

On the Scope and Limits of Generalizations in the Social Sciences*

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Abstract

This article disputes the common view that social science explanations depend on discovery of lawlike generalizations from which descriptions of social outcomes can be derived. It distinguishes between governing and phenomenal regularities, and argues that social regularities are phenomenal rather than governing. In place of nomological-deductive arguments the article maintains that social explanations depend on the discovery of causal mechanisms underlying various social processes. The metaphysical correlate of this argument is that there are no social kinds: types of social entities that share a common causal constitution giving rise to strong regularities of behavior. The article turns next to a consideration of the character of social causation and argues for a microfoundational interpretation of social causation: social causal powers are embodied in the constraints and opportunities that institutions present to individual agents. Finally, it is noted that these arguments have consequences for the credibility of social predictions: it is argued that predictions in social science are generally unreliable.

Is the social world law-governed? Are there social laws, analogous to laws of nature, that give rise to social phenomena? And is it a central task of the social sciences to discover law-like regularities among social phenomena? These are the questions to be discussed in this paper.

Some social scientists write as though the scientific credentials of their disciplines rise or fall on the strength of the law-like generalizations and regularities that they are able to identify.¹ The task of social science research is to discover the laws that govern social processes. And if a given level of analysis and description fails to produce such laws, then we need to probe more deeply until we discover the

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¹ Social scientists who have taken this view include Huntington (1968); Adelman and Morris (1967); King et al; and Zuckerman (1991). Philosophers supporting the idea that lawlike generalizations must undergird social explanations include Hempel (1942), Thomas (1979), M. Salmon (1989), Kincaid (1990), and McIntyre (1991).

underlying order.² At the other extreme, some social scientists write as though generalizations have nothing at all to do with social knowledge. All knowledge is "local knowledge" (Geertz 1983): historical, culturally specific, unique, particular, singular. On this approach, there are no interesting regularities among social phenomena, and causal explanation is an inappropriate model of explanation for the social sciences.³

I disagree with both these views. The general view I will defend is that there are regularities to be found within the social sciences, at a variety of levels of social description; that these regularities derive from features of individual agency in the context of specific social arrangements; and that discovery of such regularities is one important goal of social science research. But I also maintain that these regularities are substantially weaker than those that obtain among natural phenomena. They are phenomenal regularities, not governing regularities; and--in comparison to typical phenomenal regularities among natural phenomena--they are substantially less reliable. Moreover, I maintain that these regularities have a much more limited role within good social explanations than they are often thought to do. The upshot of these arguments is that we should have little confidence in the projectability of social regularities as a basis for prediction, and must therefore pay more attention to the specifics of the social and individual-level mechanisms that produce the regularities as well as the exceptions. And this analysis implies as well that some social scientists have drawn the wrong lessons from the legacy of logical positivism.⁴ Subsumption

² Debate about the foundations of the social sciences among philosophers has largely divided between empiricist and interpretivist positions. Philosophers of social science proceeding from an empiricist perspective have generally taken the view that generalizations are essential; whereas interpretivists have maintained instead that singular interpretation of agent's meanings is essential. Thus Richard Rudner regards the discovery of prediction-supporting generalizations as the sole means of social explanation (Rudner 1966, pp. 59-67). David Braybrooke generally endorses this conclusion as well (Braybrooke 1987, pp. 21-29). The line of argument here is squarely within the empiricist tradition, but emphasizes causal realism rather than law-like generalizations.

³ In addition to Geertz, the interpretive anti-generalization approach includes Charles Taylor (1985b), Alasdair MacIntyre (1973), Turner (1974), and Sahlins (1976).

⁴ What, then, is the right legacy? My position is that what is most worthwhile within the positivist program is epistemological, not metaphysical. The insistence on appropriate empirical controls on knowledge, the broad distinction between observation and theory, the emphasis on coherence and deductive closure--these are all abidingly important features of scientific knowledge. What does not endure, on my

under law-like regularities is not the appropriate model for social explanation. Rather, the corresponding goal ought to be premised on causal realism: discovery of mechanisms and processes that derive from agents and institutions, and that in turn produce regularities. Regularities derive from underlying causal properties, and it is the discovery of these that is the explanatory business of social science research.⁵

Much of the impulse toward emphasizing the explanatory importance of regularities in the social sciences derives from an unhelpful analogy with the natural sciences. The successes of the natural sciences have given natural scientists confidence that natural systems operate in accordance with a strict set of laws, that these laws may be given precise mathematical formulation, that they derive from the underlying real properties of constituent physical entities, and, finally, that these facts entail that the future behavior of physical systems is in principle (though perhaps not in practice) predictable. And this conception of the nature of physical systems in turn gave rise to a paradigm of scientific explanation: to explain a phenomenon is to derive the explanandum from a set of general laws and a description of the initial conditions of the system. Prediction and explanation go hand in hand, and both depend on the availability of empirically supportable general laws.⁶ For reasons that will be developed below, however, I do not believe that this is a good way of understanding social phenomena.

A related reason social scientists have thought that the search for generalizations and regularities should be central for their research and theorizing is the influence of the covering-law model of explanation. On this approach, to explain a phenomenon is to show that the phenomenon (or empirical generalization) may be inferred from one or more general laws. If the covering-law model is a correct theory of all scientific explanation, then social scientists, if they are to produce explanations at all, must arrive at generalizations.⁷ However, in *Varieties of Social Explanation* I argue for a view that places causal analysis, not subsumption under general laws, at

account, is a family of metaphysical doctrines implied by the doctrine of the unity of science.

⁵ More extensive argument for this conclusion may be found in my *Varieties of Social Explanation* (Little 1991, chapter 2). Jon Elster takes a similar position in *Nuts and Bolts for the Social Sciences* (1989).

⁶ Note, however, the powerful arguments against this general view put forward by Nancy Cartwright in *How the Laws of Physics Lie* (1983).

⁷ For a superb review of the development and criticism of the covering law model of explanation see Wesley Salmon's "Four Decades of Scientific Explanation" in Kitcher and Salmon (eds.) (1989). David-Hillel Ruben offers an interesting account of the applicability of the covering-law model for the social sciences in "Singular Explanation and the Social Sciences" (Ruben 1990).

the core of social explanation. Here the general idea is that explanation of a phenomenon or regularity involves identifying the causal processes and causal relations that underlie this phenomenon or regularity.⁸ On this approach, the central explanatory task for social scientists is to uncover causal mechanisms, not to formulate explanatory regularities that permit the deduction of observed phenomena. There are regularities that correspond to causal mechanisms, to be sure; but these may not be discernible (because of the difficulty of isolating causal factors), and they are unlikely to take the form of strong high-level regularities across social contexts (e.g. strong regularities of behavior of certain types of states, trading regimes, or popular movements).⁹

So I will argue for a position that is neither positivist nor anti-positivist. Against the main line of positivist philosophy of science, I will dispute the idea that law-like generalizations are fundamental to successful scientific explanation. But against current anti-positivist criticisms among some social scientists, I will argue for causal realism in social explanation: causal explanation is at the core of much social research, and causal hypotheses depend on appropriate standards of empirical confirmation for their acceptability. Finally, successful causal analysis permits us to arrive at statements of social regularities--this time, however, based on an understanding of the underlying processes that give rise to them. And this understanding permits us to assess the limits and conditions of the regularities we affirm, and the likelihood of failure of these regularities in various social circumstances.

It is important to emphasize that my reasons for these conclusions are not *a priori* arguments to the effect that the social sciences *must* have this or that feature (as is common in much discussion of methodological individualism, for example). Philosophy of science needs to be done in close proximity to the scientific disciplines that are its subject. The arguments here respect this requirement. The conclusions I advance depend, first, on features of the generalizations available in numerous areas of the social sciences, and second, on an empirically informed analysis of the metaphysics of social causation. The generalizations that are available in political science, economics, or sociology are weak and exception-laden, and they permit only

⁸ Other philosophers of science have argued for a similar position in the past decade. See particularly Salmon (1984) and Cartwright (1983, 1989).

⁹ There is of course still a question about the relation between simple causal relations and associated regularities. I place priority on the causal powers of the thing; nonetheless, I also recognize that if A has the causal power to bring about B, then there will be some regularity of association between A and B. But this regularity may not be observable, given data limitations. And the regularity is derivative, not constitutive, of the causal power.

tentative predictions about future developments. The burden of this paper is to offer a theory about why this is so. And the theory that I offer involves an analysis of social causation and individual agency: I argue that the only form of causal influence that social entities have is through their effects on individual action, and that this leads quite understandably to generalizations of the sort we find in the social sciences.

It might be thought that the central conclusions here have anti-naturalistic implications.¹⁰ But I do not believe that this is so. I, for one, certainly regard social phenomena as natural: they are the result of the actions and states of human beings, who are themselves natural organisms. Social phenomena supervene upon physical systems which are themselves regulated by laws of nature. But this does not entail that there are strong law-like regularities at the social level. There are many reasons why this is so. First, multiple causation with complicated INUS conditions and probabilistic causation may result in higher-level regularities being indiscernible given feasible data sets.¹¹ Second, turbulence, chaos, and sensitivity to parameter change may mean that outcomes are in principle unpredictable.¹² Third, social causation is commonly probabilistic; this means that long causal chains accumulate uncertainties, so that we cannot predict outcomes based on initial conditions. And finally there is the problem of specification. Even if our theory correctly describes the causal mechanism, it is often the case that the hypothesis must be specified in terms of a mathematical model, and this can be done in diverse ways--with different predictive consequences. Likewise, the predictions of the model depend on the specification of its parameters; but there is generally a range of plausible empirical estimates of the parameters of a model, upon which substantial differences of the model's performance may depend. So one may consistently affirm that social phenomena are natural while at the same time deny that there are laws governing social phenomena.

What are social regularities?

What is a law-like regularity? It is a universal generalization about empirical phenomena. It is one that conveys necessity. It is a generalization that supports counterfactual judgments. It is a regularity that is grounded in the real causal

¹⁰ David Thomas's *Naturalism and Social Science* (1979) provides an extensive explication of naturalism. However, I find that his account gives too much emphasis to the idea of subsuming social phenomena under general laws.

¹¹ See note 17 for definition of an INUS condition.

¹² See the recent application of the central results of the theory of chaos to theories of explanation and prediction in the philosophy of science in Hobbs (1991).

properties of the entities in question. These are reasonably familiar ideas from the philosophy of science.

Let us begin by distinguishing between what I will call "governing regularities" and "phenomenal regularities."¹³ The notion of a law of nature represents a paradigm of a governing regularity: a description of the laws that generate the behavior of a given kind of thing. A phenomenal regularity, by contrast, is a regularity of behavior that emerges from the real causal properties of a thing, but which does not itself give rise to or constrain the thing's behavior. It is a governing regularity that protons and electrons are attracted by the forces described by electrodynamics. It is a phenomenal regularity that glass flows slowly; given the real constitution of glass, it emerges that glass has many of the phenomenal properties of a liquid. (Note that the distinction between governing and phenomenal regularities is not the same as that between law-like generalizations and accidental generalizations. Phenomenal regularities support counterfactuals, so they qualify as law-like, not accidental; but neither are they essential, determining, or regulative. Likewise, the distinction does not collapse onto that between theoretical generalizations and inductive generalizations. A governing regularity could be identified inductively, and a theoretical account may give rise to a phenomenal regularity.)

Having made this distinction, the central topic of this paper divides into two questions. Are there governing laws of social phenomena? And are there phenomenal regularities? My answer to the first question is negative. The closest thing available are laws that describe common features of agency: given that persons want such and so in a given environment, thus and so emerges. It has been observed, for example, that land-tenure systems with a particular structure create common incentives for individuals wherever they are implemented; it is then a regularity of these systems that they have common features (e.g. underinvestment in capital improvements). But these regularities are strictly derivative from features of individual agency, and they do not represent governing regularities of a certain kind of social institution.

Consider an example. Are there laws of the modern state as an institution? Social scientists have discerned a variety of regularities concerning the state: states maximize revenues (Levi); state crises cause revolutions (Skocpol); states create entrenched bureaucracies; and the like. These statements represent generalizations across a number of cases, and they are intended to have counterfactual import. But it is plain that these generalizations are subordinate to our hypotheses about the underlying institutional and individual-level circumstances that give rise to the

¹³ This distinction parallels Nancy Cartwright's distinction between fundamental laws and phenomenological laws (Cartwright 1983). Another term that might be used for the latter idea is a behavioral regularity; I have chosen not to use this alternative because of its suggestion of individual behavior.

regularities of state behavior identified. States conform to regularities because they are the product of a number of agents whose purposes, powers, and opportunities are similar in many different social contexts; this leads to a regularity of state behavior.

Consider for a moment a different sort of regularity. Hand tools weigh less than 20 pounds. This is a strongly confirmed empirical regularity over its domain. But the existence of the regularity does not lead us to imagine that there are governing laws of hand tools. Instead, this regularity emerges from facts about human constitution, powers, and intentions. Tools are designed to be used by human beings in transforming nature. It is a common fact about human beings that there are limits to their strength and dexterity. Tools are designed by humans in light of these limits. It results from these facts that there will be regularities across tools.

Likewise, I suggest, for social regularities. Social phenomena are the consequence of intentional human actions (sometimes in vast numbers). Some social regularities are the intended consequence of individual action--e.g. the revenue-maximizing property of typical states follows from the interest that government officials have in maximizing the power and income of the state. Others are the unintended consequence of purposive individual action--e.g. the falling rate of profit is the unintended consequence of profit-maximizing strategies by large numbers of individual capitalists (according to Marx). But in either case the regularity is strictly derivative from the constitution and powers of the individuals whose actions give rise to the social phenomenon in question.

This leads to a fairly clear conclusion. Social regularities emerge rather than govern. The governing regularities are regularities of individual agency: the principles of rational choice theory or the findings of motivational psychology. Social regularities are strictly consequent, not governing. They obtain because of the lower-level regularities; they have no independent force (unlike a common interpretation of the force of the laws of gravitation).

Turn now to the second question: are there phenomenal regularities among social phenomena, and are these explanatory? Here the answer is, yes and no. It is evident that there are phenomenal regularities among social phenomena; but equally, these regularities turn out *not* to be explanatory. States tend to maximize revenues (Levi 1988); low income states tend to have high infant mortality rates; bureaucracies tend to resist change. These are regularities that can be discerned through empirical investigation--chiefly large multi-case studies and small comparative studies. And these are regularities that to some degree support counterfactual judgments: If India's GNP were to double, its infant mortality rate would fall. But I take the view that phenomenal regularities are not explanatory. If we want to know why windows are thicker at the bottom, it is not explanatory to offer the argument that windows are made of glass and glass flows like a liquid. Rather, we want to know what it is about the fine structure of window glass in virtue of which it flows. That is, we need to know what the causal mechanism is that gives rise to this phenomenal regularity.

Likewise, the fact that India has a high infant mortality rate is not explained by the circumstance that India's per capita GNP is low and the phenomenal regularity that "countries with low per capita GNPs tend to have high infant mortality rates." The bare discovery of a stable statistical relationship between these two characteristics is not by itself explanatory of a given country's high infant mortality rate. Instead, we need to know what it is about developing countries in typical circumstances such that low GNP is associated with high infant mortality. That is, we need to know what the causal connection is through which low GNP leads to circumstances in which infant mortality is likely to be high. But once we have identified the typical causal mechanism, we no longer need to use the phenomenal regularity to explain; we can then directly ascertain whether that mechanism is in place in India and can account for India's high infant mortality rate as the consequence of the presence of the specified causal factors. (The mechanism is presumably something like this. Low GNP causes low personal income and low state revenues. Low personal income entails low ability to pay for nutrition and health care. This leads to poor average maternal health. Low government revenues entail low ability to pay for publicly funded health and nutrition programs. This also leads to poor average maternal health. But if the state devotes a substantial fraction of its resources to public health, these causal connections do not go through, and we should expect an exception to the rule. Thus the exception of Sri Lanka, which is a country whose per capita GNP is roughly that of India, but whose infant mortality rate is comparable to that of many European countries.)

So: there are phenomenal regularities among social phenomena, and these can be discerned through familiar forms of empirical investigation; but they do not serve an important explanatory function within the social sciences.

Are there social kinds?

Another way to put the thrust of my position is to consider whether there are social kinds, analogous to natural kinds. A natural kind is a set of entities which share a common causal structure, and whose behavior can therefore be predicted on the basis of the laws that govern the behavior of such entities (Putnam 1975b). A social kind, then, would be a class of social entities that share a common causal structure; this common structure would give rise in turn to one or more governing laws. Candidates for social kinds include "riot", "revolution", "class", "religion", "share-cropping land-tenure system", "constitutional monarchy", "market economy", "nationalist political movement", "international trading regime", and "labor union". Note, to begin, that it is not the case that all scientifically useful concepts and categories designate natural kinds. The concept of a predator is useful within evolutionary theory; likewise, the concept of an acid in chemistry, an earthquake in seismology, or a gas in physics. None of these represents a genuine natural kind,

however, since there is no homogeneous causal structure that is shared by all members of the class.

I deny that any social concepts serve to identify social kinds in the strong sense outlined above. Instead, social concepts function as ideal types or cluster concepts, permitting us to classify a range of diverse phenomena under a single concept. The notion of a cluster term captures many scientific concepts--terms that encompass a variety of phenomena that share some among a cluster of properties (Putnam 1975a, pp. 50-54). An ideal-type concept is a complex description of a group of social phenomena that emphasizes some features and abstracts from others (Weber 1949). It is apparent that generalizations and predictions based on cluster concepts and ideal types demand a great deal of care. Since the entities that fall under such concepts do not share a homogeneous causal structure, we cannot infer that instances of the concept will behave in the typical way. Thus market economies have many properties in common. However, particular market economies also have causally significant differences; for example, the purported willingness of Japanese investors to consider a longer time horizon in their investments than their American counterparts could be expected to give the Japanese market economy different characteristics than the U.S. economy.¹⁴

The value in making use of cluster concepts and ideal type concepts in the social sciences is that it permits us to group social entities together in ways that emphasize their common features. This serves to suggest hypotheses about the dynamic properties of such entities. But at the same time, the fact that there are wide differences in important causal features among the entities that fall under a given concept, means that we cannot simply project the future behavior of the entity on the basis of the general features that it shares with other instances.

The metaphysical counterpart, then, to my view that there are no governing social regularities among social phenomena, is that there are no genuine social natural kinds.¹⁵

Causal explanation

In place of the goal of discovering governing regularities, I maintain that explanations in social science typically involve efforts to uncover the causal properties of social entities and processes. In *Varieties of Social Explanation* I argue that the central idea of causal ascription is the idea of causal powers and causal mechanisms:

¹⁴ Further discussion of this point can be found in my *Understanding Peasant China* (1989, chapter 6).

¹⁵ Alan Nelson argues a similar point with respect to economic kinds (Nelson 1990).

to assert that A causes B is to assert that A in the context of typical causal fields brings about B (or increases the probability of the occurrence of B). This view may be elaborated in terms of the idea of a causal chain: A causes B just in case there is a series of causal mechanisms linking the occurrence of A to the occurrence of B. This may be called "causal realism."¹⁶ Second, I argue for a microfoundational approach to social causation: the causal properties of social entities derive from the structured circumstances of agency of the individuals who make up social entities--institutions, organizations, states, economies, and the like. The mechanisms through which social causation is mediated turn on the structured circumstances of choice of intentional agents, and nothing else. (This is not equivalent to methodological individualism or reductionism because it admits that social arrangements affect individual action. This is the structuring to which I refer in this formulation.) This means that social science research that sheds light on the individual-level mechanisms through which social phenomena emerge have a foundational place within the social sciences: rational choice theory, theory of institutions and organizations, public choice theory, analytical Marxism. What these fields have in common is a commitment to providing microfoundations for social science.

My argument so far is that causal mechanisms are more fundamental than regularities of association between causal variables. But there is an apparently straightforward objection to this approach: don't causal ascriptions depend on or at least implicate regularities? Two points can be made here. First, if A has the causal power to bring about B in a wide range of causal fields, then it will be true that there will be an observable regularity of association between A and B. But this does not imply that the causal power reduced to a description of the corresponding association. Consider the statement that magnets cause forces in iron filings. This is a causal fact that gives rise to regularities, and it is easily discerned by observing regularities. But it does not reduce to a bare regularity, and it is not necessarily discovered or verified on the basis of observed regularities. More profoundly, however, it is possible that A has the causal power to bring about B in some fields and not others, with the result that it is practically impossible to observe the corresponding regularity, given data limitations. Causal stories involving complex causal diagrams, complex sets of INUS conditions, probabilistic causation, and incomplete causal fields give rise to situations in which two things may be true: A is a cause of B (in that A is an ineliminable part of

¹⁶ I make the case for this view at greater length in *Varieties of Social Explanation* (1991, chapter 2). Richard Miller has advocated a similar conception of social explanation; he writes that "an adequate explanation is a true description of underlying causal factors sufficient to bring about the phenomenon in question" (Miller 1991, p. 755).

the underlying causal diagram or INUS conditions), and there is no observable correlation between A and B.¹⁷

What, then, can we say about the causal properties of social entities? Do social entities have causal properties? Does a given state, labor organization, bank, or political party have causal properties? And do types of social entities have common causal properties? That is, do states, labor organizations, banks, or political parties have common causal properties? Consider the causal powers of the U.S. government with respect to U.S. economic activity. Various agencies have instruments of action that produce changes in economic activity. The economic variables of interest include the inflation rate, the rate of employment, and the growth rate. Changes in money supply, changes in federal spending, and changes in interest rates are all actions that government agencies can undertake that have effects on economic activity. Do these constitute causal powers in the sense described above? They do, but this judgment is attenuated by the fact that the relation between cause and effect is often highly contextual in the case of social causation. In some contexts lowering the interest rate may stimulate growth while dampening inflation; in other contexts it leaves both growth and inflation unchanged. This implies that an adequate causal analysis will not take the causal properties of the Fed as basic, but will rather involve a large number of causal factors (including the Fed's actions) which jointly produce given outcomes.

Second, we can say a great deal about the metaphysics of social causation. The discussion of microfoundations above gives the clue; the causal properties of a social entity consist in the structures that it embodies that affect the actions of individuals (through incentives, opportunities, powers, information). I assert that certain social entities have causal relevance--e.g. centralized bureaucratic states have greater capacity to collect revenues from the periphery than decentralized feudal states (Mann). What this capacity consists in, however, is not merely the observed regularity that corresponds to it. And it is not some mysterious social force inhering in the social entity itself. It is rather the specific features of these states in virtue of which the agents of the state have both the interest and the means to effectively extract revenues from actors distant from them.

The idea that a social entity has causal powers suggests that the inner constitution of the entity is such as to lend necessity to certain transitions and not others. Is there a relation among social states of affairs that conveys necessity? What features of social life would support such a judgment? What connections between states of affairs are available that would provide an interpretation of social necessity?

¹⁷ The concept of an INUS condition is the centerpiece of John Mackie's analysis of causation. It is an "insufficient but necessary part of a condition which is itself unnecessary but sufficient for the result" (Mackie 1976, p. 62).

Elsewhere I use the idea of a "logic of institutions" to attempt to capture the idea that a set of social circumstances brings about certain types of outcomes (Little 1986). I describe an institutional-logic explanation as an analysis that is concerned with determining the results for social organization and development of an entrenched set of incentives and constraints on individual action (Little 1986, p. 34). Given the stylized arrangements described in the explanans, we can expect the outcome. I maintain that this is the sole form of necessity that can be discerned among social phenomena. Note, however, that the necessity that attaches to an institutional logic is solely grounded in the intentionality of the individuals whose actions are affected by the arrangements under scrutiny. The explanatory force of an institutional logic depends fundamentally on facts about individual agency.

Consider an example. Transport systems have the causal capacity to influence patterns of settlement; settlements arise and grow at hubs of the transport system. Why so? It is not a brute fact, representing a bare correlation of the two factors. Instead, it is the understandable result of a fuller description of the way that commerce and settlement interact. Agents have an interest in settling in places where they can market and gain income. The transport system is the structure through which economic activity flows. Proximity to the transport system is economically desirable for agents: they can expect rising density of demand for their services and supply of the things they need. So when a new transport possibility emerges--extension of a rail line, steamer traffic farther up a river, or a new shipping technique that permits cheap transportation to offshore islands--we can expect a new pattern of settlement to emerge as well. This is an instance of an institutional-logic explanation.¹⁸

So far, then, my argument is two-fold: social entities have causal influence, and these causal capacities are to be explained in terms of the structuring of incentives and opportunities for agents. The causal powers or capacities of a social entity inhere in its power to affect individuals' behavior through incentives, preference-formation, belief-acquisition, or powers and opportunities. The micromechanism that conveys cause to effect is supplied by an account of the actions of agents with specific goals, beliefs, and powers. Social entities can exert their influence, then, in several possible ways.

1. They can alter the incentives presented to individuals.
2. They can alter the preferences of individuals.
3. They can alter the beliefs of individuals. (constraints on knowledge; ideology)

¹⁸ Similar examples of arguments about the logic of power relations in pre-modern societies may be found in Mann (1986).

4. They can alter the powers or opportunities available to individuals.

Lowering the prime interest rate has the causal capacity to reduce the rate of inflation. Why is this? Because rational investors lower their rate of investment in the face of lower interest rates; demand for producer goods falls; incomes for workers remain steady; and demand for goods remains flat. So prices tend to stay constant. This story accounts for the causal powers of the intervention in terms of the incentives created and strategies available to the relevant agents.

The conclusion of this line of thought is that institutions have effects on individual behavior (incentives, constraints, indoctrination, preference formation), which in turn produce aggregate social outcomes. Some social regularities follow immediately from these effects--e.g. increasing the tariff on imported running shoes leads to an increase in consumption of domestic running shoes. (This regularity derives from the fact that consumers are price-sensitive; so increasing the cost of imports leads to a shift in typical consumer's behavior.) (Naturally, we need another story to tell to indicate how institutions are embodied in the current beliefs, preferences, and behavior of existing individuals.)

We can say also, derived from this first point, that certain institutions have specific causal powers with respect to given social outcomes as a consequence of the common constitution and circumstances of individuals. The Fed has the causal power to dampen inflation, in that it can tighten the money supply; this creates an individual disincentive to purchase; this leads to reduced demand for goods; and this lessens the upward pressure on prices. This causal power is entirely derivative, however, upon facts about typical consumers. The Fed has the power to alter the environment of choice for consumers; the result of this new environment is a pattern of consumption in which demand is shifted downward.

The upshot, then, is this. Social entities possess causal powers only in a weak and derivative sense: they possess characteristics that affect individuals' behavior in simple, widespread ways. Given features of the common constitution and circumstances of individuals, such alterations at the social level produce regularities of behavior at the individual level that eventuate in new social circumstances. $S_1 \Rightarrow \{\text{structured environment of individual choice}\} \Rightarrow S_2$. Theda Skocpol's causal analysis of the state and revolution, then, does legitimately attribute causal powers to the state. But these causal powers derive entirely from the ways in which the institutions of the state assign incentives, powers, and opportunities to various individuals.

Do causal powers depend metaphysically on the existence of law-governed regularities? And does our knowledge of causal properties depend on the discovery of strong regularities? I argue that the answer to both questions is negative. The causal powers of a thing rather give rise to whatever regularities are observed; and the discovery of regularities is only one out of a number of methods by which we can

identify causal relations and powers. Regularities are symptomatic rather than criterial of causal powers and relations.

Social causal ascriptions depend on regularities; but these are not generally social regularities, but rather lawlike characteristics of agents (e.g. the central axioms of rational choice theory). I would assert, then, that the rock-bottom causal stories--the governing regularities for the social sciences--are stories about the characteristics of typical human agents. The causal powers of a particular social institution--a conscription system, a revenue system, a system of democratic legislation--derive from the incentives, powers, and knowledge that these institutions provide for participants.

Moreover, these phenomenal regularities are weak, tendential, conditional, and unreliable. So social explanation does not rest on discovering regularities, or deriving outcomes from statements of regularities. Instead, analysis of the underlying causal mechanisms, and particularly the microfoundations, is central. Social scientists would do better to embrace causal realism instead.

Prediction and social science

I have argued for three central ideas to this point: there are social regularities, but they are weak and not the central component of social explanations; second, that the explanatory work of social inquiry commonly takes the form of a search for causal relations and causal powers; and third, that causal relations among social phenomena derive their necessity through features of structured individual agency, and nothing else. Let us turn finally to the issue of prediction: what consequences do these arguments have for the reliability of predictions in the social sciences?

Some philosophers and scientists maintain that it is essential to an adequate science that it support predictions about the phenomena with which it is concerned. The thrust of much that has gone before in this paper points in the direction of rather narrow limits on the feasibility of predictions in the social sciences. The fact of the complexity of causal fields among social phenomena; the fact that social causal hypotheses are more than usually burdened by extensive *ceteris paribus* conditions; and the inexactness of social causal hypotheses; all these militate toward the conclusion that we must be skeptical about the predictions advanced by social scientists.

What is the status of prediction in the social sciences, given the analysis of social regularities sketched out above? There are generally three avenues through which scientific predictions are generated. First, there are predictions based on simple induction. We note that low-income countries usually have high infant mortality, and we predict that the as-yet unexamined low income country will have a high infant mortality rate as well. This sort of prediction depends on identifying a phenomenal regularity. Second, there are predictions based on a theory of the governing

regularities of the system in question. On my account, there are no such regularities among social phenomena, so this form of prediction is unavailable in the social sciences. Finally, it is possible to support predictions in novel circumstances on the basis of an analysis of the causal mechanisms that we can identify in the circumstance, along with a model that permits us to attempt to estimate the aggregate effects of these causal mechanisms. This is essentially the strategy of attempting to work out the institutional logic implicit in a given set of social arrangements. A special case of this last alternative is the construction of abstract models (e.g. economic models) designed to capture certain social mechanisms and permitting prediction of the future behavior of the system in question.

So social predictions may be based on phenomenal regularities; they may be based on analysis of specific causal pathways (institutional-logic explanation); and they may be based on abstract models of the workings of social sub-systems. What is the epistemic status of these sorts of predictions in the social sciences?

Begin with phenomenal regularities as a basis for prediction. According to the analysis provided above, phenomenal regularities are inductively discernible patterns that derive from the underlying causal mechanisms. The causal properties of social phenomena give rise to regularities, and it is reasonable to conclude that they are law-like (in the sense of supporting counterfactuals). The causal properties of social institutions, and the micro-mechanisms that underlie them, give rise to phenomenal laws, and these are the chief regularities identified by social scientists--not governing regularities. E.g.:

- bureaucratic states collect revenues efficiently
- low GNP is correlated with high infant mortality
- political development produces political instability

What is the scientific interest of such regularities? To begin, they are not fundamental. Rather, we are always well-served by seeking an account of the causal mechanisms that produce them; and we will better understand the scope, reliability, and variance of such regularities when we have a true theory of the underlying causal mechanisms. And second, the predictions that these sorts of regularities support are weak, unreliable, and subject to *ceteris paribus* conditions.

Second, phenomenal regularities do support predictions. If we are confident that there is a strong association between GNP per capita and infant mortality, then we are justified in predicting that if India's GNP per capita were to rise, its infant mortality rate would fall. But predictions based on such regularities are limited in just the way that any other purely statistical association is limited. The fact that 75% of philosophy professors enjoy Chinese food gives ground to a prediction that I, a philosophy professor, enjoy Chinese food. But we would be more confident in such a prediction if we knew more about the circumstances that account for the brute generalization. (If, for example, the explanation was that 75% of professors received

their graduate education in cities with excellent Chinese restaurants, this would directly causally account for the statistic. And it would likewise allow us to account for the failures of the generalization; professors who were self-taught would not be subject to the same causal influence.)

Turn now to the second possible source of predictions in social science: predictions that depend on a hypothesis about the full set of important causal factors in a given situation, and an analysis of the likely aggregate causal consequences of these factors. A common source of failures of prediction in the social sciences stems from the fact that causal hypotheses and models are generally subject to *ceteris paribus* conditions. Predictions and counterfactual assertions are advanced conditioned by the assumption that no other exogenous causal factors intervene; that is, the assertive content of the hypothesis is that the social processes under analysis will unfold in the described manner absent intervening causal factors. But if there are intervening causal factors, then the overall behavior of the system may be indeterminate. In some cases it is possible to specify particularly salient interfering causal factors (e.g. political instability). But it is often necessary to incorporate open-ended *ceteris paribus* conditions as well. And in real situations it is all too common that the *ceteris paribus* conditions of a given analysis turn out not to be satisfied. This means, in turn, that predictions based on such analysis must be understood as representing *tendencies* rather than probable outcomes.

A related problem stems from the general point that causal hypotheses and models unavoidably make simplifying or idealizing assumptions about the populations, properties, and processes that they describe. Consumers are represented as possessing consistent and complete preference rankings; firms are represented as making optimizing choices of products and technologies; product markets are assumed to function perfectly; and so on. Suppose that an economic model makes the assumption that the coefficients of production are constant. This implies that producers do not alter production technologies in the face of different price schedules for inputs. This assumption abstracts from producers' substitution behavior. But the model-builder may argue that this is a reasonable approximation in a static model; whatever substitutions occur from one period to the next will be small and will have little effect on aggregate input-output relations.

A third reason why social predictions often fail has to do with the complexity of causal fields in social phenomena. This point may be put in terms of the idea of a set of INUS conditions (or a causal diagram); the true INUS conditions for a given social phenomenon are generally very complex, with various kinds of conjunctural causation leading to complex and conditional relations between causes and effects. To the extent that a given causal hypothesis has only identified some of the causal conditions included in the true underlying causal diagram, it is foreseeable that predictions based on the hypothesis will often go wrong. Putting the point in another way, to the extent that a given causal analysis does not provide a complete representation of the full causal field, its predictions may be expected to fail on

occasion. But it is highly implausible to suppose that we have ever arrived at a complete causal field.

Fourth, the uncertainties implied by incomplete causal fields are increased by the fact that the underlying governing regularities are not deterministic. They are--as argued above--regularities of agency. If an economist is presented with a market in which there are substantial price differentials, he or she will predict that normal competitive processes will bring about an equilibrium price which is equal throughout the market. This prediction depends on the idea that rational consumers will take advantage of lower-price opportunities, thus forcing high-price providers to lower their prices. But if consumers are imperfectly rational--if, for example, they are satisficers rather than optimizers--then this prediction will not materialize.

In short, then, there are numerous reasons why we should be cautious in assessing the reliability of predictions based on a causal analysis of a social situation. This circumstance does not undermine the scientific value of the analysis, it should be noted; but it substantially undermines our confidence in the predictions the analysis gives rise to.

Turn now to predictions based on models and theories. The overall strategy is to arrive at a formal representation of what we think (some of) the underlying causal processes are within a given social context, and then use deductive and mathematical tools to predict future states of the system. This approach suggests that we can ask two sorts of questions about a model. We can ask whether the model is a good approximation of the underlying social reality--that is, the approximate truth of the theory or model. Likewise, we can ask whether the theory or model gives rise to true predictions about the future behavior of the underlying economic reality (subject to the time frame of the analysis). Each of these questions falls on the side of the truth value of the model. Another set of questions concerns the warrant of the model: the strength of the evidence and theoretical grounds available to us on the basis of which we assign a degree of credibility to the model: does available evidence give us reason to believe that the model is approximately true, and does available evidence give us reason to expect that the model's predictions are likely to be true? These questions are centrally epistemic; answers to them constitute the basis of our scientific confidence in the truth of the model and its predictions.

It is important to see that the question of the approximate truth of a model is separate from that of the approximate truth of its predictions. It is possible that the model is approximately true but its predictions are not. This might be the case because the *ceteris paribus* conditions are not satisfied, or because low precision of estimates for exogenous variables and parameters leads to indeterminate predictive consequences. Therefore it is possible that the warrant attaching to the approximate truth of the model and the reliability of its predictions may be different. It may be that we have good reason to believe that the model is a good approximation of the underlying economic reality, while at the same time we have little reason to rely on its

predictions about the future behavior of the system. The warrant of the model is high on this account, while the warrant of its predictions is low.

Points considered above concerning the need for *ceteris paribus* conditions, the need for simplifying assumptions, and the weak necessity associated with the postulated governing regularities entail that the predictions based on formal models may have substantially lower warrant than the models themselves. But there is another important consideration that is more specific to economic and social modeling techniques that undermines the warrant of predictions in the social sciences. Theories give rise to models and models produce predictions. But there is a specification problem at two levels: the model can specify the theory in various ways, leading to different predictions. And the parameters of the model themselves can be estimated in various ways--again producing different predictions. Consider the example of general equilibrium theory. General equilibrium theory represents the general hypothesis underlying applied general equilibrium models. But the application of the theory to a particular economy or policy problem is not straightforward. There is no canonical mode of representing the central economic quantities and processes. Thus utility functions can be represented in a variety of ways, and likewise with consumption and production functions. (The linear expenditure system is commonly used in computable general equilibrium models to represent consumer demand, in large part because this is a highly tractable formulation. But there are alternative non-equivalent formulations available.) So a given model represents one out of many different possible ways of implementing the general theory; and in order to arrive at an overall judgment of the credibility of the model we need to assess the adequacy of its particular implementation of supply, demand, savings behavior, and the like.

It follows from this observation that the specifics of a given model are not deductively entailed by the economic theory that underlies it. Different model-builders can have equal commitment to the general theory, while providing very different formulations of the central economic processes (e.g., utility functions, production functions, and demand functions). And the resulting models may have significantly different properties, giving rise to different predictions about the behavior of the economic system in question. (It is for reasons of this sort that Daniel Hausman refers to general equilibrium economic theory as an "inexact science;" Hausman 1992.)

Another reason why social phenomena often do not admit of confident prediction derives from the possibility of multiple equilibria and hysteresis. There are multiple equilibria in a problem of rational choice in circumstances where there are two or more positions in which each player's strategies remain unchanged, even in full knowledge of other players' strategies. In circumstances where there are multiple equilibria, game theory does not entail which equilibrium will emerge. Hysteresis refers to the possibility that the actual equilibrium is path-dependent: factors extraneous to the rational decision-making of the participants determine which point of equilibrium is selected. An example of a social situation in which there may be

multiple equilibria is the dynamics of population size, technical change, and standard of living. Rapid population growth and rapid technical change produce an equilibrium in which there is a stable and high standard of living (a high-level equilibrium); whereas rapid population growth and slow technical change may lead to a low-level equilibrium. The rate of technical change, however, depends on the rate of population growth, the current level of the standard of living *and* exogenous variables; likewise, the rate of population growth may be affected by the other two variables. So it is entirely possible that wholly extraneous historical circumstances--an external economic shock, a navigational discovery, a well-timed period of agricultural abundance, or a new type of food--may determine which path is selected--thus determining as well the eventual equilibrium outcome. (See Elvin 1973, for an argument to the effect that traditional China was caught in a high-level equilibrium trap.)

These considerations suggest that the scope of confident prediction in the social sciences is rather limited. We can often be much more confident in the approximate truth of the hypotheses, theories, and causal models that we put forward of underlying social processes than in the predictions about the future that these hypotheses give rise to.

If we were to adopt either the hypothetico-deductive model of confirmation or Popper's falsifiability requirement on theory acceptance, this would be a very serious blow to the scientific standing of social science, since on those accounts, the chief source of empirical evaluation for scientific theories is to be found in the predictive consequences of these theories. This is not the place for me to elaborate the theory of empirical evaluation that accompanies this view in any detail. Briefly, however, I maintain that the empirical status of a social theory does *not* depend on its predictive consequences (Little 1986, pp. 156-58). Instead, social scientists are able to empirically evaluate their hypotheses and theories piecemeal. The unreliability of social predictions, then, does not mean that social science hypotheses cannot be empirically evaluated. Instead, social scientists often make use of a technique of empirical evaluation that Mill described as the "deductive method": evaluation of social science theories in terms of the independent support available for their central hypotheses (Mill 1950, books III:XI and VI: IX).¹⁹

The general point of this section, then, is that there are diverse but converging reasons for being skeptical of the precision and reliability of the predictions produced by social science research.

¹⁹ See also Daniel Hausman's useful discussion of Mill's deductive method in "John Stuart Mill's Philosophy of Economics" (1981).

Conclusion

The upshot of these arguments is relatively clear. My central conclusion is that there are no governing social regularities underlying social phenomena. There are governing regularities of sorts, but they are not social (rather, they are regularities representing features of rational agency). And there are social regularities, but they are phenomenal. Therefore the social world is not a system of interrelated variables, concerning which we might aim to discover the state laws. It is rather a complex of processes subject to various causal influences, conveyed by individual agency, onto diverse and rarely predictable outcomes.

Inductive regularities among social phenomena can be discovered. But these are distinctly phenomenal laws rather than governing regularities; they have little explanatory import; and they are not particularly reliable as a basis for prediction. Better on both counts is a theory of the underlying causal mechanisms that produce them. These theories in turn need to be supported empirically. We can be most confident in statements of lawlike regularities when we have an account of the mechanisms that underlie them. This means that we need microfoundational accounts drawn from rational choice theory, theory of institutions, collective action theory, game theory, or microeconomics and microsociology.

Another important consequence of this analysis is that the predictive capacity of the social sciences is very limited. It is certainly possible to make predictions based on "logic of institutions" analysis, causal modeling, and the like, and it is reasonable to do so for a variety of purposes. However, we should not have a great deal of confidence in the resulting predictions. Causal analysis is conditioned by *ceteris paribus* clauses, incomplete causal fields, and other problems--with the result that predicted outcomes of a given analysis may well fail to obtain because the conditions are violated. And crude inductive generalizations--e.g. "recessions during election years are usually followed by change of party in office"--have limited applicability to particular cases because of the degree of variance in the evidence that supports them. So I conclude that the search for lawlike generalizations that permit confident predictions is *not* one of the most central tasks for the social sciences.

My most central conclusion, however, is metaphysical. We ought not think of the social world as a system of phenomena in which we can expect to find a strong underlying order. Instead, social phenomena are highly diverse, subject to many different and cross-cutting forms of causation. So the result is that the very strongest regularities that will be ever be discerned will remain the exception-laden phenomenal regularities described here and the highly qualified predictions of regularities that derive from institutional-logic analyses. There is no more fundamental description of the social world in which strong governing regularities drive events and processes.

Notes

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