (Kim, 1998). One solution to this problem is to keep the notion of causation univocal by restricting it to intralevel cases and provide a different, constitutive account of interlevel relations within a mechanism (Craver & Bechtel, submitted). The intuition behind top-down causation can be maintained, but expressed in terms other than causation: the causal interactions of a mechanism with its environment (including other mechanisms) alters the mechanism itself. The changed condition of the parts and operations within the mechanism then propagate causal effects within the mechanism.<sup>14</sup>

A consequence of the mechanistic approach is surrendering the view that a complete causal story can be told at the lower level—all one can account for is changes in the mechanism as the parts operate and interact with each other under the conditions in which the mechanism is operating (some of these being set by the interaction of the environment with its environment). Since it does not have the resources to describe the way in which the mechanism engages its environment, the lower-level account of goings-on inside the mechanism cannot provide a complete account of all that is happening. Our discussion of the problems with global unity theses, though, suggests that the aspirations for a complete theory should be surrendered anyway. What a mechanist requires is only that the causal effects at a given level within a mechanism can be explained—for example, that one can explain how, given the impingements on the brain from the environment, neural changes within it occur. This is precisely what molecular accounts of learning and memory strive to do (Craver & Darden, 2001). The level of neural processes inside the brain is locally constituted—it is not part of a broad level that crosses mechanisms.

## 6.2. *Within Level identities: Heuristic Identity Theory*

In characterizing mechanisms we identified both parts and their operations. The research tools for decomposing mechanisms into their parts and operations are often different. As a result, the decompositions are often developed in different disciplines. For example, cytologists using various microscopes, identified various organelles in the cell, whereas biochemists, preparing homogenates and using various assays, identified chemical reactions. One of the accomplishments of modern cell biology was to establish that different cell functions were

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<sup>14</sup> On this view, so-called *bottom-up* causation works in the same manner—the operation of parts within the mechanism alters the condition of the mechanism itself, thereby altering the manner in which it engages its environment.

performed by specific cell structures, thereby localizing the function (Bechtel, 2006).<sup>15</sup> Since localization claims maintain that it is the same entity that constitutes a particular structure and has performs a specific operation, they are identity claims in the sense advanced by the mind-brain identity theory (Place, 1956; Feigl, 1958/1967; Smart, 1959) noted above. The identity theory is often construed as advancing a reduction of psychology to neuroscience, since neuroscience is at a lower level than psychology. From the point of view of mechanistic explanation, however, we can recognize that accounts of the part of the system and the operation it is performing are at the same level. For example, initial encoding of information to be stored as long-term episodic memories (an operation described by psychology) is an operation of the hippocampus (a structure identified by neuroscientists).

Although not themselves vehicles of reduction, since they are intralevel claims, identity claims play an important role in mechanistic research and ultimately help advance mechanistic reductions. One way to see this is to consider one of the major objections that critics raised to the mind-brain identity claim. They charged that at best empirical investigation could establish a correlation between the psychologically characterized phenomenon and a brain process, an objection that has been pressed anew in recent discussions of consciousness (Chalmers, 1996). Despite the prevalence of the language “neural correlates” in recent presentations of empirical research concerning consciousness (Crick & Koch, 1998), most empirical researchers do not make a distinction between establishing a neural correlate and identifying the neural substrate. It is philosophers who insist in emphasizing that the empirical evidence cannot decide between correlation and causation. One import of making such a distinction is that a dualist can maintain that conscious states are not material phenomena at all, but are simply correlated with brain processes.

When considered in the context of how identity claims typically figure in empirical research, however, the attempt to reconstrue them as correlation claims appears radically misguided. The reason is that they typically are not the conclusions of scientific investigations but heuristics for guiding further scientific discovery (McCauley, 1981). Once an identity claim is made between a structural and a functional characterization of an entity, researchers use each characterization as a guide to elaborating the other. Discovery of an operation that cannot be linked to a part of the structure poses the question of whether that operation is indeed being performed and if so, by what component. Discovery of a component of a structure that does not seem to be performing any operation raises the question of whether it really is a working part and if so, what operation has been missed in extant functional decompositions. Such research invokes the converse of Leibniz’s law of the identity of indiscernables, focusing instead on the indiscernability of identicals: what is learned about a structure or a function under one description must apply to it under the other, or one must revise the identity claim. Correlational claims, by contrast, impose no such burden. To indicate its constructive role in guiding further research, Bechtel and McCauley (Bechtel & McCauley, 1999; McCauley & Bechtel, 2001) speak of *heuristic identity theory*. Once an identity claim has fulfilled its heuristic function of guiding discoveries both on the structural and functional sides, the identity has been woven into the science and investigators who had taken advantage of the heuristic would not be tempted to consider it a mere correlation.

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<sup>15</sup> Linking structural and functional accounts developed in different fields was one of Darden and Maull’s major examples of an interfield theory. In general, interfield theorizing often culminates in accounts of mechanisms.

As noted above, identity claims are not themselves reductive since they relate different accounts of the same entity. They do, however, directly contribute to integration between different accounts of the phenomenon, often ones developed in different disciplines with different research techniques.

## 7.0. Case Studies in Reduction and Unification Across the Disciplines

Although we noted examples from various sciences to illustrate points in the previous sections, the focus was on the conceptual account and its continuity. Looking at actual cases of reduction and unification/integration reveals that they are quite diverse. In this final section we examine four cases that have been important in the discussion of reduction and unity. In each case we ask how the foregoing discussions applies and, in the last cases, identify foci that have not been sufficiently developed in accounts to date and should serve as topics for further philosophical investigation.

### 7.1. *Temperature: Thermodynamics and Statistical Mechanics*

At the end of Section 2.3 above, we pointed to the importance of the role played by boundary conditions and bridge principles in carrying out theory reductions of higher-level laws to lower-level ones. In this first case study we revisit this feature of reductions by a deeper look at the relationship between thermodynamics and statistical mechanics, the standard example of successful theory reduction since Nagel (1961). As we saw in section 2.1, temperature in particular has long been regarded by many in the scientific and philosophical communities as completely explained in terms of the mean kinetic energy of lower-level particles (molecules):  $2E/3 = kT$ . Indeed, we now learn from some standard high school and university textbooks and from renowned physicists that temperature *just is* mean kinetic energy of the molecules that constitute the gas (Feynman, 1963, p. 39).

Several problems with this identity claim have been noted by philosophers and physicists, many of them having to do with boundary conditions. Philosopher Mark Wilson reminds us, for instance, that while the simple equality claim holds in the case of classical gases—the case Nagel emphasized—it is not anything like universal: “in point of fact, this temperature equation is generally false; the proportionality between temperature and kinetic energy is substance specific” (Wilson, 1985, p. 228).<sup>16</sup> As Nagel pointed out in developing his example, the kinetic theory of matter includes both the general postulates of statistical mechanics and more specific postulates appropriate to classical gases—those that are thermodynamically isolated, dilute, and in which the particles influence each other only by perfectly elastic collisions. The kinetic theory, of course, gives excellent predictive results for substances that are relevantly like those described by its postulates. But what about other kinds of substances or even non-dilute gases? Because of the way solids are constituted, for instance, the molecules cannot collide as they do in gases, but can only vibrate. Similar problems arise for other states of matter. It turns out that the observable macrophenomenon we call temperature is multiply realizable at the microlevel.

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<sup>16</sup> As Lord Kelvin pointed out, it is possible, of course, to construct an absolute temperature scale—a scale on which what is being measured is not relative to what is being used to measure it. This is a separate issue from the one we are raising here.

What this means for the quality of the reduction generally is not quite clear—except that there is good reason to think, as Lawrence Sklar puts it, that we “do not expect to ‘deduce’ or ‘derive’ thermodynamics from statistical mechanics in any simple minded way...” (Sklar, 1974, p. 16). In the case of temperature, there will not be just one reduction, but several, as boundary conditions for several states of matter, types of gases and for fluctuating energy situations will have to be specified. Some have argued that this situation causes no real problem for the reduction—we just need to be careful about specifying the boundaries of the reduction.

In addition, as we pointed out above, such specification relies importantly on empirical, rather than deductive, evidence. The descriptions of various states of matter and how they behave has been achieved experimentally, not deduced from the relevant lower-level theory. While statistical mechanics has thrown light on the knowledge gained from experiment, it is not the case that the relevant boundary conditions for temperature can be read off the axioms of statistical mechanics. Neither is it immediately clear how far from the ‘ideal’ boundary conditions a system can be before the lower-level laws cease to offer acceptably good predictions of the behaviour of that system at the higher level. This, too, must be investigated empirically, at least until standards are articulated.<sup>17</sup>

Given all this, even a ‘successful’ reduction in this seemingly simple case will turn out not to be as unificatory as many proponents of the theory-reduction model would have hoped. The reduction will be complicated, disjunctive, and empirically informed, rather than simple, general, and purely deductive. Indeed, the more general and unifying principles are actually those of classical thermodynamics, not the reductive bases.

It is worth noting that mechanistic reduction may provide a superior way to understand this case. The main problems noted above can be side-stepped: mechanistic reduction does not deny the importance of specifying the relevant context, neither does it demand that relations be deductive. Instead of an attempt at reduction that issues in a simple and powerful proportionality that fails to achieve full generality, a mechanistic explanation will be sensitive to boundary conditions in addition to the relations between higher- and lower-level phenomena and entities. This argues against unity, not for it, because we should not expect the physicist who works with concentrated gases to consult the physicist who works with dilute gases when she defines temperature for the systems on which she works. The simpler, better understood case has no obvious claim to epistemic superiority. On the contrary, *each* mechanistic explanation will be relatively substance specific and it is anything but clear that one is the best or more appropriate model for all the others.

The prospects for Darden and Maull-style integration also seem more promising than those for unity by theory reduction. Indeed, a great amount of integration has already taken place. Structure-function and cause-effect accounts on which relations between micro and macroproperties are specified are at the heart of thermal physics. So too are accounts from the perspective of the microlevel of the nature of features and processes at the macrolevel. These

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<sup>17</sup> We have focused on temperature because of its familiarity and centrality in the reductionism literature, but problems with entropy have also been widely discussed as a possible confounder for the reduction of thermodynamics to statistical mechanics. For discussion see Sklar (1993) and Callender (1999).

descriptions and accounts often represent the integration of different fields, of which thermodynamics and statistical mechanics are just one example.

### 7.2. *Genes: Molecular Biology and Developmental Systems Theory*

From what has been the primary exemplar case in philosophical accounts of reduction, we turn to one that we have also alluded to above and is currently capturing both scientific and popular attention in the life sciences. Very near the end of the famous paper in which the outcome of their work on the structure of DNA is announced, Watson and Crick offer the following single-sentence paragraph: “It has not escaped our notice that the specific pairing [of bases] we have postulated immediately suggests a possible copying mechanism for the genetic material” (Watson & Crick, 1953). With this was born a new emphasis on DNA as the ultimate source for knowledge about the macrofeatures of organisms. Biology soon had a new “central dogma”—DNA makes RNA makes protein—and with it an explicitly reductionist (gene-based) approach to accounting for all sorts of biological phenomena, including phenotypes (Dawkins, 1976), the evolution of morality (Ruse & Wilson, 1986), and even human belief in God (Hamer, 2004). This approach quickly led to widespread accounts of macroproperties of organisms or groups of organisms in terms of genes. Some property  $P$  could be explained by or deduced from the presence (or absence) of the gene for  $P$ . Dean Hamer’s recent claims about the gene for belief in god, or “self-transcendence,” are a good example. Hamer argues that whether or not one believes in god is best predicted by whether or not one inherits the VMAT2 gene, the ‘gene for’ belief.

The gene-based approach, however, has important problems. As Oyama, Griffiths, and Gray (2001) have pointed out, privileging DNA’s role in biological processes makes inheritance, evolution, and development, for instance, the mere passing on of DNA. On this view, DNA becomes the only relevant causal factor in these and other biological processes, and the locus of explanation for them. Richard Lewontin has pointed out on several occasions and at some length, however, that the central-dogma view cannot be the whole picture, because DNA can have no such causal efficacy. DNA, he contends, “is not self-reproducing,” “makes nothing,” and does not determine much, if anything, about organisms (Lewontin, 2000). Without the rest of the cellular machinery of proteins and enzymes, DNA produces nothing at all. To extend a well-used metaphor, if DNA *codes for* this or that protein, there must be something that *reads* the code, something that *builds* what the code specifies, and perhaps most importantly, something that *writes* the code for the next iteration. DNA cannot do all this.

Another significant problem with the gene-based approach to accounting for macrofeatures is that being in possession of the full genome sequence does not by itself tell researchers much about the properties of the organism. Far from having a gene-for map that offers one-to-one correspondence of molecules to macrofeatures, what we have learned is that a great many genes have regulatory functions—they ‘switch’ other genes on and off rather than code for the manufacture of particular proteins. It is worth quoting the following passage from Karola Stotz and Adam Bostanci (2005):

*Gene regulation* means that there is always more involved in the production of the product than the coding sequence. In the case of *alternative cis-splicing* of exons and introns, one structure contains several modules that can be alternatively spliced together.

One stretch of DNA may therefore give rise to several proteins. *Overlapping genes* and *alternative reading frames* entail that the “same” DNA sequence can yield different products. *Cotranscription* of adjacent DNA sequences blurs the boundaries between structural “genes”. In the case of *trans-splicing*, one might say that two “genes” (if a gene is defined as a unit of transcription), are involved in coding for a single protein (or more than one products [sic] as in the case of *alternative trans-splicing*). Mechanisms such as *exon scrambling*, *exon repetition*, or *antisense-trans-splicing* further increase the divergence of DNA sequence and protein product. *mRNA editing* exchanges single nucleotides in the linear sequence. Last but not least, *protein splicing* changes the final product once more, but in this case by splicing so-called ‘inteins’ in and out of the final polypeptides of which proteins are composed.

The phenomenon of gene regulation clearly shows that in order to have good explanations of what genes are doing, we need to know what is being regulated and how. These explanations ask for more context than is available at the level of the molecular gene alone, and often come from physical chemistry rather than from genetics. This further suggests that privileging the gene as the locus of explanation is premature in at least some cases. There are also higher levels to consider: How did the genotype-phenotype map get to be the way it is? Why and how is it stable across generations?

Recently, developmental systems theory has emerged as a competitor for gene-based thinking about developmental biology. Proponents of developmental systems theory argue that development cannot be understood outside the framework of its neighbor disciplines and processes, and thus that the causal contexts of heredity and evolution cannot safely be ignored if developmental processes are to be explained. On this view, molecular genetics is just one part of a long and complex story—a story in which genetic goings-on do not make up the only plot.

The developmental systems approach rejects simple reduction of macrofeatures to molecular genetics and urges that there are very often several causal factors in a given developmental process. This viewpoint makes room for the kinds of alternatives to reduction discussed above. Mechanistic reduction, in particular, seems useful for explaining developmental processes in ways that do not neglect epigenetic influences. Mechanistic explanations, by their nature, account for phenomena in context and across levels or organization, rather than privileging a particular level.

This approach is exemplified by recent work on heterochrony—changes in the timing of events or processes during organismal development—as it applies to evolution. Researchers who have investigated differences in organisms that arise as a result of heterochrony have recognized that heterochrony is often not driven by the mere presence of some gene or other. Rather, there may be differences in the timing of gene expression or of the rates of expression. These processes are very often described in mechanistic terms (see, for instance, Wray & Love, 2000; Tautz, 2000; and the review article by Smith, 2003), and researchers have not generally assumed or argued that in those cases where heterochrony can be mechanistically related to particular genes, gene products, or differences in the timing of gene expression, the observed differences can be explained at the molecular level. Even with the molecular part of the story in hand, if we are to apply what we know to evolutionary development, we will still want to know whether and how

heterochrony leads to major evolutionary transitions, how the developmental process is regulated for embryos, and at what level(s) of organization this regulation is orchestrated. It is interesting to note that at present the best-known candidate for a developmental regulator in at least some organisms is the so-called somite clock. It is a kind of feedback mechanism responsible for the timing of segmentation in the vertebrate embryo that is usually described as operating at the cellular, rather than molecular, level (Pourquié, 1998; Dale & Pourquié, 2000).

There is also a strong case to be made that the proponents of developmental systems theory are calling for an explanatory strategy like the one advocated by Darden and Maull. We can see molecular genetics, embryology, cell biology, and other disciplines as fields that all have some relation to development, and the search for a better understanding of developmental systems as an attempt to specify interfield relations for particular developmental processes. There is no reason, however, to assume beforehand that the field concerned with the lowest level of organization is epistemically prior or more basic. Take, as a simple example, the well-known case of inheritance among diploid organisms. Studied from a molecular level, we only learn about gene variation at certain loci. Couple this knowledge, though, with the study of cellular mechanisms and we can begin to see why Mendel's second law holds: the process of meiosis regularly distributes each allele such that the assortment is independent of every other allele. Population genetics tells us still more of the story, informing us as to what the distributions of alleles will be when no outside forces are operating.

Choosing any one of these levels as primary artificially limits the inquiry in ways that may not be heuristically justifiable. At the cellular level, we can ask structure-function questions of the molecular level, as well as cause and effect questions. From the molecular and cellular perspectives we can ask about the physical processes that underlie the regularities captured by population genetics. We can also hope, as developmental systems theorists do, that not limiting ourselves to a single perspective will result in interfield theories that parlay knowledge at these various levels into a more thoroughgoing account of evolutionary development.

It is important to note that in the case of heterochrony and in the case of diploid inheritance, molecular genetics does not provide a sufficient account on its own. Rather, it requires interfield connections with developmental and evolutionary biology or explanations that pay attention to the important connections between the molecular, cellular, phenotypic, and population levels.

### *7.3. Historical Archaeology: Physical and Social Sciences*

So far we have focused on the explanatory gain that results from integration of fields—interfield theories and accounts of mechanism enable investigators to answer a multitude of questions that they could not otherwise address. But there is an additional virtue, one that has been clearly brought out by Alison Wylie (1999) in her account of historical archaeology. Drawing upon the insights of Ian Hacking (1983) on how scientists triangulate independent research techniques to secure reliable evidence even when they cannot directly establish the reliability of any one technique, Wylie shows how historical archaeologists are affecting such triangulation. The approaches of traditional history, which relies primarily on the analysis of documents, and archaeology, which has relied on the analysis of material remains of societies, are radically different. In many cases there is no potential for integrating them. Prehistoric civilizations have

left no written documents and they have been the province of archaeologists. The material remains of more recent societies are often destroyed and historians have relied primarily on the analysis of documents to describe their history. But there are a range of early human societies for which both documents and material remains can be recovered. While practitioners of traditional history and traditional archaeology have tended to insist on the primacy of their own tools of investigation, starting after World War II a number of investigators attempted to integrate the two and have adopted the name *historical archaeology* for this integrated investigation.<sup>18</sup> In the U.S., for example, historical archaeologists tended to focus on early European settlement and the effects of these on native American peoples as well as subsequent expansion of the frontier and urbanization of the continent. Its institutional structure did not materialize until the late 1960s. They have attempted to weave together results from analysis of documents and archaeological remains.

As Wylie notes in describing the sometimes tempestuous relations between historical archaeologists and their home disciplines,

A recurrent theme [sounded by advocates of historical archaeology] . . . is an insistence that when events and conditions of life or historic periods are at issue, vastly more can be achieved by making conjoint use of the evidential, methodological, and theoretical resources of archaeology and documentary history than can be achieved by either field working in isolation from the other (Wylie, 1999, p. 305).

What is significant is that the attempts to integrate sources often forced revisions in the accounts compiled from one source alone. By drawing upon archaeological methods to study the artifacts of a society, one is not just a filling in the historical record but procuring “substantially different, potentially transformative insights about the recent past” (p. 305). This stems from the fact that archaeology can provide evidence of people who do not show up in documentary records, illustrating the ways they lived their lives, which then provides a different perspective on the documents left by the cultures in question.

Wylie’s particular interest in historical archaeology is its potential to provide an illuminating example of how integrating the modes of investigation from multiple disciplines can both provide epistemic warrant beyond what each alone can produce and serve as a heuristic to encourage new inquiry. The key idea behind increased epistemic warrant is Whewell’s (1840) notion of consilience of induction according to which results secured through independent lines of inquiry are more likely to be true than those relying on just one line of investigation. Wylie notes, however, that one cannot just assume that because evidence is advanced in two different disciplines that it represents independent evidence and emphasizes the need to tease apart difference in causal processes, independence of background knowledge and theories invoked, and disciplinary independence. These must be evaluated case by case. But she argues that historical archaeology does offer cases of such independent convergence of evidence and offers the convergence in dating by reliance on tree ring counts, radio-carbon decay, magnetic orientation, and evolution of stylistic traditions in documents:

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<sup>18</sup> The Society for Historical Archaeology was established in 1967 and began publishing the journal *Historical Archaeology* that year (see Schuyler, 1978, for a discussion of these events in the U.S. and related developments in other countries during the same period).

The disciplines that supply the relevant technologies of detection are certainly institutionally autonomous, and the content of their theories is substantially independent; it is unlikely that the assumptions that might produce error in the reconstruction of a date using principles from physics will be the same as those that might bias a date based on background knowledge from botany or socio-cultural studies of stylistic change. Finally, this independence in the content of the auxiliaries and in their disciplinary origins is especially compelling because it is assumed to reflect a genuine causal independence between the chemical, biological, and social processes that generated and transmitted the distinct kinds of material trace exploited by different dating techniques (p. 310).

Securing different forms of evidence that can be used to evaluate and revise claims made by any one form of evidence is clearly an important aspect of integrating sciences that applies broadly. In entering the *terra incognita* (de Duve, 1984, p. 11) that then existed between classical cytology and biochemistry, pioneers in cell biology drew upon two new tools recently developed in physics and chemistry—the electron microscope and the ultracentrifuge. Each presented its own risk of artifact but their combined use, including the use of one to calibrate results from the other, provided investigators with the opportunity to develop an integrated structural and functional account of many basic cell mechanisms (Bechtel, 2006). Integration thus can serve both an explanatory and an evidential role.

#### 7.4. Language: Linguistics and Psycholinguistics

So far our examples have stemmed predominately from the physical and biological sciences, but we end with one that bridges into traditional areas of the humanities. This case also provides us a glimpse into the dynamics of integrating research efforts across disciplines. Many disciplines in the humanities, social sciences, and engineering focus their attention on products produced, intentionally or unintentionally, by human beings. Literary, artistic, philosophical, and technical products typically are constructed intentionally by their authors. Languages and other symbol systems are typically not constructed intentionally, but are nonetheless the products of human activity. How do the disciplines that study these products relate to other disciplines in the physical, biological, and behavioral sciences? We will follow the analysis of Abrahamsen (1987) to discuss one such case: the relationship between linguistics (concerned with the formal structure of human languages) and psychology, especially cognitive psychology (concerned with the mental processes that enable cognitive systems, including humans, to perform their activities). Note that these are different enterprises and typically try to account for different phenomena using different theoretical constructs and appealing to different sources of evidence. Linguists are principally concerned with the structure of language, advance grammars to account for such structure, and test their grammars by their capacity to generate all and only the sentences of a particular language. Psychologists, on the other hand, attempt to explain the mental processes that enable individual language users to comprehend or produce sentences of their language.

Abrahamsen (1987) identifies three patterns in the relationship between linguistics and psychology in the 20<sup>th</sup> century: (1) boundary maintaining, in which the two disciplines pursued their inquiries independently, (2) boundary breaking, in which one discipline tried to usurp the territory of the other, and (3) boundary bridging, in which practitioners of the disciplines

collaborated rather than competing for the same territory. Boundary-breaking episodes often attract the greatest attention. At the turn of the 20<sup>th</sup> century, psychology was a new and rapidly advancing discipline that attracted a number of young linguists seeking to move beyond the older traditions in their own discipline. What they encountered in psychology, however, was not a single view they could take back to linguistics but competing conceptual frameworks—notably the mechanistic cognitive framework of Johann Herbart and the antimechanistic idealist perspective of Wilhelm Wundt. Wundt (1900) himself addressed a host of issues in both linguistics proper (grammatical structure, phonological systems) and psycholinguistics (language acquisition, speech errors) whereas Herbart influenced linguistics through the applications of his work by the linguist Hermann Paul (1880). As Blumenthal (1987) describes, these two approaches conflicted—Herbart's approach proceeded bottom-up from sentence elements invoking associations techniques whereas Wundt's started with unified, often creative, mental representations and proceeded top-down. The conflict within psychology, according to Blumenthal, soon left linguists disillusioned and many opted to divorce linguistics from psychology (McCauley, 1987).

The second round of boundary breaking interactions followed Chomsky's introduction of transformational grammar (Chomsky, 1957). Chomsky viewed his approach to grammar not only as a revolution against structuralism in linguistics proper but also as a revolution against behaviorism in psychology (Chomsky, 1959). Many psychologists, themselves striving to break free of the behaviorist tradition, eagerly followed Chomsky's lead. Notably, Miller (1962) sought to provide evidence for the psychological reality of transformations. This time it was psychologists who were to be disillusioned, as Chomsky repeatedly revised his grammars regardless of the evidence psychologists offered for their psychological reality (Reber, 1987; see also McCauley, 1987). Chomsky continued to break boundaries by characterizing many of his ideas as contributions to psychology, including his nativism, competence-performance distinction, and construal of linguistic grammars as accounts of human linguistic competence (Chomsky, 1965, 1966, 1986; see discussion in Abrahamsen, 1987).

Abrahamsen contrasts such instances of boundary breaking relations with ongoing boundary-bridging interaction between linguistics and psychology. She proposes that a boundary-bridging relation often holds between psycholinguistics, as a subdiscipline of psychology, and linguistics. In this boundary bridging research, psycholinguists rely on linguists to provide specialized descriptions of, for example, phonemes, distinctive features, and phonological rules, while psycholinguists provide linguists with explanations (e.g., of universal characteristics of phonological systems) and evidence (e.g., for the psychological reality of certain linguistic accounts).<sup>19</sup> Abrahamsen observes, however, that the psycholinguist must often reformat the account provided by the linguist in order to make use of it. Some linguistic theories (e.g., augmented transition network grammars; lexical-functional grammars) require less adjustment than others (e.g., Chomsky's Standard Theory). Abrahamsen comments:

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<sup>19</sup> Abrahamsen generalizes this framework to many interdisciplinary relations. Subdisciplines of the physical sciences obtain specialized descriptions from the biological sciences, while biological sciences in turn appeal to these subdisciplines for explanation and evidence. The same, she proposes, is true of subdisciplines of the biological sciences with respect to the behavioral sciences, and of the subdisciplines of the behavioral sciences with respect to the cultural product disciplines (mathematics and engineering, humanities, and social sciences).

The psychological studies benefit from ongoing involvement of linguists who are willing to consider psychological goals in addition to their own native goals as linguists. When these linguists carry out their work of linguistic description, they must satisfy two sets of constraints simultaneously, producing descriptions that can be easily applied in behavioral research as well as satisfy criteria of linguistic adequacy (p. 373).

While boundary breaking research as characterized by Abrahamsen would promote a unificationist conception of science, boundary-bridging research has far more limited aspirations. In some cases a cultural product discipline such as linguistics may simply provide a description of the phenomena for which psychologists then offer a mechanistic explanation. In other cases the understanding of the mechanism may explain certain linguistic phenomenon (e.g., multiply center embedded sentences such as *the dog the cat the mouse squeaked ate chased* are uncommon because they exceed the working memory capacity of humans). The results are interfield theories, not theory reductions.

## 8.0. Conclusions

Visions of unifying all the sciences have been popular ever since the work of the ancient Greek philosophers. Such aspirations were prevalent in many of the historical proposals for unity with which we began this chapter. But the quest for unity can take many forms, often achieving integration rather than true unification. Perhaps the strongest vision of unity appeared in the theory-reduction model of the logical empiricists. This model was attractive because it suggested that logic might provide a powerful way to unite the results of all scientific inquiries by showing higher-level theories to be derivable from lower-level ones. Not only were serious objections raised against this model, but as we have seen, much of the unity that appears to result is illusory. Even in the exemplar case of temperature, the bridge principles and boundary conditions have to be established empirically for each type of material in which heat is realized. For many years worries about multiple realizability provided the principal objections to the applicability of the theory-reduction account. A more troubling concern is that any lower-level theory that will provide a foundation from which to derive all higher-level theories will look very unlike contemporary lower-level theories, since it will have to incorporate all knowledge acquired at the higher levels. Altogether, the various objections to the theory-reduction have succeeded in moving it off center-stage in discussions about unity of science.

The problems confronting the theory-reduction model have led some philosophers to abandon the ideal of unity altogether. Cartwright emphasizes the plurality of models that investigators need to deal with the actual world, while Dupré focuses on the need for multiple different ways of categorizing phenomena, each of which is useful for different purposes. Kitcher remains a strong defender of the objective of theoretical unity, but even he has reduced it to the status of a regulative ideal. Still other philosophers, as we have shown, have adopted a reversionary perspective of advocating integration rather than advocating unity. This was the point of Darden and Maull's notion of an interfield theory—it integrates by bridging fields rather than establishing one complete unified theory. It is also exemplified in the notion of reduction which we have identified in the new mechanistic accounts of scientific explanation.

On mechanistic accounts, explanation consists in demonstrating how the orchestrated operation of the components of a mechanism enable the whole mechanism to perform a function in its environment. The conditions imposed on the mechanism from its environment remain a critical part of the explanation, so the higher-level account remains an autonomous component of any explanation. Further, there is no promise that the knowledge of how components behave in a mechanism will be unified with knowledge about how those components behave in other conditions. Lastly, organization turns out to be crucial in getting mechanisms to perform their function, and despite some key theoretical advances in understanding how negative and positive feedback systems enable dynamically organized mechanisms to maintain themselves, this inquiry is still in an early stage. Nonetheless, as the developments in the life sciences in the 20<sup>th</sup> century illustrate, there is great explanatory gain to developing models of mechanisms that integrate knowledge over several levels of organization. In discussing the more restrictive type of reduction that is achieved through understanding a mechanism, we also noted the need to rethink levels from the rather global perspective embraced in the theory-reduction account to a far more restricted sense in which the constituents of a given level are only determined as one takes a mechanism apart and establishes its working parts. Further, we noted that not all integration in mechanistic explanations is reductive—sometimes claims linking two characterizations of the same entity (e.g., a functional and a structural account) play an important heuristic role in fostering the development of science.

The kind of knowledge that results when investigators focus on mechanism is illustrated in the developmental systems account of how genetic information is linked to knowledge of biological traits—it is linked via an understanding of genetic regulation that relies on knowledge of the cellular machinery (especially the machinery of protein synthesis) which makes development possible. Our last two brief case studies bring out yet other important aspects of integration: the use of integration to overcome epistemic limitations and advance the epistemic warrant of research techniques and theories in each discipline and the dynamics of the process of interdisciplinary exchange (including boundary breaking as well as boundary bridging endeavors). Although we cannot follow up on these threads here, they point to very promising directions for further philosophical investigations of scientific integration.

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