Prediction

(forthcoming in *The Philosophy of Science: An Encyclopedia*, ed. Jessica Pfeifer and Sahotra Sarkar. New York: Routledge, Inc.)

Jeff Barrett and P. Kyle Stanford
Department of Logic and Philosophy of Science
University of California, Irvine

Introduction

Whether one predicts rainfall, recessions, or racetrack winners, predicting an event or state of affairs often, perhaps even typically, involves saying that it will happen before it occurs, and this common association is presumably responsible for the idea that predictions must be about the future. But in scientific contexts one often characterizes a theory's predictions as its implications or entailments without regard for temporal constraints, as when one says a successful theory of cosmology predicts the existence of cosmic background radiation at all times. The language of prediction is also used to describe declarative assertions about past and present events made in light of a theory, as when evolutionary theory was used to predict that marsupial mammals must once have lived in what is now Antarctica and left fossilized remains there. A temporal element might be preserved by insisting that these are really cases of 'postdiction' or 'retrodiction' or even shorthand predictions about future evidential findings. But perhaps these comfortable extensions of predictive language more naturally suggest that the central element in prediction is not temporal but epistemic. To predict is to make a claim about matters that are not already known, not necessarily about events that have not yet transpired.

Of course prediction cannot be as simple as that, because one way to know something is to predict it correctly on the basis of a well-confirmed theory. Predictive language seems most appropriate in cases when one makes claims about unknown matters using tools (like inductive generalization, scientific theorizing, or sheer guesswork) that can be contrasted with more direct methods of ascertaining the same information (like simply observing in the right place and/or at the right time and/or under the right conditions, or looking for physical traces of some past state of affairs). Although specific philosophical and scientific conceptions of what is immediately given in experience or known directly have shifted over time, predictive language has continuously respected the fundamental idea that a prediction is a claim about unknown matters of fact whose truth or falsity has not already been independently ascertained by some more direct method than that used to make the prediction itself (see PHENOMENALISM; PHYSICALISM).

As this account suggests, successful prediction is valuable because it goes beyond what is already known most directly, but this same feature renders prediction inherently risky. The most interesting and useful predictions typically concern matters to which

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more direct intersubjective access is ultimately expected, so prediction is characteristically something that one can be caught out on given the shared standards of the community of inquirers.

This idea that scientific prediction involves risk led Karl Popper (1963) to single out the willingness to make risky predictions as what distinguishes genuine science from pseudoscience (see POPPER, KARL RAIMUND). Pseudoscientific theories, he suggested, typically include the resources to explain any outcome in their intended domain of application after it is known. Marxist history, Freudian psychoanalysis, and Adlerian 'individual' psychology were among Popper's favorite examples. He urged that such theories should not be regarded as genuinely confirmed by passing tests that they could not possibly have failed. Confirmation, or for Popper 'corroboration', requires that a theory succeed where it might have failed (see CORROBORATION). Thus, Popper argued, genuine science requires theories that rule out some states of affairs and make risky predictions about unknown cases, exposing themselves to the serious possibility of refutation.

In empirical science, the requirement of shared epistemic access to the success or failure of a prediction means that the fate of a prediction is typically decided in the court of experiment and observation.

The problems of induction

The Scottish Empiricist David Hume may have posed the problem of the rational justification for prediction in its starkest form. Hume's empiricism led him to regard the most general problem about knowledge to be how we come to know anything whatsoever "beyond the present testimony of our senses, or the records of our memory" (1977 [1748], 16). Hume pointed out that the mere occurrence of one event or sense impression never deductively implies that another will occur. From this he concluded that it must be on the basis of experience that one learns which particular events reliably cause, precede, or are otherwise associated with others. One is thereby able to make predictions about events or states of affairs beyond those immediately perceived (see EMPRICISM).

But how can one possibly justify assuming that the regular associations or even causal relationships that have been noted between past events will persist into the future? Again there is no logical contradiction in supposing that things will change. That the sun will not rise tomorrow, Hume notes (1977 [1748], 15), is no less intelligible a proposition than that it will rise—indeed, the future will almost certainly be quite unlike the past in innumerable particular respects. And any attempt to justify this assumption by appeal to past experience of uniformity in nature, Hume claims, will be "going in a circle, and taking that for granted, which is the very point in question" (1977 [1748], 23). That the future has been like the past in the past only constitutes evidence about what one's own future will be like if one already assumes that how things have been in the past is a good guide to what they will be like in the future, which was the very assumption needed to justify the inferential practice in the first place.

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Efforts to solve or dissolve Hume's problem of induction are a topic of continuing debate (see INDUCTION, PROBLEM OF). For his part, Hume concluded that there can be no rational justification whatsoever for predictions concerning unexperienced matters of fact, and he took this to illustrate that reason or rational justification does not play anything like the role usually supposed in the cognitive lives of human beings. In his 'skeptical solution' to the problem, Hume argues that what generates expectations about unknown cases is a primitive or instinctive psychological disposition he calls custom, which is not itself mediated by any process of reasoning at all. Custom leads one, automatically and without reflection, to expect an event of type B on the appearance of an event of type A just in case B's have followed A's reliably in the past. Thus, Hume offers a naturalistic *explanation* of the psychological mechanism by which empirical predictions are made but not any rational *justification* for this practice. But this is not to say that it is a mistake to rely on custom: not only do we have no choice in the matter, Hume argues, but "Custom … is the great guide of human life. It is that principle alone which renders our experience useful to us.…Without the influence of custom, we should be entirely ignorant of every matter of fact, beyond what is immediately present to the memory and senses" (1977 [1748], 29). The fact that there is no rational justification for such an important and useful cognitive function, he suggests, simply illustrates that Nature has secured "so necessary an act of the mind, by some instinct or mechanical tendency" rather than leaving it "to the fallacious deductions of our reason" (1977 [1748], 37). The most central aspects of human cognitive lives, he suggests, are neither products of, nor even subject to reason. Instead they are "a species of natural instincts, which no reasoning or process of the thought and understanding is able, either to produce, or to prevent" (1977 [1748], 30).

A further problem of inductive justification, arguably anticipated in Hume's treatment, is clearly articulated by Nelson Goodman (Goodman 1954). Here the problem is not how to justify the belief that unexperienced cases will resemble experienced ones, but how to understand, categorize or describe experienced cases so as to know just what it would be like for unexperienced cases to resemble them. Present inductive evidence fully supports the claim that all emeralds are green, for example, but it equally well supports the claim that they are all grue, where 'grue' means 'green if first observed before 2050 and blue if not observed before'. Those who believe that emeralds are grue rather than green, however, will have expectations concerning the appearance of emeralds that diverge significantly from the customary one starting in 2050. Nor can one say that the predicate 'grue' is somehow artificially conjunctive or really disguises a change, Goodman argues, for it is only relative to a set of predicates that regards green and blue as natural categories that it does so. If one takes 'grue' and, say, 'bleen' (understood as 'blue if first observed before 2050, and green if not observed before 2050') as natural or primitive predicates for a language, it will be 'green' that must be defined in an artificially conjunctive way (i.e. 'grue if first observed before 2050 and bleen if not). But of course, it was the choice of 'green' and not 'grue' as natural, primitive, or singularly appropriate for law-like generalization for which a defense was sought in the first place. Goodman thus argues that any attempt to use inductive evidence to project future or unknown cases relies on a set of entrenched predicates, and it is

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controversial whether the entrenchment of one set of predicates rather than another can be rationally defended. Like Hume's custom, Goodman's entrenchment may offer a kind of naturalistic explanation of how humans come to make the predictions they do, but not one that seeks or provides any rational justification for the practice.

Models of empirical prediction

Hume's empiricist approach to the foundations of knowledge proved attractive to such later theorists of science as the logical empiricists, many of whom held that the aim of empirical science was to determine the dependence of observable phenomena on one another; indeed, some famously insisted that every meaningful statement derived its meaning from its implications regarding observable phenomena (see COGNITIVE SIGNIFICANCE; VERIFICATIONISM). On this broad view, empirical predictions were required to be statements (i) in a specified observation language, (ii) entailed by one's theory together with one's past observations, (iii) concerning unobserved but observable phenomena. It is important to recognize, however, that the logical empiricists did not always agree even among themselves about how to characterize the nature of empirical predictions. To take just one example of controversy, in Carnap's *Aufbau* (1967) the empirical predictions made by a scientific theory do not concern the "given" of sense experience but rather concern structural features of the intersubjective domain constructed from experience (see CARNAP, RUDOLF).

Carl Hempel's (1965) model of scientific knowledge was both deeply influenced by the earlier logical empiricist tradition and itself widely influential in turn. In the simplest, deductive-nomological, case predictions and explanations are logical deductions of the form

$$\begin{array}{l} C_1 \wedge C_2 \wedge \cdots \wedge C_k \\ L_1 \wedge L_2 \wedge \cdots \wedge L_r \\ \hline E \end{array}$$

where $C_1 \wedge C_2 \wedge \cdots \wedge C_k$ are statements of particular occurrences (*e.g.*, the positions and momenta of certain celestial bodies at a time), $L_1 \wedge L_2 \wedge \cdots \wedge L_r$ are general laws (*e.g.*, those of Newtonian mechanics), and E is the sentence stating whatever is being, in Hempelian terms, explained, predicted, or postdicted (e.g. the time of the next solar eclipse). Hempel also allows for what he calls inductive-statistical predictions where the argument has the same basic form, but the laws invoked are statistical probability statements. Here a specific event is not logically implied by the boundary conditions and laws, but only supported to a certain degree (1965, 175-177). For Hempel, the conclusion of any argument of this form qualifies as a prediction if E refers to an occurrence at a time later than that at which the argument is offered. A fascinating and controversial feature of this account is the symmetry it asserts between prediction and

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explanation: to explain an event by appeal to a set of laws and conditions is simply to show that it could have been predicted using them (see HEMPEL, CARL GUSTAV).

More recent accounts of empirical prediction have moved progressively further away from the logical empiricists' original requirement of a neutral "sense-datum" language for reporting observation or representing experience. On Bas van Fraassen's constructive empiricism, for example, presenting an empirical theory involves specifying a model for the language of the theory: a domain of objects together with a description of the properties they can have and the relations they can bear to one another. In presenting the theory, one also specifies those substructures of the model that are candidates for representing observable phenomena. The theory is empirically adequate just in case the appearances given in phenomenal experience are isomorphic to the observable substructures of the model (van Fraassen1980, 64; see EMPIRICISM; INSTRUMENTALISM). As in the empiricist tradition more generally, then, the distinction between observable and unobservable phenomena does significant work here, but this distinction is not drawn in linguistic terms. Rather, for van Fraassen, the distinction is supposed to be grounded in the actual observational capacities of human observers, and it is natural science itself which tells us what those observational capacities are (see PHENOMENALISM; PERCEPTION).

The naturalistic suggestion that observability is a question to be settled by natural science is perhaps promising. But how could one's best theories determine what is observable? If they characterize important features of the natural world and one's place in it, then they also might be expected to specify how and the circumstances under which reliable inferences from measurements are possible for human observers. It is presumably in just those circumstances for which one's theories indicate that measurements will provide the resources for reliable inferences about the presence or absence of some entity that one is inclined to characterize the entity as observable. On such a naturalistic view, an empirical prediction might in principle concern any feature of the world that one's best theories indicate can be reliably detected.

But herein also lies a problem for the naturalist. What one judges to be observable will depend on one's current best understanding of the natural world, but this best understanding will itself depend on what one believes one has observed. Since the naturalist's account of what is observable itself depends on the theories the naturalist accepts, observations cannot test the truth or falsity of theories in any direct or simple way. As W. V. O. Quine (1951) and others have noted, one can always respond to a failed test of a theory by blaming background assumptions, presumably including the assumptions used to characterize what empirical observations are and the conditions under which they can be reliably made, rather than admitting that a particular prediction was mistaken. But if empirical predictions need never be given up, then they cannot, strictly speaking, test the theory that makes them.

In practice, however, this general epistemic problem is more often a point of logic rather than a real obstacle to naturalistic inquiry, as Quine himself noted in developing his own naturalistic position. Testing a given empirical prediction to the satisfaction of

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the scientific community requires only that there be a sufficient context of shared background assumptions to provide the understanding of and the rules for the empirical test. The understanding and rules might be implicit, they might change over time, and they might be subject to challenge, but none of this undermines the possibility of testing predictions in principle and, consequently, the possibility of testing the theories that make them. That empirical predictions are in fact often taken by the scientific community to be thoroughly tested and that theories are in fact accepted or rejected on this basis suggests that there are often, perhaps typically, unambiguous standards for checking them.

The Epistemic Significance of Prediction

As the preceding discussion of the relationship between theories and their predictions suggests, testing a theory's predictions is often taken to be a crucial aspect of how it is confirmed or disconfirmed. The most persistent question here concerns whether the ability to predict novel phenomena is of fundamental significance in the testing and confirmation of specific theories in the special sciences; that is, whether it counts in favor of a theory's confirmation that it has predicted novel phenomena rather than merely accommodating, explaining, or anticipating phenomena already known to occur. In this context the relevant sense of prediction involves not anticipating when and where familiar phenomena will recur but rather discovering the existence of phenomena unlike those that are already familiar.

The roots of this debate reach back at least to the foundations of modern science itself; perhaps its most famous iteration pitted William Whewell against John Stuart Mill, who expressed amazement at Whewell's view that

> an hypothesis…is entitled to a more favourable reception, if besides accounting for all the facts previously known, it has led to the anticipation and prediction of others which experience afterwards verified. Such predictions and their fulfillment are, indeed, well calculated to impress the uninformed…But it is strange that any considerable stress should be laid upon such a coincidence by persons of scientific attainments (System of Logic, III, xiv, 6; cited in Musgrave 1974, 2).

Mill's amazement notwithstanding, versions of this Whewellian intuition have been defended by 'persons of scientific attainments' as otherwise diverse as Clavius, Descartes, Leibniz, Huygens, Peirce, and Duhem. By contrast Mill himself defended the view that confirmation depends only on the match between a theory's entailments and the phenomena. While decidedly less popular, this competing view has also recruited influential champions, such as John Maynard Keynes (see Giere 1983, Section 3).

Enthusiasts have sometimes gone so far as to claim that only predictions of novel phenomena are of any confirmational significance at all or that any prediction of a novel phenomenon is of greater confirmational significance than any amount of accommodation of existing evidence. But the claim of a special confirmational significance for prediction does not require such extremes. For prediction as such to

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enjoy a special confirmational privilege it seems sufficient that predicting a given phenomenon provides (or would have provided) greater confirmation for a theory that does so than the mere accommodation of that same phenomenon does (or would have). A view having this consequence, including the extremes just described, may be described as a form of predictivism. Predictivist themes have recently loomed large in debates over the progressiveness of research programs, the adequacy of various approaches to confirmation (especially Bayesianism), and the so-called miracle defense of scientific realism.

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Imre Lakatos is widely credited with having reintroduced this concern over the confirmational significance of novel prediction, specifically in connection with his ‘methodology of research programs’ (see LAKATOS, IMRE). Lakatos’s bold claim was that it is only the ability of the successive theories in a research program to make successful novel predictions that bears on its progressiveness or acceptability. But even Lakatos’s own work includes several competing lines of thought about the nature of novelty (see Gardner 1982, 2-3). At times he seems to construe the novelty of a prediction for a theory purely temporally, though his most famous account holds novel prediction to consist in predicting phenomena that are “improbable or even impossible in the light of previous knowledge” (1970, 118), and he later accepted Elie Zahar’s revisionist proposal that the novelty of a fact for an hypothesis requires only that it “did not belong to the problem-situation which governed the construction of the hypothesis” (1973, 103). Each of these lines of thought has been more fully developed by later thinkers even as they have lost any immediate connection to concerns about the evaluation of research programs.

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The second issue of recent interest concerns whether standard philosophical approaches to confirmation can recognize a special confirmational significance for novel prediction; and if not, whether this weighs against such approaches to confirmation or against the legitimacy of predictivist intuitions instead. Such approaches are described as taking into account only ‘logical’ and not ‘historical’ relations between theory and evidence, or alternatively, only the content of theories and evidence and not historical facts about them. It has sometimes been claimed that a logical approach to confirmation is strictly inconsistent with predictivism; but this is too strong, for the fact of successful prediction can itself simply be treated as part of the evidence supporting a theory. In Bayesian terms, one need only treat the fact that a novel result was predicted as part of the evidence on which the theory’s probability is conditionalized to allow a special confirmational role for novel prediction. This brute force solution to the problem invites the complaint that a special epistemological significance for novel prediction still finds no expression in the formal machinery of either Bayesian or any other extant ‘logical’ accounts of confirmation. But even this is far from uncontroversial (see BAYESIANISM; CONFIRMATION THEORY).

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There has been widespread discussion among Bayesians concerning the nature and plausibility of the further assumptions that must be granted in order to accord novel prediction a special confirmational significance within the Bayesian framework. Central to this discussion has been Glymour’s (1980) ‘problem of old evidence’: the Bayesian

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approach to confirmation suggests that known evidence cannot provide any support for a theory because probability one is conferred on that evidence by background knowledge alone (see Bayes's theorem below). To make matters worse, it is difficult to see how one could conditionalize on or even specify what one's background knowledge 'would have been' without the evidence in question. Indeed, it has variously been argued that Bayesianism is legitimate because it recognizes a special confirmational significance for novel prediction, that it is legitimate because it does not, that it is illegitimate because it does, and that it is illegitimate because it does not (see Brush [1995]).

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Finally, it has sometimes been argued that the ability of a theory to make successful novel predictions is the one form of scientific success for which only the truth of a theory can provide any explanation. This grounds a specific form of the traditional miracle or explanationist argument for scientific realism on behalf of theories enjoying success in making novel predictions. Leplin's (1997) account of novel prediction, for example, is an explicit effort to pick out just those forms of scientific success that only the truth of the successful theory could explain (see REALISM).

A related point of contention concerns whether predictivist convictions have in fact played any role historically in the confirmational judgments made by actual scientific communities. Theorists have appealed to such famous cases of novel prediction as the Poisson 'bright spot' by Fresnel's formulation of the wave theory of light, the gravitational bending of light by the general theory of relativity, and the existence and properties of three new elements by Mendeleev's periodic law to argue that particular novel predictions have or have not been accorded exceptional confirmational weight by actual scientific communities relative to the mere accommodation of existing evidence (see Scerri and Worrall [2001] for references and discussion). Such claims about scientific practices are also invoked to either bolster or defuse the further claim that an adequate account of confirmation will have to respect predictivist intuitions. This historical debate serves to underscore the contentious character of the explanandum for which accounts of novel prediction are supposed to provide explanations. For even if it is true that scientific communities have not historically weighted novel predictions over other kinds of evidence, this itself would seem to call for some kind of explanation, in light of the grip that predictivist intuitions seem to hold on ordinary thinking about confirmation.

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Indeed, any serious assessment of the epistemic significance of novel prediction seems to invite a stark conflict of powerful normative intuitions. There is something especially impressive about such famous cases of novel prediction as gravitational light bending and the Poisson bright spot, but it seems perfectly fair to ask why the temporal order or other historical circumstances of discovery should have any bearing on the confirmational significance of the evidence for a theory. After all, whether a phenomenon was already known or not does not have any impact whatsoever on how convincing the theory's account of that phenomenon is. Why should it make any difference whether the data were predicted by a theory or acted as a constraint on the development or selection of that same theory in the first place? The theory's fit to the data, the auxiliary assumptions required to obtain that fit, the theory's intrinsic

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plausibility, and the like remain precisely the same, whatever the order, manner, or other circumstances of their discovery. It seems perverse to treat such apparent historical accidents as relevant to the degree of confirmation conferred on that theory by the evidence at hand.

Predictivism's defenders have turned, therefore, to specifying criteria for genuinely novel prediction in a way that seeks to avoid dependence on apparently arbitrary or epistemically insignificant features. Meanwhile, their opponents have sought to show that the apparent significance of novel prediction is a product of its confusion, conflation, or frequent association with something else that is of genuine epistemic importance. It is, however, sometimes hard to see more than a rhetorical or terminological difference between the positions of those who seek to creatively refine the conception of novel prediction so as to guarantee its epistemic significance and those who seek to explain the apparent importance of novel prediction as dependent upon the genuine epistemic significance of something else altogether. Sometimes both camps appeal to the same or similar relationships between theory and evidence, and it is not always clear whether a given author even means to be explaining the epistemic significance of novel prediction or explaining it away. A similar ambiguity infects the discussion of the confirmational role of novelty in actual historical cases.

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Complicating this dialectical situation are two competing strands of thinking about the epistemic significance of novel prediction, whether real or apparent. The first, sometimes called 'heuristic', strand holds that the epistemic significance of genuine novelty is a matter of the independence, in some sense, of a given result from the formulation of the theory for which it counts as a novel prediction. Various formulations count a given phenomenon as novel for a given theory if and only if it was not part of the 'problem-situation' that led to the theory (Zahar 1973), was not actually used in the formulation of the theory (Worrall 1978; 1989), was not known to some theorist who formulated the theory (Gardner 1982), or fits the hypothesis despite the latter's not having been designed for that purpose (Campbell and Vinci 1983). These accounts differ most centrally in the precise role known data must play in the formulation of an hypothesis in order for it to lose the special confirmational signficance associated with novel prediction.

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The second approach, sometimes unfortunately called 'epistemic', proposes instead that novelty should be understood as a matter of unexpectedness or low probability in light of what is believed absent the theory. Examples include Lakatos's construal of novel prediction as the prediction of phenomena that are "improbable or even impossible in the light of previous knowledge" (1970, 118) and Musgrave's suggestion that a novel prediction of a theory is one that either conflicts with or is at least not also made by its competitors or predecessors (1974; see also Popper 1963, 36). These accounts differ centrally over what should form the foundation for the expectations that a predicted phenomenon must violate in order for it to enjoy the exceptional confirmational significance associated with novel prediction, and thus recall the 'problem of old evidence' for Bayesianism.

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Each approach is sometimes motivated by canvassing weaknesses or challenges to various versions of the other, but it is worth noting that each encapsulates one of two quite different phenomena that might reasonably be called novel prediction. The first is aimed at a theory's entailment of a result not involved in its own development, that is, a result that is novel for the theory, while the second concerns a theory's prediction of phenomena unlike those with which an epistemic community is already familiar, that is, a 'novelty' for an epistemic community.

Among theorists who seem inclined to argue that the apparent epistemic significance of novel prediction is a product of its confusion with some other condition of genuine epistemic significance, the most influential proposal has been that the evidence must provide a 'severe test' of the theory; that is, one that the theory is likely to fail if it is in fact false (see Popper 1963; Horwich 1982; Giere 1984; Mayo 1991). Other analyses propose alternative sources of confusion such as the assurance novel prediction typically provides that the hypothesis be well supported by earlier subsets of the data as well as by the whole (Schlesinger 1987), that there be no opportunity for 'fudging' the hypothesis to fit the data (Lipton 1991), or that the hypothesis itself not be an arbitrary conjunction of facts (Lange 2001).

One natural way to unify these divergent intuitions about the epistemic significance of novelty is to suggest that each is concerned to rule out a different possible explanation of the evidential situation that would undermine the support a given piece of evidence would otherwise provide for a given theory. The idea here is that the various attempts to define novelty and to explain or to explain away its confirmational significance appeal to different ways in which the prima facie support that evidence provides for a theory can be undermined by further information, such as finding out that the theory was constructed, manipulated, or chosen so as to yield its supporting data, that there are reasons besides the theory to expect the results reported in the data, that the theory itself is simply an arbitrary conjunction of unrelated facts, and so on. If this view is right, the various competing accounts of novel prediction reflect the variety of possible confirmation-undermining explanations of the evidential situation, and it was a mistake all along to insist on just a single criterion of 'genuine' novel prediction or even a single analysis of its epistemic significance, real or apparent. This suggests that paradigmatic cases of novel prediction like the Poisson bright spot are particularly impressive precisely because they preclude nearly all of the most likely confirmation-undermining possibilities. In a similar spirit, Leplin's effort to pick out the sort of predictive success that could only be plausibly explained by the truth of a theory that enjoys it includes criteria of both heuristic and epistemic varieties.

Any such pluralistic proposal regarding the epistemic significance of novelty must face up to an argument given by Horwich (1982). Horwich grants that the confirmation provided for a theory by a given result would be compromised by the existence of plausible competing explanations for that same result, and he endorses a Bayesian version of the 'severe tests' conception of confirmational significance. But he denies that the explanations ruled out by the heuristic novelty of a result (e.g. that the theory was formulated or even manipulated so as to entail the result) are actually competitors to the

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Any effort to cast the net of epistemic significance for novel prediction so as to include heuristic novelty must face up to an argument given by Horwich (1982).

theory itself as explanations of the available data. Instead, he suggests, the explanations ruled out by heuristic novelty are explanations of why a given theory fits the data as well as it does, and thus do not compete with the theory itself to explain the data at hand. For this reason, he suggests, both heuristic novelty and the explanations of our evidential situation that it is able to preclude do not carry any genuine confirmational significance.

But what about the latter explanatory demand alone? Even if the fact that a theory was formulated or manipulated so as to entail the data simply offers an explanation for why the theory fits the data, this would seem to compete with the alternative explanation that the theory fits the data because it is true. Horwich resists this suggestion by way of an intriguing analogy designed to illustrate that even different explanations of the same state of affairs need not always be genuine competitors. He points out that ‘being out of gas’ and ‘having a broken starter’ compete to explain why Smith’s car won’t start: both could obtain, but the probability reasonably assigned to one will be dramatically lowered upon learning that the other is true, because they answer the same explanatory demand in the same way. But not all explanations of the same state of affairs compete in this way. Consider, he suggests, the following explanations for the fact that his car is green: is it because he only buys green cars or because the previous owner painted it green? In this case, the candidate explanations do not compete, in the sense that the fact that one obtains does not reduce the probability that the other also obtains. He further suggests that the explanations of fit precluded by heuristic novelty and that provided by the truth of a theory are like the second case and not the first: the fact that a theory fits the data because one requires or even manipulates the hypothesis to ensure that this is so simply does not compete with the explanation that the resulting hypothesis fits the data because it is true. If so, there is no prior reason to think that hypotheses that merely accommodate existing data are less likely to be true than those that successfully predicted the same data as novel phenomena.

It is far from clear that this claim must be accepted as it stands. Perhaps there is no competition if data simply constrained the formation of a theory in the first place. But if one manipulated a theory’s variable parameters to get it to fit the data, and if one believed that such adjustment could have accommodated most any data of the kind in question, this might indeed seem to compete with the claim that the theory fits the data because it is true. But even if one accepts Horwich’s claim, all need not be lost for heuristic novelty. It remains possible that one might find a promising inductive justification for the epistemic significance of heuristic novelty. But this would require making the case that theories successfully predicting heuristically novel phenomena go on to enjoy especially impressive track records.

How to make predictions

In the spirit of Hume’s skeptical ‘solution’ to the problem of induction, one might wonder whether questions about the rational justification of predictions are not best dealt with by investigating how successful predictions have in fact been made. What inferential techniques and assumptions are actually used to move from known facts to predictions about unknown cases?

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It may help to start with simple cases. Hans Reichenbach took the aim of inductive inference to be finding series of events whose frequency of occurrence converges toward a limit (see REICHENBACH, HANS). If this is all one wants, one might simply keep track of the relative frequencies in any series of data one finds interesting. Suppose for example, one wants to determine the probability of tossing a coin and having it come up heads. Start tossing it, keeping track of the relative frequency of heads to all tosses. If there is a well-defined relative frequency in the limit as one tosses the coin and never stops, then one would be guaranteed to find it this way. If the limiting relative frequency is undefined, then, for Reichenbach, it makes no sense to assign probabilities to the possible outcomes of any toss of the coin, and there is no solution to the problem of induction for that particular series of events. That is, the world is predictable insofar as it is sufficiently ordered to enable one to construct limiting relative frequencies from empirical data. While Reichenbach admits that "we do not know whether the world is predictable," if it is, then keeping track of relative frequencies is guaranteed eventually to deliver the right probabilities and this sort of inductive inference will work; and if the world is not predictable, then nothing will work. And if something besides this sort of inductive inference does reliably work to predict future events, then this sort of inductive inference would track the success of the alternative method and warrant its use (Reichenbach 1938, 350).

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While raw relative frequencies sometimes are the best estimates of probabilities, several caveats are in order. Although Reichenbach's procedure is guaranteed to deliver the actual relative frequencies in the long run if there are any, one never in fact performs an infinite number of observations calculating relative frequencies at each step, and it is not even clear that this is in principle possible (a real coin would not survive). Furthermore, a series of events might very well have well-defined limiting relative frequencies but be such that one would not find them in the short to medium run.

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This last caveat helps to illustrate an important general principle: the kinds of tools that allow one to make predictions based on past evidence require the use of background assumptions about how a given segment of the time series of events one observes relates to the time series more generally. In this case, if the local relative frequencies are approximately equal to the relative frequencies in future segments of the time series, then keeping track of local relative frequencies clearly provides a good way of making future predictions. As a solution to the general problem of induction, of course, such an assumption simply begs the question. But it would perhaps be surprising to find that accurate predictions could be made without any background assumptions whatsoever. Thus, one might usefully classify methods of prediction by the type of background assumptions that must be satisfied for the method's predictions to be reliable.

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One might, for instance, roughly distinguish basic tools, which make predictions on the basis of empirical data with only the most basic statistical assumptions, from model-based tools, which make predictions by estimating unknown parameters in more complex or intricately structured predictive models. (See Hamilton [1994] for a generous sample of both sorts of predictive tools.) Reichenbach's suggestion that one take local

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relative frequencies as probability estimates is an example of a basic predictive tool. Here a prediction is a bet that one’s short-to-medium-run evidence faithfully reflects longer-run relative frequencies. While one may never be certain that this is the case, it is easy to imagine evidence for or against the claim that the assumption is reasonable in a given context. If it is, one can confidently use this basic tool for empirical prediction.

Another relatively basic predictive tool is Bayesian updating. This tool requires that one have prior probabilities to be updated on the basis of one’s new evidence. This is an advantage in that one can often make use of more of one’s relevant prior beliefs in making predictions and a disadvantage in that one must have an appropriate set of prior probabilities in order to use the predictive tool at all.

There are two steps to Bayesian updating. One first calculates the probability of the hypothesis under consideration H being true given evidence E using Bayes’ theorem

$$\Pr(H|E) = \frac{\Pr(H)\Pr(E|H)}{\Pr(E)}$$

For a Bayesian subjectivist $\Pr(H)$ is one’s prior degree of belief in H, $\Pr(E|H)$ is the degree to which H being true would explain evidence E, and $\Pr(E)$ is one’s prior degree of belief that E would occur. Bayes’ theorem follows from the axioms of probability theory and the definition of conditional probability.

The total probability theorem can be used to expand $\Pr(E)$ yielding Bayes’ theorem in a form that is often more useful

$$\Pr(H|E) = \frac{\Pr(H)\Pr(E|H)}{\sum_i \Pr(H_i)\Pr(E|H_i)}$$

where the H_i form a mutually exclusive and exhaustive set of possible hypotheses (which typically includes H) and $\Pr(E|H_i)$ is a measure of how well each rival hypothesis would explain the occurrence of E.

Once one has calculated the old probability of hypothesis H given evidence E, one updates the probability of H given that E has in fact occurred. In the simplest case, where one’s evidence it itself certain, one might use strict conditionalization

$$\Pr_{new}(H) = \Pr_{old}(H|E)$$

For a subjective Bayesian, $\Pr_{new}(H)$ represents the degree of belief one ought to have in H after evaluating evidence E. The justification here is given in a series of Dutch-book arguments where one shows why an agent would accept irrational wagers guaranteed to lose money if the agent adopts an incompatible strategy for revising

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degrees of belief (see DUTCH BOOK ARGUMENT; see also Howson and Urbach [1989]).

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The use of prior probabilities in Bayesian updating requires stronger initial assumptions than Reichenbach’s method. But in return it allows one to make inferential use of more of one’s relevant background beliefs about the nature of the world and the context in which a prediction is made.

Because predictive tools are characterized by the background assumptions needed to apply them, basic prediction and model-based prediction differ only in the number, detail, and complexity of the background assumptions they require. In model-based prediction one starts with a model for one’s empirical data that one believes would make reliable future predictions if its parameters were correctly set. As one might expect, model-based predictive tools are both numerous and diverse. Adopting a predictive model might involve anything from assuming that one’s data should fit to a straight line (the parameters to be estimated here might be the line’s slope and y-intercept) to assuming that one’s data should fit a broad range of specific parameters in a detailed causal description of the system being observed (the parameters to be estimated here might be the state of a physical system at a time and its Hamiltonian). Stronger background assumptions may permit the use of predictive tools that yield more detailed and/or accurate predictions, but the stronger assumptions are also more likely to be mistaken.

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The methods one might use to set the parameters of a predictive model are similarly diverse. One might use basic predictive tools in order to estimate the values of the unknown parameters in a more complex predictive model or one might very well use another model to estimate these parameters. And the accuracy of one’s subsequent predictions will depend upon whether the background assumptions required by the models are satisfied, the number of parameters estimated, the accuracy of the estimations, and the sensitivity of the models to specific failures in accuracy. In short, then, the reliability of the predictions of one’s model will typically depend upon a host of non-trivial background assumptions.

One might take from Hume’s problem of induction the general lesson that information about the past cannot guide rational expectations about the future without some additional background assumptions about the system under consideration. It is perhaps not surprising that there are genuine choices to be made concerning what background assumptions and associated predictive tools are applicable in a given context. In real cases when one justifies a particular set of background assumptions the issue is not whether the future will resemble the past in some vague general sense; rather, one typically finds a variety of concrete argumentative and evidential considerations weighing in favor of competing ways in which it might be expected to do so. And theories in the particular sciences discussed below are typically associated with one or more predictive models requiring various sets of both substantive and controversial background assumptions.

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Prediction in the empirical sciences

Classical Mechanics and Chaotic Systems

Newton's classical mechanics is perhaps the most influential example of a predictive theory. The theory is deterministic in that the physical state (*i.e.*, the position and momentum of each particle) of a closed and finite physical system at a time $S(t_0)$ together with the energy properties of the system uniquely determines the physical state at all other times. Since the position of each particle can be given by three coordinates and the momentum can be given by three coordinates, the complete state of an N-particle system can be given by $6N$ coordinates or a single point in a $6N$ dimensional phase space. As the system evolves, the point representing its state in phase space moves in a continuous way. The past, present, and future history of a particular closed system is represented by the curve in phase space that represents the state of the system at each time. The dynamics can be represented as a set of differential equations that have as solutions the possible phase space trajectories of the system. Since the history of the system is fully determined by the initial state $S(t_0)$ and the system's dynamical properties, this information and sufficiently precise calculations would, at least in principle, enable one to predict with perfect accuracy the state of the system at any time (see CLASSICAL MECHANICS).

This ideal is compromised in application by the fact that observational error is always introduced in measuring continuous quantities like position, momentum, and energy with limited precision. Moreover, computational errors are nearly always introduced by rounding, since analytic solutions to the dynamical laws are rare in general and almost never perfectly applicable. Nonetheless, classical mechanics allows one to make very accurate empirical predictions in a wide variety of contexts, and Newton's Principia famously employs his theory of mechanics to explain and to predict the future motions of the five primary planets, the moon, the satellites of Jupiter and Saturn, the precession of the equinoxes, tidal phenomena in the Earth's seas, and the motions of comets. And it does all this so successfully that many thought he had determined, as Edmund Halley wrote in his ode honoring Newton's accomplishment, "Jove's calculation and the laws/That the creator of all things, while he was setting the beginnings of the world, would not violate" (Newton 1999 [1713], 379).

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Notwithstanding these and other remarkable successes, there are severe limits to prediction in classical mechanics that are consequences of the theory's nonlinear dynamics. The problem is that phase space trajectories that are initially close may diverge exponentially with respect to time. Thus, the inevitable small errors in determining the initial state of a system or introduced in computation may generate large predictive errors. And it can happen that the expected error becomes so large over even relatively short times that one can predict almost nothing concerning the future state of the system from its current state. A chaotic system is one that exhibits such exponential sensitivity to initial conditions. More precisely, the chaotic domain of a system is that region of its phase space where the trajectories associated with infinitesimally displaced initial conditions separate from each other exponentially in time.

Chaotic behavior is exhibited by many familiar physical systems (Ott 1993). A dripping water faucet, for example, will often exhibit nearly equal times between drips for low, even inflows, then shift to an unpredictable sequence of times between drops when the inflow is increased. Chaotic behavior is also exhibited by some chemical and biological systems. And curiously, there is reason to expect the motions of the planets in the solar system, the paradigm example of clockwork regularity, to exhibit chaotic behavior. Given that the best estimates of the relevant continuous physical parameters are approximate and that numerical methods for performing computations invariably introduce error, there are strict limits on the reliability of the predictions concerning the motions of the planets obtainable from classical mechanics.

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The behavior of chaotic systems is not, however, entirely unpredictable. In many cases, a chaotic system can be characterized for the purposes of prediction by its attractors, those sets of points in phase space (whether singular points, limit cycles, or more complex regions) which are attractive to all neighboring trajectories. Knowing the type and location of the attractors can allow one to predict the long-term behavior of even a chaotic system, although the nature and precision of such predictions will depend on the existence and type of attractors exhibited by the system. Some dynamical systems are associated with limit sets that are asymptotically attractive to neighboring trajectories but contain trajectories that are locally divergent within the attractive set. Such a limit set is called a strange attractor. If the system begins within the region attracted by the strange attractor, one would be able to predict convergence to the attractor, but virtually nothing concerning the behavior within the attractor. Such attractors may even be typical in nonlinear systems of order higher than two (Cook 1994).

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A rather different predictive problem in classical mechanics concerns the fact that the theory is only deterministic for finite, closed physical systems. Consider a particle that is moving at 1 m/s at $t = 0$, then is accelerated to 2 m/s at $t = 1/2$ second, to 4 m/s at $t = 3/4$ second, to 8 m/s at $t = 7/8$ second, etc. After $t = 1$ second, the particle will be further than any finite distance from where it started. Since any possible physical history can in classical mechanics be run backwards as well as forwards, consider the time-reversed version of this history. Here the particle starts further than any finite distance from a system, say one whose behavior one wishes to predict, and ends up crashing into it and ruining the prediction. In this case, one would not be able to make any predictions concerning the behavior of the system whatsoever even after taking into account every particle that has a well-defined position at the beginning of the time-reversed story (Earman 1986).

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Quantum mechanics

Quantum mechanics is the most successful empirical theory ever, but unlike classical mechanics, it typically allows predictions that are only probabilistic (see QUANTUM MECHANICS). In quantum mechanics the state of a physical system S is given by a vector ψ_S in an appropriate vector space. ψ_S is sometimes called the wave function of S. The state of the system almost always evolves in a linear, deterministic

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way that depends only on the energy properties of the system. In non-relativistic quantum mechanics this deterministic evolution is described by the time-dependent Schrodinger wave equation. Given the standard way of interpreting the quantum-mechanical state, a physical system typically fails determinately to have or determinately not to have a given classical physical property at a time. But systems are found to have the determinate property being measured when a measurement is made. On the von Neumann-Dirac collapse formulation of quantum mechanics this is explained by the collapse of the quantum mechanical state on measurement: when a system S, initially in state ψ_S, is measured, S instantaneously and randomly jumps to a state where the property being measured is determinate (see QUANTUM MEASUREMENT PROBLEM). Which state S jumps to is taken to be an irreducible matter of chance. The probability of ending up in the determinate-property state χ_S is determined by the geometric relationship between the vectors ψ_S and χ_S (the probability is equal to $|\langle\psi|\chi\rangle|^2$). It is because the collapse dynamics is random that one is typically limited to making only probabilistic predictions concerning the results of future observations.

While there is disagreement concerning how one ought to understand quantum mechanics generally, and the collapse dynamics in particular, there is nearly universal agreement that one will never be able to make empirical predictions that do better than the standard quantum probabilities (Albert 1992). In this sense, the quantum probabilities are taken to represent a fundamental limitation to empirical prediction in physics.

Biological and Social Sciences

It is sometimes claimed that biological sciences in general, and evolutionary biology in particular, are not predictive, but this is at worst simply false and at best a simplistic description of a complex situation. There is no question, however, that there are systematic differences between the predictive capabilities characteristic of biological sciences and those familiar from the physical sciences. Some of these are illustrated by an example Mary Williams borrows of characteristic prediction in evolutionary theory: "Sexual dimorphism in the length and color of the furry body covering of bumblebees should show a latitudinal and altitudinal gradient among species of bumblebees, with tropical and low altitude species having more dimorphism" (Williams 1982, 293). As the example suggests, biological predictions more typically concern groups, species, populations, or ensembles than individual entities, and they more often describe unknown past or present states of affairs than future ones. Moreover, the intended scope of predictive generalizations in biology is typically not spatiotemporally unrestricted: they are at a minimum restricted to circumstances in which particular (often unique and evolutionarily contingent) causal mechanisms operate, and they are typically exception-ridden, or asserted ceteris paribus, even within the scope of such intended domains. Mendel's Law of Segregation can be used, for example, to make reliable predictions (of ratios for populations or probabilities for individuals), but even these predictions are both restricted to contexts in which a particular, evolutionarily contingent causal mechanism is operating (i.e. sexual reproduction) and are subject to exceptions (e.g. meiotic drive) even

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in that domain. Perhaps the most interesting question, then, concerns the source of these characteristic differences in predictive capabilities (see POPULATION GENETICS).

The first and most obvious is the relatively greater causal complexity of the natural systems studied by biological sciences as compared with those in the restricted domains in which physical science is able to deliver precise and accurate predictions. As Mitchell (2003, Ch. 5) effectively documents, the relevant complexity here is of several kinds, including the compositional complexity of actual biological systems, the number and variety of causal processes operating in them, the sometimes dramatic and sudden shifts in the relationships between key variables across different ranges of their variation, and the characteristic embeddedness of biological entities in levels of organization with multiple weak, non-additive forces and redundant mechanisms operating both within and between these various levels. The impact of such causal complexity is amplified by the long time-scale of evolutionarily significant effects, during which such complexity must be modeled, controlled, or eliminated to allow effective prediction. Also significant is the characteristic contingency of the states of affairs studied by evolutionary biology and other historical sciences on the occurrence of particular (often rare) events, which themselves constrain and shape the course of future evolutionary change. Perhaps the simplest example here is the effect of mutation: at a given time different mutational substitutions of just a single amino acid can easily lead to extremely different evolutionary outcomes over relatively short time-scales, but the process of mutation is treated as a random variable by evolutionary theory, either because it is genuinely indeterministic or because one does not yet know enough about the process or relevant conditions in particular organisms to predict what, when and how particular mutations will occur with any precision. It is for all these sorts of reasons (although not always in these terms) that philosophers of science have enthusiastically debated whether there are laws of any traditional variety in biological science and the ultimate source of the indeterministic character of evolutionary theory.

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Taking these sorts of complexity and contingency into account suggests that the physical analogue for biological sciences is not predicting the speed and position of a ball rolling down an inclined plane, but something more like predicting the path of a bag of feathers dumped out of an airplane. Physical sciences are able to make some predictions about what will happen in these circumstances, but these predictions will be relatively weak, more likely to concern the feathers as a group, and will only be reliable ceteris paribus or subject to the operation and/or interference of particular causal mechanisms that affect their trajectory (e.g. prevailing weather conditions). Of course, the extent to which these features are characteristic of the predictive application of the physical sciences generally is also controversial (see Cartwright 1983), but biological sciences appear to fare even worse in natural settings and to be even less amenable to the construction of specialized contexts which make precise and powerful prediction possible.

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Alexander Rosenberg (1985, 1994) and others have argued for a related but perhaps more fundamental kind of limitation on the predictive capabilities of biological sciences. Rosenberg suggests that such predictive limitations arise because in typical cases (arguably excluding some parts of molecular genetics) biologically significant categories must be characterized functionally or teleologically, and there is a wide diversity (which seems unlikely to admit of finite specification even in a disjunctive list) of possible physical realizations of such functionally characterized entities. Put another way, the claim here is that (i) there are no type-identities between important explanatory biological categories (like fitness, mimicry, temperature-regulating mechanism, balanced polymorphism, regulatory gene) and the mechanistic physical descriptions of the tokens instantiating these functional kinds in particular cases, and (ii) that it is these mechanistic descriptions that offer power and precision in predicting the range of conditions under which a mechanism will operate, what causal factors might interfere with it, its probable evolutionary trajectory, and the like. Rosenberg thus argues that the goals of prediction and explanation pull in different directions here, and that the diversity and potential infinity of the possible realizations of functionally characterized biological kinds conspire to ensure that biological sciences must retain their weakly predictive character if they are to remain useful to us.

Perhaps a useful example of how this can be so is provided by Fisher's (1930) famous explanation of why the sex ratio in sexually reproducing diploid species at reproductive age is typically 1:1. Briefly, the explanation is that (assuming equal parental cost to produce offspring of either sex, ignorance of offspring quality, and setting aside complications), no matter what the mating system, a parent will spread more copies of its genes by producing offspring of the less numerous sex: since every successful mating requires the genetic contribution of exactly one member of each sex, members of the less numerous sex are more easily able to obtain multiple successful matings, be more choosy about mates, or enjoy whatever reproductive advantages members of that sex enjoy in the mating system. Even if a species' mating is structured in such a way that a few successful males do all the mating and most males do not mate at all, when males are less numerous than females a parent will do better on average by producing sons with a proportional chance of being one of the lucky few than daughters who are guaranteed to mate. Therefore, it pays to produce members of whichever sex is more rare, ensuring strong selective pressure against any mechanism that favors producing male offspring over female or vice versa.

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Contrast this explanation of the sex ratio with that provided by conjoining the physical histories of each organism in each species. There is certainly a sense in which the sex ratio is thereby explained, for this complex history (including the random segregation of sex-determining chromosomes, the details of development and survivorship for each organism, and even the survival and propagation of the specific genetic, physiological, and ontogenetic mechanisms responsible for sex-determination in each species in the first place) deductively imply that sex ratios are what they are (near 1:1 in each case). But this mechanistic explanation, provided in terms that would increase predictive power and precision regarding individual cases, provides nothing like Fisher's insight into the evolutionary reasons for the emergence and persistence of the 1:1 sex ratio. Similarly, a detailed molecular description of the operation of a particular DNA-repair mechanism may permit effective prediction of the conditions under which the mechanism will operate, but it will be unable to explain why such a mechanism exists

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and persists and perhaps even obscure the fact that it is a mechanism for repairing DNA (and thereby minimizing mutational changes) in the first place. Of course, without this functional characterization there would seem little sense left to be made of the question of under what conditions such a mechanism will operate (or do so effectively). Such examples illustrate why the relative predictive weakness of biological sciences might not simply be an unfortunate consequence of increased complexity and contingency in their domains of application, but also an aspect of those sciences intimately bound up with what renders them useful.

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An interesting further question concerns to what extent any of these predictively limiting aspects of biological science also underlie the relative predictive weakness of the social sciences. Causal complexity and contingency are often invoked in this connection, perhaps most famously in an influential argument due to Karl Popper (1957; helpfully discussed by Rosenberg [1993]) to the effect that, because the growth of scientific knowledge has persistently exerted dramatic effects on the course of history and human affairs, the unpredictable trajectory and directions of such growth precludes even the possibility of a predictively robust social science. However, the failure of type identities between important explanatory categories has also been invoked to explain the relative predictive weakness of the social sciences (see Rosenberg 1994). Here it is the intentional explanatory categories of the social sciences which are supposedly both ineliminable and multiply realized by tokens that are heterogeneous in the terms of more predictively precise and powerful sciences, including biology itself (but cf. Nelson 1990). These and closely related considerations are invoked to support a variety of predictively relevant conclusions concerning the social sciences, including the claims that the kinds of predictive limitations discussed above will prove to be ineliminable from them, that social scientific predictions will remain merely 'generic' or qualitative, that genuinely scientific and predictive social science will have to eliminate any appeals to intentional notions, that the study of social phenomena is autonomous and cannot be understood in terms of aggregate actions and dispositions of individuals, and that social inquiry must or should be restricted to the interpretive study of others.

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See, also, BAYESIANISM; CAUSALITY; CONFIRMATION THEORY; DETERMINISM; EMPRIRICISM; INDUCTION, PROBLEM OF; LAWS OF NATURE; LOGICAL EMPIRICISM; PHENOMENALISM; POPPER, KARL RAIMUND; THEORIES; and VERIFICATIONISM.

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