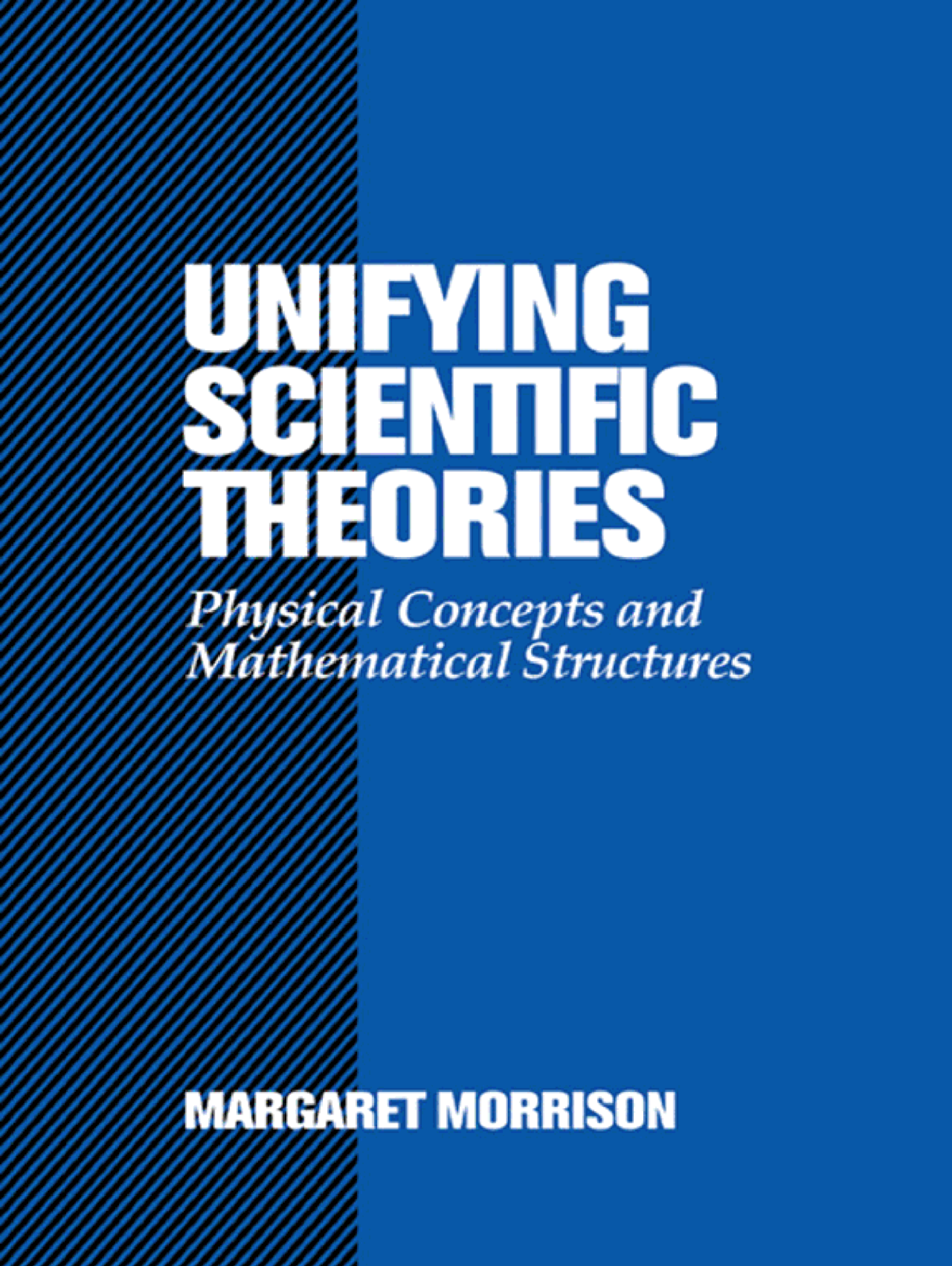

UNIFYING SCIENTIFIC THEORIES

Physical Concepts and Mathematical Structures

MARGARET MORRISON

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Introduction

"Unity" has become a much-maligned word in history and philosophy of science circles, the subject of criticism that is both normative and descriptive. The concepts of "unity of science" and "unity of method" and even the notion of a "unified theory" have been criticized for being either politically undesirable (Dupré 1996) or metaphysically undesirable (Galison and Stump 1996), or else simply nonexistent – the products of a misrepresentation of scientific practice. Critics of unity claim that when we look at scientific practice we see overwhelming evidence for disunity, rather than the coherent structure we have been led to believe characterizes science. Although some of these arguments are extremely persuasive, the desire to banish unity altogether has resulted, I believe, in a distortion of the facts and a misunderstanding of how unity actually functions in science. It is simply a mistake to deny that science has produced unified theories. So where does the evidence for disunity come from? In order to answer this question, we need to look to theory structure as a way of clarifying the nature of that unity. The task then, as I see it, is not so much one of defending a strong version of unity at all costs, but rather of providing an analysis of how it is achieved and how it functions. To that end I have chosen to focus on theory unification as the basis for my discussion of unity. Not only do unified theories provide the foundation for a more general notion of scientific unity, but also there has been a great deal of attention paid to theory unification in the philosophical literature (e.g., Friedman 1983; Glymour 1980; Kitcher 1989).

Although there are undeniable instances of theory unification, to ignore instances of disunity in science would also be to disregard the facts. So instead of trying to counter examples of disunity with ones of unity, I want to show that once we have some understanding of (1) how unity is produced, (2) its implications for a metaphysics of nature and (3) its role in theory construction and confirmation, it will cease to occupy the undesirable role attributed to it by the advocates of disunity. This book addresses, primarily, these three issues, but not by providing a general "theory" of unification, because no such account is, I think, possible. Instead, I shall draw attention to some general features of unified theories, thereby providing the reader with some insight into the complex nature of theory unification and the philosophical consequences that result from a better understanding of the process. One such consequence is the decoupling of unification and explanation. Rather than analysing unification as a special case of explanatory power, as is commonly done in the literature, I claim that they frequently have little to do with each other and in many cases are actually at odds.

If one were asked to list the most successful scientific theories of the modern era, two obvious entries would be Newtonian mechanics and Maxwell's electrodynamics. The feature common to both is that each encompasses phenomena from different domains under the umbrella of a single overarching theory. Theories that do this are typically thought to have "unifying power"; they unify, under a single framework, laws, phenomena or classes of facts originally thought to be theoretically independent of one another. Newton's theory unified celestial and terrestrial mechanics by showing how the motions of both kinds of objects could be described using the law of universal gravitation. Maxwell's theory *initially* brought together electromagnetism and optics by demonstrating that the calculated velocity for electromagnetic waves travelling through a material medium (an aether) was in fact equal to the velocity of light – that light waves and electromagnetic waves were in fact motions of one and the same medium. Later versions of the theory did not rely on the aetherial medium to achieve this result; the value for V was derived from the field equations expressed in the abstract mechanics of Lagrange. In both Newton's theory and Maxwell's theory the unification consists, partly, in showing that two different processes or phenomena can be identified, in some way, with each other – that they belong to the same class or are the same kind of thing. Celestial and terrestrial objects are both subject to the same gravitational-force law, and optical and electromagnetic processes are one and the same.

Because each of these theories unifies such a diverse range of phenomena, they have traditionally been thought to possess a great deal of explanatory power. For example, we can "explain" the nature of radio waves by showing that they are simply a type of electromagnetic radiation; and we can explain the tides by demonstrating that they are a manifestation of gravitational force. In fact, much of the literature in the philosophy of science has analysed unification exclusively in terms of explanation; see especially Friedman (1983) and Kitcher (1981, 1989). A unified theory is simply one that explains several different phenomena using the same laws. And, frequently, what it means for a theory to have explanatory power is analysed in terms of its ability to unify. The best explanation then typically will be the one that unifies the greatest number of phenomena. Accounts of explanation, such as the deductive nomological (D-N) model, that focused primarily on explanation as deduction or derivability could also account for unification within those parameters. Although certain formal and material constraints needed to be fulfilled if the explanation was to count as a unification, the point remained – unification was a special case of explanation.

In the case of the D-N model, we explain a particular phenomenon, termed the explanandum, by showing how it is derivable from a set of laws and initial conditions that, taken together, constitute the explanans. We know that the unification produced by Newton's theory involved a synthesis of Galileo's laws of terrestrial to furnish a field-theoretic description of the phenomena. This is accomplished without any explanation of how electromagnetic waves are propagated through space. Despite its prominence, electric displacement is given no theoretical explanation in Maxwell's mature theory; that is, he offers no physical dynamics explaining the nature of displacement. And to the extent that unification relies on mathematical structures like Lagrangian mechanics, it becomes easy to see how explanatory detail is sacrificed for the kind of generality and abstraction that facilitate unification.

Given this separation of unification and explanation, together with the role of mathematical structures in unifying theories, it becomes necessary to rethink the impact of theory unification for a metaphysical thesis about unity in nature. Again, whether or not the former provides evidence for the latter cannot be answered in a general way. Their connection will depend on the kind of unity a particular theory exemplifies. In the examples, I distinguish two different types of unity: reductive unity, where two phenomena are identified as being of the same kind (electromagnetic and optical processes), and synthetic unity, which involves the integration of two separate processes or phenomena under one theory (the unification of electromagnetism and the weak force). I claim that in the latter case there is no ontological reduction, and consequently the unification offers little in the way of support for claims about a physical unity in nature. Although reductive unity does seem to involve an ontological component, any conclusions we draw about ontology must ultimately depend on the way in which the unity was achieved. In other words, are there good physical reasons for thinking that two processes are one and the same, or have they simply been brought together with the aid of an abstract mathematical structure or model?

Briefly, then, my thesis is twofold: First, unification should not be understood as a form of explanatory power, for the mechanisms that facilitate the unification of phenomena often are not the ones that could enable us to explain those phenomena. Second, although unification is an important part of the scientific process, an analysis of how it takes place reveals that it can in some instances have very few, if any, implications for a reductionist metaphysics and an ontological unity of nature.

I begin with a discussion of the various ways in which "unity" has been conceived throughout the history of science and philosophy. The historical portion of Chapter 1 focuses on Kepler, Kant and Whewell. Although all three agreed on the importance of theoretical unity as a goal to be pursued, they had different metaphysical views about the source of that unity and how it functioned in an explanatory capacity. I have chosen to highlight Kant and Whewell because both figure prominently in philosophical discussions of unity, and both are often invoked in attempts to justify the search for unified theories as *the* methodology for the sciences. Kepler is similarly important because his search for connections between bodies, together with the system of quantifiable forces that he so diligently sought after, represents what has come to be seen as the paradigm of modern mathematical physics. The remainder of the first chapter presents a brief analysis of "unity" as conceived by the founders of the *International Encyclopedia of Unified Science* (Neurath et al. 1971), writers who were responsible for laying the foundations for a twentieth-century philosophy of science. And finally I shall discuss some of the philosophical arguments that have recently been put forward linking explanation, unification and truth. This issue of whether or not unified theories are more likely to be true will be addressed in greater detail in Chapters 2 and 6, where we shall examine Friedman's and Kitcher's accounts of unification. I shall highlight some difficulties with each approach – problems that arise in the application of these philosophical views to particular instances of theoretical unity.

In addition to addressing certain philosophical arguments regarding the nature and status of unification and explanation, we shall also examine several instances of unification encompassing both the physical and biological sciences. These cases will be presented in Chapters 3–5 and 7. What I hope my investigation will reveal are the ways in which theoretical unification takes on different dimensions in different contexts. What this means is that there is no "unified" account of unity – a trait that makes it immune from general analysis. Nevertheless, there are certain features that all unified theories possess, features that enable us to distinguish the process of unifying from that of simply explaining and conjoining hypotheses. Highlighting these will allow us to free theory unification from the kind of metaphysical speculation that fuels the desire for disunity in science. One of the implications of my analysis is that the unity/disunity debate rests on a false dichotomy. Describing science as either unified or disunified prevents us from understanding its rich and complex structure; in fact, it exhibits elements of both. Acknowledging the role played by each will allow for an appreciation of unity and disunity as essential features of both science and nature.

1

The Many Faces of Unity

1.1. Kepler: Unity as Mathematical Metaphysics

In the *Mysterium cosmographicum* Johannes Kepler claimed that it was his intention to show that the celestial "machine" was not a kind of divine living being,

> but a kind of clockwork insofar as the multiplicity of motions depends on a single, quite simple magnetic and corporeal force, just as all the motions of a clock depend upon a simple weight. And I also show that this physical cause can be determined numerically and geometrically. (Kepler 1938, xv:232)

His research began with a specification of certain astronomical hypotheses based on observation; that was followed by a specification of geometrical hypotheses from which the astronomical ones would follow or could be calculated. Those geometrical hypotheses were grounded in the idea that God created the solar system according to a mathematical pattern. Given that assumption, Kepler attempted to correlate the distances of the planets from the sun with the radii of spherical shells that were inscribed within and circumscribed around a nest of solids. The goal was to find agreement between the observed ratios of the radii of the planets and the ratios calculated from the geometry of the nested solids. Although unsuccessful, Kepler remained convinced that there were underlying mathematical harmonies that could explain the discrepancies between his geometrical theory and ratios calculated from observations.

Part of Kepler's unfaltering reliance on mathematical harmonies or hypotheses was based on their direct relationship to physical bodies. He considered a mathematical hypothesis to be physically true when it corresponded directly to physically real bodies. What "corresponding directly" meant was that it described their motions in the simplest way possible. Hence, according to Kepler, physical reality and simplicity implied one another; and it was because nature loves simplicity and unity that such agreement could exist. (Here unity was thought to be simply a manifestation of nature's ultimate simplicity.) Perhaps his most concise statement of the relationship between truth and simplicity or between the mathematical and the physical can be found in the *Apologia*, where Kepler distinguished between "astronomical" and "geometrical" hypotheses:

> If an astronomer says that the path of the moon is an oval, it is an astronomical hypothesis; when he shows by what combination of circular movements such an oval orbit may be brought about, he is using geometrical hypotheses. . . . In sum, there are three things in astronomy: geometrical hypotheses, astronomical hypotheses, and the apparent motions of the stars themselves; and, consequently, the astronomer has two distinct functions, the first, truly astronomical, to set up such astronomical hypotheses as will yield as consequences the apparent motions; second, geometrical, to set up geometrical hypotheses of whatsoever form (for in geometry there may often be many) such that from them the former astronomical hypotheses, that is, the true motions of the planets, uncorrupted by the variability of the appearances, both follow and can be calculated.[1]

One was able to discover the true motions of the planets by determining their linear distances and using simplicity as the guiding principle in interpreting the observations.

Much of his early work in constructing physical theories (before the development of his laws of planetary motion) was dominated by the desire to provide a unified explanation of the causes of planetary motion. The Neoplatonic sun, to which he added a force that pushed the planets along in their orbits, served as the primary model for his solar hypothesis. But the foundation for that hypothesis was the metaphysical principle that one ought to reduce several explanatory devices to a single source. That principle, in turn, was based on Kepler's ideas about the Trinity. The sun served as the principle that unified and illuminated matter in the way that the Trinity symbolized the indivisible, creative God. Kepler then transformed the theological analogy into a mathematical relation in which solar force, like the light in a plane, was assumed to vary inversely with distance. The idea was that there existed one soul at the centre of all the planetary orbits that was responsible for their motions. God the Father created spirit in the same way that the sun dispersed spirit, and the sun emitted a moving force in the ecliptic in accordance with the same mathematical function as light propagating in a plane.

The important relation here, of course, was between mathematical simplicity and unity and the way in which those notions were used to both construct and justify astronomical hypotheses. As mentioned earlier, there was a direct relation between the symmetry of the mathematical relations used to describe physical bodies and the metaphysical underpinnings of those relations found in the Trinity. In his account of the interspacing of solid figures between planetary spheres, Kepler claimed that it ought to follow perfectly the proportionality of geometrical inscriptions and circumscriptions, and "thereby the conditions of the ratio of the inscribed to the circumscribed spheres. For nothing is more reasonable than that the physical inscription ought exactly to represent the geometrical, as a work of art its pattern" (1938, vi:354). And in analogy with the Trinity, he remarked that

> there exists everywhere between point and surface the most absolute equality, the closest unity, the most beautiful harmony, connection, relation, proportion and commensurability.

> And, although Centre, Surface and the Interval are manifestly Three, yet they are One, so that no one of them could be even imagined to be absent without destroying the whole. (Kepler 1938, vi:19)

Here we see an explicit statement of how unity and simplicity could be, in some cases, manifestations of the same thing. The unifying axiom that the planets were united by a single force, rather than a multiplicity of planetary "souls" acting in isolation, was, of course, also the simplest hypothesis. Hence, simplicity and unity were represented as oneness. In other contexts, however, unity and simplicity were related to each other via a kind of interconnectedness, the one as a manifestation of the many. For Kepler, the latter was apparent in the notion of the Trinity, but we can perhaps see it more clearly in the idea of a nation-state that embodies many people and perhaps many cultures, all of which are united in one identity – citizens of that state. It was that combination of unity and simplicity as a form of *interconnectedness* that provided the empirical basis on which Kepler's astronomical hypotheses were justified.

Although Kepler saw the truth of a physical or astronomical hypothesis as metaphysically grounded in its simplicity or unity, the latter also had to be revealed empirically. Not only did the phenomena have to be describable using mathematically simple relations, but the interconnectedness among those descriptions had to be manifest at the empirical level in order for the hypothesis to be justified. Such was the case in Kepler's famous argument for the elliptical orbit of Mars. Indeed, it was his belief that "physical" hypotheses regarding the quantifiable forces exerted by the sun on the motions of the planets could, in fact, be proved or demonstrated. And it was the idea that "one thing is frequently the cause of many effects" that served as the criterion for the truth or probability of a hypothesis, particularly in the *Astronomia nova*. The key to the argument in Kepler's famous "war on Mars" was the geometrical relation that facilitated the combination of two quantifiable influences of the sun on the planet, the first being the planet's orbit around the sun, and the second its libratory approach to and recession from the sun. Once those two were combined, Kepler could *justify* not only the elliptical orbit of Mars but also the fact that its motion was in accordance with the area law. The synthesis consisted in showing (1) that although libratory motion obeyed a law of its own, it was exactly because of the motion of libration that the planet described an elliptical orbit, and (2) that the second law or area law was valid only for an elliptical orbit. Kepler saw his argument as producing an integrated unity founded on mathematical simplicity. Let us look briefly at the physical details to see how they fit together.

Kepler's dynamical account of libration was modelled on magnetic attraction and repulsion. In *Astronomia nova*, planetary motion was explained by the joint action of the sun and the planets themselves, whereas in his later work, the *Epitome*, the entire action was attributed to the sun. The motive radii of the sun's species not

1.2. Kant: Unity as a Heuristic and Logical Principle

Within the Kantian framework it is the faculty of reason that is responsible for synthesizing knowledge of individual objects into systems. An example is Kant's notion of the "order of nature", an entire system of phenomena united under laws that are themselves unified under higher-order laws. This systematic arrangement of knowledge is guided by reason to the extent that the latter directs the search for the ultimate conditions for all experience – conditions that are not, however, to be found within the domain of experience itself. That is, we could never unify all our knowledge, because such a grand unification could never be found in experience. Hence, the quest for unity is one that, by definition, is never fulfilled; it remains simply an ideal or a goal – in Kant's terms, a "problem" for which there is no solution. What reason does, then, is introduce as an ideal or an uncompletable task a set of rational conditions that must be satisfied for all of our knowledge to constitute a unified system. Examples of such conditions are (1) that we act as though nature constitutes a unified whole and (2) that we act as if it is the product of an intelligent designer. Consequently, this ideal *regulates* our search for knowledge and directs us toward a unified end. The fact that we can never achieve this complete unity should not and cannot be an obstacle to our constant striving toward it, for it is only in that striving that we can achieve any scientific knowledge.

To the extent that complete unity is not attainable, reason is said to function in a "hypothetical" way; the conditions referred to earlier take on the role of hypotheses that function as methodological precepts. Consequently, the systematic unity that reason prescribes has a *logical* status designed to secure a measure of coherence in the domain of empirical investigation. Kant specifically remarks that we would have no coherent employment of the understanding – no systematic classifications or scientific knowledge – were it not for this presupposition of systematic unity. But how can something that is in principle unrealizable, that is merely and always hypothetical, function in such a powerful way to determine the structure of empirical knowledge? Part of the answer lies in the fact that the search for unity is an essential logical feature of experience.

The notion of a *logical* principle serves an important function in the Kantian architectonic. Principles of reason are dependent on thought alone. The logical employment of reason involves the attempt to reduce the knowledge obtained through the understanding "to the smallest number of principles (universal conditions) and thereby achieve the highest possible unity" (Kant 1933, A305). Although we are required to bring about this unity in as complete a form as possible, there is nothing about a logical principle that guarantees that nature must subscribe to it. In that sense the logical employment and hypothetical employment of reason describe the same function. The logical aspect refers to the desire for systematic coherence, and the hypothetical component is a reminder that this ultimate unity as it applies to nature always has the status of a hypothesis. The principle that bids us to seek unity is necessary insofar as it is definitive of the role of reason in cognition; without it we would have no intervention on the part of reason and, as a result, no coherent systematization of empirical knowledge. In other words, it is a necessary presupposition for all inquiry. And as a logical principle it specifies an ideal structure for knowledge in the way that first-order logic is thought to provide the structure for natural language.

One of the interesting things about the requirement to seek systematic unity is that it not only encompasses a demand for a unified picture of experience but also involves what Kant classifies as "subjective or logical maxims" – rules that demand that we seek not just homogeneity but also variety and affinity in our scientific investigations and classifications. These maxims are the principles of genera (homogeneity), specification (species) and continuity of form (affinity). Homogeneity requires us to search for unity among different original genera; specification imposes a check on this tendency to unify by requiring us to distinguish certain subspecies; and continuity, the affinity of all concepts, is a combination of the previous two insofar as it demands that we proceed from each species to every other by a gradual increase in diversity. Kant expands on this point in the *Jäsche Logic* (sec. 11), where he discusses the concepts "iron", "metal", "body", "substance" and "thing". In this example we can obtain ever higher genera, because every species can always be considered a genus with respect to a lower concept, in the way iron is a species of the genus metal. We can continue this process until we come to a genus that cannot be considered a species. Kant claims that we must be able to arrive at such a genus because there must be, in the end, a highest concept from which no further abstraction can be made. In contrast, there can be no lowest concept or species in the series, because such a concept would be impossible to determine. Even in the case of concepts applied directly to individuals, there may be differences that we either disregard or fail to notice. Only relative to *use* are there "lowest" concepts; they are determined by convention insofar as one has agreed to limit differentiation.

These logical maxims, which rest entirely on the hypothetical interests of reason, *regulate* scientific activity by dictating *particular* methodological practices. Again, this connection between logic and methodology is a crucial one for Kant. At the core of his view of science as a systematic body of knowledge lies the belief that science must constitute a logical system, a hierarchy of deductively related propositions in ascending order of generality. The act of systematizing the knowledge gained through experience enables us to discover certain logical relations that hold between particular laws of nature. This in turn enables us to unify these laws under more general principles of reason.

This classification process, which includes the unification of dissimilar laws and diversification of various species, exemplifies Kant's *logical* employment of reason. A properly unified system exhibits the characteristics of a logical system displaying coherence as well as deductive relationships among its members. Scientific theories are themselves logical systems that consist of classificatory schemes that unify our knowledge of empirical phenomena. Kant recognizes, however, that reason cannot, simply by means of a logical principle, command us to treat diversity as disguised unity if it does not presuppose that nature is itself unified. Yet he claims that

> the only conclusion which we are justified in drawing from these considerations is that the systematic unity of the manifold of knowledge of understanding, as prescribed by reason, is a *logical* principle. (Kant 1933, A648/B676)

This leaves us in the rather puzzling position of having logical or subjective maxims whose use is contextually determined, while at the same time upholding an overriding principle of unity in nature as prescribed by reason. In other words, Kant seems to sanction the idea of disunity while at the same time requiring that we seek unity. At A649 he discusses the search for fundamental powers that will enable us to unify seemingly diverse substances. Again the idea of such a power is set as a problem; he does not assert that such a power must actually be met with, but only that we must seek it in the interest of reason. As Kant remarks at A650/B678, "this unity of reason is purely hypothetical". Yet in the discussion of logical maxims the principle of unity seems to take on a more prominent role. His example concerns a chemist who reduces all salts to two main genera: acids and alkalies. Dissatisfied with that classification, the chemist attempts to show that even the difference between these two main genera involves merely a variety or diverse manifestations of one and the same fundamental material; and so the chemist seeks a common principle for earths and salts, thereby reducing them to one genus. Kant goes on to point out that it might be supposed that this kind of unification is merely an economical contrivance, a hypothetical attempt that will impart probability to the unifying principle if the endeavour is successful. However, such a "selfish purpose" can very easily be distinguished from the *idea* that requires us to seek unity. In other words, we don't simply postulate unity in nature and then when we find it claim that our hypothesis is true.

> For in conformity with the idea everyone presupposes that this unity of reason accords with nature itself, and that reason – although indeed unable to determine the limits of this unity – does not here beg but command. (Kant 1933, A653/B681)

Put differently, the overall demand of reason to seek unity is the primary goal of all cognition in the attempt to reconstruct nature as a logical system. The mere fact that we engage in cognitive goals implicitly commits us to the search for unity. Within that context there are several different methodological approaches that can be employed for achieving systematic classification of empirical knowledge. Reason presupposes this systematic unity on the ground that we can conjoin certain natural laws under a more general law in the way that we reduce all salts to two main genera. Hence, the logical maxim of parsimony in principles not only is an economical requirement of reason but also is necessary in the sense that it plays a role in defining experience or nature as a systematically organized whole. Hence, what appear to be conflicting research strategies, as outlined by the subjective maxims, are simply different ways that reason can attain its end. For example, the logical principle of genera responsible for postulating identity is balanced by the principle of species, which calls for diversity; the latter may be important in biology, whereas the former is more important for physics. But Kant is no reductionist; the idea of a "unified knowledge" is one that may consist of several different ways of systematizing empirical facts.

The logical maxims are not derived from any empirical considerations, nor are they put forward as merely tentative suggestions. However, when these maxims are confirmed empirically, they yield strong evidence in support of the view that the projected unity postulated by reason is indeed well-grounded. But in contrast to the strategy described earlier, the motivation behind the unifying methodology is not based on utilitarian considerations; it is not employed because we think it will be successful. Nevertheless, when we do employ a particular maxim in view of a desired end and are successful in achieving our goal, be it unity, specification or continuity, we *assume* that nature itself acts in accordance with the maxim we have chosen. On that basis we claim that the principles prescribing parsimony of causes, manifoldness of effects and affinity of the parts of nature accord with both reason and nature itself.

We must keep in mind, however, that although these principles are said to "accord with" nature, what Kant means is that although we must *think* in this way in order to acquire knowledge, there is also some evidence that this way of thinking is correct. The latter, however, can never be known with certainty, because we can never know that nature itself is constituted in this way. From the discussion of the logical employment of reason we know that in order to achieve the systematic unity of knowledge that we call science it is necessary that this unity display the properties of a logical system. In other words, if one agrees with Kant that science is founded on *projected* systematization and that this system is ultimately reducible to logical form (non-contradiction, identity and deductive closure over classification systems), then the principles that best cohere with the demand of systematic unity recommend themselves. Parsimony, manifoldness and affinity are not only methodological principles for organizing nature according to our interests; they are also the most efficient way of realizing the one interest of reason – the systematic unity of all knowledge. Because we empirically verify the extent to which this unity has been achieved, we are thereby supplied with the means to judge the success of the maxims in furthering our ends (Kant 1933, A692/B720), but ends that we, admittedly, never attain. We employ a particular maxim based on what we think will be the most successful approach in achieving systematic unity given the context at hand.

As mentioned earlier, the motivating idea for Kant is the construction of a logical system rather than the realization of a metaphysical ideal regarding the unity of nature. Kant is silent on the question of whether or not this notion of systematization constitutes the basis for scientific explanation. Although it seems clear that classification of phenomena does serve some explanatory function, there is nothing in the Kantian account of unity to suggest that it is in any way coincident with explaining or understanding the nature of phenomena. In essence, the Kantian account of unity constitutes a methodological approach that is grounded in the basic principles of human reason and cognition. The unity has a hypothetical and presuppositional status; it is an *assumption* that the world is a unified whole, rather than a metaphysical principle stating how the world is *actually* structured. In that sense it is simply an idealization that is necessary for scientific inquiry.

Kant's views about the role of ideas in producing unity both in and for science were taken up in the nineteenth century by William Whewell. His views about unity as a logical system were also adopted, albeit in a different form, in the twentieth century by Rudolph Carnap. Unlike Kant, Whewell took a more substantive approach by linking his notion of unity (termed the consilience of inductions) to explanation by way of a set of fundamental ideas: Each member in the set of ideas would ground a particular science. Consequently, Whewell also adopted a much stronger epistemological position by claiming that unified or consilient theories would have the mark of certainty and truth.

1.3. Whewell: Unity as Consilience and Certainty

In the *Novum Organon Renovatum* William Whewell discusses various tests of hypotheses that fall into three distinct but seemingly related categories. The first involves the prediction of untried instances; the second concerns what Whewell refers to as the consilience of inductions; the third features the convergence of a theory toward unity and simplicity. Predictive success is relatively straightforward and encompasses facts of a kind previously observed but predicted to occur in new cases. Consilience, on the other hand, involves the explanation and prediction of facts of a kind different from those that were contemplated in the formation of the hypothesis or law in question. What makes consilience so significant is the finding that classes of facts that were thought to be completely different are revealed as belonging to the same group. This "jumping together" of different facts, as Whewell calls it, is thought to belong to only the best-established theories in the history of science, the prime example being Newton's account of universal gravitation. But Whewell wants to claim more than that for consilience; he specifically states that the instances where this "jumping together" has occurred

> impress us with a conviction that the truth of our hypothesis is certain. . . . No false suppositions could, after being adjusted to one class of phenomena, exactly represent a different class, where the agreement was unforeseen and uncontemplated. That rules springing from remote and unconnected quarters should thus leap to the same point, can only arise from that being the point where truth resides.[6]

Finally, such a consilience contributes to unity insofar as it demonstrates that facts that once appeared to be of different kinds are in fact the same. This in turn results in simpler theories by reducing the number of hypotheses and laws required to account for natural phenomena. Hence, unity is a step in the direction of the goal of ultimate simplicity in which all knowledge within a particular branch of science will follow from one basic principle.

"conception" is introduced that is not contained in the bare facts of observations. The conception is the new fact that has been arrived at through a reinterpretation of the data using the relevant methods. This new element or conception can then be superimposed on existing facts, combining them in a unique way. Such was the case with the ellipse law governing the orbit of Mars. What Whewell describes are methods for data reduction that facilitate the formulation of a conception; but one need not employ all of these methods in order to arrive at a conception. For example, after trying both circular and oval orbits and finding that they did not agree with observations of the observed longitudes of Mars (or the area law), Kepler was led to the ellipse, which, taken together with the area law, gives the best agreement with the available observations. Some methods of data reduction were employed, because the object of the exercise was to find a structure that would fit with the observations. As we saw earlier, there was a convergence of numerical results in establishing the ellipse law, which led Kepler to believe that he had hit on the right formulation. Although we don't have predictions of different *kinds* of data or classes of facts, as in the case of a true consilience, we do have better predictions for not only Mars but also Mercury and the earth. In that sense, then, there is a colligation of facts made possible by the introduction of the conception (i.e., the ellipse) based on the idea of space.

So the induction does not consist in an enumerative process that establishes a general conclusion; rather, the inductive step refers to the suggestion of a general concept that can be applied to particular cases and can thereby unify different phenomena. According to Whewell, this "general conception" is supplied by the mind, rather than the phenomena; in other words, we don't simply "read off" the conception from the data. Rather, it requires a process of conceptualization. The inference that the phenomena instantiate this general conception involves going beyond the particulars of the cases that are immediately present and instead seeing them as exemplifications of some ideal case that provides a standard against which the facts can be measured. Again, the important point is that the standard is constructed by us, rather than being supplied by nature. That the conception presents us with an "idealized" standard is not surprising, because the mathematical methods used to arrive at it embody a great deal of generality – generality that obscures the specific nature of the phenomena by focusing instead on a constructed feature that can be applied across a variety of cases.[8] It is this issue of generality that I want to claim is crucial not only to the unifying process but also to the connection (or lack thereof) between unification and explanation. My focus is not so much the notions of data reduction, as described by Whewell, but more general mathematical techniques used to represent physical theories. The importance of calling attention to Whewell's methods is to emphasize the role of mathematics generally in the formulation of specific hypotheses. The more general the hypothesis one begins with, the more instances or particulars it can, in principle, account for, thereby "unifying" the phenomena under one single law or concept. However, the more general the concept or law, the fewer the details that one can infer about the phenomena. Hence, the less likely it will be able to "explain" how and why particular phenomena behave as they do. If even part of the practice of giving an explanation involves describing how and why particular processes occur – something that frequently requires that we know specific details about the phenomena in question – then the case for separating unification and explanation becomes not just desirable but imperative.

It has been claimed by Robert Butts, and more recently by William Harper, and even by Whewell himself, that in a consilience there is an *explanation* of one distinct class of facts by another class from a separate domain.[9] However, it is important here to see just what that explanation consists in. As Butts has pointed out, we cannot simply think of the explanatory power of consilience in terms of entailment relations, because in most cases the deductive relationship between the consilient theory and the domains that it unifies is less than straightforward. The best-known example is Newton's theory and its unification of Kepler's and Galileo's laws. That synthesis required changes in the characterization of the nature of the physical systems involved, as well as changes in the way that the mathematics was used and understood, all of which combined to produce nothing like a straightforward deduction of the laws for terrestrial and celestial phenomena from the inverse-square law. Given that consilience cannot be expressed in terms of entailment relations, is it possible to think of the connection between explanatory power and consilience in terms of the convergence of numerical results? Such seems the case with Maxwellian electrodynamics, in which calculation showed that the velocity of electromagnetic waves propagating through a material medium (supposedly an electromagnetic aether) had the same value as light waves propagating through the luminiferous aether. That coincidence of values suggested that light and electromagnetic waves were in fact different aspects of the same kind of process. However, as we shall see in Chapter 3, whether or not this kind of convergence constitutes an explanation depends on whether or not there is a well-established theoretical framework in place that can "account for" why and how the phenomena are unified. The latter component was in fact absent from Maxwell's formulation of the theory. Yet the theory undoubtedly produced a remarkable degree of unity.

A similar problem exists in the Kepler case. Recall, for instance, the way in which Kepler's first and second laws fit together in a coherent way, given the physics of libratory motion. Although there was an explanatory story embedded in Kepler's physics, it was incorrect; hence, contrary to what Whewell would claim, a coincidence of results by no means guarantees the truth of the explanatory hypothesis. Although the convergence of coefficients may count as a unification of diverse phenomena, more is needed if one is to count this unification as explanatory. This is especially true given that phenomena are often unified by fitting them into a very general mathematical framework that can incorporate large bodies of diverse data within a single representational scheme (e.g., gauge theory, Lagrangian mechanics). And mathematical techniques of the sort described by Whewell are important for determining a general trait or tendency that is common to the data while ignoring other important characteristics.

But Whewell himself seems to have recognized that more was needed if consilience was to count as truly explanatory; specifically, one needed a *vera causa* to complete the picture. In the conclusion to the section on methods of induction he remarks that those methods applicable to quantity and resemblance usually lead only to laws of phenomena that represent common patterns, whereas inductions, based on the idea of cause and substance, tend to provide knowledge of the essential nature and real connections among things (Whewell 1967, p. 425). Laws of phenomena were simply formulae that expressed results in terms of ideas such as space and time (i.e., formal laws of motion). Causes, on the other hand, provided an account of that motion in terms of force.

Unfortunately, Whewell was somewhat ambiguous about the relations between causes and explanations and sometimes suggested that the inference to a true cause was the *result* of an explanation of two distinct phenomena; at other times he simply claimed that "when a convergence of two trains of induction point to the same spot, we can no longer suspect that we are wrong. Such an accumulation of proof really persuades us that we have a *vera causa*".[10] Although the force of universal gravitation functioned as just such a true cause by explaining why terrestrial and celestial phenomena obeyed the same laws, gravitation itself was not well understood. That is, there was no real explanatory mechanism that could account for the way that the force operated in nature; and in that sense, I want to claim that as a cause it failed to function in a truly explanatory way. Hence, even though a *why* question may be answered by citing a cause, if there is no accompanying answer to the question of *how* the cause operates, or what it is in itself, we fail to have a complete explanation.

With hindsight, of course, we know that Whewell's notion of unity through consilience could not guarantee the kind of certainty that he claimed for it. Regardless of whether or not one sees Whewell's account of consilience of inductions as a model for current science, it is certainly the case that Whewell's history and philosophy of the inductive sciences provided a unity of method that at the same time respected the integrity and differences that existed within the distinct sciences. It provided not only a way of constructing unified theories but also a way of thinking about the broader issue of unity in science. Each science was grounded on its own fundamental idea; some shared inductive methods (e.g., means, least squares), but only if they seemed appropriate to the kind of inquiry pursued in that particular science. In that sense, Whewell was no champion of the kind of scientific reductionism that has become commonplace in much of the philosophical literature on unity. Consilience of inductions was a goal valued from within the boundaries of a specific domain, rather than a global methodology mistakenly used to try to incorporate the same kinds of forces operant in physics into chemistry (Whewell 1847, p. 99).

Now let us turn to another context, one in which the focus is not on unified theories specifically but more generally on unity in science defined in terms of unity of method. I am referring to the programme outlined in the *International Encyclopedia of Unified Science*, a collection of volumes written largely by the proponents of logical empiricism and first published in 1938. Although there were similarities to Whewell's attempt to retain the independence of particular sciences, the proponents of that version of the unity of science (Neurath) claimed that the localized unity achieved within specific domains carried no obvious epistemic warrant for any metaphysical assumptions about unity in nature. Their desire to banish metaphysics also resembled the Kantian ideal of unity as a methodology. What is especially interesting about that movement, as characterized by each of the contributions to the *Encyclopedia*, is the diversity of ideas about what the unity of science consisted in. Although that may seem the appropriate sort of unity for an encyclopedia, more importantly it enables us to see, in concrete terms, how unity and disunity can coexist – evidence that the dichotomy is in fact a false one.

1.4. Logical Empiricism: Unity as Method and Integration

It has frequently been thought that the unity of science advocated by the logical empiricists had its roots in logical analysis and the development of a common language, a language that would in turn guarantee a kind of unity of method in the articulation of scientific knowledge. In his famous 1938 essay "Logical Foundations of the Unity of Science", published in the *International Encyclopedia of Unified Science* (Neurath et al. 1971), Rudolph Carnap remarks that the question of the unity of science is a problem in the logic of science, not one of ontology. We do not ask "Is the world one?", "Are all events fundamentally of the same kind?". Carnap thought it doubtful that these philosophical questions really had any theoretical content. Instead, when we ask whether or not there is a unity in science we are inquiring into the logical relationships between the terms and the laws of the various branches of science. The goal of the logical empiricists was to reduce all the terms used in particular sciences to a kind of universal language. That language would consist in the class of observable thing-predicates, which would serve as a sufficient reduction basis for the whole of the language of science. Despite the restriction to that very narrow and homogeneous class of terms, no extension to a unified system of laws could be produced; nevertheless, the unity of language was seen as the basis for the practical application of theoretical knowledge.

We can see, then, that the goal of scientific unity, at least as expressed by Carnap, is directly at odds with the notion of unity advocated by Whewell. The kind of reductionist programme suggested by the logical unity of science would, according to Whewell, stand in the way and indeed adversely affect the growth of knowledge in different branches of science. According to him, the diversity and disunity among the sciences were to be retained and even encouraged, while upholding a unity within the confines of the individual branches of science.

But, as with the problem of the unity of science itself, within the logical-empiricist movement there were various ways in which the notion of unity was understood, even among those who contributed to the *International Encyclopedia of*

Unified Science. Views about what constituted the unity of science and how the goal was to be pursued differed markedly from the more traditional account of logical empiricism and the reductionism expressed by Carnap. For example, John Dewey, a contributor to the *Encyclopedia*, saw the unity of science as largely a social problem. In addition to a unification of the results obtained in science there was also the question of unifying the efforts of all those who "exercise in their own affairs the scientific method so that these efforts may gain the force which comes from united effort" (Dewey 1971, p. 32). The goal was to bring about unity in the scientific attitude by bringing those who accepted it and acted upon it into cooperation with one another. Dewey saw this problem as prior to the technical issue of unification with respect to particular scientific results. Unlike Carnap, who concerned himself with more technical problems and the development of methods that would achieve a logical reduction of scientific terms, Dewey believed that the unity-of-science movement need not and should not establish in advance a platform or method for attaining its goal. Because it was a cooperative movement, common ideas ought to arise out of the very process of cooperation. To formulate them in advance would be contrary to the scientific spirit.

Dewey saw the scientific attitude and method as valuable insofar as such practice had brought about an increase in toleration; indeed, in his view that attitude formed the core of a free and effective intelligence. Although the special sciences can reveal what the scientific method is and means, all humans can become scientific in their attitudes (i.e., genuinely intelligent in their ways of thinking and acting), thereby undermining the force of prejudice and dogma.

Yet another account of the unity of science was articulated by Otto Neurath, who saw unified science as a type of encyclopedic integration. It certainly was no accident that the *International Encyclopedia of Unified Science* brought together authors with diverse views on the topic of unity, but views that nevertheless could be integrated together in a way that could achieve a common goal. Hence, the *Encyclopedia* itself stood as the model for a unified science as envisioned by Neurath. Each contribution from a given scientific field was brought together with others that expressed diverse opinions within a wider set of agreements, agreements that lent unity and the spirit of Deweyan cooperation to the project.

But how should one understand unity as encyclopedic integration? At what point do differences begin to obscure the unified core that binds together the diversity of opinion and method? If one adopts a Whewellian approach, the answer is relatively straightforward: Unity existed within each science, and across domains there was a common approach to the discovery of knowledge that had its origin in the doctrine of fundamental ideas. However, for Neurath, as well as some of his fellow contributors, the aim of the *Encyclopedia* was to synthesize scientific activities such as observation, experimentation and reasoning and show how all of those together helped to promote a unified science. Those efforts to synthesize and systematize were not directed toward creating *the* system of science, but rather toward encouraging encyclopedism as both an attitude and a programme. One starts with property to be identified with explanation? If we think of the laws of Newtonian mechanics as allowing us to derive both the fact that the planets obey Kepler's laws and the fact that terrestrial bodies obey Galileo's laws, then, claims Friedman, we have reduced a multiplicity of unexplained independent phenomena to one. Hence, our understanding of the world is increased through the reduction in the total number of independent phenomena that we must accept as given. *Ceteris paribus*, the fewer independent phenomena, the more comprehensible and simple the world is. By replacing one phenomenon or law with a more comprehensive one, we increase our understanding by decreasing the number of independently acceptable consequences.

Friedman's strategy for giving a precise meaning to this notion of reduction of independent phenomena was shown to be technically flawed by Philip Kitcher (1976). However, in addition to the technical difficulties, some of Friedman's more intuitive claims regarding the connection between unification and explanation are by no means unproblematic. In his discussion of the kinetic theory of gases, Friedman claims that the theory explains phenomena involving the behaviour of gases, such as the fact that they approximately obey the Boyle-Charles law, by reference to the behaviour of the molecules that compose the gas. This is important because it allows us to deduce that any collection of molecules of the sort that compose gases will, if they obey the laws of mechanics, also approximately obey the Boyle-Charles law. The kinetic theory also allows us to derive other phenomena involving the behaviour of gases – the fact that they obey Graham's law of diffusion and why they have certain specific-heat capacities – all from the same laws of mechanics. Hence, instead of these three brute facts, we have only one: that molecules obey the laws of mechanics. Consequently, we have a unification that supposedly increases our understanding of how and why gases behave as they do. The unifying power of the mechanical laws further allows us to integrate the behaviour of gases with other phenomena that are similarly explained.

The difficulty with this story is that it seems to violate Friedman's second condition for a theory of explanation: that it be objective and not dependent on the changing tastes of scientists and historical periods. We know that the kind of straightforward mechanical account that he describes is not the technically correct way of explaining the behaviour of gases, given the nature of quantum statistical mechanics. Although it may have been a perfectly acceptable explanation at that time, today we no longer accept it as such. But perhaps what Friedman had in mind was the claim that because the laws of mechanics unify a number of different domains, they themselves can be thought to provide an objective explanation of the phenomena. That is, the search for unifying/explanatory theories is more than simply an objective methodological goal. When we do find a theory that exhibits the kind of powerful structure exemplified by mechanics, we typically want to extend the notion of objectivity beyond the idea of unifying power simpliciter to the more substantive claim of "unification through mechanical laws". Hence, it is the explanatory theory, in this case mechanics, that provides us an objective understanding of the phenomena. And to the extent that mechanics is still used to explain phenomena in certain domains, it should be considered objective. However, as the historical record shows, the importance of mechanical explanation is indeed linked to specific historical periods, and in many cases when we make use of mechanical explanations we do so on the basis of expediency. We know that the accurate "objective" description is either too complicated or not really required for the purposes at hand. Hence the notion of changing tastes of scientists is one that is directly linked to theory change, and to explanation as well. That is, with every theoretical change comes a change regarding what constitutes an "objective explanation". And although this encompasses much more than "matters of taste", decisions to accept an explanation as merely useful are ones that are nevertheless contextual and hence pragmatically determined.

We need only look at the development of physics in the nineteenth century to see how contextually based mechanical explanation had become even at that time. As a mathematical theory, mechanics was able to deal not only with rigid bodies, particle systems and various kinds of fluids but also, in the form of the kinetic theory of gases, with the phenomenon of heat. Unfortunately, however, the kinetic theory was far from unproblematic. Maxwell had shown that the system of particles that obeyed the laws of mechanics was unable to satisfy the relation between the two specific heats of all gases (Maxwell 1965, vol. 2, p. 409). In other words, the equipartition theorem, which was a consequence of the mechanical picture, was incompatible with the experimentally established findings about specific heats. Similarly, the second law of thermodynamics could not be given a strict mechanical interpretation, because additional features, like the large numbers of molecules, needed to be taken into account. A statistical description involving an expression for entropy in terms of a molecular distribution function was thought by some, including Boltzmann, to provide a solution to the problem. Some favoured mechanical explanation, while others opposed it.

The place of mechanical explanation in Maxwellian electrodynamics was also unclear. Although the theory was developed using a series of mechanical models and analogies, the final formulation of the theory was in terms of Lagrange's dynamical equations. The advantage of that method was that it enabled one to proceed without requiring any detailed knowledge of the connections of the parts of the system, that is, no hypotheses about the mechanical structure were necessary. Mechanical concepts were used by Maxwell as a way of showing how electromagnetic phenomena *could* be explained, but they by no means formed the core of the theory. In addition, difficulties in establishing a mechanical account of the behaviour and constitution of material bodies led the theorist Willard Gibbs to exercise extreme caution in his claims about the validity of the theory presented in *Statistical Mechanics* (Gibbs 1902). Too many unresolved difficulties in the theory of radiation and the specific-heats problem made many sceptical of the legitimacy of mechanical descriptions of physical phenomena.

Others, like Ernst Mach, and to some extent Pierre Duhem, disliked mechanical explanations for what were largely philosophical reasons. Because of Mach's unfaltering reliance on empiricism/phenomenalism as the proper method for acquiring scientific knowledge, mechanism and its hypotheses about the ultimate constituents of matter simply lacked the kind of justification that he thought a proper scientific account should have. There simply was no legitimate means of knowing whether or not mechanical phenomena could provide the ultimate explanatory ground. In the end, the mechanical explanation that had proved so powerful since the time of Newton gave way to quantum mechanics and eventually quantum field theory, with the old ideal of a mechanically based physics being gradually abandoned. My point, then, is that we can, at most, claim objectivity for only a certain kind of understanding that arises from within the confines of a specific theory at a particular time in history. The fact that the theory may provide a unified account of the phenomena does not eliminate the fact that the acceptability of its explanatory structure is historically situated and may be a matter of what particular individuals see as appropriate. It isn't the case that mechanics wasn't an explanatory theory; rather, its explanatory power was ultimately linked to the very factors that Friedman wants to rule out: changing tastes and historical periods.

What content can we give, then, to the claim that the unifying/explanatory programme provided by the mechanical paradigm satisfied the goal of objectivity? It clearly was the case that preference for mechanical explanations was tied to the preferences of specific groups and/or individual scientists.[12] However, neither that nor the fact that the mechanical conception of nature was historically rooted need detract from its objectivity. Decisions either to pursue or to abandon the search for mechanical explanations were firmly rooted in scientific successes and failures, as well as in philosophical presuppositions about the correct methodology for science. In that sense the objectivity of mechanical explanation was and is ultimately linked to the objectivity of the scientific method. Although there can be no doubt that mechanics was a very powerful foundation for physics, its broad unifying/explanatory power was not, as we have seen, based on a wholly consistent foundation. The difficulties with the kinetic theory, together with the historical contingency that accompanied mechanical explanation, paint a picture of objectivity quite different from the one Friedman suggests; they reveal an objectivity that was grounded in localized requests for information determined by the particular theories and periods in history.

One might want to argue that the objectivity Friedman claims for his account is nothing more than a *measure* of the reduction of independent facts through the use of more basic, comprehensive ones (i.e., it is an objective fact whether or not such a reduction has occurred). Hence, if this is a constraint on explanation/understanding, it is also an objective fact whether or not this reductivist goal has been attained. But surely this feature cannot be divorced from the fundamental worth of the reducing theory. Even though, as Friedman notes, the basic phenomena may themselves be strange or unfamiliar, one nevertheless expects a fundamental coherence between theory and experiment. That coherence was simply absent from the kinetic theory and its mechanical structure. In that sense, objectivity cannot be merely a procedural feature of an explanatory theory that involves reduction of facts, but instead it must integrate the philosophical theory of explanation and the scientific explanatory theory. In other words, one's theory of explanation must employ concepts and constraints that are applicable to the ways in which scientific theories themselves evolve. That evolution has had a history that has influenced the kinds of explanations that have been deemed acceptable; it is only by taking account of that history that we can begin to see how "objectivity" has emerged. Instead of characterizing objectivity as something transcendent to which theories and explanations aspire, we must recognize that part of what makes an explanation "objective" is its acceptability in the context in which it is offered, something that will, undoubtedly, have a temporal dimension.

The difficulty at the core of Friedman's account is his identification of explanation and unification. Some of these issues arise again in the context of his more recent account of unification and realism, as discussed in Chapter 2. For now, suffice it to say that it is, and should be, an objective question whether or not a theory has unified a group of phenomena. Moreover, the unification should not depend on historical contingencies. It is simply a fact that Newton's theory unified celestial and terrestrial phenomena and that Maxwell's theory unified electromagnetism and optics. Yet we no longer accept the physical dynamics required to make those theories explanatory. By separating explanation and unification we can retain our intuitions about the context independence of theory unification while recognizing the historical aspects of explanation. Although the broader notion of unity in science may have several different interpretations, there nevertheless seem to be good reasons for thinking that theory unification is more clear-cut. We ought to be able to determine, in a rather straightforward way, the extent to which a particular theory has unified different domains. Indeed, much of this book is dedicated to showing how that can be done.

Another attempt to "objectify" explanation has been proposed by Clark Glymour (1980). He claims that there are two different reasons for belief in a scientific theory: reasons provided by the explanations the theory gives and reasons provided by the tests the theory has survived. The two qualities that explanations have that lend credence to theories are their ability to eliminate contingency and their unifying power. For example, Glymour claims that perhaps the most comprehensive way to explain the ideal-gas law is to show that it simply is not possible for a gas to have pressure, volume and temperature other than as the gas law requires. So instead of demonstrating that a regularity is a necessary consequence of a theory, one shows that the regularities are necessary in and of themselves. One thereby explains the regularity by identifying the properties it governs with other properties "in such a way that the statement of the original regularity is transformed into a logical or mathematical truth" (Glymour 1980, p. 24). Consequently, the statements that identify properties are, if true, necessarily true, and thereby transform the contingent regularity into a necessary truth. A simple example hinges on the identification of gravity with curved space-time. Provided this identification is true, then if general relativity is true, the identification is necessarily true.

Why is this so? It is so because on such a picture the field equation of general relativity states an identity of properties, and hence if it is true, it is necessarily so. As a result, the equation of motion of the theory, because it is a consequence of the field equation, is also necessary. In physics, these identities usually are definitional in form, but are expressed in terms of a mathematical equation. For instance, consider one of the field equations of electrodynamics, $\operatorname{div} \mathbf{B} = 0$ where $\mathbf{B}$ is the magnetic-flux intensity; if we introduce the vector potential $\mathbf{A}$ and claim that $\mathbf{B}$ is equivalent to curl $\mathbf{A}$ we get $\operatorname{div} \operatorname{curl} \mathbf{A} = 0$. Because the divergence of a curl is always zero, we have a mathematical identity that supposedly affords an explanation of the Maxwell field equation. Moreover, because the field equation follows as a necessary consequence of the mathematical identity, it is also necessary.

Although this scheme provides a relatively straightforward and powerful explanatory strategy, it implicitly assumes a direct and unproblematic correspondence between the mathematical structure of our theories and the physical systems represented by the mathematical formalism. Although the nature of this correspondence is one of the most important unanswered questions in philosophical analyses of mathematical physics, there are some partial answers to the question that would seem to caution against taking Glymour's analysis as a general scheme for providing explanations. If we think about the use of mathematical structures like group theory and the Lagrangian formalism, we quickly see that what is established is, at best, a structural similarity between the mathematical framework and a physical system. Although it was Lagrange's intention to provide an account of mechanics, he wished to do so by eliminating the Newtonian idea of force, replacing it with the kinetic potential L (excess of kinetic energy over potential energy). But in modern physics, the uses to which Lagrange's equations are put extend far beyond mechanics, making the Lagrangian formalism a method for framing equations of motion for physical systems in general, rather than providing mechanical explanations of phenomena.

Both the breadth of the Lagrangian method and its weakness as an explanatory structure come from the use of generalized coordinates q_i used in place of rectangular coordinates to fix the position of the particle or extended mass [where $x = x(q_1, q_2, q_3)$, and so on for y and z]. It is important to note that the interpretation of these coordinates can extend well beyond simply position coordinates; for instance, in the Lagrangian formulation of electric circuits given by Maxwell, the q_i terms were interpreted as quantities of electricity with unspecified locations. The q_i terms then are functions of time and need not have either geometrical or physical significance. In modern accounts they are referred to as coordinates in a configuration space, and the $q_i(t)$ terms as equations of a path in configuration space. Hence, because no conclusions about the nature of a physical system (other than its motion) can be reached on the basis of its Lagrangian representation, it seems unreasonable for us to argue from a mathematical identity to a necessary physical truth on the basis of identification of physical and mathematical quantities. Similarly, consider the Fourier series, as used in the study of heat diffusion.

relation. For example, in Maxwell's first paper on electromagnetism he utilized a formal analogy between the equations of heat flow and action at a distance, yet no physical conclusions followed from that similarity.

I also want to claim, contra Glymour, that any identification of unification and explanation that might prove possible ought to involve more than the application of a common set of principles to diverse circumstances. The reasons *why* these principles are applicable must emerge at some level within the theory if it is to be truly explanatory. My reasons for holding such a view have to do with a belief that general principles fail to be explanatory in any substantive sense. They enable us to classify and systematize phenomena and may be thought of as the starting point for scientific explanation, but they do not provide details about *how* particular processes take place over and above a descriptive account of the relations among various quantities. Take, for instance, the different ways in which classical analytical mechanics can be formulated – the Newtonian, Lagrangian and Hamiltonian approaches. Each provides a general method for handling particular aspects of the same physical problem or different kinds of problems. However, the decision to employ any one of them depends not only on the nature of the object under investigation but also on the kind of prior information we possess. If we are unsure about the forces acting on a particular system, the Newtonian method with tell us nothing about them; we will simply be unable to apply the parallelogram rule. The Hamiltonian and Lagrangian formulations will tell us something about the evolution of the system – they will allow us to characterize stable states as those for which potential energy is at a minimum – but will tell us nothing about the specific mechanisms involved in the processes that interest us. One might want to object that Newtonian mechanics explains a startling amount about the motions of falling bodies, the tides and planetary motions by showing how each is an instance of the law of universal gravitation. The explanatory relation in this case amounts to an accurate calculation of these motions based on the relations specified by the inverse-square law. But here again there is nothing specific in the theory about how or why the mechanism operates – something that was, at the time the *Principia* was published, clearly a legitimate topic for explanation. By contrast, general relativity does provide an explanatory framework for understanding gravitation. My point, then, is not just that the division between explanation and unification is not uncommon in unified theories, but on the basis of the unifying process we have no principled reason to expect it to be otherwise.

Most modern physical theories seek to unify phenomena by displaying a kind of interconnectedness, rather than a traditional reduction of the many to the one. Two distinct but related conditions are required for this interconnectedness to qualify as representing a unification. First, the mathematical structure of the theory must be general enough to embody many different kinds of phenomena and yet specific enough to represent the way in which the phenomena are combined. The second, related condition refers to the "rigidity" as opposed to the "flexibility" of a theory.[14] In the latter case the theoretical structure does little to resist the multiplication of free parameters in order to account for distinct phenomena. Rigidity, on the other hand, not only minimizes the number of free parameters in the theory's domain but also rules out the addition of supplementary theoretical structure as a way of extending the theory's evidential base. These requirements are definitive of the unifying process, but as such they have very little to say about the nature of scientific explanation.

My discussion of unification in the subsequent chapters is motivated not only by what I see as errors and omissions in current philosophical analyses of the subject but also by historical investigation of what exactly was involved in paradigm cases of unification in both the physical and biological sciences. I want to stress at the outset that my emphasis is on the process of theory unification, something I want to distinguish from a metaphysical or even methodological thesis about the "unity of science" or a "unity of nature". What I want to show is that the methods involved in unifying theories need not commit one to a metaphysics of unity, of the kind that, say, Kepler advocated. As we saw earlier, Kepler's mathematical physics was rooted in the corresponding belief that nature was harmonious; hence there was a kind of one-to-one correspondence between the mathematical simplicity of physical laws and the mathematical simplicity of nature. Although some might claim that the motivation for theory unification embodies a belief in something like Keplerian metaphysics, I want to argue that there are good reasons, despite the presence of unified theories, for thinking such a belief to be mistaken. It is perfectly commonplace to have a high-level structural unity within a theoretical domain in the presence of a disunity at the level of explanatory models and phenomena. In addition to the electroweak case, population genetics, which is discussed in Chapter 7, is a case in point.

The purpose of this overview has not been to set out particular accounts of unification as models for the cases I intend to discuss. My intention has rather been to present a brief sampling of some ways in which unity and unification have been characterized throughout the history of science and philosophy and to give some sense of the diversity present in accounts of unity. I have also attempted to lay some groundwork for my argument that unity and explanatory power are different and frequently conflicting goals. Undoubtedly, strands of each of the views I have discussed can be found in the examples I shall present, something that serves to illustrate my point, namely, that although unified theories themselves may share structural similarities, no hard and fast conclusions can be drawn from that about nature itself. This is partly a consequence of the methods involved in theory unification, but it is also due to the fact that unity in science and nature can take on many disparate and contradictory interpretations and forms.

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Unification, Realism and Inference

The question that occupies most of this chapter is whether or not the first word in the title – unification – bears any relation to the other two, and if so, how that relation ought to be construed. As mentioned in the introductory remarks, a common approach to fleshing out the notion of unification is to link it to explanation. A unified theory is thought to be one that can explain phenomena from different domains by showing either that the phenomena are essentially the same (e.g., light waves are simply electromagnetic waves) or that diverse phenomena obey the same laws, thereby suggesting some link between them. This explanatory power supposedly provides good evidence that the theory is true; hence, the best explanation, which typically will be the one that reveals some unity among the phenomena, should be seen as more likely to be true than its competitors. Of course, not all "best explanations" will perform a unifying function. There may be only one explanation of a particular phenomenon, and hence, by default, it will have to be considered the best. So embedded in the debate are two issues, one linking unity to explanatory power, and the other linking the concept of "best explanation" to increased likelihood of truth. This practice of drawing inferences to truth on the basis of explanatory power has been dubbed "inference to the best explanation" (IBE) and has been advocated by, among others, Harman (1965) and Thagard (1978).

More recently, however, there have been forceful criticisms by van Fraassen (1980), Cartwright (1983) and Friedman (1983) of the link between IBE and truth and its use as a methodological rule that forms the basis for inference. The complaints are varied. Some, particularly van Fraassen, emphasize the fact that explanation has to do with providing answers to "why" questions or organizing and systematizing our knowledge – pragmatic features that do not provide evidence for the literal truth of the background theory used in the explanation. Cartwright has argued that truth and explanation are, in fact, inversely related: Explanatory power requires broad general laws that do not accurately describe physical processes. But even for those who disagree about the pragmatic status of explanation or its relation to truth, the best available explanation may not be the one that we would want to accept, even provisionally. Friedman opposes IBE on the ground that it provides no guidance on the issue of whether we should construe theoretical structure literally or instrumentally. It simply fails to explain why theoretical structure should *ever* be taken literally. For example, consider two attitudes one might have toward the molecular model of a gas: Either one can be a realist and claim that gases really are just configurations of molecules, and the former can be reduced to or identified with the latter, or one can simply believe that the function of the kinetic theory is to supply a mathematical model for the observable behaviour of gases by associating gases or their properties with mathematical aspects of the model. In this case there is a mapping or correlation of the two domains, but not a literal identification; we have a representation, but not a reduction. We can think of the phenomenological and theoretical domains as being two structures $\mathcal{B}$ and $\mathcal{A}$. The realist sees the relation between these two as that of model to sub-model; $\mathcal{B}$ is a sub-model of $\mathcal{A}$, and hence the objects in $\mathcal{B}$ are identified with their counterparts in $\mathcal{A}$. The anti-realist, however, claims only that $\mathcal{B}$ is embeddable into $\mathcal{A}$; there is a mapping from one domain to the other, but no literal identification is made.

The important question, of course, is when to adopt one attitude rather than another. Part of Friedman's objection to IBE is that it provides no guidance on this issue. Regardless of whether we interpret theoretical structure as a mere representation of observable phenomena or as a literal reduction, we enjoy the same consequences vis-à-vis the observable realm. That is, we get the same explanations of the observable phenomena, the only difference being that the anti-realist says that the phenomena behave "as if" they were composed of molecules, rather than actually believing that to be so. In addition, we may have only one explanation of a particular phenomenon, one that might not be acceptable for a variety of reasons; nevertheless, if we apply the rule of IBE we are forced to accept it. Friedman's solution to this problem consists not in giving up this method of inference but rather in restricting its applicability. He argues that theoretical inference can be sanctioned when accompanied by unification, thereby linking unity, explanation and truth. Inference to the "unified explanation" is touted as superior because we get an accompanying increase in the confirmation value of the phenomena to be explained and greater confirmation than would accrue to the previously unconjoined (or non-unified) hypotheses. For instance, if we conjoin the atomic theory of molecular structure and the identification of chemical elements with different kinds of atoms, we can explain chemical bonding. This imparts more confirmation to the assumption that gases are simply molecular systems, a hypothesis that is also confirmed by the gas laws themselves.

Friedman provides persuasive arguments to suggest why one ought to be a realist about certain bits of theoretical structure that figure in the process of unification. Realism allows a literal interpretation of the relevant structure, which in turn affords our theories their unifying power and subsequently their confirmation. In other words, we simply cannot conjoin or unify hypotheses that we do not interpret literally, and, on his view, a literal interpretation requires realism. Without this ability to unify, there is no basis for increased confirmation and hence no basis for belief. Any theoretical structure not participating in unification can be

UNIFYING SCIENTIFIC THEORIES

Physical Concepts and Mathematical Structures

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