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The Skeptical Perspective: Science without Laws of Nature

1. Interpreting the Practice of Science

It is a fact about humans that their practices are embedded in interpretive frameworks. This holds both for individuals and for groups engaged in a common enterprise. Of course any sharp distinction between practice and interpretation, whether drawn by participants or third-party observers, will be somewhat arbitrary. Nevertheless, drawing some such distinction is useful, perhaps even necessary, for those who, while not direct participants in a practice, seek to understand it from their own perspective.

Such is the situation of historians and philosophers of science regarding the practice of science and the concept of a 'law of nature.' The claim of some philosophers, for example, that scientists seek to discover laws of nature, cannot be taken as a simple description of scientific practice, but must be recognized as part of our interpretation of that practice. The situation is complicated, of course, by the fact that, since the seventeenth century, scientists have themselves used the expression 'law of nature' in characterizing their own practice. The concept is thus also part of the interpretative framework used by participants in the practice of science. That shows that the concept sometimes lives in close proximity to the practice, but not that it is divorced from all interpretive frameworks.

Insisting on the interpretive role of the concept of a law of nature is important for anyone like myself who questions the usefulness of the concept for understanding the practice of science as a human activity. I realize full well that many others do not share this skeptical stance. Being part of the characterization of

the goals of science is but one interpretive role played by this ubiquitous concept. Laws played an essential role in Hempel's [1948, 1965] influential analysis of scientific explanation, and they continue to play a central role in more recent accounts [Salmon, 1984]. Nagel's [1961] classic analysis of theoretical reduction focuses on the derivation of the laws of one theory from those of another theory. Even critics of these analyses, including radical critics [Feyerabend, 1962], have generally focused on other features and left the role of laws unexamined. A concern with the status of laws has inspired many investigations into the confirmation or falsification of universal statements. Laws also figure in contemporary analyses of the concept of determinism [Earman, 1986]. And scientific realism is often characterized in terms of the truth, or confirmation, of laws referring to theoretical entities.

It is thus not surprising that, like Kant two centuries ago, many contemporary philosophers take it as given that science yields knowledge of claims that are at least universal, and perhaps necessary as well. Their problem, like Kant's, is to show how such knowledge is possible.[1] Doubting that such knowledge is actual, I have little interest in rebutting arguments that it is possible. More serious would be claims that knowledge of universal and/or necessary laws is not only actual, but necessary for understanding the practice of science. But I shall not here be concerned to rebut such arguments.

I will begin by advancing some general reasons for skepticism regarding the role of supposed laws of nature in science. Then I will outline an alternative interpretive framework which provides a way of understanding the practice of science without attributing to that practice the production or use of laws of nature as typically understood by contemporary philosophers of science. Finally, I will sketch explanations of how some expressions can play a fundamental role in science without being regarded as 'laws,' and how one can even find necessity in nature

1 Among recent philosophers, David Armstrong [1983] seems to me to come closest to the Kantian stance.

without there being 'laws of nature' behind those necessities. I shall thus be offering an interpretation of science even more radical than what David Armstrong once called the 'truly eccentric view … that, although there are regularities in the world, there are no laws of nature' [1983, p.5]. On my interpretation there are both regularities and necessities, but no laws.

2. Historical Considerations

One way of understanding the role that a concept plays in an interpretation of a practice is to examine the *history* of how that concept came to play the role it now has. Through the history one can often see the contingencies that led to that concept's coming to play the role it later assumed and realize that it need not have done so.

Of course there is a standard answer to this sort of historical argumentation. The origins of a concept, it is often said, are one thing; its validity quite another. Philosophy is concerned with the validity of a concept, whatever its origins.[2] But this answer rings somewhat hollow in the present context. It is typically assumed that we need a philosophical analysis of the concept of a law of nature *because* that concept plays an essential role in our understanding of science.[3] Inquiring into how the concept came to play its current role may serve to undercut this presupposition.

2 This answer is an obvious generalization of Reichenbach's [1938, Ch. 1] famous distinction between the contexts of discovery and justification for scientific hypotheses.

3 Armstrong, for example, writes [1983, p. 4]: 'If the discovery of the laws of nature is one of the three great traditional tasks of natural science, then the nature of a law of nature must be a central ontological concern for the philosophy of science.' Similarly, John Earman describes the concept of laws of nature as 'a notion that is fundamental to the study not only of determinism but to the methodology and content of the sciences in general' [1986, p. 81].

Among the characteristics attributed to laws of nature by contemporary philosophers of science, several are especially prominent. Laws of nature, it is typically said, are *true* statements of *universal* form. Many would add that the truths expressed by laws are not merely contingent, but in some appropriate sense *necessary* as well. Finally, laws are typically held to be *objective* in the sense that their existence is independent of their being known, or even thought of, by human agents.[4]

These characteristics, I believe, came to be associated with some scientific claims not simply through reflection on the practice of science, but in large part because of particular circumstances obtaining in Europe in the seventeenth century when modern science began to take the form it now exhibits. Unfortunately, there seem to be few sources that focus directly on this question, and undertaking such a study is beyond both the purposes of this paper and my own expertise. So here I can offer only some suggestions and a few references.[5]

The main sources for the use of 'laws of nature' as a concept to interpret the practice of science are to be found, it seems, in the works of Descartes and then Newton. For both, the laws of nature are prescriptions laid down by God for the behavior of nature.[6] From this premise the predominant characteristics of laws of nature follow as a matter of course. If these laws are prescriptions issued by God the creator of the universe, then of course they are true, hold for the whole universe, are necessary in the sense of absolutely obligatory,[7] and independent of the

4 These are a subset of the assumed characteristics of laws that van Fraassen [1989, p. 38] picks out as pre-eminent.

5 Why the question of the origin of the notion of laws of nature has received so little attention from historians of science is itself a subject for still further speculation. My guess is that the correctness of the idea has been so taken for granted that few have felt the need to inquire into its origins.

6 That our modern use of the concept of 'laws of nature' is directly traceable back to Descartes and Newton, and flowed from their conceptions of the Deity, was argued both by Zilsel [1942] and Needham [1951].

7 Here it is important to observe the medieval distinction between what is necessary for God's creations from what is necessary for the deity itself.

wishes of humans, who are themselves subject not only to God's laws of nature, but to His moral laws as well.

There is at least one place in Newton's writings where this line of reasoning is explicit. In an unpublished draft of Query 31 of the *Optics*, dating from around 1705, Newton draws on his conception of the deity to support the universality of his laws of motion. 'If there be an universal life and all space be the sensorium of a thinking being who by immediate presence perceives all things in it,' he wrote, 'the laws of motion arising from life or will may be of universal extent.'[8] The modesty with which the connection is here asserted was appropriate. What *empirical* evidence did Newton have for the universality of his laws of motion? Only terrestrial motions, such as falling bodies, projectiles, and pendulums, and the motions of the Sun, Moon, planets, and, allowing the investigations of Edmund Halley, perhaps comets as well. The fixed stars posed a definite problem, for what prevented the force of gravity from pulling all the stars together into one place? Newton had need of his God.

Despite some arguments to the contrary, it seems pretty clear that the idea of laws of nature as emanating from the Diety did not originate with Descartes and Newton, or even in the seventeenth century at all.[9] Nor were all earlier uses of such notions

Both Descartes and Newton were 'voluntarists' in that they believed God could have chosen other laws for the world. Descartes notoriously even held that the laws of arithmetic and geometry could have been different if God had so willed.

8 Quoted by Westfall [1971, p. 397]. I owe this reference to Brooke [1991, p. 139]. Chapter VII of Westfall's book, particularly the last five pages, makes a strong case for the role of Newton's conception of the deity in his willingness to abandon direct mechanical interaction in favor of apparent action at a distance.

9 Both Zilsel [1942] and Needham [1951] claimed that the idea of God's laws for nature originated with the rise of powerful centralized governments in the early modern period. Thus Zilsel [1942, p. 258] argues unequivocally that 'the concept of physical law was not known before the seventeenth century' and suggests, more tentatively, that 'the doctrine of universal natural laws of divine origin is possible only in a state with rational statute law and fully developed central sovereignty' [p. 279]. Oakley [1961] objects that the idea existed long before in a theological tradition. Ruby argues that already in the thirteenth century Roger Bacon used the notion in a way that 'resembled that of modern science' [1986, p. 350] [p. 301 in this volume]. The comments which follow are based on my reading of all of the above mentioned authors.

necessarily connected with that of a personified lawgiver. The distinction between divine laws for humans as opposed to laws for the rest of animate or inanimate nature can be traced back at least to Roman thinkers. On the other hand, by the thirteenth century, Roger Bacon seems to have thought of the laws of optics, reflection and refraction, in very much the secular way that became commonplace in the nineteenth. Galileo is famous for his employment of the 'two books' metaphor in which God is portrayed the author of both the Bible and the 'Book of Nature.' But the idea of 'laws of nature' in the sense of Descartes and Newton seems not to have been part of his understanding of the new science. Robert Boyle, who shared many of Newton's theological beliefs, nevertheless urged caution in using the notion of laws of nature on the grounds that, strictly speaking, only moral beings, and not inanimate matter, can appreciate the meaning of laws. One finds similar qualms in the writings of Aquinas.

There is another factor in the story which seems relatively distinct from theological influences, namely, mathematics. Would the concept of laws of nature have gained such currency in the absense of simple mathematical formulae which could be taken express such laws? And do not the qualities of universality and necessity also attach to mathematical relationships? These questions are as difficult as they are relevant. Galileo had the mathematical inspiration, but apparently did not think of the book of nature as containing 'laws.' Kepler, on the other hand, thought of laws in somewhat the same way as Descartes and Newton. Clearly the theological and mathematical influences both push in the same direction. In any case, the one does not exclude the other. Perhaps both were necessary for the notion of a law of nature to have developed at all.[10]

In the end one may still ask why Descartes and Newton were so strongly inclined to interpret various mathematical formulae as expressions of God's laws for nature when thinkers a century earlier or a century later were far less inclined to do so. I would suggest the influence of the bloody religious conflicts exhibited in the Thirty Years War and the English Revolution respectively. These conflicts made it very difficult for anyone in France or England then to think about nature in significant ways without considering the possible role of God.[11] What matters, however, is not which ideas one can find when. At almost any period in history one can find a vast range of ideas existing simultaneously. The important question is which of the variety of ideas available at an earlier period got adopted and transmitted to later periods and shaped later interpretations. Here there can be no serious doubt that for Descartes and Newton the connection between laws of nature and God the creator and lawgiver was explicit. Nor can there be any doubt that it was Newton's conception of science that dominated reflection on the nature of science throughout the eighteenth century and most of the nineteenth as well.[12]

The secularization of the concept of nature's laws proceeded more slowly in England than on the continent of Europe. By the end of the eighteenth century, after the French Revolution, Laplace could boast that he had no need of the 'hypothesis' of God's existence, and Kant had sought to ground the universality

10 I own consideration of the importance of mathematics in this history to conversations with Rose-Mary Sargent. She also pointed out that Boyle's cautions regarding the use of the notion arose partly from his conviction that mathematical relationships abstracted too much from the complexities of nature.

11 Toulmin [1990] has recently emphasized the role of the Thirty Years War on Descartes' thinking.

12 The influence of Newton's conception of science on later British thought hardly needs documenting. For its influence on French Enlightenment thought, see Gay [1969, Book 3, Ch. 3].

and necessity of Newton's laws not in God or nature, but in the constitution of human reason. Comte's positivism found a large audience in France during the middle decades of the nineteenth century. But, in spite of the legacy of Hume, whether the laws of nature might be expressions of divine will was still much debated in the third quarter of the nineteenth century in Britain. Here the issue was whether Darwin's 'law of natural selection' might just be God's way of creating species. Not until Darwin's revolution had worked its way through British intellectual life did the laws of nature get effectively separated from God's will.[13]

It is the secularized version of Newton's interpretation of science that has dominated philosophical understanding of science in the twentieth century. Mill and Russell, and later the Logical Empiricists, employed a conception of scientific laws that was totally divorced from its origins in the theological climate of the seventeenth century. The main issue for most of this century and the last has been what to make of the supposed 'necessity' of laws. Is it merely an artifact of our psychological makeup, as Hume argued, an objective feature of all rational thought, as Kant argued, or embedded in reality itself?

My position, as outlined above, is that the whole notion of 'laws of nature' is very likely an artifact of circumstances obtaining in the seventeenth century. To understand modern science we need not a proper analysis of the concept of a law of nature, but a way of understanding the practice of science that does not simply presuppose that such a concept plays any important role whatsoever.

3. The Status of Purported Laws of Nature

What is the status of claims that are typically cited as 'laws of nature' – Newton's Laws of Motion, the Law of Universal Gravitation, Snell's Law, Ohm's Law, the Second Law of Thermody-

13 For an appreciation of the intensity of these debates see Brooke [1991, Ch. VIII] and Desmond and Moore [1991]

namics, the Law of Natural Selection? Close inspection, I think, reveals that they are neither universal nor necessary – they are not even true.[14]

For simplicity consider the combination of Newton's Laws of Motion plus the Law of Universal Gravitation around the year 1900, before the advent of relativity and the quantum theory. Could one find, for example, any two bodies, anywhere in the universe, whose motions exactly satisfied these laws? The most likely answer is 'no'. The only possibility of Newton's Laws being precisely exemplified by our two bodies would be either if they were alone in the universe with no other bodies whose gravitational force would effect their motions, or if they existed in a perfectly uniform gravitational field. The former possibility is ruled out by the obvious existence of numerous other bodies in the universe; the latter by inhomogeneities in the distribution of matter in the universe. But there are other reasons as well for doubting the precise applicability of the laws. The bodies would have to be perfectly spherical, otherwise they could wobble. They could have no net charge, else electrostatic forces would come into play. And they would of course have to be in 'free space' – no atmosphere of any kind which could produce friction. And so on and on.[15]

Many excuses have been given for not taking more seriously the lesson that, strictly speaking, most purported laws of nature seem clearly to be false. A recent one is that the laws actually discussed by scientists are not the 'real' laws of nature, but at

14 This thesis was argued thirty years ago by Michael Scriven [1961] and more recently by others including Nancy Cartwright [1983, 1989] and myself [1988a]. Even Armstrong [1983, pp. 6-7] and Earman [1986, pp. 80-81] admit the strict falsity of the traditional examples of laws.

15 For a more extended discussion of the strict falsity of the law of the pendulum see [Giere, 1988a, pp. 76-78]. That classical mechanics has be superseded by relativity theory and quantum mechanics does not materially change the argument. Cartwright [1983, 1989] provides examples from quantum theory. Similar examples could be developed for relativity theory as well.

best 'near' laws.[16] Here I wish only to examine a view that does take the lesson seriously, but remains still too close to the traditional view. This is the view, developed by Coffa [1973] and Hempel [1988], that laws are expressed not by simple universal statements, but by statements including an implicit 'proviso.' As I understand it, Coffa's and Hempel's account is that purported statements of laws of nature of the form 'All bodies, ..., etc.' are to be interpreted as really of the form 'All bodies, ..., etc., with the proviso that ...' My objection to this interpretation is that it is impossible to fill in the proviso so as to make the resulting statement true without rendering it vacuous.

This problem is particularly evident in cases where the implicit proviso must be understood to be expressed in concepts that are not even known at the time the law containing the implicit proviso is first formulated. Thus most of the laws of mechanics as understood by Newton would have to be understood as containing the proviso that none of the bodies in question is carrying a net charge while moving in a magnetic field. That is not a proviso that Newton himself could possibly have formulated, but it would have to be understood as being regularly invoked by physicists working a century or more later.[17] I take it to be a *prima facie* principle for interpreting human practices that we do not attribute to participants claims that they could not even have formulated, let alone believed.

It is important to realize that my objection is not just that the proviso account introduces indefiniteness into our interpretation of science. One of the major lessons of post-positivist philosophy of science is that no interpretation of science can make everything explicit. Important aspects of the practice of science must remain implicit. The issue is where, in our interpretation of science, we locate the unavoidable indefiniteness. The proviso account locates indefiniteness right in the formulation of what, on that account, are the most important carriers of the

16 For an elaboration of this view see Swartz [1985] and Swartz in this volume [Ch. II. pp. 67-91].

17 For a more extended development of this objection see [Giere, 1988b].

content of science, namely, its laws. I think a more faithful interpretation would locate the indefiniteness more within the practice of science and leave its products, including its public claims to knowledge, relatively more explicit.[18]

4. Models and Restricted Generalizations

Let us return to the example of Newton's equations of motion together with his equation for the force of gravity between two bodies. My reference here to Newton's *equations* of motion rather than his *laws* of motion is deliberate. Everyone agrees that Newton used these equations. The issue is how to interpret them, whether as 'laws,' which was Newton's interpretation, or as something else.

Interpreting the equations as laws assumes that the various terms have empirical meaning and that there is an implicit universal quantifier out front. Then the connection to the world is relatively direct. The resulting statement is assumed to be either true or false.

On my alternative interpretation, the relationship between the equations and the world is *indirect*. We need not initially presume either a universal quantifier or empirical meaning. Rather, the expressions need initially only be given a relatively abstract meaning, such as that m refers to something called the mass of a body and v to its velocity at a specified instant of time, t. The equations can then be used to construct a vast array of abstract mechanical systems, for example, a two-body system subject only to mutual gravitational attraction. I call such an abstract system a *model*. By stipulation, the equations of motion describe the behavior of the model with perfect accuracy. We can say that the equations are exemplified by the model or, if we wish, that the equations are true, even *necessarily* true, for the model. For models, truth, even necessity, comes cheap.

18 Cartwright [1983] holds the superficially similar view that lower level laws, such as Snell's Law, are to be understood as *ceteris paribus* laws of the form: 'Everything else being equal, ..., etc.' But she does not claim that such laws are true, only that they are explanatory in a way not compatible with a covering law model of explanation. I would prefer a more radical interpretation that does away with law talk even though this departs from the way scientists themselves often present their science. I think this can provide us (philosophers) with a better understanding of what they (scientists) are doing.

The connection to the world is a provided by a complex relationship between a model and an identifiable system in the real world. For example, the earth and the moon may be identified as empirical bodies corresponding to the abstract bodies in the model. The mass of the body labeled m_1 in the model may be identified with the mass of the earth while the distance r in the model is identified with the distance between the center of the earth and the center of the moon. And so on. Then the behavior of the model provides a representation of the behavior of the real earth-moon system. For the purposes of understanding the relationship by which the model represents the real system, the concept of truth is of little value. A model, being an abstract object rather than something linguistic, cannot literally be true or false. We need another sort of relationship altogether.

Some friends of models invoke *isomorphism*, which is at least the right kind of relationship.[19] But isomorphism is too strong. The same considerations that show the strict falsity of presumed universal laws argue for the general failure of complete isomorphism between scientific models and real world systems. Rather, models need only be similar to particular real world systems in specified respects and to limited degrees of accuracy. The question for a model is how well it 'fits' various real world systems one is trying to represent. One can admit that no model fits the world perfectly in all respects while insisting that, for specified real world systems, some models clearly fit better than others. The better fitting models may represent more aspects

19 Van Fraassen [1980, 1989], for example, defines scientific realism as that one of a family of models is exactly isomorphic with the system it is intended to represent. I have objected [Giere, 1985; 1988a, Ch. 4] that this is too strong a requirement for a reasonable realism.

of the real world or fit some aspects more accurately, or both. In any case, 'fit' is not simply a relationship between a model and the world. It requires a specification of which aspects of the world are important to represent and, for those aspects, how close a fit is desirable.

In this picture of science the primary representational relationship is between individual models and particular real systems, e.g., between a Newtonian model of a two-body gravitational system and the Earth-Moon system. But similar models may be developed for the Earth-Sun system, the Jupiter-Io system, the Jupiter-Sun system, the Venus-Sun system, and so on. Here we have not a universal law, but the *restricted generalization* that various pairs of objects in the solar system may be represented by a Newtonian two-body gravitational model of a specified type. Restricted generalizations have not the form of a universal statement plus a proviso, but of a conjunction listing the systems, or kinds of systems, that may successfully be modeled using the theoretical resources in question, which, in our example, are Newton's equations of motion and the formula for gravitational attraction.

Other pairs of objects in the solar system cannot be well represented by the same sort of model, the Earth-Venus system, for example. Moreover, although one could in principle construct a single Newtonian model for all the planets together with the sun, the resulting equations of motion are not solvable by any known analytical methods. One cannot even solve the equations of motion for a three-body gravitational system, one intended to represent the Earth-Jupiter-Sun system, for example. Here one must approximate, for example, by treating the influence of the Earth as a perturbation on the motion of the two-body Jupiter-Sun system. Such approximative techniques have been part of Newtonian practice since Newton himself, but have been largely ignored by the tradition that interprets Newton's equations of motion as expressing universal laws of nature.

It is typically said to be a major part of Newton's success that he 'unified' the behavior of celestial and terrestrial bodies.

The equations of motion used to build models of the Jupiter-Sun system may also be used to construct models to represent the behavior of balls rolling down an inclined plane, pendulums, and cannon balls. This was a considerable achievement indeed, but it hardly elevates his equations of motion to universal laws. It had yet to be shown that similar models could capture the comings and goings of comets, and the fixed stars were beyond anyone's reach. What Newton had in 1687 were not God's all encompassing laws for nature, but a broad, though still restricted, generalization about some kinds of systems that could be modeled using the resources he had developed. That he had fathomed God's plan for the universe was an interpretation imported from theology.

5. Principles versus Laws

It may reasonably be objected that focusing simply on Newton's equations of motion does not do justice their role in the science of mechanics. They seem somehow to capture something fundamental about the structure of the world. One might express similar feelings about the Schrödinger equation in quantum mechanics. The problem is to capture this aspect of these fundamental equations without lapsing back into the language of universal laws.

An interpretative device that has considerable historical precedent would be to speak of Newton's *Principles of Motion* and the *Principle of Gravitational Attraction*. The title of his book, after all, translates as *The Mathematical Principles of Natural Philosophy*.[20] Whether or not thinkers in the seventeenth, or even eighteenth, century recognized any significant distinction between 'laws' and 'principles,' we can make use of the linguistic variation. Principles, I suggest, should be understood as rules devised by humans to be used in building models to

20 Recall also that Descartes' main work in natural philosophy was titled *The Principles of Philosophy*.

represent specific aspects of the natural world. Thus Newton's principles of mechanics are to be thought of as rules for the construction of models to represent mechanical systems, from comets to pendulums. The rules instruct one to locate the relevant masses and forces, and then to equate the product of the mass and acceleration of each body with the force impressed upon it. With luck one can solve the resulting equations of motion for the positions of the bodies as a function time elapsed from an arbitrarily designated initial time.

What one learns about the world is not general truths about the relationship between mass, force, and acceleration, but that the motions of a vast variety of real world systems can be successfully represented by models constructed according to Newton's principles of motion. And here 'successful representation' does not imply an exact fit, but at most a fit within the limits of what can be detected using existing experimental techniques. The fact that so many different kinds of physical systems can be so represented is enough to justify the high regard these principles have enjoyed for three hundred years. Interpreting them as universal laws laid down by God or Nature is not at all required.[21]

21 It is worth noting that in the twentieth century the expression 'principle of relativity' has had considerable currency, as in the title of the well-known collection of fundamental papers by Einstein and others [Einstein *et. al.* 1923]. Einstein himself [1934] distinguished between what he called 'constructive' theories and 'principle' theories. The special theory of relativity, he claimed, was of this latter type. One of its principles is the 'principle of the constancy of light in vacuo' [1934, p. 56]. I doubt, however, that Einstein's intent corresponds to my own, given that he describes the advantages of principle theories over constructive as being 'logical perfection and security of the foundations' [p. 54]. He seems to think of his principles' as expressing deep general truths about the world, and, like Newton, draws on religious, though not theological, inspiration.

6. Necessity Without Laws

Traditionally it has been the supposed universality of laws of nature that has seemed to require their necessity. For, as Kant argued, how could a universal association be just a regularity? For an association to be truly universal, he thought, there must be something making it be so. Thus, denying the existence of genuine universal laws in nature makes it possible to deny the existence of necessity as well. But such denial is not required. It is also possible to deny the existence of universal laws of nature while affirming the existence of causal necessities.[22]

Consider a model of a harmonically driven pendulum of the sort that one would use to represent the motion of a pendulum on a typical pendulum clock. Solving the classical equations of motion for the period as a function of length (assuming that the angle of swing, θ, is sufficiently small that $\cos\theta \approx 1$) yields the familiar result that the period is proportional to the square root of the length. Now this model provides us with a range of possible periods corresponding to various possible lengths. These possibilities are built into the model. But what of the real world?

Suppose the actual length of the pendulum on my grandfather clock is L. The model permits us to calculate the period, T. It also permits us to calculate a slightly greater period T' corresponding to a slightly greater length, L'. Suppose the clock is running slightly fast. I claim that turning the adjusting screw one turn counter-clockwise would increase the length of the pendulum to L', and this would increase the period to T', so that the clock would run slightly slower. This seems to be a claim not about the model but about the real life clock in my living room. Moreover, it seems that this claim could be true of the real life clock even if no one ever again touches the adjusting screw. These possibilities, it seems, are in the real physical system, and are not just features of our model.

22 These two positions are represented by van Fraassen [1980, 1989] and Cartwright [1983, 1989] respectively.

There are, of course, many arguments against such a realistic interpretation of modal (causal) claims. Here I will consider only the empiricist argument that there can be no evidence for the modal claim that is not just evidence for the regularity relating length and period for pendulums. The inference to possibilities, it is claimed, is an unwarranted metaphysical leap. Moreover, I will not try to argue that this empiricist interpretation is mistaken; only that it is no less metaphysical than the opposing view.

I claim that by experimenting with various changes in length and observing changes in period one can effectively *sample* the possibilities that the model suggests may exist in the real system. That provides a basis for the conclusion that these possibilities are real and have roughly the structure found in the model. The empiricist argument is that the most one can observe is the actual relationship between length and period for an actual series of trials with slightly different initial conditions. So the issue is whether experimentation can reveal real possibilities in a system or merely produce actual regularities in a series of trials. Whichever interpretation one favors, one cannot claim that the latter interpretation is somehow less metaphysical than the former. It is just a different metaphysical view. I think the modal realist interpretation provides a far better understanding of the practice of science, but that is not something one can demonstrate in a few lines, or even a whole paper.

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The Middle Ground: Resiliency and Laws in the Web of Belief

1. Introduction

In the final section of "Two Dogmas of Empiricism" – entitled "Empiricism without the Dogmas" Quine paints a striking picture of the pragmatics of belief:

> The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric which impinges on experience only along the edges. Or, to change the figure, total science is like a field of force, whose boundary conditions are experience. A conflict with experience at the periphery occasions readjustments in the interior of the field. Truth values have to be redistributed over some of our statements. Reevaluation of some statements entails reevaluation of others, because of their logical interconnections – the logical laws being in turn simply further statements of the system, certain further elements of the field. Having reevaluated some statements we must reevaluate some others, which may be statements logically connected with the first or may be statements of logical connections themselves. But the total field is so underdetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to reevaluate in the light of any single contrary experience.

But however attractive this picture may be, Quine does not offer any methodology for modeling and mapping the networks of belief and representing the place of laws within them.

We believe that the best framework for a precise realization of these ideas is the theory of personal probability. The question then arises how to map a network of degrees of belief in a way which reveals the weak and strong resistances to disconfir-

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