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Lancelot Law Whyte (1967)

UNDERDETERMINATION THESIS, DUHEM-QUINE THESIS

Underdetermination is a relation between evidence and theory. More accurately, it is a relation between the propositions that express the (relevant) evidence and the propositions that constitute the theory. The claim that evidence underdetermines theory may mean two things: first, that the evidence cannot prove the truth of the theory, and second, that the evidence cannot render the theory probable. Let us call the first deductive underdetermination and the second inductive (or ampliative) underdetermination. Both kinds of claims are supposed to have a certain epistemic implication, namely that belief in theory is never warranted by the evidence. This is the underdetermination thesis.

DEDUCTIVE UNDERDETERMINATION

Deductive underdetermination is pervasive in all interesting cases of scientific theory. If the theory is not just a summary of the evidence, the evidence cannot determine, in the sense of proving, the theory. For instance, no finite amount of evidence of the form Aa_i & Ba_i can entail an unrestricted universal generalization of the form *All A's are B*. Deductive underdetermination rests on the claim that the link between evidence and (interesting) theory is *not* deductive. What is the epistemic problem it is supposed to create? Given that the link is not deductive, it is claimed that we can never justifiably believe in the truth of a theory, no matter what the evidence is. However, it would be folly to think that deductive underdetermination creates a genuine epistemic problem. There are enough reasons available for the claim that belief in theory can be justified even if the theory is not proven by the evidence: Warrant-conferring methods need not be deductive.

Deductive underdetermination speaks against simplistic accounts of the hypothetico-deductive method, which presuppose that the epistemic warrant for a theory is solely a matter of entailing correct observational consequences. Two or more rival theories (together with suitable initial conditions) may entail exactly the same observational consequences. Given the above presupposition, it follows that the observational consequences cannot warrant belief in one theory over its rivals. Though simplistic accounts of the hypothetico-deductive method need to be jettisoned, there are ways to meet the challenge of deductive underdetermination, even if we stay close to hypothetico-deductivism. Since theories entail observational consequences only with the aid of auxiliary assumptions, and since the available auxiliary assumptions may change over time, the set of observational consequences of a theory is not circumscribed once and for all. Hence, even if, for the time being, two (or more) theories entail the same observational consequences, there may be future auxiliary assumptions such that, when conjoined with one of them, they yield fresh observational consequences that can shift the evidential balance in favor of it over its rivals. Besides, a more radical (though plausible) thought is that theories may get (indirect) support from pieces of evidence that do not belong to their observational consequences.

INDUCTIVE UNDERDETERMINATION

Inductive underdetermination takes for granted that any attempt to *prove* a theory on the basis of evidence is futile. Still, it is argued, no evidence can confirm a theory or make it probable, or no evidence can confirm a theory more than its rivals. This claim is rather odd. In all its generality, it is a recapitulation of inductive skepticism. If induction lacks justification, then no inductively established theory is warranted by the evidence. Yet induction does not lack justification. In any case, according to recent externalist-reliabilist theories of justification, belief in theory is justified if induction is reliable; and there is no argument that it is *not*. If inductive scepticism is set aside, inductive underdetermination must relate to problems with the theory of confirmation. For on *any* theory of confirmation, the evidence (even if it is restricted to observational consequences) can render a theory probable or more probable than its rivals. That is, the evidence can raise the probability of a theory. So inductive underdetermination must rest on some arguments that question the confirmatory role of the evidence vis-à-vis the theory. There is a battery of such arguments, but they may be classified under two types.

The first capitalizes on the fact that no evidence can affect the probability of the theory unless the theory is assigned some nonzero initial probability. In fact, given the fact that two or more rival theories are assigned different prior probabilities, the evidence can confirm one more than the others, or even make one highly probable. The challenge, then, is this: Where do these prior probabilities come from? A total denial of the legitimacy of any prior probabilities would amount to inductive skepticism. Inductive underdetermination would be inductive skepticism. The more interesting version of inductive underdetermination does not challenge the need to employ prior probabilities, but rather their epistemic credentials. If, it is argued, prior probabilities have epistemic force, then the evidence can warrant a high degree of belief in a theory (or greater degree of belief in a theory than its rivals). But, it is added, how can prior probabilities have any epistemic force?

The subjective Bayesians' appeal to subjective prior probabilities (degrees of belief) accentuates rather than meets this challenge. Bayesians typically argue that, in the long run, the prior probabilities wash out: even widely different prior probabilities will converge, in the limit, to the same posterior probability, if agents conditionalize on the same evidence. But this is scant consolation because, apart from the fact that in the long-run we are all dead, the convergence-of-opinion theorem holds only under limited and very well-defined circumstances that can hardly be met in ordinary scientific cases. The alternative is to claim that prior probabilities have epistemic force because they express rational degrees of belief, based, for instance, on plausibility or explanatory judgements. This claim faces many challenges, but its defense might well be necessary for blocking the epistemic implications of inductive underdetermination. In its favor, it can be said that rational belief in theory is not solely a matter of looking for strict observational evidence.

The second type of argument rests on the claim that theories that purport to refer to unobservable entities are, somehow, unconfirmable. The problem is supposed to be that since there cannot be direct observational access to unobservable entities, no observational evidence can support the truth of a theory that posits them, and no evidence can support a theory more than others that posit different unobservable entities. The distinctive element of the second type of argument is that the resulting inductive underdetermination is selective. It does *not* deny that observational generalisations can be confirmed. Hence, it does not deny that the evidence can confirm or render probable observational theories. It denies that the same can be the case for theories that refer to unobservable entities.

Even if a sharp distinction between observable and unobservable entities were granted (though it is by no means obvious that it should), this selective inductive underdetermination has a bite only if the methods that lead to, and warrant, belief in observable entities and observational generalizations are different from the methods that lead to, and warrant, belief in theories that posit unobservable entities. Yet the methods are the same. In particular, explanatory considerations play an indispensable role in both cases. In the end, this kind of selective inductive underdetermination undermines itself: it either collapses into inductive skepticism or has no force at all.

EMPIRICAL EQUIVALENCE

It is commonly argued that there can be totally empirically equivalent theories— that is, theories that entail exactly the same observational consequences under any circumstances. In its strong form, this claim (let's call it the Empirical Equivalence Thesis, *EET*) asserts that *any* theory has empirically equivalent rivals (some of which might be hitherto unconceived). *EET* is an entry point for the epistemic thesis of total underdetermination: that there can be no evidential reason to believe in the truth of *any* theory. But there is no formal proof of *EET*, though a number of cases have been suggested ranging from Descartes' "evil demon" hypothesis to the hypothesis that for every theory T there is an empirically equivalent rival asserting that T is empirically adequate yet false, or that the world is *as if* T were true. One can, of course, argue that these rival hypotheses have only philosophical value and drive only an abstract philosophical doubt. In science, it is often hard to come by just one totally empirically adequate theory, much less a bunch of them.

Yet it seems that there is a genuine case of empirical equivalence of theories of quantum mechanics. Alternative interpretations of the quantum-mechanical formalism constitute empirically equivalent but different theories that explain the world according to different principles and mechanisms. The most typical rivalry is between the orthodox understanding of quantum theory—the "Copenhagen interpretation," according to which a particle cannot have a precise position and momentum at the same time—and the Bohmian understanding of quantum theory—the hidden-variables interpretation, according to which particles always have a definite position and velocity, and hence momentum. On Bohm's theory, particles have two kinds of energy: the usual (classical) energy and a "quantum potential" energy. More recently, there have been three particularly well-developed theories (the Bohmian quantum mechanics, the many-worlds interpretation, and the spontaneous-collapse approach) such that there is no observational way to tell them apart. And it seems that there *cannot* be an observational way to tell them apart. This situation is particularly unfortunate, but one may respond that the ensued underdetermination is local rather than global; hence the possible skepticism that follows is local.

The Duhem-Quine thesis has been suggested as an algorithm for generating empirically equivalent theories. Briefly put, this thesis starts with the undeniable premise that all theories entail observational consequences only with the help of auxiliary assumptions and concludes that it is always possible that a theory, together with suitable auxiliaries, can accommodate *any* recalcitrant evidence. A corollary, then, is that for any evidence and any two rival theories T and T', there are suitable auxiliaries A such that T' and A will be empirically equivalent to T (together with its own auxiliaries). Hence, it is argued, no evidence can tell two theories apart. It is questionable that the Duhem-Quine thesis is true. There is no proof that *nontrivial* auxiliary assumptions can always be found.

But let us assume, for the sake of the argument, that it is true. What does it show? Since the Duhem-Quine thesis implies that any theory can be saved from refutation, it does create some genuine problems to a falsificationist (Popperian) account of theory testing— that is, the view that theories are tested by attempting to refute them. If attempted refutations are the sole test for theories, two incompatible theories that are not refuted by the evidence are equally well tested by it. But the Duhem-Quine thesis does not create a similar problem to an inductivist. From the fact that any theory can be suitably adjusted so that it resists refutation it does not follow that all theories are equally well confirmed by the evidence. An inductivist can argue that the empirical evidence does not lend equal inductive support to two empirically congruent theories. It is not necessarily the case that the auxiliary assumptions that are needed to save a theory from refutation will themselves be well supported by the evidence. Since it is reasonable to think that the degree of support of the auxiliary assumptions associated with a theory is reflected in the degree of support of the theory, it follows that not all theories that entail the same evidence are equally well supported by it.

EET has generated much philosophical discussion. An argument favored by the logical positivists is that such cases of total underdetermination are illusions: the rival theories are simply notational variants. This move presupposes that theories are not taken at face value. For anyone who does not subscribe to a verificationist criterion of meaning, this move is moot. It does make sense to say that there *can* be distinct but totally empirically equivalent theories. The hard issue is not to exclude their possibility on a priori grounds but to find ways to distinguish their epistemic worth, should we find ourselves in such a predicament.

Another move, favored by Quine, is to go for pragmatism: The balance is shifted to the theory *we* (our community) favor, simply because it is *our* theory. This raises the spectre of epistemic relativism. Yet another move is to go for skepticism: among rival totally empirically equivalent theories one is true, but we cannot possibly come to know or justifiably believe which this is. This skeptical answer might be supplemented with some differential stance towards the rival theories, but this differential treatment will not be based on epistemic reasons but rather on pragmatic considerations. Indeed, social constructivists have seized upon this in order to claim that social, political, and ideological factors break observational ties among theories: hence, they argue, belief in theory is socially determined.

The general problem with the skeptical move is that it rests on a restricted account of what counts as evidence (or reason) for justified belief; it counts only observations as possible epistemic reason for belief. But rational belief may well be a function of other epistemic reasons—for instance, the theoretical virtues that a theory possesses. This last thought ushers in yet another possibility: that empirically equivalent theories may well differ in their explanatory power. Insofar as explanatory power can offer epistemic credentials to a theory, it can break supposed epistemic ties among totally empirically equivalent rivals. This move makes rational belief a more complex affair and tallies with the intuitions of scientific and common sense. Yet it faces the problem of justifying the claim that theoretical virtues are epistemic reasons— that is, that a virtuous theory (a theory with great explanatory power) is more likely to be true than a less virtuous one.

This is not an unsolvable problem. There are, broadly, two ways to tackle it. One is to argue (rather implausibly) that some theoretical virtues are constitutive marks of truth. The other is to argue for a broad conception of evidence that takes the theoretical virtues to be empirical and contingent marks of truth. A central element in this latter argument is that theories can get extra credence by entailing novel predictions—that is, predictions such that information about the predicted phenomenon was not previously known and not used in the construction of the theory. In the end, the epistemic relations between evidence and theory cannot be exhausted by their logico-semantic relations.

See also Confirmation Theory; Scientific Realism.

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Stathis Psillos (2005)

UNITY AND DISUNITY IN SCIENCE

Unity covers a wide range of loosely connected ideas in science, differently analyzed by different interpreters. Generally, they are expressions, or echoes, of the idea that science can succeed in providing one consistent, integrated, simple, and comprehensive description of the world. This entry will provide a historical perspective on such ways of thinking about unity in science. (Readers should bear in mind that the real history is much more complex and interesting than the following microsketch, which is intended only to introduce the leading ideas.)

MECHANISMS AND LAWS

The scientific revolution of the seventeenth century involved consolidation of the "mechanical (or corpuscularian) philosophy" according to which natural phenomena are to be understood in terms of shaped matter in motion, with the natural world likened to a giant mechanism. Natural philosophy could look for unity in this regard by thinking of the parts of the world machine as all governed by the same simple set of rules or laws. Isaac Newton's mechanics could be seen in this regard as a paradigm of unification, showing how the same laws covered motion in both the heavens and on Earth.

But there was a monkey wrench in this mechanist paradigm: Newton's law of gravity involved "action at a distance," inadmissible by most seventeenth-century interpreters as a legitimate mechanical principle. Mechanism required contact action. Newton's official response was that "I make no hypotheses," that is, no hypotheses or speculations about what the underlying real mechanism of gravity might be. Instead, he presented his mechanics as "mathematical only," that is, mathematical principles by which motions can be reliably and accurately described but with no pretense to describing what makes things move as they do. Accordingly, some of Newton's successors thought of unity in theory and in science in terms of a simple set of general, mathematical laws that integrate, by covering, a wide range of phenomena that otherwise might seem independent, and all this without any thought of underlying mechanisms. This will be referred to as the "nomological attitude."

These two ideas, seeing disparate phenomena as manifestation of one underlying mechanism or covered by one set of simple laws, interacted and intertwined during the eighteenth and nineteenth centuries. For example, James Clerk Maxwell worked to treat first electric and magnetic effects and then discovered he could also cover optical phenomena, thinking of all of these first as manifestations of one underlying mechanism, developing the laws that might govern such a mechanism, and then letting go of the postulated underlying mechanism as unverifiable speculation in favor of the general laws that had emerged. Heinrich Rudolf Hertz maintained that Maxwell's theory is Maxwell's equations, and eventually Albert Einstein's special relativity did in the speculated stuff of electromagnetic mechanisms, the luminiferous aether.

The opposition of mechanisms versus laws also played out, with the opposite result, during the second half of the nineteenth century over the issue of atoms. The predictive and explanatory success of chemistry, as well as the nascent kinetic theory (statistical mechanics), emboldened some to see atoms and molecules as real cogs in the cosmic machine. Others scoffed at postulation of things too small to see or individually detect as "metaphysics," not science. Continuum mechanics and even contact action presented severe problems for an atomistic theory. The speculated indivisibility of atoms, though mentioned by some, was not really the issue. Rather, it was whether one could correctly think of the underlying order in terms of discrete parts interacting in something like the mechanist tradition or whether this should be seen, at best, as a kind of pretty imaginative picture, while scientific truth was exhausted by mathematical laws in the nomological tradition.

The issue of atoms came to a head in the first decade of the twentieth century in the work augmented and integrated by Jean-Baptiste Perrin. Perrin catalogued the astonishingly numerous and diverse facts that could be encompassed by postulating atoms: constant ratios in chemistry, relative atomic weighs, diffusion and other fluctuation phenomena, osmotic pressure, behavior of electrolytes, specific heat, behavior of thin materials, even why the sky is blue. Perrin tabled sixteen independent ways of reaching the same estimate of Avogadro's number. Einstein's theory of Brownian motion proved especially effective—in a sense one could "see" the causal effects of individual molecular collisions. A vast range of otherwise diverse observable phenomena were unified in the sense of interpreting them as the manifestation of the properties and behavior of atoms. By 1913 most of the physics community accepted atoms as real.

Electric, magnetic, and optical phenomena unified by Maxwell's laws. Perrin's diverse phenomena unified by postulation of atoms. Though they are in some ways polar attitudes, mechanistic and nomological thinking really cannot operate without one another. To provide unifying explanations, mechanisms need to be governed by laws, and laws, if they are to do more than exhaustively list superficially observable phenomena, must at least have the form of describing some conceptually more economic structure.

REDUCTIONISM

The nineteenth century saw explosive development of the natural sciences, emboldening some toward the end of the century to speculate that physics was almost completed with little left to do but to work out the applications to other natural phenomena. Contrary to what one might have imagined, the shocks of relativity and quantum mechanics in the first quarter of the twentieth century initially encouraged rather than tempered such scientific utopian attitudes. Some strands of positivism in the second quarter of the century described unity of science in terms of unity of language and methods; others took the spirit of unification to its logical extreme, emphasizing axiomatic formulation and developing the idea of reduction of all natural phenomena to "fundamental physics" in the spirit of the logicists' hope of reducing all of mathematics to logic. By the 1950s and 1960s reductionistic thinking had taken a deep hold on much thinking in both philosophy and science, no doubt encouraged by advances within science in subjects such as quantum chemistry and microbiology. Unity now took the form of (expected) chains of reductive definitions, identifying not just complex physical, but biological, psychological, and social phenomena with the behavior of physical parts, everything ultimately to be described in terms of the laws of fundamental physics.

Again a monkey wrench, or this time two, brought the reductionist juggernaut to a halt. In the 1970s and 1980s philosophy of science became acutely aware of difficulties with the whole reductionist program. The reversal began with the collapse of the two show cases: claimed deductive reduction of thermodynamics to statistical mechanics and of Mendelian to molecular genetics. Temperature is in fact realized by mechanisms in addition to mean kinetic energy, and in principle could be realized in indefinitely many ways. There is no neat one trait–one gene correlation and the developmental effects of any one bit of DNA depend, not just on its genetic, but on its overall environmental context. If temperature and genes are multiply realizable by disparate physical constructs, then surely also, for example, are mental states. Higher level objects and phenomena may still all be physically realized, but in such diverse ways that the program of reduction by definitions and deduction loses plausibility. *Unity* no longer seems such an apt term.

This first basis for some kind of disunity was followed in the 1980s and 1990s by a second. Nancy Cartwright, Ronald N. Giere, and others have pointed out that, whatever the ultimate aims of science or of some scientists might be, the science we actually have, now or any time in the foreseeable future, hardly follows the pattern of calculation of phenomena from universally applicable, exact, true laws or of description in terms of mechanisms known or even believed to operate exactly as described. Rather, science uses laws in the construction of idealized models, always limited in scope, and even where they apply never exactly correct. Rather than providing descriptions that set out exactly what the phenomena are, the laws of science are only true, or at least only exactly true, of the idealized models that in turn enable us to understand phenomena and their hidden sources in terms of the idealizations to which the phenomena are similar. For the puny minds of even the best physicists, to understand the fluid properties of water we need to resort