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Underdetermination as an Epistemological Test Tube:

Expounding Hidden Values of the Scientific Community

Abstract: Duhem-Quine underdetermination plays a constructive role in epistemology by pinpointing the impact of non-empirical virtues or cognitive values on theory choice. Underdetermination thus contributes to illuminating the nature of scientific rationality. Scientists prefer and accept one account among empirical equivalent alternatives. The non-empirical virtues operating in science are laid open in such theory choice decisions. The latter act as an epistemological test tube in making explicit commitments to how scientific knowledge should be like.

1. Introduction

The underdetermination thesis states that any given set of data can always be represented by different, conceptually incompatible accounts. I will elaborate this claim later but it is clear already from this brief description that the thesis proceeds on the assumption that the evidence is given. The issue is to assess the impact of the evidence on the credibility of theoretical principles. The reputation of the underdetermination thesis has undergone various changes in the past decades. It was considered highly plausible in the days of Logical Empiricism (with its strict distinction between facts of experience and pragmatic criteria) and during the period of the theory-change debate (with its emphasis on historical contingency). Since then, its reputation has dropped off (see Laudan & Leplin 1991). The high point in its esteem during the 1970s marked the inception of social constructivism with its attempt to fill the room left open to human choice by underdetermination with social factors. The relativism inherent in social constructivism is a historical product, although not a logical consequence, of the underdetermination thesis.

My claim is that the underdetermination thesis is basically correct, provided that it is construed adequately. Underdetermination is not an inevitable feature of scientific theorizing; it follows rather from a specific methodological orientation, namely, the commitment to hypothetico-deductive testing. Furthermore, underdetermination does not open the floodgates to relativism but rather plays a positive and fruitful role in epistemology by pinpointing the impact of non-empirical virtues or cognitive values on theory choice. Rather than being a threat to scientific rationality, it contributes to illuminating what scientific rationality actually is.

My chief argument is aimed at analyzing the role and function of underdetermination in the philosophy of science. I wish to expound a constructive role for Duhem-Quine underdetermination, which serves to bring to light the non-empirical epistemic commitments which prevail in the scientific community. Scientists faced with empirically equivalent alternatives cannot make their choice by invoking empirical adequacy as a yardstick. Yet the observation is that scientists prefer and accept one account under such conditions. The non-empirical virtues operating in science are laid open in the choice between Duhem-Quine alternatives. Such virtues form an indispensable part of how scientific knowledge is conceived.

2. Methodological Transitions: Inductivism and Hypothetico-Deductivism

The rise of the underdetermination thesis is in large measure the result of a major methodological reorientation in the 19th century. The dominant methodological approach in the 17th and 18th centuries was inductivism, elaborated systematically by Francis Bacon. According to Bacon, the chief distinction of knowledge was its objectivity. That is, scientific knowledge should faithfully represent the phenomena and not be distorted by human choices or fantasies. Acceptable scientific hypotheses are determined, without remainder, by the properties of the objects investigated and completely free of any human addition.

Bacon designed a two-way test and confirmation procedure whose early steps consisted in eliminating all subjective influences like predilections and prejudice. Rather, hypotheses are supposed to be generated by a carefully regulated process of observation and experimentation. This process of inductive ascent is intended to make sure that the ensuing hypothesis relies exclusively on the facts of observation. The complementary step of deductive descent involves the derivation of observations other than those used in the formation of the hypothesis. The hypothesis is assessed by comparing the anticipated consequences with the actual observations (Carrier 2006, 18-26).

In the course of the 19th century, inductivism was gradually replaced by a hypothetico-deductive understanding of empirical examination in science. Hypothetico-deductivism dispenses with the first inductive step: constraints on hypothesis formation are abolished and methodological considerations begin only when the hypothesis has been formulated. Hypothetico-deductive assessment involves the tentative or hypothetical adoption of an assumption and its evaluation by deducing empirical consequences that can be compared with experience.

The hypothetico-deductive methodology was advocated as the only legitimate test and confirmation procedure in science by Pierre Duhem and was later defended by Karl Popper, Carl Hempel, Hans Reichenbach and others. This approach grew out of the realization that the accepted theories of 19th century science were in no way confined to hypotheses that could be reconstructed as being suggested by experience. Rather, theories of optics, electromagnetism or thermodynamics aimed to capture processes that go far beyond the reach of the senses. Such theories are not inductively formed, but rather free creations of the human mind.

Consequently, hypothesis formation is handed over to psychology by hypothetico-deductivism; methodological considerations only come into play when the assumptions are there and ready to be judged. This reorientation becomes manifest in Reichenbach's widely received distinction between the context of discovery and the context of justification (Reichenbach 1938, 3-7). The former contains the pathways pursued for arriving at a certain conjecture, and it says nothing about the viability of the conjecture. Supporting reasons and confirming evidence are part of the context of justification which includes the arguments or data on which the validity of the assumption is intended to rest. The separation of the two contexts is often passed as a hallmark of rationality in general, but it is peculiar, in fact, to a hypothetico-deductive setting and has no backing in an inductivist framework (Carrier 2006, 35-38).

Adopting the hypothetico-deductive framework had a liberating impact on scientific thought. Hypotheses were admitted as legitimate candidates for empirical examination that would have been ruled out right away within inductivist confines. In addition, the unprejudiced evidential basis, as demanded by Baconian inductivism, is impossible to establish. Rather, hypotheses are needed for structuring the data and the relevant evidence is produced by applying theories.

Hypothetico-deductivism clearly has its merits, chiefly by removing methodological obstacles to using scientific theories for exploring nature's workings. However, a price needs to be paid. Namely, the standards brought to bear in hypothetico-deductivist testing are lowered in one respect. It is no longer required that a hypothesis be suggested by the data. The spectrum of legitimate assumptions that are licensed, as it were, by methodology to be subjected to empirical examination is greatly enlarged. It is this expanded leeway on which the underdetermination thesis thrives. Underdetermination is an unintended by-product of the methodological transition from inductivism to hypothetico-deductivism.[i]

3. Duhem's Master Argument for Underdetermination

The chief argument for the underdetermination of theory by evidence was conceived by Pierre Duhem early in the 20th century. In a two-pronged approach, Duhem showed that confirmation and refutation of particular hypotheses in a hypothetico-deductive framework cannot be grounded on logic and experience alone. In the most basic hypothetico-deductive framework, a hypothesis entails an observational consequence whose verification is then supposed to give credit to the hypothetical premise. As a result of this fallacious move, a successful test of this sort can never establish the truth of the hypothesis. In particular, the same observational consequences may equally follow from a different hypothesis incompatible with the first.

The history of science testifies that this limitation is not an abstract proviso from the logic textbook. Duhem refers to the geocentric astronomy of antiquity as his chief example. Apollonius of Perge had demonstrated around 200 B.C. that two distinct assumptions about the annual revolution of the solar orbiting sphere were equally fit for reproducing the astronomical record. The eccenter hypothesis located the Earth at a distance from the center of the solar orb, whereas the alternative epicycle hypothesis adhered to the Earth-centered solar orb but placed an additional epicyclic sphere at its circumference (Duhem 1908, 3-27; Kuhn 1957, 66-69). With respect to conceptual structure, the two assumptions are certainly dissimilar. It truly makes a difference regarding the structure of the planetary system whether one orb or two are attributed to the Sun. Yet terrestrial experience is at a loss to distinguish between them. In the wake of this finding, astronomy was thought to be incapable of elucidating celestial motions in all respects and was taken to be constrained to "saving the phenomena."

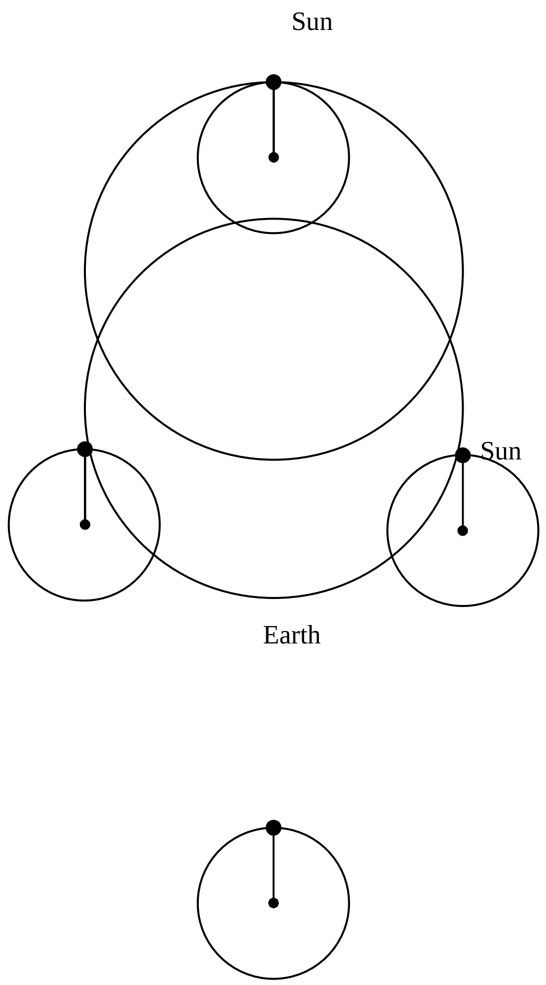

Figure 1: Empirically equivalent accounts of annual solar motion

The second half of Duhem's argument concerned hypothetico-deductive refutation and was intended to show that no hypothesis can be refuted by relying on logic and experience only. Its basis is the observation that in scientific practice no isolated hypotheses are put to empirical scrutiny but rather collections of hypotheses. Theoretical principles need to be articulated with the help of auxiliary assumptions in order to yield definite empirical consequences in the first place whose verification requires a number of observation theories (employed in setting up and running the pertinent instruments). For this reason, an anomalous observation merely demonstrates that at least one of the assumptions invoked in the test procedure is mistaken. Logic and experience fail to single out the culprit (Duhem 1906, X.2, X.10).

As a result, more than one option for fixing a flaw should exist. Consider again the example of early astronomy. The most cherished principle of astronomy until the inception of the scientific revolution concerned the uniformity of celestial motions. This principle was called into doubt by the observation that in the northern hemisphere winter lasts six days shorter than summer. Apparently, the Sun moves faster in the winter—which contradicted the uniformity principle. The two hypothetical constructions mentioned saved this principle at the expense of adjusting other assumptions. The eccenter model dropped the idea that the Earth was located at the center of all celestial motions, while the epicycle model abandoned the conception that celestial bodies performed only one uniform motion. Both models proved expedient for directing the refuting power of the anomaly away from the *prima-facie* relevant principle and toward other less than pivotal beliefs (Carrier 2006, 43-54).

The upshot is that hypothetico-deductive confirmation fails to make sure that the supported hypothesis is, in fact, true. Rather, alternative assumptions may yield the same empirical consequences and thus be supported by the same facts. The conclusion advocated by Duhem is that theories never lose their dependence on human imagination. Nature always leaves room for alternative accounts. Neither proofs nor refutations are part of scientific method.

4. Duhem, Quine, and Beyond: The Career of Underdetermination

The notion of underdetermination suggested by the Duhemian treatment encompasses the following characteristics. First, Duhem refers to real-life theories, not to artificially contrived toy models. Duhem’s other examples are taken from the practice of 19th century optics and electrodynamics. In other words, the alternatives he presented were genuinely and interestingly different. Second, the possibility of alternative accounts is stressed but no recipe for actually constructing them is provided. Duhem's idea is that demonstrating the existence of alternatives in some cases makes it plausible that the failure to come up with suitable deviant explanations in other cases is due to a lack of imagination. Third, Duhem's underdetermination thesis is confined to a given range of phenomena. When this range is expanded, the empirical equivalence between two alternative accounts may collapse. For instance, both the wave and the particle theories of light were able to accommodate the whole of geometric optics, but this equivalence broke down when the causes of refraction were taken into consideration. Foucault's experiment showed in the mid-19th century that the undulatory account was empirically superior (Duhem 1906, X.2).

In the more recent debate about underdetermination, a distinction is drawn between "in-principle equivalence" and "temporary indistinguishability." According to the first construal, underdetermination refers to all possible experience; it means that conceptual and logical analyses can establish that under no circumstances the two alternatives will diverge from one another in empirical respect. The second construal is weaker and says that within a given realm the same empirical consequences ensue. This temporary indistinguishability is actually the feature Duhem had in mind; it involves a restriction to a fixed realm of data. Transient underdetermination is sometimes mistaken as uninteresting and trivial (Laudan & Leplin 1993, 8-10). The claim I wish to maintain is that this triviality charge is misguided and that temporary underdetermination plays an important epistemological role: it serves to illuminate the bearing of non-empirical virtues in judging hypotheses and adopting theories.

The canonical formulation of the underdetermination thesis goes back to Willard Van Orman Quine, without reference to Duhem and with a stronger emphasis on the malleability of the theoretical description and on the in-principle character of underdetermination. This thesis is generally known as Duhem-Quine underdetermination; it comes in the two variants suggested already by Duhem, namely, with an emphasis on confirmation or refutation, respectively. The confirmational version says that any given set of data can always be represented by different, conceptually incompatible accounts; the refutational variant suggests that any hypothesis can be maintained in the face of arbitrary evidence if one is prepared to adjust the system of beliefs, maybe profoundly, in other respects. The refutational version is more basic since it implies the confirmational one but is not implied by the latter. If there is a choice between adjusting two different hypotheses so as to cope with an empirical difficulty, it follows that there is a choice between two distinct but empirically equivalent sets of statements. By contrast, if there is a choice between two such systems, it does not follow that any particular statement from this set can be retained.

Quine gives several formulations of the refutational version. „Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. … Conversely, by the same token, no statement is immune to revision" (Quine 1953, 43). Analogously: "Just about any hypothesis, after all, can be held unrefuted no matter what, by making enough adjustments in other beliefs—though sometimes doing so requires madness" (Quine & Ullian 1970, 79). In the following quote, Quine suggests the transition from the refutational to the confirmational variant. "Discarding any particular hypothesis is just one of many ways of maintaining consistency in the face of a contrary observation; there are in principle many alternative ways of setting our beliefs in order" (loc. cit. 103).

Duhem had only claimed that there is more than one option for adapting a theoretical system to an anomaly; Quine goes beyond Duhem's claim by saying that each and every assumption can be saved at will in the face of whatever evidence, which entails that the room left to human choice is considerably enlarged. The general argument advanced by Quine in favor of the underdetermination thesis always relies on the logical inconclusiveness of hypothetico-deductive testing: confirming a hypothesis by verifying its observational consequences is tantamount to inferring the truth of the premise from the correctness of its consequent. Let me examine the nature of the underdetermination thesis by looking more closely at typical examples that emerged in the debate.

The discussion of underdetermination in the second half of the 20th century is dominated by two strategies. The first one is characterized by attempts to articulate the thesis and to make the claims involved in underdetermination more tangible and concrete, which required immersing in the relevant nitty-gritty. The second line of reasoning accepted underdetermination as an established principle and used it as a premise of far-reaching conclusions regarding theory construction and theory change. In some of the subsequent examples, the articulation of the underdetermination thesis is taken in the service of a philosophical project of theory construction or theory change so that the two strategies to be outlined are not mutually exclusive.

A standard scenario for articulating underdetermination involves the distinction between the observed and the unobserved behavior of entities. The argument is conceived in an egocentric version by Hans Reichenbach (1938, 136-140) but was soon generalized to an intersubjective setting. Here is a general recipe for constructing an empirically equivalent alternative to an arbitrary theory *T*. Assume that a set of events or processes, as described by *T*, displays certain properties or regularities. Accept *T* for observed events or processes and stipulate deviant properties or regularities for unobserved events and processes (Kukla 1993, 4-5). It goes without saying that such a move requires the adaptation of causal principles. In particular, we are led to attribute a certain causal power to the act of observation. Yet such a feat is not ruled out by any experience; given that similar maneuvers are under debate in some interpretations of quantum mechanics, it is not even completely implausible.

It is true, such changes caused by the act of observation cannot be attributed to a physical mechanism that could be captured theoretically or integrated into the system of knowledge. But what matters with respect to underdetermination is that the introduction of such a nomological split between observed and unobserved events is not thwarted by any observation. After all, Quine explicitly concedes that some of the adjustments necessary to regain empirical equivalence require "madness" (see section 4). What militates against such a split is "the postulate of the homogeneity of causality" (Reichenbach 1938, 139), whose adoption is favored by the goal or virtue of conceptual parsimony. If parsimony is accepted as a cognitive goal, it is preferable to employ one uniform account, rather than two heterogeneous ones, once the experiences can be accommodated equally both ways.

Another standard strategy for producing empirically equivalent alternatives is quantity replacement. The model of this strategy is provided by what Clark Glymour has called "deoccamization," which is supposed to indicate that the use of Ockham's razor is suspended. It amounts to supplanting a theoretical quantity of a theory by a fixed combination of several other quantities, none of which has separate empirical bearing. For instance, each occurrence of the term "force" in Newtonian mechanics is replaced by the expression "force_1 + force_2" without appending any distinctive features to either partial force. It is clear that dividing up the Newtonian force conceptually in this fashion will produce no empirically tangible distinctions (Glymour 1980, 30-32).

Examples of this replacement-strategy can be found in procedures developed in the 1950s for interpreting theories in an instrumentalist fashion. One such procedure is to transform a theory into its Ramsey-sentence in which all its theoretical terms figure as existentially quantified variables. The replacement strategy serves to transform the underdetermination thesis into a proven theorem. By appeal to "instrumentalist algorithms," empirically equivalent alternatives to any given theory can be constructed (Kukla 1993, 4). The drawback is that these rephrased versions are completely dependent conceptually on their respective originals. As Larry Laudan and Jarrett Leplin objected, these alleged alternatives are utterly parasitic upon their originals in substantive respect and hide this mimicry by setting up conceptual ornaments. In fact, as the criticism continues, we are not dealing with alternative accounts at all but rather with different interpretations of the same theory. And it is clear that interpreting a theory differently will not alter its empirical consequences (Laudan & Leplin 1991, 456-457; see also Norton 2008, 33-36).

The conclusion of the critics is that the underdetermination thesis is either trivial or false. Adolf Grünbaum argues that underdetermination in general only obtains if no constraints of plausibility are placed on the necessary adjustment strategies. Saving a hypothesis or theory may demand adopting weird and empirically unsupported assumptions—which involves a trivialization of the underdetermination thesis. By contrast, if minimum standards of reasonableness are upheld, the existence of empirically equivalent alternatives is a contingent matter—which means that underdetermination as a general claim is mistaken (Grünbaum 1963, 106-115). In the same vein, Laudan and Leplin conclude that the underdetermination thesis is trivial if transient indistinguishability, instrumentalist analogs, and the like are accepted as relevant alternatives. Rather, the thesis ought to require that to each theory an empirical equivalent alternative can be constructed that is conceptually autonomous and qualifies as a "genuine theory." Yet there is no guarantee whatsoever that alternatives in this demanding sense can always be specified (Laudan & Leplin 1991, 455-456; Laudan & Leplin 1993, 8-11).

I have attempted to show in this brief reconstruction that the underdetermination thesis has gained in stringency during its career. For instance, it has changed its modality from the claimed *possibility* of pointing out suitable alternatives to the requirement of the provable, i.e., *necessary*, existence of such alternatives. Yet the original commitment to interestingly different alternatives has been maintained, which then saddles the advocate of underdetermination with the formidable task of specifying algorithmic procedures for producing conceptually non-derivative, genuinely distinct theoretical alternatives. Yet it helps to recall that Quine did explicitly waive the standards of plausibility and common sense for the envisaged empirically equivalent alternatives (see sec 4). There is no doubt that Quine's version of the underdetermination thesis licenses recourse to what the mentioned critics would unhesitatingly call "trivial revisions" (Klee 1992, 488-489). My claim is that nevertheless the underdetermination thesis serves a non-trivial epistemological function.

Before I go into this claim, let me briefly address the second historical thread in the career of the thesis. This second tradition of philosophical reception takes underdetermination as an established feature of scientific theorizing and uses it as a premise for far-reaching conclusions regarding methodology. Imre Lakatos invokes the underdetermination thesis as a pivotal presupposition in his methodology of scientific research programs. Lakatos gives Duhem's irrefutability argument a positive twist: the impossibility of targeting a specific hypothesis on the basis of an anomaly issues a license to adhere to a set of principles as the "hard core" of a research program. Such principles are constitutive of a program and are never abandoned on empirical grounds. The reason for this irrefutability is neither their established truth nor their methodological deficiency (as in Popper), but rather the firm determination of the proponents of the program not to admit any counterevidence and to direct the force of anomalies away from these principles (Lakatos 1970, 18-19, 48-49, 96-100).

In this way, Lakatos turned underdetermination into a constructive element of scientific change: underdetermination creates room for human imagination to determine the pathways of science. By the same token, it is not the facts that decide about theories but rather non-empirical virtues. Lakatos suggested non-empirical but epistemic achievements (like the prediction of novel phenomena in contrast to the explanation of known features) as yardsticks for theory choice. I will take up this line of reasoning in the next section.

Whereas Lakatos focuses on cognitive values and the growth of knowledge, others bring to bear non-epistemic criteria of choice. Otto Neurath is the prime mover of this tradition. In his view, Duhem has demonstrated that there is room for theoretical pluralism in science so that it is legitimate to select systems of statements according to practical demands. Neurath's recommendation is to fill the Duhemian leeway by adopting theories that are most suitable for guiding action. This train of thought has been taken up among social constructivists. A lot of sociological factors are claimed to be influential on the assessment of hypotheses. Professional interests of the scientific community are among them but also interests of wider parts of society. Science is part of sociopolitical power play and the latter clearly affects the core business or the "internal" challenges of science. Underdetermination creates room for admitting social factors to science (Bloor 1976, Chap. 1; Bloor 1981, 203-204).

5. In Defense of Duhem-Quine Underdetermination

The thesis I wish to outline is intended to illuminate the constructive role underdetermination can play in philosophy of science. Duhem-Quine underdetermination can be understood as setting limits on hypothetico-deductive testing, but it can also be construed as a positive claim about options left to scientific theorizing by experience. I suggest regarding underdetermination as an epistemological test tube. If scientists are faced with empirically equivalent alternatives, the commitment to empirical adequacy fails to establish the superiority of any account at hand. Yet the scientific community regularly makes a choice in such cases. Regarding the examples sketched in section 4, the vote of scientists is directed quite unanimously against nomological splits and deoccamized theories. These choices are necessarily guided by non-empirical virtues and the cognitive value employed obviously is conceptual parsimony. By applying Ockham's razor we go beyond what is presented to us by nature and bring to bear values which have to do with our understanding of how scientific knowledge should be like.

It is clear that in the more usual cases empirical adequacy constitutes a major benchmark for assessing theories. But focusing on Duhem-Quine alternatives is comparable to a solar eclipse in which stars near the sun, invisible otherwise, are disclosed to our eyes. Similarly, non-empirical values that are usually operative in a hidden way are laid open in the choice between empirically equivalent alternatives. Underdetermination serves the function of making such non-empirical commitments in science explicit. Additional standards need to be invoked for selecting a theory as the adequate or correct one to the exclusion of their empirically equivalent alternatives. Underdetermination provides us with an opportunity to elucidate in which sense the preferred accounts are superior.

If underdetermination is viewed from this more positive perspective, it is no longer necessary to distinguish between trivial and interesting alternatives. Duhem-Quine alternatives need not be serious competitors. Even seemingly ridiculous rivals, at once dismissed by any scientist, may point to non-trivial virtues brought to bear on theory choice. Although some of the alternatives used for this purpose may not deserve serious consideration, the criteria made explicit with their help may yet merit attention. They bring to light a commitment to the sort of knowledge we strive for.

The non-empirical values involved become more significant if we turn to Duhem-type examples, that is, to competing real-life accounts that were indistinguishable in empirical respect. Think of the struggle between Ptolemaic geocentrism, Copernican heliocentrism and Tychonic geoheliocentrism in late 16th century astronomy. All three accounts yielded the planetary motions with roughly the same accuracy; they can be considered empirically equivalent in practical respect. What we observe in the debate is the influence of two methodological yardsticks, namely, explanatory power and the coherence with established background knowledge. Theories with great explanatory power need a minimum of independent principles to account for a broad class of phenomena in an accurate fashion. Copernican astronomy excelled in this respect as regards the qualitative features of planetary motion. For instance, the periodic occurrence of retrograde motion and its observable properties could be accounted for by invoking nothing but the core principles of the Copernican theory whereas Ptolemy needed additional, tailor-made assumptions for every single aspect of the phenomenon (Carrier 2001, 81-92). However, geocentric astronomy outperformed its Copernican rival regarding its coherence with the accepted Aristotelian physics. The Aristotelian account of the origin and nature of the weight of heavy bodies could not be squared persuasively with the assumption that the Earth is revolving around its own axis and located at a distance from the center of the universe (see Carrier 2001, Chap. 6; Carrier 2008, 276-277). Copernican theory suffered from its incompatibility with the physics of the period.

The quick acceptance of the geoheliocentric account can be attributed to the impact of the very same criteria. The Tychonic compromise system preserved the explanatory achievements of the Copernican approach and remained in agreement with most of the received physics and cosmology (and later proved even able to digest Galileo's discovery of the phases of Venus and the moons of Jupiter). The upshot is that, first, the scientific community did make a choice between empirically equivalent alternatives and that, second, the criteria operative in this choice were explanatory power and coherence with background knowledge.

Another pertinent case comes from space-time theory and concerns the so-called conventionality of physical geometry. This conventionality claim involves the specification of alternative space-time structures that are compatible with the same set of spatiotemporal measurements. Henri Poincaré, Rudolf Carnap, Hans Reichenbach and others set out schemes which appeal to distortions of measuring rods or light rays and involve different values of curvature as emerging from the same data. According to standard general relativity, gravitation is incorporated into the space-time structure; it is not a force driving particles off their course but rather affects what the straightest possible course is under particular conditions. By contrast, the Newtonian theory had regarded gravitation as a force acting in a fixed background space-time. The conventional alternatives to standard general relativity introduced an arbitrary background space-time and accounted for the deviations between the behavior of measuring rods or moved particles as predicted on this basis and as actually observed by assuming a force field that affects rod lengths and particle trajectories. The catch was, though, that no sources of this field could be identified; the empirically equivalent alternatives needed to draw upon effects without causes. Of course, conventionalists understood this perfectly well but insisted that preservation of causality is not required by the facts of nature (Grünbaum 1963, 66-75; see Carrier 1994, 239-241). Quite right, but the universal dismissal of any one of these non-standard alternatives testifies that theory assessment in science brings in non-empirical criteria—among them the preservation of causality (that made itself felt already in the nomological-split scenario, see section 4).

Quantum mechanics is another case in point. David Bohm's alternative account to standard quantum mechanics works with so-called nonlocal hidden variables which are assumed to affect or fix the usual quantum properties. The picture behind Bohm's theory is completely at variance with standard quantum mechanics; it drops wave-particle duality, for instance. Yet the two are indistinguishable in observational respect. Bohm's theory is in much better agreement than standard quantum mechanics with the world-view of classical physics, yet it suffers from the deficiency that the theoretical extras it needs to introduce for this purpose cannot be identified separately in experience. Bohm's "particle configurations" and his "quantum potential" are inaccessible through measurement. Accordingly, the criterion of testability militates against the theory, and this fault is one of the reasons for its bad reputation among physicists.

The historical fact is that the scientific community does not rest undecided, wavering helplessly between different Duhem-Quine options. They rather pick an account. Theory choice is pervasive in science. This includes the mentioned examples. Around 1600 the geoheliocentric theory was preferred to its rivals, general relativity is accepted at the expense of its conventional alternatives, and standard quantum mechanics is retained in spite of the Bohmian challenge. The decisions of the scientific community transcend what can be justified by relying on logic and experience alone. Theory choice goes beyond the austere hypothetico-deductivist framework and bears witness to the influence of non-empirical virtues.

6. Platonism and Aristotelianism as Divergent Approaches to Nature

Let me outline an example of a different kind which addresses the more general attitude we adopt in approaching nature. In the past two decades, the so-called model-debate has illuminated how difficult it is to apply theories to phenomena. Models need to be built that not only include principles of the pertinent theory and initial and boundary conditions (as it was thought earlier) but also nomological ingredients of different origins, experiential regularities, rules of thumb and the like (Morrison 1999). The variegated details of the phenomena typically escape the grip of comprehensive theory which merely addresses the generic features of the situation. However, the conceptual structure of the models used for coping with the phenomena is typically still shaped by general theory. Their conceptual backbone derives from theory; the necessary adjustments are made by way of modifying this theory-based structure. Here is an example:

The “orifice problem” in hydrodynamics concerns the determination of the amount of liquid that streams out of a small circular hole in a tank. The received treatment goes back to Evangelista Torricelli and Daniel Bernoulli. In principle, the flow of fluid through the hole can be determined by invoking the conservation of mechanical energy. The kinetic energy of the jet (½ mv^2) equals the potential energy of the fluid in the container (mgh), which yields $v = \sqrt{(2gh)}$ (with *v* as the velocity of the jet and *h* as the height of the tank). The discharge per unit time as estimated on this theoretical basis is Q = vA (with the hole area *A*). But in fact, the flow is considerably smaller. A correction factor needs to be added to the theoretical estimate that varies with the profile of the opening and may amount up to a 40% flow reduction.

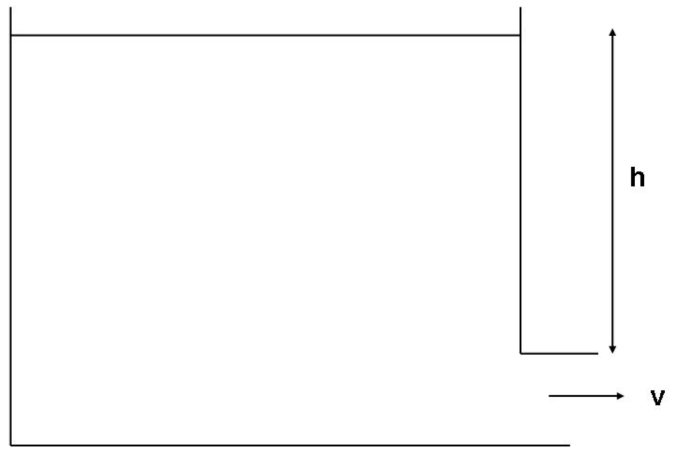

Figure 2: The orifice problem (see Bod 2006, 15).

The effect was described already by Torricelli and designated as "*vena contracta*" or "contracted vein." The qualitative explanation is not difficult: in streaming out, the liquid converges on the opening so that the area left for the issuing jet is less than the width of the orifice. A kind of fluid congestion is built up before the opening and impedes the flow through it. But there is still no reliable quantitative estimate of the reduction on a theoretical basis. Rather, the correction factor is assessed empirically for various orifice shapes (Bod 2006, 14-15).

On the usual approach, the model combines elements of theoretical derivation with elements of empirical adjustment. However, the empirical claims of the model are strongly affected by the correction factor and thus dominated by observation. Thus, it appears possible and perhaps even preferable to replace the cumbersome combination by a more uniform phenomenological account which yields directly and without a detour through theory the measured rates for various tanks and openings.

One of the reasons for adhering to theory is that the generalization of theory-shaped models is easier than the transfer of phenomenological models to new cases encountered. Phenomenological models are shaped conceptually by the demands of the problem-situation at hand. As a result, each such phenomenon needs to be approached on its own terms. The drawback is that results gained in this phenomenological fashion cannot be transferred to different problem-situations; the latter need to be addressed completely afresh. By contrast, the approach based on "theoretical derivation cum empirical correction" singles out those elements that remain unchanged in a whole class of phenomena so that large parts of the model can be reused in other accounts. This is evident in the orifice example. When the problem has been solved for circular holes, transfer of the solution to rectangular holes is quite easy. Within a phenomenological approach, by contrast, the latter problem would be entirely new; it had to be attacked without benefiting from the solution to the former (Bod 2006, 16-17).

It can be gathered from this example that a phenomenological approach and a theory-shaped approach can easily come out empirically equivalent and that the preference for one of them is rather based on heuristic reasons (which do not detract from underdetermination). In contrast to the earlier examples of specific empirically equivalent accounts, these two approaches exemplify divergent attitudes toward nature, the opposition, namely, between Platonism and Aristotelianism. Platonism is committed to the rule of fundamental law; the universal is supposed to pervade the whole of nature. But this approach works only imperfectly and with limitations. Aristotelianism insists on the basic and unique character of specific cases; the differences among the particulars outweigh their shared features.[ii] But this attitude suffers from pragmatic inconvenience.

This suggests that the overweighing preference within the scientific community for the theory-centered approach is not dictated by nature. Experience doesn't favor the Platonic rule of law over the Aristotelian reign of the particulars. It is we who decide and adopt the generic mode of description. This decision is not driven by reasons of empirical adequacy but by considerations of convenience. Having a general recipe and adjusting it to new conditions is easier than devising a new scheme from scratch for new situations—although the tailor-made scheme may be less complex in each individual case than the adjusted general recipe. What is effective here again is a commitment to conceptual parsimony or heuristic fruitfulness.

7. The Invisibility of Underdetermination

I take it that these examples and considerations suffice to make it plausible that underdetermination is far from trivial. There are significant choices in science that are not fixed by a prior commitment to empirical adequacy. But why, then, is underdetermination denied by so many scientists and philosophers of science? A widespread attitude on this matter is that it is difficult enough to identify one account that is in accordance with the phenomena; thinking up an alternative is often impossible (Laudan & Leplin 1993, 12). In this vein, it has been claimed that major features of quantum physics and general relativity are actually dictated by the facts of nature.

For instance, Steven Weinberg contends that quantum mechanics is imposed upon us by nature. The argument is that physicists tried to resist its adoption and attempted to modify its principles so that the break with classical physics would be avoided. But all these endeavors failed. The attempts to revise quantum mechanics ended up with accounts that were either inconsistent or logically equivalent to quantum mechanics. In this matter, nature leaves us no choice (Weinberg 1992, 85-88). Likewise, judging from Albert Einstein's own account, the development of general relativity came close to a stringent logical process in whose course no explanatory alternatives showed up. General relativity proves inevitable, once special relativity is presupposed, the principle of equivalence is adopted, Newtonian gravitational theory is assumed as a limiting case and general covariance is required. On this basis, Einstein was able to infer his field equations. In Einstein's own judgment, no significant freedom of choice was left. It is true, the concepts used for grasping the phenomena are created by scientists but nature shows no lenience regarding divergent conceptual inventions. The freedom of theory construction, Einstein argues, is not the freedom of the poet but the freedom of a person faced with a puzzle. Any proposed solution is the result of a creative process but only one such proposal eventually matches all relevant constraints (Einstein 1936, 69). In sum, we can be fortunate if we manage to come up with one suitable account. The idea of an embarrassment of riches is removed from reality.

It should be acknowledged that logic and experience set limits to cooking up alternative accounts and that it has hardly been established in the discussion that arbitrary assumptions can be upheld "come what may." Half a century of failed attempts to integrate quantum mechanics into the conceptual space of classical physics tell a different lesson. Yet whereas the freedom of choice has been overestimated by some philosophers and sociologists of science, it has been underrated by many scientists.[iii] Arguments like the ones rehearsed are typically based on much more than logic and experience. They rather invoke substantive principles (like general covariance) and methodological commitments (like conceptual uniformity and parsimony).[iv] Arguments to this effect presuppose and include the non-empirical values that Duhem-Quine underdetermination place in the limelight. Commitments to such values are certainly second nature to scientists and may appear as a matter of course from their point of view. But they are still highly significant for the analysis of the process of the growth of scientific knowledge.[v]

8. Conclusion

The underdetermination thesis says that any given set of data can always be represented by empirically equivalent, conceptually distinct accounts. The range of relevant options is dependent on how empirical equivalence and conceptual distinctiveness is spelled out but irrespective of such details, the thesis establishes a leeway for scientific theory when faced with the verdict of nature. The reason for the significance of this leeway is that the criteria appealed to in picking an account from the collection of empirically admissible options bear witness to our epistemological intuitions. The pertinent non-empirical criteria uncover the features of experience we consider worth knowing.

For instance, ever since the scientific revolution we have looked for regular, repeated events, rather than the extraordinary. This was different in the Middle Ages when deviations from the usual course attracted particular interest. Scientific knowledge emphasizes the generic traits of the phenomena and treats the differences in detail by adjusting comprehensive approaches (see section 6). Moreover, the cases discussed indicate that scientists are aiming at a clarification of causal mechanisms and attempt to increase the explanatory power of science by unifying the system of knowledge. In addition, coherence with background knowledge and testability are considered significant virtues, too.

It is an important objective of philosophy of science to clarify the intuitions concerning the properties or virtues of scientific knowledge. Such a clarification can be followed by attempts to connect these epistemic distinctions of scientific knowledge with more overarching goals of science. This is done in arguments that intend to show that the evaluation criteria employed in science are truth-conducive. Be that as it may, epistemic values of the sort mentioned are constitutive of what we understand by scientific knowledge. They are thus the basis of normative judgments about the legitimacy or inadequacy of assessments within science. Epistemic values shed light on how science functions in epistemic respect.

It deserves notice, in addition, that epistemic values express non-empirical virtues, to be sure, but do not appeal to social interests, pragmatic constraints or aesthetic predilections. Rather, epistemic values bring out a commitment to knowledge that represents the object in some sense and does not depend on us, on our wishes and fears. The idea associated with epistemic values like explanatory power, testability, and novel predictive success is that they are suited to guide scientific research on its way toward objective or truthful representation. Epistemic values are distinct from pragmatic virtues, which are directed at guiding human action (and express, for instance, a preference for accounts that can be handled easily); epistemic values are also distinct from sociopolitical objectives aiming at the promotion of particular social groups or at technological use.

Epistemic values are intended to characterize endeavors that are committed to capturing what is out there or what exerts a resistance to human intentions and endeavors. The influence of such values on scientific research is no reason for relativist concerns. They are values that prevail in the scientific community and transcend, accordingly, any particular society or culture. They bring out what kind of knowledge the scientific community takes to be significant or worth knowing. It hardly justifies relativist worries that the striving for particular kinds of knowledge is not forced upon us by nature but is subject to human deliberation. Duhem-Quine underdetermination can serve as a touchstone for clarifying the notion of “epistemic significance”; it thereby performs an important epistemic function.

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[i] It is true that depending on how stringent the notion of „hypotheses suggested by observation“ is conceived, underdetermination may also arise within the inductivist framework. Accordingly, the more precise claim I wish to make is that the tighter the conditions placed on hypothesis formation the less pronounced is the ensuing underdetermination.

[ii] At least this is how Aristotle’s position in *Metaphysics* 1038-1041 is often interpreted: essences are in the individuals, not in the universals; the particular is primary and the universal is no more than a collection of individuals; the universal does not exist separately of the particular.

[iii] It is not obvious how underdetermination fares in the course of scientific progress. The broader the evidence, the more data need to be reproduced by an alternative account which makes it more difficult for underdetermination to occur (see Fahrenbach, this volume). By contrast, scientific progress produces an increasingly complex web of intercon-

nected beliefs. The more theories are involved, the more hypotheses can be adjusted, and the larger is the room left for coping with anomalous findings. No clear tendency seems to emerge from these contrastive factors.

[iv] In some cases, lack of imagination or knowledge determined the judgment—as in Weinberg's denial of alternatives to standard quantum mechanics. Bohm's account represents such an alternative (see section 5).

[v] "Kuhn-underdetermination" is another variant of underdetermination which serves the same aim of bringing to light non-empirical commitments of the scientific community. Kuhn-underdetermination concerns the comparative evaluation of empirically different rival accounts whose assets and liabilities occur in different fields or different respects. As a result, a weighting of the features involved is necessary. In order to arrive at a clear evaluation, a judgment is needed that the success in one field or respect is more important or less relevant than the failure in a different field or respect. Kuhn-underdetermination shows that logic, experience and epistemic values are insufficient to single out one account as the superior one and thus discloses the impact of non-epistemic criteria. Yet unlike Duhem-Quine underdetermination, it does not proceed from empirical equivalence. Examples of Kuhn-underdetermination occur more frequently than cases of Duhem-Quine underdetermination (Carrier 2008).