CHAPTER SEVEN

There are no Laws of the Social Sciences

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7.1 The Significance of the Question

What is at stake in the question of whether there are laws of the social sciences? It is tempting to suppose that what is at stake is something about the nature of human beings and human history; for example, whether our history proceeds according to an inevitable, predetermined course. But this would be a mistake. The existence of laws in a given domain is consistent with indeterminism in that domain (Earman, 1986). Moreover, the nonexistence of social-scientific laws would not guarantee indeterminism, for it could be that all human events are predetermined by laws of nature that can be articulated only in microphysical terms, and not in terms of concepts of the social sciences. Whether there are or are not laws of the social sciences, we cannot infer much about the nature of human history from this fact alone.

A different answer is that what is at stake is whether the social sciences really deserve the name "science." If the social sciences do not, or cannot, discover laws, then perhaps they should not be considered truly scientific. Why should we believe this? A tempting line of thought is the following: Science is about prediction and explanation. Phenomena are explained by subsuming them under laws of nature, and they are predicted by showing that, given the current conditions and the laws of nature, such-and-such is bound to happen (Hempel and Oppenheim, 1948). Hence, a field of study can only perform the tasks of science if it is capable of discovering laws.

The view of science presupposed by this answer has been influential in the history of the philosophy of science. But it is overly simplistic, and ought to be rejected. For one thing, it is far from clear that any good scientific explanation must cite laws. Over the past few decades, philosophers of science have proposed a variety of models of explanation according to which this is not so. Salmon (1994) proposes a model according to which explanation involves locating the event to be explained in a

network of causal processes and interactions, and need not involve citing any laws; Kitcher (1981) argues that explanation involves showing how to unify a large body of phenomena by bringing a unified set of argument-patterns to bear on it, where these patterns need not appeal to laws; van Fraassen (1980) defends a pragmatic theory of explanation in which laws play no special role. Moreover, it is not at all clear that the other characteristic activities of science, such as prediction, can only be performed by a field of study that recognizes laws of nature; for arguments that this is not so, see van Fraassen (1989) and Giere (1999). It seems hard to deny that in order to make predictions, one needs reliable information about regularities, or causal mechanisms, or statistical distributions, with relevance for future occurrences. But even if this is so, it needn't follow that *laws* are required, for the concept of a law of nature involves more than this (for more on what is distinctive about laws, see section 7.2).

Why, then, does it seem so tempting (and why, historically, have so many been tempted) to suppose that a field of study cannot count as science unless it discovers laws? I suspect the answer is that there has traditionally been a strong tendency to think of physics, which appears to be strongly focused on discovering laws of nature, as the very paradigm of a science. Other fields of study get to count as sciences only to the extent that they emulate the methods, theory forms, and successes of physics. To give in to this temptation is to put oneself in the difficult position of having to say, for every putative branch of science, either that its theories, hypotheses, and results can be forced into the pigeonhole provided by the model of physics, or else that it does not really qualify as scientific. And a careful examination of contemporary biology and psychology, and much of the social sciences as well, shows how difficult it is to do the first, and how appalling it would be to do the second. It is of course very difficult to say with any precision what makes a field of study scientific, but a reasonable starting point would be the idea that a discipline can qualify as a science if it: has certain general aims, including understanding and prediction of phenomena; uses certain methodologies, including careful observation, statistical analysis, the requirement of repeatability of results, and perhaps controlled experimentation; and has certain forms of institutional organization, including distribution of intellectual authority across a large body of scientists who are (at least nominally!) not beholden to political, religious, or other extra-scientific authorities with respect to the content of their scientific opinions. By this kind of standard, much work in the social sciences deserves to be called scientific (see Kincaid, 1996). It does not immediately follow, and should not be decided in advance, that any field of study that measures up according to this kind of standard must produce hypotheses and theories that mimic the typical form of fundamental theories in physics – in other words, hypotheses and theories that posit laws of nature (among other things). Hence, it should remain an open question whether the social sciences must discover laws (or at least putative laws) in order to qualify as scientific.

One can assume a demarcation criterion according to which discovering laws is a necessary condition for being scientific, and then ask whether there are social-scientific laws in order to assess the so-called social "sciences." Or, one can assume that at least some work in the social sciences is genuinely scientific, and then ask whether there are social-scientific laws in order to assess the aforementioned demar-

cation criterion. The view to be defended here is that we should go the second route. There are no social-scientific laws, I shall argue, and what follows from this is not that the social sciences aren't really sciences, but that any view of science that entails that science is essentially about discovering laws must be rejected.

7.2 What is a Law?

Before we can come to grips with the question of whether there are laws of the social sciences, we need to have some idea of what a law is. Unfortunately, no answer is possible that is simultaneously informative and uncontroversial. The concept of a law of nature has proven terribly difficult to explicate, and the literature on laws is rife with disagreement (for some of the controversy, see Swoyer, 1982; Armstrong, 1983; Earman, 1986; Carroll, 1994; Lewis, 1994; Cartwright, 1999). In this section, I will present a working characterization of laws that is more or less uncontroversial, though not terribly deep. Fortunately, it will be sufficient to inform the argument below.

To begin with, I shall assume that there are such things as laws of nature, and that physics, at least, is engaged in the project of trying to discover laws. This view can be supported by noting that the succession of fundamental physical theories since Newton is a succession of theories positing laws of nature, and each of these theories enjoyed considerable empirical success for a while. Classical mechanics posited Newton's three laws of motion and various special-force laws such as the law of universal gravitation and Hooke's law. Classical electromagnetism posited Coulomb's law, the Biot–Savart law, the Lorentz force law, and Maxwell's equations, the latter being standardly interpreted as laws even though the word "law" does not occur in their names. More recently, it has been less common to use the word "law" in names of fundamental physical principles, but Einstein's field equations are commonly described as the laws of general relativity, and the Schrödinger equation as the basic law of (nonrelativistic) quantum mechanics.

To be sure, there are philosophers of science who have argued that this is a misinterpretation, that neither physics nor any other science should be understood as a search for the laws of nature, and that there are probably not any such things as laws of nature anyway (van Fraassen, 1989; Giere, 1999). I think these arguments fail, but for present purposes, it isn't necessary to refute them. For if there are no laws, then it follows trivially that there are no laws of the social sciences. There is an interesting question here that is specifically about the social sciences only if the physical sciences, at least, *can* discover laws.

So, what is a law of nature? To begin with, laws of nature are closely related to regularities. Some philosophers maintain that laws are regularities of a certain sort (Lewis, 1994; Earman, 1986, ch. 6), while others deny that they *are* regularities but insist that they *entail* regularities (Dretske, 1977; Tooley, 1977; Swoyer, 1982; Armstrong, 1983). We can distinguish three kinds of regularities. First, there are strict regularities: universally quantified conditionals, holding throughout the universe – an example is "Anything made of copper conducts electricity." Secondly, there are statistical or probabilistic regularities, characterizing an unrestricted domain; for example, "Any atom of uranium-238 has a probability of 0.5 of decaying within any

time-interval of 4.5 billion years."[1] Thirdly, there are what might be called "hedged regularities," which are sometimes called "*ceteris paribus* regularities."[2] These are (statistical or nonstatistical) regularities that are qualified by admitting that they have exceptions in various circumstances, and these circumstances are not made explicit but are given the label "disturbances" or "interferences."[3] Most philosophers who have written on the topic of laws of nature allow that laws may be (or may entail) either strict or statistical regularities. Some allow that laws may also be (or entail) hedged regularities (Armstrong, 1983; Fodor, 1991; Pietroski and Rey, 1995; Lipton, 1999; Morreau, 1999). Others have argued that *all* laws of nature are (or entail), at best, hedged regularities (Cartwright, 1983; Lange, 1993).

Almost everyone agrees, however, that just being a regularity is not sufficient for being a law of nature (but for the contrary view, see Swartz 1985). It seems that there are more regularities than there are laws. For example, consider the proposition that every sphere of solid gold has a diameter of less than one kilometer. This has the form of a strict regularity, and it is quite plausible that it is true. (If it turns out not to be, then presumably, we can just substitute some larger diameter and make it true.) But it is implausible to suppose that this proposition states (or is entailed by) a law of nature. No law that we have any inkling of rules out gargantuan spheres of gold; it just happens to be the case that there are no such spheres. More generally, a regularity (strict, statistical, or (perhaps) even hedged) can be true "just by accident," because of the way the world happens to be as a matter of brute fact. So there is more to being a law than just being a regularity.

Exactly what it takes to be a law, beyond being (or entailing) a regularity, is a matter about which there is wide disagreement. But a couple of things are uncontroversial. First, laws are logically and mathematically contingent, and can only be known *a posteriori*.[4] Secondly, laws have what John Carroll (1994) calls a "modal character." They "govern" the course of events, in that they constrain the scope of what is physically or naturally possible. The cash-value of this idea includes at least the fact that laws have a certain explanatory power and a certain counterfactual robustness. The former means that a law is a suitable general principle to appeal to in order to explain particular phenomena.[5] The latter means that in reasoning about

1 Examples of statistical regularities with restricted domains include "Three-quarters of the socks I now own are black" and "30 percent of American adults are cigarette smokers." (I do not know whether either of these is true.) I am excluding such regularities from consideration here, because they do not seem to have the kind of generality required by a law of nature; rather, they are descriptions of local conditions that are effects of a number of contingent causes.

2 As many writers have noted, the literal meaning of "*ceteris paribus*" – other things being equal – is not exactly appropriate here; "there being no interfering or disturbing conditions" would be better. For this reason, I will stick to the term "hedged." The topic of hedged regularities will be explored further below.

3 If these conditions were made explicit, then we would really have a strict or statistical law. For example, if we are told that all F's are G's, unless there is an interference, and every interference is of type H, then we have the strict regularity: all F's not in the presence of H are G's.

4 This is distinct from the somewhat more controversial claim that laws are *metaphysically* contingent. See Swoyer (1982) for an argument that laws are metaphysically necessary though knowable only *a posteriori*.

5 Of course, it is important to distinguish this claim from the very strong claim that scientific explanation always must appeal to a law.

hypothetical, counterfactual situations, we tend to hold constant the laws of the actual world, and it is reasonable for us to do so.

How to account for this "modal character" is a matter of dispute. In the early modern period, natural philosophers such as Boyle and Newton maintained that the laws represented free legislative decrees of God, which could not be violated because they were backed by the divine will. Much more recently, many philosophers have argued that the modal character of laws can only be accounted for by supposing laws to be facts of a fundamentally different kind than facts about the actual antics of particular natural objects, and regularities and patterns in these antics. For the latter concern only what happens to be the case in the actual world, and as such have no implications for what would have happened had circumstances been different, or for the ultimate reasons why things are as they are. Since laws are expected to have such implications, they must be a fundamentally different kind of feature of the world – such as relations among universals (Dretske, 1977; Tooley, 1977; Armstrong, 1983), the essences of natural properties (Swoyer, 1982), the natures and capacities of kinds of systems (Cartwright, 1983, 1999), or irreducible, *sui generis* modal principles (Carroll, 1994). Philosophers of a more Humean bent, on the other hand, have argued that the "modal character" of laws can be adequately accounted for by supposing them to be regularities or patterns in the actual course of events that are among the world's deepest or most pervasive structural features. (There is more than one way to work out the details here; Skyrms (1980) describes one, and Lewis (1994) another.) Such regularities have explanatory and counterfactual import, according to this view, not because they have metaphysical natures that confer such import on them but, rather, because our practices of explanation and counterfactual reasoning place great importance on such pervasive structural aspects of the world. All parties to the dispute, however, agree that any successful account of the nature of laws must provide some explanation of what it is that gives laws their peculiar significance for explanation and counterfactual reasoning.

It might be thought that the question of whether there are social-scientific laws should be settled by consulting the most successful contemporary theories from the social sciences, and seeing whether or not they say that anything is a law. It is important to see why this is not a promising way to settle the issue. Being *called* a law by scientists, or being given a name with the word "law" in it (e.g., "the law of universal gravitation"), is neither necessary nor sufficient for being a law. Schrödinger's equation is not called "Schrödinger's law," and textbooks on quantum mechanics do not always explicitly call it a law. However, it plays a role within quantum mechanics that is analogous to the role played in classical mechanics by Newton's laws of motion, and for this reason it seems to deserve to be thought of as a law. On the other hand, many things commonly called "laws" by scientists are actually mathematical truths and so, unlike laws of nature, they are necessary and *a priori*. (One example is the Hardy–Weinberg law in population genetics.) Thus scientists do not use the word "law" uniformly to mark the distinction that we are interested in here. This is no cause for blame; the purposes of scientists typically do not require the use of a standard term for marking this distinction. The upshot is that deciding what, according to a given scientific theory, is a law and what is not is a task that requires some philosophical interpretation.

The interpretation required can be subtle. It is tempting to suppose that any general principle or regularity that is true, logically contingent, and plays a role in explanation and counterfactual reasoning counts as a law of nature. Surely (one might think) the social sciences discover such general principles; for social scientists engage in explanation and their explanations often make appeal to general regularities. So how could anyone deny that there are laws of the social sciences? The problem with this quick argument is that while it is plausible that every law is a logically contingent general principle that plays an important role in explanation and counterfactual reasoning, it does not follow that every such principle is a law, and there are reasons to doubt that this is so. Consider the following regularity: "All seawater is salty." It supports counterfactuals: If the glass from which I am now drinking had been filled with water taken straight out of the Atlantic, I would be making an awful face right now. It can play a role in explanation; for example, in explaining why the oceans are populated with organisms with certain physiological characteristics. But it would be very odd to call this regularity a law of nature (or even a law of geology); surely it is just a contingent fact resulting from what the initial conditions of the earth just happened to be, and these conditions could have been otherwise without there being any violation of the laws of nature.

This general principle about seawater is not a law because it is too fragile. It would be upset by differences in the contingent facts about our world that need not imply any violation of the laws of nature. The question of whether there are social-scientific laws depends on whether there exist, within the realm of the social sciences, principles that are robust enough not to count as nonlaws for the same reason that our regularity concerning seawater does, and at the same time logically contingent. Before attacking this question, though, it still needs further clarification.[6]

7.3 Distinguishing some Questions

The question of whether there are any laws of the social sciences is ambiguous, and needs to be sharpened up. To see this, consider the analogous question: "Are there

6 The preceding discussion might suggest that what it takes for a proposition to be a law, over and above it's being a logically contingent generalization and playing an important role in explanation and counterfactual reasoning, is *nonfragility* – that is, the property of being such that it would still be true even if circumstances differed in some nomologically possible way. This is not a view that I would endorse, however. For one thing, it would be viciously circular if proposed as a definition of "law of nature," since it uses the phrase "nomologically contingent," which just means "consistent with, but not entailed by, the laws of nature." For another, this kind of fragility is no doubt something that comes in degrees, so that if we adopted this view, we would be committed to the view that lawhood comes in degrees. Many philosophers would be happy with this result (see, e.g., Lange, 2000). But it seems to me that the laws that have lawhood "to the highest degree" – the fundamental laws of physics – play roles within the practice of physics that are so special and so important (I have in mind the roles played by laws in characterizing symmetry principles and in constraining the probability measures used in statistical mechanics) that it makes sense to reserve the term "law" for them. This is admittedly a terminological preference, and the arguments to follow do not depend on it: Even if we agree to use the term "law" in a broad sense, in which it is correct to speak of degrees of lawhood, the arguments of sections 4–6 show that there cannot be laws (of any degree) in the social sciences.

laws of physics?" A quick and obvious answer is: "Yes, of course: There are Newton's second law of motion, Coulomb's law, Boyle's law, and so on." But now consider a follow-up question: "Were there any laws of physics one million years ago?" None of the aforementioned laws had been thought up one million years ago, which tempts one to answer negatively. But a negative answer implies that laws of physics have come into being because of the evolution and activities of human beings – which seems outrageous! But there is an easy resolution: The question "Are there laws of physics?" is ambiguous between:

(a) "Have physicists discovered any laws?"

and

(b) "Are there, really, any laws that are within the subject matter of physics?"

These questions are in the present tense, but they can be put into the past tense. Our temptation to say that there were no laws of physics one million years ago is explained by our taking the question to be the past-tense version of (a), and the intuition that this could not be right comes from thinking of the question as the past-tense version of (b). Once the question is disambiguated, there is no problem.

That easy resolution is not quite good enough, though, as we can see by noting that all of the "laws" of physics given as examples above are no longer thought to be (universally) true. So citing them does not suffice for an affirmative answer to (a). It would be nice if we could justify an affirmative answer to (a) by citing more up-to-date laws, but alas, things are not so nice. Which up-to-date laws would we cite? Those of quantum field theory? Those of general relativity? Each has a good claim, but they could not both be right, since quantum field theory and general relativity are incompatible with each other. Physicists and physics fans hope that one day, we will be able to answer (a) affirmatively by citing "real" laws of physics. But we are not there yet, and it is difficult to give any reason – other than pious optimism – for believing that we ever will be. This makes it look as if we are not really justified in giving affirmative answers to either (a) or (b). Yet, a glance at the historical development of modern physics makes it hard to deny that physics has something important to do with laws. To do justice to this consideration, we can distinguish a third sense of the question "Are there laws of physics?":

(c) Do the successful theories of physics posit laws?

Here, "successful" means counting as successful by the evidential standards of science; a theory need not be true to be successful, and a theory that is successful can nonetheless be overturned by a later theory if the latter is even more successful. The answer to (c) is clearly affirmative. A sensible conclusion, then, would be that if we want to know whether there are laws of physics, we need to get more precise about what we mean. If we mean (a), then the answer is "Probably not yet, but maybe some day." If we mean (b), then the answer is "We do not really know yet." If we mean (c), then

the answer is "Yes, certainly." Which answer we get thus depends crucially on the way in which we disambiguate the question.

From these considerations, we should learn that what answer we give to the question "Are there laws of the social sciences?" depends on how we disambiguate that question. Again, we have three options:

(A) Have the social sciences discovered any laws?
(B) Are there really any laws within the subject matter of the social sciences?
(C) Do the successful theories of the social sciences posit laws?

To anticipate, I will argue for the following: We have pretty good reasons for thinking that the answer to (B) is negative. The empirically successful theories of the social sciences can, generally, be plausibly interpreted as not positing any laws, and since we have good reason to believe that the answer to (B) is "No," it is more charitable to interpret social-scientific theories as not positing laws. Hence, we should so interpret them, and so we should answer (C) in the negative. (The reason why interpretation is needed to answer (C) was explained above: to find out whether a scientist has posited a law, it is not sufficient to look and see whether and how she uses the word "law.") It follows straightforwardly that the answer to (A) is negative as well.

Question (B) concerns laws that are "within the subject matter of the social sciences" – What does that mean, exactly? It is hard to give a satisfying positive answer, but it is not hard to give examples of things that would clearly not count. For example, suppose that it is a law of nature that every social class is such that the bodies of all of its members move in such a way that their collective center of mass has an acceleration proportional to the sum of all forces acting on the bodies of its members. This law concerns social classes, which fall within the subject matter of sociology, but clearly the law itself is not a law of sociology: It is a special case of a law of physics, restricted to physical objects that are picked out using a social-scientific concept. Suppose, for the sake of argument, that it is a law of biology that the process of respiration always includes the Krebs cycle. Then it is arguably a derivative law that all heads of state undergo respiration that includes the Krebs cycle. But this is just a rather arbitrarily restricted special case of a biological law, rather than a law of political science, even though it is formulated using the term "head of state." In each case, the putative "law" is about some kind of thing that falls within the subject mater of a social science, but the law itself holds in virtue of some fact in the subject matter of the natural sciences. Speaking roughly and intuitively, it is not because something is a social class that it obeys the first law mentioned above; rather, it is because it is a set of objects with physical properties. It is not because someone is a head of state that he or she obeys the second law just mentioned; it is because he or she is a biological organism. For a law to be a law within the subject matter of the social sciences, it must be a law that applies to the kinds of things studied by the social sciences *because of* their properties or natures *qua* social entities. This is a crudely stated principle, and it could use a more careful and detailed explication. But it will serve well enough for the purposes of the discussion to follow.

7.4 The Case against Social Laws

Let us introduce the term "social laws," to refer to laws wholly within the subject matter of the social sciences. Then the negative answer to question (B) can be expressed by denying that there are any social laws. This denial, I claim, is supported by the following argument:

(i) If there are any social laws, then they are hedged laws.
(ii) There are no hedged laws.

Therefore, there are no social laws.

By "hedged law," I mean a law that is, or entails, only a hedged regularity (in the sense described in section 7.2), rather than a strict or statistical regularity. The argument is sound if both of its premises are true. In section 7.5, I will argue that premise (i) is true, and in section 7.6, I will argue that premise (ii) is true.

7.5 Why Social Laws must be Hedged

It will help to start by considering an example. One familiar putative example of a social-scientific law comes from economics – the law of supply and demand:

> If the supply of a commodity increases (decreases) while the demand for it stays the same, the price decreases (increases); if the demand for a commodity increases (decreases) while the supply remains the same, the price increases (decreases).

(There are more careful ways of formulating this putative law – see Harold Kincaid's companion chapter (chapter 8, this volume) for more details – but the remarks to follow would apply equally well to a more careful formulation.) This claim is not a bad rule of thumb for predicting and explaining various economic phenomena. But there are numerous kinds of cases in which it will be false. These include cases in which the government imposes price controls, in which either vendors or consumers are ignorant about changes in supply or demand, in which there is widespread irrationality on the part of either vendors or consumers, in which humanitarian feelings on the part of many vendors motivate them not to raise prices on goods, such as medical supplies, for which there is a pressing need, and so on. So the law cannot be a strict law. One might hold out hope of reformulating it as a statistical law, but in order to do this, one would need to find probabilities of all the kinds of phenomena that can lead to violations. Since these phenomena are so diverse and can have such diverse causes, this seems hopeless. One would need to find the probability of, for example, an outbreak of a terrible disease; one would then need to find the conditional probability that humanitarian concerns would motivate vendors of medical supplies not to increase prices, were such an outbreak to occur; and so on. It seems that, at best, the law of supply and demand is a hedged law. The regularity it describes holds, unless for some reason it does not, and we cannot specify all of the possible conditions under which it will not.

It might be objected that the last claim is false: We *can* specify the class of conditions under which the law of supply and demand will hold, and the class of conditions under which it will not. Since we can do this, we can reformulate the law as a strict law, simply by appending "As long as conditions C obtain . . ." to the front of the statement of the law, where C stands for the set of conditions under which it will not be violated. What would conditions C be? They would have to be sufficient to rule out *all* factors that can interfere with the working of the market in such a way as to disturb the regularities posited by the law of supply and demand. The list of disturbing factors offered in the preceding paragraph was only a start. Who knows how many others there may be? Consider that the regularities described by the putative law are regularities in the mass behavior of groups of human beings; psychological quirks, external pressures of a noneconomic variety, failures of communication, natural disasters, cultural norms, and cultural conflicts can all potentially have a disturbing influence on the regularities in this behavior. But in many cases, such factors will not disturb these regularities. The problem is to characterize the class of possible conditions that will disturb the regularities, so that we can state the law in a form that applies only to the others. We could do this by defining conditions C as those conditions in which there are no factors of any kind that result in violations of the regularities described in the law of supply and demand. But if we do this, then we render this putative law a tautology: "These regularities will obtain unless for some reason they do not."[7] But short of doing this, there seems to be no way of adequately characterizing the conditions C; the range of possible interfering factors is indefinitely large and indefinitely varied. It seems that the law of supply and demand can only be a hedged law.

The issues that we have encountered in this examination of a single example illustrate a more general characteristic of social phenomena, which strongly suggests that there are no nonhedged laws to be found in the social realm. The kinds of system that are studied by the social sciences – markets, states, social classes, political movements, and so on – are *multiply realizable*. Every social system is constituted by a large and complex physical system. This is because a social system is made of a group of humans and their environment, which is in turn constituted ultimately out of physical parts, perhaps elementary particles. (There is an interesting question about what exactly the relation of constitution is here. Is a social system *identical* with the system of physical particles out of which it is constituted? Or is it a distinct thing that supervenes on the latter? Or what? Fortunately, we need not settle this issue here. I will assume, however, that a social system *supervenes* on the large and complex physical system out of which it is composed, in the sense that there could not be a difference in the social system without a physical difference somewhere in the physical system that constitutes it.) The multiple realizability of a kind of system studied by the social sciences is the fact that there is a large and heterogeneous class of kinds of physical systems that could constitute a social system of a given kind. An example due to John Searle (1984) is *money*; there is a huge class of kinds of physical objects that

7 You might think this is unfair: "Unless for some reason they do not" is not obviously equivalent to "unless they do not." But this point doesn't help; see the discussion of the proposal of Pietroski and Rey in the following section.

could count as money. *A fortiori*, there is a tremendous variety of kinds of physical system that could constitute a *market*. Examples are easily multiplied.

Suppose that we have a kind of social system studied by the social sciences; call it kind F. Further suppose that we are interested in the behavior of F-systems under circumstances C. Consider the hypothesis that it is a law that in circumstances C, F-systems will exhibit behavior G. Now, suppose that C and G, like F, are kinds (of circumstances, of behavior) that belong to the classificatory schemes of the social sciences. So, C might be the circumstance of currency inflation, but it will not be the circumstance of having a collective center of mass that moves uniformly. This is necessary to guarantee that the regularity proposed by our hypothesis is "within the subject matter of the social sciences" in the sense described above. Since all these kinds are multiply realizable, there is a great plurality of kinds of physical system that could be a system of kind F in circumstances C. How such a system will evolve is sensitive to the details of how the underlying physical system will evolve. Since all we know about this physical system is that it belongs to one of a certain enormous and heterogeneous class of kinds of physical system, there is a great variety of different ways in which it might evolve. It is very implausible that there is any single answer to the question of how an F-system will behave in circumstances C, beyond that it will continue to conform to the truths of logic and the laws of physics. But if G specifies only that the system continues to conform to the truths of logic, then our hypothesis is not contingent, so it cannot be a law. And if it specifies only that the system will continue to conform to the laws of physics, then our regularity is not within the subject matter of the social sciences. So our hypothesis will admit of exceptions; it will not state a strict law.

This argument is extremely abstract and it is very quick. But consideration of a concrete example helps to drive the point home. Suppose that F stands for currency markets, and C stands for the condition of inflation. One kind of physical system that can constitute an F-system in circumstances C is a population on a planet in the direct path of an enormous comet that will arrive within a few hours. (Other examples include populations about to be stricken by a new and terrible virus, populations in the paths of hurricanes, and so on.) Such a physical system is bound to evolve in such a way that, in a short amount of time, it will belong to no recognizably economic kind at all.

This is a rather brutal example (both in terms of its content and the rhetorical use to which I am putting it), but it is easy to imagine more subtle ones. For example, imagine a population in which the molecules in the brains of all individuals are coincidentally arranged in such a way that very soon, they will undergo psychological changes that completely revise their behavior patterns, so that they all eschew their former acquisitive ways and become ascetic humanitarians. This scenario is a far-fetched one, but improbable as it is, there is no obvious reason why it should be nomologically impossible, and it does supply an example of a kind of physical system that would constitute a system of kind F in circumstances C, whose future evolution is likely to be quite different, economically speaking, from what we would expect. Any kind of behavior G, belonging to a social-scientific classificatory scheme (rather than, say, a physical one), which would apply correctly to the future state of such a system as well as to the future state of a more "normal" currency market in condi-

tions of inflation, would have to be extremely descriptively weak. It would have to cover cases in which everybody becomes extremely altruistic, as well as cases in which everybody gets obliterated, and cases in which things proceed as we normally expect them to do. If G is so broad, then our hypothesis – that all F-systems in circumstances C exhibit behavior G – would have to be completely uninformative. Again, the case described is an extreme one. But if it is so easy to imagine circumstances in which the antecedent of our hypothesized social-scientific regularity is fulfilled but bizarre phenomena (the obliteration of all life; the sudden widespread imitation of Mahatma Gandhi) result, how much more plausible it is that there exist extremely complex physical circumstances in which this antecedent is fulfilled and what occurs is something more ordinary, yet still not what would be predicted by any given, reasonably informative, social-scientific hypothesis?

The basic point here is that no matter which social-scientific kinds F, C, and G are, there are likely to be kinds of physical system that constitute social systems that instantiate F and C but, under normal physical evolution, lead to bizarre outcomes, which will not be covered by G (unless G is so weak that our hypothesis is a truth of logic or a law of physics). One natural response is to point out that just because there are such kinds of physical system, it does not follow that any real social system will ever actually be constituted by one. So, there might not actually be any exceptions to our hypothesis. The problem is that even if this is so, still our hypothesis will state a regularity that does not seem to count as a law: Even though it is true, it would have been violated if the actual circumstances had been different in a way that there is no reason to regard as impossible. The regularity will be fragile, in a sense that seems incompatible with being a law. Another natural response is to revise the hypothesis, by building into the circumstances C the requirement that the F-system not be constituted by any of the kinds of physical system that will lead to bizarre behavior. But since *any* social kind is multiply realizable, this is not likely to succeed unless we define the circumstances C in physical terms – any definition of C that is couched solely in terms of social-scientific kinds is multiply realizable, so the same problem will arise again. And if we define C in physical terms, then we no longer have a regularity that could be a *social* law. In order to rescue our hypothetical social regularity, then, it seems that we must hedge it: "F-systems in circumstances C will exhibit behavior G, *unless something goes wrong*."

7.6 The Case against Hedged Laws

It has been argued that hedged regularities cannot be discovered by science, because they are not empirically testable. The hedge functions as an "escape clause" that allows any hypothesized hedged regularity to escape empirical refutation: Whenever you discover a counterexample, claim that there has been an interference of some kind, so that the case is outside the scope of the hypothesis and does not falsify it (Earman and Roberts, 1999). Although I think that there is something important to this line of thought, I will not pursue it here. I will focus not on the empirical testing of hedged laws, but on the supposed hedged laws themselves. There are no hedged laws, because any hedged law would be (or would entail) a hedged regularity, and there is no coher-

ent concept of a hedged regularity that could be (or could be entailed by) a social law.

A hedged regularity takes the following form:

> Whenever A happens, B happens, unless there is an interference.

What does the "unless" clause add to the content of the statement? This depends on how "interference" should be understood. There are a number of possibilities:

1 "Interference" means any event or circumstance that we can identify as a cause of B failing to happen, even in the face of A.
2 "Interference" refers to an event or circumstance in a certain, definite class *I* of events or circumstances:
   (a) where we can identify the class *I* independently of saying that it is the range of cases in which there is an exception to the A–B regularity;
   (b) where we cannot so identify the class *I*, but nonetheless we understand what "interference" means in this context;
   (c) where we have no idea how to identify the class *I* or any implicit understanding of the range of cases that would count as an interference.
3 "Interference" just means any case in which A happens but B fails to happen.

I can offer no proof that this list of possibilities is exhaustive, but it is hard to imagine what a further alternative would be. None of these possibilities provides us with a coherent concept of a hedged regularity that would allow us to make sense of a hedged social law.

I will consider the easy cases first. In case 3, our hedged regularity is a tautology: "Whenever A happens, B happens, unless B does not happen." So it could not be a law. In case 2(c), the regularity need not be a mere tautology, but in stating it, we have no idea what we are stating. No particular fact is stated by the hedged-regularity statement. But that just means that no fact is stated by it. In case 2(a), what we really have is a strict regularity: "Whenever A happens and no event in the class *I* occurs, B happens." We have just stated this strict regularity in an abbreviated form.

Case 1 is more complicated. It echoes a proposal of Pietroski and Rey (1995) concerning how we should understand hedged laws. The problem with it is that it trivializes the notion of a hedged regularity. Consider an example. Every material object either exhibits ferromagnetism, or fails to do so. It is plausible that for every object, there is some feature of its molecular constitution and structure that explains why it does or does not. So, for every spherical object S, it is true either that S exhibits ferromagnetism, or else that there is some property of it (some feature of its molecular constitution and structure) that explains why it does not. Hence, if we understand "interference" in the sense of case 1, then we can say that every spherical object is ferromagnetic unless something interferes. But it would be absurd to suppose that this fact is a "hedged regularity"; at any rate, if we do, then we are going to find hedged regularities everywhere. Whenever it is true that any object that fails to have property G does so because of some factor that explains why it fails to have property G, we will have a hedged regularity to the effect that every F is a G, unless something

interferes, *no matter what F is*, and even if F is completely irrelevant to G (cf., Earman and Roberts, 1999, pp. 452–4).

What remains is case 2(b). It is, in essence, the possibility favored by Marc Lange (1993) in his general discussion of hedged laws. For Lange, the content of a hedged law is, for example, that every F is a G unless there is an interference, and we understand the meaning of "interference" even if we cannot give a precise, illuminating definition of it. There is nothing particularly troubling or unusual about this; we understand the meanings of plenty of words, even though we cannot give informative definitions of them all in a noncircular way. As Wittgenstein argues, language is a rule-following activity in which a person can engage even if she cannot state every rule in a complete form that leaves nothing to interpretation. Why should we demand explicit definability of "interference" in other terms, when we cannot consistently make this demand for every word that we use? Why does "interference" deserve to be picked on more than, say, "force" or "distance"?

This is an interesting and subtle proposal. I will not try to refute it here (but for an objection, see Earman and Roberts, 1999, pp. 449–51). However, even if the general point is a good one, it does not seem to be of any help with respect to the kinds of hedged regularities that would be expressed by social-scientific laws. Lange illustrates his proposal with the example of a law from phenomenological physics, the law of thermal expansion. This law says that when a metal bar is heated or cooled, it undergoes a change in length that is proportional to the change in temperature. Exceptions can occur when some kind of external stress force is being exerted on the bar; for example, by someone pounding on the ends of the bar with a sledge hammer. So the law must be hedged. (This example might be called "Lange's hedge slammer.") According to Lange, a physicist can have an implicit understanding of what sort of thing would count as an interference covered by the hedge, even if she is unable to sit down and write out a complete list of those things. That is, she can tell in advance whether any particular specified condition is covered by the hedging clause, without having to check experimentally to see whether the law is violated in that condition (Lange, 1993, p. 233).

Be that as it may, things are different with the case of hedged social laws. As we saw above, social systems are constituted by physical systems of enormous complexity, and social-scientific kinds are multiply realizable, so the kinds of situations in which any social regularity might be violated include extremely complex states of the underlying physical system which cannot be characterized in terms of social kinds. It is implausible that a social scientist proposing a hedged social law could have the kind of implicit grasp of all possible such conditions that Lange's physicist has, and still less plausible that it is possible to tell in advance whether or not a specified physical state of the underlying physical system is covered by the hedging clause. Lange's proposal thus does not help with the case of hedged social-scientific laws.

Hence, there appears to be no way of understanding what a "hedged regularity" is which will allow us to make sense of a hedged social law. If we want to find a coherent and charitable interpretation of the contemporary social sciences, then, we had better find a way of interpreting their hypotheses and theories as doing something other than positing hedged laws.

7.7 Why Social Science need not Posit Laws

Consider one example of highly successful social-science research: Jeffrey Paige's (1975) study of revolutions in agrarian societies. (Paige's study is analyzed by Kincaid (1996, pp. 70–80), and here I rely on Kincaid's analysis.) Paige identified a variety of factors that tend to influence the degree and kind of collective political activity among cultivators. For example, Paige finds that commercial hacienda systems tend to lead to agrarian revolt, and plantation systems tend to lead to labor reforms instead (Kincaid, 1996, p. 77). These tendencies are not uniform; for example, the occurrence of a given type of political action is made more likely if the same kind of event has occurred in the same society in the recent past ("contagion effects"; Kincaid, 1996, p. 78). Paige's results were arrived at by a painstaking classification and statistical analysis of a large body of carefully collected data. Kincaid argues persuasively that Paige's methodology is scientifically sound and that we are justified in believing his conclusions.

The question before us now is how we should interpret Paige's results. One interpretation is that Paige has discovered a number of social laws governing agrarian societies. The structure of his inference is this: (1) He gathered data on the statistical distributions of various kind of political events across agrarian societies, and identified certain complex statistical relations; for example, that there is a positive correlation between hacienda systems and agrarian revolt, and that this correlation is affected in certain ways when one conditionalizes on the presence or absence of various kinds of political actions having occurred in the recent past. (2) From this statistical data, Paige inferred that it is a law that, for example, hacienda systems lead to agrarian revolt, although this is a hedged law rather than a strict one. (3) From this law, in turn, we can now derive predictions and explanations of particular political events.

If the above arguments are sound, then this interpretation of Paige's results cannot be right. But what alternative is available? Well, why not take the results at face value, as very precise and informative statistical information about the characteristics of the actual agrarian societies that Paige studied? As such, they may well be projectible to other agrarian societies, including future societies. This projectibility would amount to its being rationally justified to expect similar statistical patterns to prevail elsewhere. This would be useful for predictive purposes, and on many models of explanation, it could be explanatory as well (e.g., Salmon's (1971) statistical-relevance model and Kitcher's (1981) unification model). None of this requires that anything be considered a law, if law is understood in the general way sketched in section 7.2. On this interpretation, the idea that Paige's results are put to predictive and explanatory work by inferring, from his statistical conclusions, *first* to a hedged law, and *then* to particular predictions and explanations, is just an inferential detour. Moreover, it is an ill-advised detour, since it takes us through a hedged law-statement, which could not be an accurate description of anything in the world, since there is no such thing as a hedged law. It is thus possible to interpret Paige's results in a way that does not deny them predictive or explanatory import, but does refrain from positing any social-scientific laws.

Prediction and explanation require reliable sources of information about the world, in the form of strict or statistical regularities. Laws of nature are regularities that have certain features: they are global or universal, and robust, in the sense that they do not depend on contingent details of particular systems of objects, and they would not be upset by changes in the actual circumstances that are physically possible. In order to have explanatory and predictive value, though, a regularity (strict or statistical) need not have these special features. Hence, not only does social science have no laws; it needs no laws.

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