Unifying Scientific Theories

Physical Concepts and Mathematical Structures

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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 only led the planets around it but also repelled and attracted them depending on whether a planet displayed its "friendly" or "hostile" side toward the sun, that is, depending on which magnetic side was facing the sun. Kepler hypothesized that the source of that magnetism lay in magnetic fibres that passed through the planets. However, the planets themselves did not "exert" any force; rather, the action of the sun communicated a certain "inclination" to the fibres of the planetary body such that its entire libration derived from the sun. Hence, libration was not the result of any action or motion of the planet itself.[2] In order to give an exact account of the mechanism of orbital motion it would be necessary to determine the variations in the propelling and attracting forces throughout the entire path. That would require that one calculate the angles that the radius vectors of the sun made with the magnetic fibres of the planet. The sines of the complements of those angles (the cosines) would provide a measure of those portions of the forces that acted on the planet.[3]

Once Kepler developed the mechanism responsible for libration and proved that it was measured by the versed sine of the arc traversed by the planet, he was able to formally establish that an elliptical orbit resulted as a consequence of libration.[4] From there Kepler went on to prove his second law, which describes the relationship between the time taken by a planet to travel a particular distance on its orbit and the area swept out by the radius vector. Again, the key to the synthesis was that the law, as Kepler formulated it, was valid *only* for an elliptical orbit, thereby establishing an interconnectedness between the dynamics of libratory motion and the geometry governing the motions of the planets. The relationships were all confirmed empirically, making the argument one that was not based solely on formal geometrical constraints, but one that united the physical and mathematical components of celestial phenomena in a simple coherent way. That unity created a kind of justification that not only applied to Kepler's laws themselves but also extended to the metaphysical thesis regarding the relation between mathematical simplicity and truth. In other words, it was a justification determinable through the agreement of results. His laws governing planetary orbits described the simplest possible paths consistent with libratory motion, and the convergence of those results provided further evidence that his physical hypothesis was true. Kepler began with the belief that nature was founded on or determined by mathematical harmonies, and the correspondence of empirical observations with laws based on those harmonies further reinforced the idea of nature's unity and mathematical simplicity.

It is exactly this kind of context, where one sees a particular law or principle yielding different kinds of interconnected results (i.e., the dynamics of libratory motion facilitating the derivation of Kepler's first and second laws in a way that made each interdependent on the other), that we typically take as exemplifying at least some of the qualities of a unified theory.[5] But in the assessment of such a theory, in the determination of its truth or confirmation value, one must be cautious in locating the truth component in the proper place. In other words, if we look at the

unity displayed by Kepler's account of planetary motion, it is tempting to describe its explanatory power as being grounded in the dynamics of libratory motion. And when Kepler succeeded in deducing an elliptical orbit from his physics, instead of just arriving at it through a process of observation coupled with manipulation of mathematical hypotheses designed to fit the facts, one is tempted to say that the physical basis of the theory must be true. Tempting as this may be, the question is whether or not we can, or even should, infer, as Kepler did, on the basis of this type of interconnectedness, the truth of the physical hypothesis.

With hindsight we know that Kepler's physics was mistaken, despite the fact that it yielded laws of planetary motion that were retained as empirically true approximations within Newtonian mechanics. The example, however, raises a number of issues that are important for understanding how one ought to think about unification. First, it seems that given what we know about the history of physics, it becomes obvious that one should not, as a general principle, attribute truth to a unifying hypothesis such as Kepler's theory of libratory motion simply because it yields a convergence of quantitative results. Second, and equally important, is the question of whether unification can be said to consist simply in a mathematical or quantitative convergence of different results or whether there needs to be an appropriate dynamical or causal explanation from which these results issue as consequences. The answer one gives to this latter question is significant not only for the link between explanatory power and unification but also for the connection between unified theories and the broader metaphysical thesis about unity in nature. That is, if unity is typically accompanied by an underlying physical dynamics, then it becomes necessary to determine whether or not the unifying power provides evidence for the physical hypothesis from which it emerges. Finally, it certainly is not an uncommon feature of scientific theories that they display, at least to some degree, the kind of interconnectedness present in Kepler's account of planetary motion. Yet his account is not typically thought to be a truly "unified" theory, in that it does not bring together *different* kinds of phenomena. For example, electromagnetic phenomena were thought to be radically different from optical phenomena until they were unified by Maxwell's theory and shown to obey the same laws. In fact, most instances of what we term "unification" are of this sort. Is it necessary, then, to specify particular conditions that must be satisfied in order for a theory to be truly unified, or is the notion of unification simply one that admits of degrees? In other words, do all theories unify to a greater or lesser extent? Each of these issues will be discussed in later chapters, but by way of contrast to Kepler's metaphysical/mathematical picture of unity and simplicity let us turn to Kant's account, which accords to unity a strictly heuristic role by characterizing it as a regulative ideal that guides our thinking and investigation about experience in general and scientific investigation in particular. What is interesting about Kant's notion of unity is that it carries with it no metaphysical commitments; yet it is indispensable for scientific research and the more general quest for knowledge.

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.

Part of what is involved in a consilience of inductions is what Whewell refers to as "the colligation of ascertained facts into general propositions". This also takes place through a three-step process that involves (1) selection of the idea, (2) construction of the conception and (3) determination of the magnitudes. These steps have analogues in mathematical investigations that consist in determining the independent variable, the formula and the coefficients. It was Whewell's contention that each science had its own fundamental idea; the study of mathematics was based on the ideas of number and space, whereas mechanics relied on the idea of force to make it intelligible. Similarly, the idea of polarity was predominant in the study of chemical phenomena, and ideas of resemblance and difference were crucial to the study of natural history and the classificatory sciences.

Once the requisite idea was chosen, one could then proceed to the construction of a conception, which was a more precise specification of the idea. For example, a circle or a square is a kind of spatial configuration, and a uniform force is a particular manifestation of the general notion of a force. So if we have a phenomenon like the weather and we are trying to establish some order that will assist in predictions, we must decide whether we wish to select (1) the idea of time, and introduce the conception of a cycle, or (2) the idea of force, accompanied by the conception of the moon's action. One selects the appropriate conception by comparing it with observed facts, that is, determining whether or not the weather really is in fact cyclical by comparing the supposed cycle with a register of the seasons. The idea is the core concept that grounds each field of inquiry. When we achieve a consilience of inductions, the result is that two different conceptions governing different classes of facts are seen in a new way, either as belonging to a totally new conception or as manifestations of one or the other of the original conceptions. The important point is that consilience does not involve a "jumping together" of two distinct ideas from different branches of science. The classes of facts usually are drawn from within an individual science.

There is no definitive method for selecting the right idea, nor the right conception for that matter. The only requirement or rule is that the idea must be tested by the facts. This is done by applying the various conceptions derived from the idea to the facts until one succeeds in uncovering what Whewell refers to as the "law of the phenomena".

Although my intention is not to provide a complete analysis of Whewell's account of induction and unification, I do think it important to discuss briefly the mathematical representation of this procedure, in an attempt clarify how unification takes place. The interesting question is whether the convergence of numerical results that occurs in a consilience of inductions is what ultimately constitutes unity or whether there are further implications for the supposed connection between explanation and unification.

In a section entitled "General Rules for the Construction of a Conception" Whewell describes the process as the construction of a mathematical formula that coincides with the numerical expression of the facts. Although the construction of the formula and the determination of its coefficients have been separated into two steps, Whewell claims that in practice they are almost necessarily simultaneous. Once one selects the independent variable and the formula that connects the observations to form laws, there are particular technical processes whereby the values of the coefficients can be determined, thereby making the formula more accurate. These include the methods of curves, of means, of least squares and of residues. In the case of the method of curves, we have a specific quantity that is undergoing changes that depend on another quantity. This dependence is expressed by a curve. The method enables us to detect regularities and formulate laws based not only on good observations but also on those that are imperfect, because drawing a line among the points given by observations allows us to maintain a regular curve by cutting off the small and irregular sinuosities. When we remove the errors of actual observation by making the curve smooth and regular, we are left with separate facts corrected by what Whewell calls their "general tendency"; hence, we obtain data that are "more true than the individual facts themselves" (Whewell 1847, vol. 2). The obstacles that prove problematic for the method are ignorance of the nature of the quantity on which the changes depend and the presence of several different laws interacting with one another.

The method of curves assumes that errors in observation will balance one another, because we select quantities that are equally distant from the extremes that observation provides. In cases where we have a number of unequal quantities and we choose one equally distant from the greater and smaller, we use the method of means rather than the method of curves. The implicit assumption, again, is that the deviations will balance one another. In fact, the method of means is really just an arithmetical procedure analogous to the method of curves, with one significant difference: In the method of curves, observation usually enables us to detect the law of recurrence in the sinuosities, but when we have a collection of numbers we must divide them into classes using whatever selection procedures we think relevant.

The method of least squares is also similar to the method of means. It allows us to discover the most probable law from a number of quantities obtained from observation. The method assumes that small errors are more probable than larger ones, and it defines the best mean as that which makes the sum of the squares of the errors the least probable sum. Finally, the method of residues involves an analysis of unexplained facts that have been left as residue after the formation of a law governing changes of a variable quantity. The residue is analysed in the same way as the original observations until a law is found that can account for it. This continues until all the facts are accounted for.[7]

The notable feature present in these methods, and what is important for our purposes here, is the level of generality that is introduced in order to assist in the formulation of laws of phenomena. Although it is true that induction is the process by which one formulates a general proposition from a number of particular instances, the difference in these cases is that the general conclusion is not simply the result of a juxtaposition or conjunction of the particulars. In each case a

"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 a certain group of scientific statements that may or may not be axiomatizable; they are then combined with others expressed in similar form. Such an encyclopedic integration of statements stresses the incompleteness of an encyclopedia, rather than the completeness of a system of knowledge.

The idea is perhaps better expressed by analogy with an orchestra, in which the different instruments play not only in harmony but also in discord with one another (Kallen 1948). Orchestration requires diversity in order to sustain itself; even the state of perfect harmony cannot be achieved without the necessary independence of each member of the group. In that sense the orchestration is self-directed from within, rather than imposed from without. Indeed, this notion of an orchestration of the sciences involves the kind of unity of action that forms the core of Neurath's vision of the *Encyclopedia*. Similarly, the unity of language expressed in Neurath's physicalism acts more as a coordinating mechanism than as an imposed structure that must be adhered to.[11] In other words, its task is to facilitate integration, not impose it.

If Neurath's physicalism is in fact non-constraining in that it does not impose a particular structure on how scientific practice/theorizing is carried out, then it begins to have quite radical implications for the philosophy of science. Philosophy ceases to be a kind of meta-science that dictates acceptable principles of rationality and method and becomes instead a form of inquiry whose normative force arises not out of first principles but out of practical cooperation with other disciplines. As a result, the normative component can be only pragmatically motivated; it is fallible and continuously evolving as inquiry itself evolves. Philosophy can provide the tools for analysis and contribute to the synthesis of reason and practice, but it in no way provides a "grounding" for scientific activity that transcends the practice itself.

In essence, then, what emerges from this version of the logical-empiricist or scientific-empiricist picture is a kind of unity through disunity. There is nothing to suggest that the kind of localized systematization of the sort favoured by Whewell would necessarily be ruled out; one could quite legitimately have unity within a particular science, physics for example, that emerges as the result of unifying theories, without any extension of this unity to other domains. But there is also nothing in this view of unity as orchestration to suggest that unity within a science in the form of reduction to a few basic principles is necessarily a goal to be desired or sought after. Clearly, this empirical approach to unity is at odds with the grand systematization desired by Kant, as well as the picture of a mathematically harmonious universe endorsed by Kepler. Yet one sees elements of each of those accounts in contemporary scientific and philosophical attitudes toward unity. The power of gauge theory in the development of quantum field theory pays homage to the mathematical idea of symmetry that formed the core of Kepler's physics. Molecular biology gives credence to the Kantian desire for systematization of knowledge across theoretical boundaries. Fields like astrophysics and cosmology, whose theoretical foundation is based in diverse and sometimes mutually contradictory models, lend support to the image of science as orchestration, whereas some areas in

high-energy physics are more accurately depicted by the Whewellian model that stresses unity only within a particular domain. For example, there may be no reason to assume that gravity can be incorporated into the unified structure that describes electromagnetism and the weak force.

Mirroring these various scientific attitudes are current philosophical accounts regarding the nature of unification and its connection to explanation. Aside from the discussion of Whewell, and perhaps Kepler, I have said very little about the connection between explanatory power and unity. As a way of briefly addressing that issue and setting the problem in its philosophical context, let us consider some of the points and questions that have emerged in the debate about the nature of unification and explanation. As discussed briefly in the Introduction, one common way of characterizing unification is through explanation; theoretical unity is thought to exist when, in a manner similar to Whewell's consiliences, a theory can explain a number of diverse facts. Similarly, the explanatory power of a theory is sometimes defined in terms of its ability to unify diverse facts under one law. We shall next look briefly at the merits of these views. More detailed accounts of unification and its connection with explanation, specifically those of Kitcher and Friedman, will be discussed in later chapters.

1.5. Unity as Explanation

Much of the debate about the character of explanation centres on the tension between the search for an objective criterion that can define scientific explanation and a subjective component that seems necessary if we are to link explanation with some notion of understanding. In his frequently quoted article "Explanation and Scientific Understanding", Michael Friedman (1974) tries to establish a link between these two goals and lists three desirable properties that a philosophical theory of explanation should have. First, the conditions that specify what counts as a good explanation should be sufficiently general so that most, if not all, scientific theories that we consider explanatory should come out as such. Second, explanations should be objective – they should not depend on the idiosyncrasies and changing tastes of scientists and historical periods. In other words, there is no room for what Friedman calls non-rational factors, such as which phenomenon one happens to find more natural, intelligible or self-explanatory. To the extent that there is an objective and rational way in which scientific theories explain, a philosophical theory of explanation should tell us what it is. Finally, the theory providing the explanation should connect explanation and understanding by telling us what kind of understanding scientific explanations provide and how they provide it.

Friedman attempts to articulate just such a theory by isolating what he sees as a particular property of the explanation relation. This property should be possessed by most instances of scientific explanation and should be one that is common to scientific theories from various historical periods. The property in question is the "unification" achieved by particular scientific theories – the ability to derive large numbers of diverse phenomena from relatively few basic laws. How exactly is this 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 **B** is the magnetic-flux intensity; if we introduce the vector potential **A** and claim that **B** is equivalent to curl **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.

The structure of the mathematical expression $\sum_r a_r \cos rx$ is that of a presumably convergent series of periodic functions. The sum itself represents the temperature of a point in the body, but this doesn't imply that heat is thought to be composed of an infinite series of basic states. Alternatively, this kind of mathematical structure may be more successfully used in a Fourier-series solution to the wave equation, where the periodic functions refer to harmonics in the study of acoustics.

We can see, then, that the use of mathematical identities as a way of eliminating contingency and providing explanations of physical phenomena or their relations can in fact have the *opposite* effect from the one Glymour proposes. Often an identification of a phenomenon with a particular mathematical characterization is highly contingent, and the generality of such frameworks is such that they provide no unique or detailed understanding of the physical systems that they represent. That is to say, we can predict the motions of phenomena from dynamical principles, but we have no understanding of the causes of motion.[13] Hence, there is no guarantee of explanatory power resulting from the mathematical description afforded by our theories. In fact, as we shall see in later chapters, the generality provided by such mathematical structures can actually detract from rather than enhance the theory's overall explanatory power. It is also worth pointing out that in order for the identities Glymour speaks about to attain the status of a necessary truth, one must first assume that the theoretical specification of the identity is itself true. Of course, if one knew that, then the epistemological problems associated with explanation and its link to evidence would simply vanish.

Finally, there is the additional form of understanding and explanation involving what Glymour calls the recognition of a pattern and the demonstration that diverse phenomena are of a kind that exhibit a common pattern. Examples are Newtonian mechanics and Copernican astronomy. In the Newtonian case, unity is achieved by generating diverse regularities from a single scheme that specifies the acceleration of any body in a system of n point particles in terms of their masses and mutual distances, assuming that they are subject only to mutual gravitational attraction. Once the value of n is specified, the scheme furnishes a linear second-order differential equation. Because there are infinite numbers of possible geometrical configurations and velocities of the n particles, each differential equation will have an infinity of solutions. One can apply this scheme to various values of n and classes of initial conditions for n-particle systems to yield the theorems of celestial mechanics. This achieves a unification insofar as the evidence bears on each of the laws connected in the pattern; without this kind of common pattern there is no reason to assume that the evidence for one group of laws has any connection with any other group. Moreover, it is not simply the case that Kepler's laws are logical consequences of Newton's dynamical theory together with universal gravitation. Instead, on Glymour's account, the pieces of evidence about various systems that satisfy Kepler's laws (the primary planets, the satellites of Jupiter and Saturn, and the moon) all lead to *instances* of the law of universal gravitation, and as a result all provide tests of the law itself. In that way the explanatory power of a theory, in this case Newton's theory, can be connected with the other reason for acceptance, namely, an accumulation of successful tests.

Although I have no dispute with this latter characterization of the relation between unification and *evidence*, it isn't immediately clear that it provides an account of explanation. Glymour himself wants to maintain that explanation can be a variety of things: unification, the description of causal connection, or several others. I want to claim that the kind of unity Glymour describes does not constitute an explanatory relation, nor does it allow us to distinguish between theories that are truly unified and phenomena/theories that merely share a number of common patterns. On this account, Kepler's celestial mechanics qualifies as a unified theory, but not to the degree that Newton's mechanics does. Difficulties then ensue about how to measure the level of unity achieved in a particular context. Instead, I want to claim that truly unified theories display a particular feature in virtue of which the phenomena are joined together, enabling diverse phenomena to be combined into a single theoretical framework. It is this *combining* of phenomena through a particular parameter in the theoretical structure that constitutes an important part of the unifying process, a process that is represented in the mathematical framework of the theory.

This process of combining is crucial for differentiating among the various ways of thinking about the unifying process. First, there is the rather straightforward case in which different phenomena obey the same laws, an example being the inverse-square law governing the forces on both electrical and mechanical phenomena. Second, there is the type of unity provided by Newtonian physics, in which phenomena previously thought to be different were shown to be similar in kind (gravitating bodies) and to obey the same force law. This differs from the first example in that existential claims about the same force presumably involve more than just the fact that it can be mathematically represented as an inverse-square law (i.e., that the same formal relations hold for a variety of different contexts and phenomena). Lastly, we have Maxwellian electrodynamics, in which light and electromagnetic waves are shown not only to have the same velocity but also to be manifestations of the same process; light waves are identified with electromagnetic waves. This differs from the modern unification of the electromagnetic and weak force, in which the implication is that at high energies the forces combine, but the theory nevertheless retains two distinct coupling constants, one for each of the forces, thereby making the unity somewhat of a promissory note.

I want to argue that in true cases of unification we have a mechanism or parameter represented in the theory that fulfills the role of a necessary condition required for seeing the connection among phenomena. In electrodynamics that role was played by electric displacement or the displacement current, which provided the foundation for a field-theoretic account of the phenomena. In the electroweak theory the mixing of the weak and electromagnetic fields is represented by a parameter known as the Weinberg angle. No such mechanism presents itself simply by showing that two kinds of forces can be described using the same formal

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.

2

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*