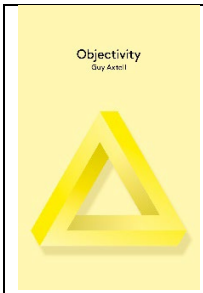

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3 Objectivity in the Natural Sciences

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Why science is objective is usually explained through appeal to scientific method, and more specifically to testing of hypotheses or theories by relevant evidence. Science has a very impressive resume of formalized standards bearing on its methods of inquiry. Experimental science uses *controls* to aid comparison between groups that do or do not get exposed to the hypothesized causal factor. It uses *randomization* of the members put into the control an experiment groups in order to remove selection bias, *blinding* to remove confirmation bias, *statistical sampling* to increase confidence that samples match the diversity of the target

population, *repetition* to attain greater reliability of results and inferences, etc. Issues about scientific methodology, however, are the start and not the finish of debates over objectivity and the sciences.

At the risk of oversimplifying 20th century debates in the philosophy and methodology of the sciences, the first half of this century witnessed the dazzling rise of a strongly objectivist account of scientific reasoning and scientific change. The second half by contrast witnessed its downfall amidst the 20th century's sharpest debates over scientific rationality and objectivity. For several decades beginning in the 1960's, the 'Science Wars' became especially rancorous over issues of scientific authority, objectivity, and rationality. This occurred as the empiricist conception of scientific theory construction faced challenges by a wide range of critics. Discussions of scientific reasoning and what demarcates it from non-scientific ways of seeking knowledge and understanding became much more widespread across academic fields. The discussion was widespread because the logical positivist (or to use a somewhat broad term, logical empiricist) account of scientific rationality and objectivity had strong normative implications for *all* fields. Science studies, a fledgling field drawing upon the history and sociology of science, challenged the positivist's conception of the rationality of scientific change by holding it up against science's own history and practice. Criticisms of positivism led to re-examining the self-image of the scientist much as cognitive psychology in this same era led a re-examination of received views about human beings as *homo sapiens* — the uniquely rational animal.

Our previous discussions of realism might lead the reader to surmise that objectivism about scientific methodology is synonymous with scientific realism. This would be a mistaken, however, because empiricism is often seen as a competitor of realism, and yet can be highly

objectivist. The breakdown of the logical empiricist model of scientific objectivity was due to many factors. But there are some candidates for who fired the shots heard round the world that set off the Science Wars and introduced us into a 'post-positivist' era. The most often cited is Kuhn's *The Structure of Scientific Revolutions* in 1962 (hereafter *SSR*), which attacked the 'standard view' that theory-choice in science proceeds purely logically or algorithmically. We will refer to the standard, objectivist account as the "logicist" conception of scientific method and reasoning. The chapters of Part Two will primarily discuss issues surrounding the rise and fall of logicists objectivism, and how debate over objectivity in the natural and social sciences has progressed over the past century.

3.1 Challenges to Empiricism

Kuhn problematized the logicist understanding of the objectivity or rationality of scientific change, providing a very different picture than that of the cumulative or step-wise progress of theoretical science. Theories often compete, and when consensus builds around one competitor it may be for a variety of reasons other than just the direct logical implications of experimental successes and failures. Kuhn pitted the study of the actual history of science against what Hans Reichenbach referred to as the "logical substitutes" logicists used to reconstruct the rationality of theory change. Such substitutes were promoted to show that theory confirmation is a purely epistemic affair, where purely epistemic implies purely inferential. But where the positivists saw logical compulsion as the reason for theory change, Kuhn saw psychology in values as relevant to how theories are actually confirmed or accepted by the scientific community.

This message was not well received, especially since positivists drew a sharp distinction between logic and psychology, and restricted psychology's relevance only to the *context of*

discovery, or hypothesis *formation,* maintaining that the *context of justification*—the confirmation or rejection of theories— is fully objective. Kuhn similarly saw factors the standard received view treated as "external history of science" protruding on theory choice, whereas the positivist view held that change, if rational, should turn only upon purely epistemic "internal history of science." Positivists tended to depict scientific progress as cumulative, whereas to Kuhn to see change coming in a more punctuated manner. Most of the time fields of science are "paradigm bound," and progress pretty much as positivists claim. But when a prevailing scientific paradigm becomes too weighted down by problems and anomalies, times may be ripe for new theories and new ways of thinking about scientific methodology and axiology. Positivists, of course, recognized scientific "revolutions" in specific fields such as biology (the rise of evolutionary theory) or particle physics (the rise of relativity theory and of quantum mechanics). But Kuhn shocked and dismayed proponents of the standard view by claiming that "paradigm change" during such periods of scientific revolution depended on all sorts of factors that did not fall into logic or the "internal" history of science.

Debates over the special or non-special status of scientific methods and results are always ongoing, but in the past two decades I can report that the rancor has subsided somewhat, with moderation on the side both of science's special authority and of its critics. Debates seem no longer led by sharply opposed supporters of objectivism and relativism as they were a half-century ago, when positivist philosophers squared off against a variety of challengers. This is not to say that philosophy of science today is less *vibrant*, only that it is less divided along lines of the champions and challengers of locisist objectivity. The logic/psychology, and rational/social dichotomies are more likely today to be viewed as false dichotomies, and the rational relocated within the social rather than being sharply contrasted with it. That the social aspects of practice

enable objectivity means that objectivity is not understood in the negative terms positivists favored, of being value-free, untouched by any but strictly epistemic reasons. So a thesis of this chapter is that logicist support for scientism and objectivism has largely given way to a weaker but more sustainable conception of scientific rationality. This latter, more sustainable conception is one in which draws upon and draws together both the logical reasoning that scientists and practice-focused social epistemology.

3.2 The Logicist Understanding of Scientific Objectivity

Positivism is the idea of empirically-based or descriptive knowledge, and its recognized mid-19th century "father" was Augusta Comte, who saw human thought progressing step-wise from a mythico-religious, through a metaphysical, and finally to a scientific stage of development. Logical positivism was a quite influential movement in Anglo-American philosophy in the twentieth-century. Its proponents wouldn't endorse Comte's "Law of Three Stages" explaining human social development up to the final scientific stage of history. For to the extent that that such a claim about broad historical stages seems grand and speculative it is also out of keeping with a strict empiricist attitude of sticking to the facts and abjuring metaphysical speculation. But the influence of positivism nevertheless expanded in England and the United States. The "verification principle" or verification theory of meaning advocated by Britain's A. J. Ayer held that a statement can only be cognitively meaningful if it has verifiable empirical implications. Failing this test of testability one's claim must either be just a tautologous analytical claim, or else a "metaphysical" one. If the former, it is essentially empty of empirical content. The truths of logic and mathematics are analytic and without empirical or sense-world content. If on the other hand one's claim purports to be about fact but no way of verifying the claim is conceivable,

then it is only a pseudo or a metaphysical claim, a claim that the strict verificationists held is literally senseless or meaningless. Positivism swept across American thought after several members of the Vienna Circle and Berlin Circle of scientifically-minded philosophers interested especially in analyzing advancements in physics escaped the rise of Nazism and migrated to the United States, taking up posts in several universities prior to WWII.

The influence of logical positivism/logical empiricism peaked around midcentury, in works like Hans Reichenbach's (1951) *The Rise of Scientific Philosophy.* Yet by the time this triumphant note is sounded the challenges to logicists objectivity were already mounting. Reichenbach, an accomplished mathematician and physicist, wanted to reconnect the relationship between science and philosophy that he saw as having been largely severed after Kant. As laudable a goal as this was, he together with Ayer and Rudolph Carnap were charged with underestimating both the range of philosophy's legitimate concerns and the legitimate reach of the history and sociology of science.

We will first revisit some of the history of this period and especially Vienna Circle member Rudolf Carnap's influential account of scientific rationality, since it is pivotal for understanding both positivist objectivism and the problems that increasingly beset the movement. Carnap argues for a minimalist conception of philosophy according to which most traditional philosophical questions are regarded as pseudo-questions. Assimilating physical theory as closely as possible with logic and mathematics, the task of philosophy is identified with and restricted to the study of the "logic of science." On the positivist view, philosophy of science is philosophy enough, and this underscores not just their strict empiricism but also their scientism. One can see from these claims both how "Cartesian" Carnap's project was in attempting to provide certain foundations for scientific knowledge, but also how far Carnap

diverged from Descartes through his dismissal of metaphysics. Descartes claimed to find certainty in a set of metaphysical claims derived *a priori* (which is to say by the mind's own resources), and thus still allowed philosophy to be a handmaiden to theology. Carnap's scientization of philosophy by contrast insisted that "Philosophy is to be replaced by the logic of science -- that is to say, by the logical analysis of the concepts and sentences of the sciences, for the logic of science is nothing other than the logical syntax of the language of science."[1]

Alberto Coffa (1976) describes the linguistic world of knowledge as Carnap develops it in *The Logical Syntax of Language* (1936) as a two-tiered, *Upstairs-Downstairs world.* Upstairs is the object language, where reside the scientists, those whose task it is to find out the facts about the world. Downstairs is the metalanguage, and there dwell the philosophers, whose task it is to keep the house of knowledge clean. By and large Upstairs people can build their houses any way they want; they can build their languages and assign meanings to theoretical terms as suits their purposes. But Carnap's two-tier model of the language of science imposes law and order on the Downstairs project. That project is limited to coordinating the theoretical and observational terms, and the rules for relating them. What Coffa thinks inspired Carnap's restriction of Downstairs projects was not just his verification principle, but his more general distrust of a philosopher's ability to say anything interesting qua philosopher about things other than language.[2]

Theory construction and testing according to Carnap depends upon a sharp and clear distinction between theoretical and observational terms, as well as clear rules by which

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[1] R. Carnap, (1936).

[2] J. A. Coffa (1976), 207. See also Coffa (1991).

“theoretical laws” (laws which contain theoretical or unobservable terms) can be related directly back to “empirical laws” (laws which contain only observable terms or state only directly observable regularities). It may have been various worries about the clarity of the theory/observation distinction in science, and about the meaning of theoretical terms that originally led philosophers of science like Carnap and W.V.I. Quine to an interest in questions of language.

Syntax is the study of the rules or grammar that determines how sentences are formed. Carnap first gave a syntactic and later a semantic account of the logic of science. The fundamental idea behind the *syntactic* conception of the language of science is that physical scientific theories are collections of statements that can have a formal representation as axiomatic systems, much as logic or mathematics can. Carnap's first formulated position restricted philosophy by claiming that its inquiry "need concern only syntactical properties of various forms of language, and pragmatic reasons for preferring one or another form for given purposes." Quine later argued that Carnap’s syntactical account greatly underestimated the problems of coordination, as well as of reference. If the claim is that we can always clearly separate the logical structure of a theory (what is essentially analytic) from its factual content (what is empirical or synthetic), then it is a claim directly challenged by Quine. Quine asked, can we always distinguish between “truths which are…grounded in meanings independent of matters of fact, and truths which are…grounded in fact”? Quine thought not, and he identified dependence on such an analytic/synthetic distinction, along with the assumption of the “reducibility” of meaningful statements to terms which refer to immediate experience, as two of what he called the “dogmas of empiricism.” If theory formation and concept formation go hand in hand then neither can be carried on for long in isolation from the other.

In the wake of such challenges to empiricism, it became more difficult for positivists to maintain that the only philosophical problems that needed to be addressed concern the logical structure of the language we adopt. The syntactic account made it altogether too easy just to say that scientists could avoid all problems of metaphysics and treat epistemological problems as little more than chores of the fine-tuning the language of science. Looking back in his autobiography, Carnap discusses the more general reasons for these revisions:

A few years after the publication of the book (*The Logical Syntax of Language*), I recognized that one of its main theses was formulated too narrowly. I had said that the problems of philosophy or of the philosophy of science are merely syntactical problems: I should have said in a more general way that these problems are meta-theoretical problems…Later, we saw that the meta-theory must include semantics and pragmatics; therefore the realm of philosophy must likewise be conceived as comprising these fields.[3]

Carnap is explaining why his later account came to recognize semantic and pragmatic dimensions of science, but he is not fully acknowledging how this reflected serious modifications to the logicist account of scientific objectivity. Recognizing questions of semantics opens the door to ontological concerns raised by employing theoretical terms. Pragmatics is the study of the practical circumstances in which we put language to use. But while

[3] Schilpp (1963), 56.

Carnap was liberalizing his view to accommodate problems such as the one result raised by Quine, is not clear that he could do so without undermining the logicist account of objectivity. Critics point out that Carnap uses "pragmatics" as a catch – all term that contains a number of questions about scientific methodology and axiology (goals, aims, values) that his critics would say are pertinent to theory construction and assessment.

The realm of external questions and pragmatic decisions became for Carnap a place-holder for a variety of "meta-scientific" questions and assumptions the logical empiricists were being forced by their critics to take more seriously.

Hilary Putnam writes, "it is clear that explanation has to be partly a *pragmatic* concept. To regard the 'pragmatics' of explanation as no part of the concept is to abdicate the job of figuring out *what makes explanations good.* More precisely: the issue is... Whether our theory does justice to [pragmatic features] or relegates them to mere 'psychology.'" (1976, 42). Science is not a purely formal set of inference and observation procedures. Putnam relates the changes in Carnap's views to the impending collapse of the positivist's fact/value dichotomy.[4] The positivist view that debates about values are not rationally decidable cannot be reconciled with the key tenet of post-positivism, that scientists *qua* scientists make value judgments. Richard Bernstein also criticizes the claim that "pragmatics" refers only to what Carnap came to call "external questions"--questions one could harmlessly be conventionalist about. Even if the

[4] For Putnam (1993), "positivism was fundamentally a denial of entanglement, an insistence on sharp dichotomies: science-ethics, science-metaphysics, analytic-synthetic." Whatever the fate of metaphysics may be, issues of ontology cannot be reduced merely to those of semantical rules.

pragmatics of language is something that can be ignored in mathematical proof, it cannot be easily dismissed in the context of scientific explanation. Bernstein points out that since the distinction of syntax, semantics, and pragmatics was introduced into philosophy by Morris early in the 20th century, "it has become a virtual dogma that there is a clear hierarchical ordering among these three disciplines. First come syntax, and semantics, and finally pragmatics; pragmatics is dependent on semantics, and semantics is dependent on syntax." (2010, 119). Challenges to the view that science is value-free stand this hierarchy on its head, arguing that questions about science's aims and methods—questions that do not fit easily under in any of Carnap's three recognized areas where philosophy is pertinent to science, are important to our understanding of scientific reasoning.

To give concrete examples, the logicist seems committed to the claim that a choice between empirically equivalent theories in physics, and the choice between axiomatic consistent systems in mathematics, are merely conventional matters or mere matters of expediency. Yet where a range of "super-empirical" theory virtues beyond the primary theory virtue of empirical adequacy are acknowledged, we can employ them in many cases to provide *theoretical* and not merely pragmatic reasons for the adoption of one system over the other. For example, Lorentz's mature theory based on Newtonian mechanics and Einstein's special theory of relativity are thought to have been empirically equivalent theories around 1905. Ptolemaic and Copernican accounts of the motions of the planets, at the time they were debated, differed only quite marginally in what they predicted and in a degree of arc beyond the powers of observation at the time. Yet even before further developments helped provide fuller confirmation the Copernican system won out within the scientific community over the Ptolemaic system, and special relativity theory over Lorentz's. Such changes in theory are not for reasons of the kind that get

characterized as pragmatic by Carnap—reasons of expedience—but for reasons pertaining to substantive explanatory differences and theory-virtues.

Carnap tried to draw a sharp distinction between epistemologically-relevant "internal" questions, and epistemologically-irrelevant "external" ones, and to refer all questions of values to the latter. Yet his critics point out that revisions to his early views made "internal" scientific discourse depend more rather than less heavily upon answers to or decisions on "external" decisions. This expose the manner in which logicist objectivism showed itself to be self-refuting. Ernan McMullin makes this objection, especially well. "The term, 'external'", he writes, "was obviously an unhappy choice, as things would turn out. The questions Carnap dubs 'external' would be external to science only if theory-decision is external to science." Carnap extended the dependence of the conduct of theoretical science upon purportedly arbitrary decisions on external questions. Yet this revision to his account "implied the pragmatic 'external' criteria are the appropriate ones for deciding on the acceptability of the linguistic frameworks of science, that is, of scientific theories...." If both evaluation and valuing are involved in scientific reasoning,[5] then value judgment in just the sense that positivists deplored does play a central role in science. In

[5] The positivists' views about evaluative discourse were radically split between "evaluation" and "valuing." Evaluation, or *using a standard*, was open to rational criticism because that is a matter of instrumental rationality: the fitting of means to a given end; evaluation is only a kind of grading or applying an existing standard. But valuing—*choosing* a standard—is typically presented as altogether different, since this kind of discourse is thought to be beyond rational discourse and essentially an emotive matter. This dichotomy for some time helped to insulate the logicist's "value-free" conception of objectivity, but McMullin is here challenging it.

this case, “The attempt to construe all forms of scientific reasoning as forms of deductive or inductive inference fails.”[6]

3.3 The Verification and Falsification of Scientific hypotheses

Trying to verify one’s causal hypotheses and theories through their observable consequences would seem to suggest a broadly inductivist approach to questions of scientific theory change. The very term *theory confirmation* lends itself to this view, and there is an inductivist philosophy of science going back through Isaac Newton to Francis Bacon, whose *New Organon* was to be a method of scientific discovery. Sir Karl Popper’s influential “falsificationist” account of scientific reasoning contrasts with positivist verificationism. Popper thought science should abandon inductivist pretensions and the whole notion of a logic of scientific discovery. Popper speaks of the growth of scientific knowledge as occurring through a process not of verification, but of "conjectures and refutations."

The similarities between Popper’s falsificationism and positivist verificationism are as important to see as the differences. Both adhered what is called the Hypothetico-Deductive (H-D) method in the hard sciences. If one wants to test a causal hypothesis they can try to deduce from it some prediction of what should occur, and then run a test to find out whether or not the predicted event is observed. Both verificationists and falsificationists like Popper typically draw a sharp distinction between logic and psychology, and say it’s mostly a matter of individual psychology how a scientist originally comes up with a hypothesis to test. The “context of justification” for strict empiricists is by contrast strictly a matter of logic and evidence.

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[6] McMullin (1982), 6, and 13-14.

On closer scrutiny Popper's views still differed markedly from those of the verificationists and inductivists. Popper emphasized that if scientists try to *verify* their hypotheses through testing, they are bucking up against logic. The syllogism, 'If H (hypothesis) then C (observable consequence or prediction); C is observed; therefore H is true,' is *invalid.* It is essentially the *fallacy of affirming the consequent*. One can't really *prove* any theory through its observable consequences, in the normal deductive sense of "prove." Theory confirmation as the inductivists understood it is too easy. Popper thought science should rather work *with* the flow of logic by trying to falsify rather than confirm causal hypotheses. The syllogism, 'If H (hypothesis) then C (observable consequence or prediction); but not C; therefore H is false,' is the *valid* syllogistic form, *modus tollens.* So there is a kind of asymmetry to the logic of testing, and asymmetry showing us that claiming to have disconfirmed or refuted a causal hypothesis is far easier to justify then is claiming to have *confirmed* a hypothesis through its observable consequences. Accordingly, science is most rigorous and scientists most objective, Popper insisted, when they put their own conjectures (theories or hypotheses) to severe tests by honestly trying their best to falsify them. What survives such rigorous attempts at falsification is by no means necessarily true, but shows itself to be a theory worth holding onto.

Popper's description of himself as a falsificationist does not imply that he found evidential merit *only* in failed experiments. Hypotheses that survive severe tests are stronger than they were before the tests. Positive or expected results can result in a certain sense of "corroboration" of the guiding hypothesis, but he was pointing out that how much corroboration or inductive support a positive test supplies is quite hard to figure. 'Confirming evidence' only counts when it is a result of a serious attempt at falsification. The only exception is when the test results confirm an especially bold or risky prediction made from a theory. So a key theory virtue

for Popper was *boldness*—the degree to which the prediction deduced from the hypothesis is something that we should be quite *surprised* to observe, were the hypothesis false. Bold predictions allow for severe tests and surviving a bold prediction, it is universally agreed, provides a substantial degree of corroboration of the guiding hypothesis although it still does not *prove* it. To give an example of bold prediction, take the case of Edmond Haley On the basis a retrospective study of comet-sighting and his application of Johannes Kepler's laws of planetary motion, Edmond Halley came to believe that the comets of 1456, 1531, 1607, and 1682 all followed similar paths and were in fact the same comet. Performing his calculations he predicted that this comet on a roughly 76 year orbit around the Sun would be visible from Earth in a certain sector of the heavens, within a narrow time frame. Then he died. But the British Society remembered Halley's bold prediction, and when in 1758 some 16 years after his death it arrived, just as he had predicted, the matter was considered settled and the comet has since been named after him, Halley's Comet. This seems to be an example of confirmation through bold, testable prediction, for even before 19 and 20th century sightings the possibility that it was a different comet that just *happened* to appear where and when Halley expected to observe one was just too great a longshot to be taken seriously. But in other cases there very well may be other viable explanations for a phenomena than the one being tested, and so a positive test result adds only piecemeal corroboration for the hypothesis. The bolder the prediction, the stronger the corroboration flowing from positive results in experimental science.

Popper's way of addressing the problem of demarcation by supplying what he thought of as clear marks of science is closely related to this. For a theory to be testable implies that it is potentially falsifiable, refutable. Pseudo-sciences like astrology do just about anything to make their explanations *immune* to potential empirical refutation; genuine science, by contrast, is eager

and willing put its theories to the test, and to move on from them if they fail. The willingness to take risks also accounts for the *growth* of scientific knowledge, in contrast to the stagnant state of the beliefs and 'theory,' if one can even call it that, which attends astrology.

Although Popper was surely right that the hallmark of science is not trivial verifications, there are nevertheless serious problems with his own positive account of scientific method, and of the demarcation of science from pseudo-science. Firstly, contemporary inductivists are not without resources. They argue persuasively that adherence to the H-D model ignores the prior probability of the hypothesis being examined, and fails to accommodate testing of statistical hypotheses, hypotheses on which the predicted outcome is not deduced from the hypothesis but only rendered more or less probable by it. Yet statistical hypotheses are pervasive in the sciences, and this explains why Bayes' theorem is an attractive way of understanding the logic of evaluating scientific hypotheses.[7]

Secondly, philosophers attentive to methodological (sometimes called confirmation) holism in science point out how rare Popper's "crucial tests" of a theory are, and dispute the decisive, 'quick kill' of a causal hypothesis that falsifications think should eventually from a failed prediction. Going back to physicist-philosopher Pierre Duhem's work around 1900, a negative test result shows that *something* is wrong, but, Duhem argued, logic does not dictate that the hypothesis *itself* is falsified. One may rationally maintain the hypothesis if willing to

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[7] Some Bayesian philosophers of science even claim that it can provide a rigorous approach to theory confirmation, saving a good deal of the 'algorithmic' conception of confirmation. Bayesians can share Kuhn's rejection of the H-D method while offering a probabilistic interpretation of the Kuhnian theory virtues. See W. Salmon (2006).

make changes elsewhere in one's system. This is because the predicted observation is really deduced not just from the hypothesis (H) by itself, but from the hypothesis together with various "auxiliary assumptions" which may include assumptions about interpreting results, measurements, machinery, and the like. The real logic of it is, "If (H and A) then C; Not C; Therefore not (H and A)," (again a *modus tollens* argument). So something is indeed shown wrong with the *combination* (H and A) by a failed test, but the fault does not necessarily with H, one's guiding hypothesis. One might plausibly make changes elsewhere in the system, while retaining the causal hypothesis. On the holistic picture of scientific reasoning auxiliary assumptions are always operating in scientific testing, helping scientists organize and interpret their findings. Duhem's methodological holism is grounded in the practice of science, and more specifically in scientific language and the scientist's use of instruments and measurement techniques, since these too are marked by theoretical interpretation.

Popper and Imre Lakatos both maintained that we can distinguish between modifications of hypotheses which are normatively permissible and those which being purely *ad hoc,* do little or nothing to support their scientific status, but only seem to insulate it from falsification.[8] Lakatos grappled with this problem and came to admit more aspects of confirmation holism. While he was in some ways bitterly opposed to Kuhn's account, which he at one point characterized as making paradigm change a matter of 'mob psychology,' Lakatos agreed with Kuhn that a certain degree of tenacity towards a favored hypothesis is normal, and to be expected in science. It is not necessarily a sign of irrationality to hang onto a favored hypothesis in the face of initially disappointing experimental results. Indeed Lakatos held that most new research programs are born into a 'sea of anomalies,' because they are grappling with the same problems

[8] Quine quoted from McMullin, 57.

that are showing the prevailing research program to be problematic. Without allowing for a healthy degree of tenacity, many scientific programs would be abandoned far too soon, before they even have a chance to build a track record of successes. But permissible tenacity does have limits, and Lakatos still agreed with Popper that increasing empirical content and avoiding recourse to certain kinds of *ad hoc* adjustments to a theory are things that mark off science from pseudo-science.[9]

3.4 Theory Choice and the Underdetermination Problem

Verificationist and falsificationist versions of logical empiricism both adhered to the hypothetico-deductive (H-D). This method is still considered basic to science, but philosophers of science today take better notice that the method tends to ignore the possibility of competing hypotheses, and the seriousness of what is called the underdetermination problem. The underdetermination problem is the problem that theories are underdetermined by the data that they explain or account for. As a problem of practice Duhem described particular sciences like physics falling into a situation of local underdetermination whenever multiple theories could claim empirical adequacy and no scientific consensus prevailed as to the best theory. But as a problem for philosophical accounts of scientific rationality and objectivity, the underdetermination problem has been especially acute for those accounts of the empiricists that rely upon the H-D method. For as Rosenberg puts it, the underdetermination problem purports to

[9] *Ad hoc* adjustments to a theory are ones that appear to have no independent support, their recommending grace being that they 'save' the hypothesis or theory from refutation. Ad hoc adjustments typically insulate a theory from experimental disconfirmation, which Popper saw as a sign of pseudo-science.

show both that "falsifications do not undermine one particular statement and [that] confirmations do not uniquely support one particular set of statements"[10]

This is how Larry Laudan formalizes the most widely recognized form of underdetermination thesis:

(HUD) For any finite body of observable evidence, there are indefinitely many mutually contrary theories or hypotheses, each of which together with auxiliary assumptions logically entails that evidence.[11]

(HUD) firstly highlights a key weakness of inductivism: Accepting positive results as strong evidence of the truth of one's hypothesis overlooks the fact that there might be many other possible but undreamt-of hypotheses that could have predicted the same thing. This problem of unrecognized competitors means that positive test result still may not discriminate in favor of your actual hypothesis and against these possible alternative explanations for what is observed. If there may be many other incompatible hypotheses that would also predict the same observation, we seem to have little grounds for concluding that our causal hypothesis is uniquely best. "Positive" results must remain permanently subject to reconsideration if a body of evidence is always potentially consistent with a variety of different theoretical explanations.

Theory, then, is always something that goes *beyond* evidence, to provide a specific way of explaining it, or of basing forward-looking predictions upon it. Scientific theories are not merely the products of observation even if some philosophies of science have tried to absolve scientists from criticism by presenting theory construction as simply 'falling out' from the facts

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[10] Rosenberg, 286.

[11] Laudan, (1990), 323.

themselves. Recognizing the problem of underdetermination of theory by evidence as Duhem does is likely enough to undermine this naïve inductivism. The underdetermination thesis suggests that by evidence alone we cannot tell whether a theory is true, but only whether the theory is empirically adequate, that is, whether if fits the known facts, or in the phrase going back at least to Newton, whether it "saves the phenomena." In various fields of science there are sometimes for short or long periods of time multiple theories that can claim "empirically adequacy." We might say that when worries become serious about what theory virtues should be recognized in a particular scientific discipline, or how they should be applied to a choice between extent competing theories, then this scientific community is operating in a situation of "local" underdetermination.

The Underdetermination Problem, however, is also, and perhaps equally, a problem for Popper's falsificationism. This is because the underdetermination problem leads to a need to adopt a kind of methodological or confirmation holism. The underdetermination problem and the holist's responses to it have both sometimes been presented in radical and exaggerated ways, holism especially in connection with Quinean meaning holism. But we needn't examine these radical versions to make our point: Even very moderate conceptions of the underdetermination problem and methodological holism imply "that falsifications do not undermine one particular statement and confirmations do not uniquely support one particular set of statements"[12].

So if we start from Duhem and the underdetermination problem, it seems we will be led to a more holistic account of theory confirmation or assessment than either inductivismism or Popperian falsificationism allow for. Scientific reasoning is not reducible to the set of rules that

[12] Rosenberg (2011), 286.

identification of scientific objectivity with the H-D method would suggest. Application of objective procedures is an essential part of scientific rationality, but only one part. For at least some instances of theory-choice we will need to recognize a range of theory virtues *beyond* empirical adequacy as desiderata of theory choice, and arguably also of the epistemic status of theories.

The theoretical virtues are standards that regulate discursive interactions in scientific communities. Often in the literature these aids to consensus-building are called *cognitive values*. But both the specific theory virtues which should be acknowledged, and how objective theory selection is that is based upon "super-empirical" theory virtues have been matters of much controversy. The holist is not trying to put empirical adequacy on all fours with these other theory virtues; the question is really how regularly empirical adequacy is available to settle theoretical disputes. The more basic tenet that confirmation holists insist upon, and has been a key tenant of post-positivist philosophy of science, is that scientists *qua* scientists make value judgments. Kuhn put it by arguing that theory-choice makes use of 'values at work' in science, and others like McMullin have refined Kuhn's list. Kuhn acknowledged at least five such "values at work" in science: 1. accuracy; 2. consistency (both internally and with other currently accepted theory); 3. scope (meaning that a theory's consequences should extend beyond the data it is required to explain); 4. simplicity; and 5. fruitfulness (leading into new concerns for further research).[13] We will adhere to McMullin's preference for calling them 'theory virtues' rather

[13] Source, "Thomas Kuhn" *Stanford Encyclopedia of Philosophy,*

http://plato.stanford.edu/entries/thomas-kuhn/

than ‘values’ because this description “draws attention to their status as attributes at once objective and desirable”.[14]

Anti-realists like the way that underdetermination challenges scientific theories as candidates for true depiction of the way the world is. Proponents of constructive empiricism see this as driving a sharp distinction between acceptance (use) of the non-observed or non-observable entities posited in a scientific theory, and believing those entities to exist. Critical realists such as McMullin like the way that cases of underdetermination of theory by data leads us to expand our notion of evidence to include explanatory theory virtues. These include the historical or diachronic theory virtues like *fertility*: characteristics that help us evaluate the progressiveness of a research program over time. The importance to scientific reasoning of the diachronic theory virtues that McMullin highlights seem undeniable, as does the expanded view of evidence associated with moderate confirmation holism. But in contrast to those including van Fraassen and McMullin who have used the underdetermination problem to support their favorite view of scientific axiology, it could be argued that we should agree with Darling (2002) that “Even if Duhem is right and scientists do encounter underdetermination every day in their

[14] McMullin (1995), 31 & (2009, 501). Rosenberg (2011, 212-213) agrees that, “[B]esides the test of observation, theories are also judged on other criteria: simplicity, economy, explanatory unification, precision in prediction…consistency with other already adopted theories…amount of allowable experimental error, etc.” Kuhn would agree, and was even quite conservative in holding that there is a stable set of theory virtues recognized in science.

routine activities, this does not straightforwardly entail any grand conclusions about realism or anti-realism".[15]

3.5 Holism and Historicism

Popperian falsificationism emphasizes the "crucial experiment" and its associated notion of swift and final refutation of a theory faced with recalcitrant evidence from a disappointing test result. By contrast, confirmation holists claim that "The finality of an experiment is historical, not logical."[16] This indicates how confirmation holism, as one of the defining characteristics of post-positivist philosophy of science, is associated with a "historical turn" in post – positivist philosophy of science. Firstly, the unit of evaluation for the holist is significantly broader than the hypothesis alone. Lakatos identifies it with a "research program" that develops over time. Evaluation of such a broad unit, in contrast with an isolated causal hypothesis, is not likely to be subject to a crucial experiment. Scientific rationality **"**works much slower than most people tend to think, and even then, fallibly. Minerva's owl flies at dusk" (87), meaning that the final or wisest evaluation of a theoretical approach assesses its 'track record' retrospectively over the course of its attempts to rectify itself and to solve problems in its field. Also for the holist, evaluation proceeds largely on a *comparative* basis: We rarely if ever abandon a theory until a better one is available, since a theory supplies organizing principles which help us make sense of our observations. So for historicists like Lakatos, "Science is not simply trial and error, a series of conjectures and refutations" (4).

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[15] Darling (2002), 532.

[16] W.V.I. Quine (2008), 173.

Lakatos also had a response to Kuhn. He held that Kuhn's account dividing periods of paradigm – bound *normal science* from other periods of *revolutionary science* interrupted by a *scientific revolution* is inaccurate, since only rarely in science is there the near monopoly that Kuhn describes as normal science. He saw science not in terms of Kuhn's *successive paradigms*, but in terms of *competition between rival research programs*. Both terms again posit a much broader focus than just the individual theory, so both writers were in some sense confirmation holists. Yet Lakatos argues that "The history of science refutes both Popper and Kuhn. On close inspection both Popperian crucial experiments and Kuhnian revolutions turn out to be myths: what normally happens is that progressive research programmes replace degenerating ones" (6). But holism is sometimes charged with making the falsification of scientific theory not only more difficult, but actually impossible.[17] It is Lakatos' theory virtues — his criteria for assessing the progressiveness of research programs over time — that allows him to avoid the charge that confirmation holism makes the elimination of theories impossible. Lakatos is able to support scientific rationality and to employ normative criteria for theory-choice, instead of having to present only a sociological or 'decisionist' account as many other historicists of 1970s and 1980s did.

Lakatos argued that, consistent with only a moderate historicism about epistemic norms, and a moderate confirmation holism, there were still objective criteria of theoretic and empirical

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[17] Against what he thought were the excesses of confirmation and meaning holism, Popper argued that the fact that we are successful at "attributing our refutations to definite portions of the theoretical maze…must remain inexplicable for one who adopts Duhem's and Quine's views on the matter." Popper (1963), 243.

failure that could be recognized if we attend to the manner in which adjustments are made to a program over a course of time. What he called his *sophisticated methodological falsificationism* retained many 'hard' Popperian elements of demanding bold and testable hypotheses wherever possible. But since increasing empirical content, which drives new predictions and problems to test, occurs over time, Lakatos' account also integrated what we might call the 'soft' Kuhnian elements of holistic assessment of theories. This means assessment by a broader array of theory virtues than empiricists of his day were apt to recognize. Concern with the adjustments made over a course of time led Lakatos to describe differences between what he called progressive and degenerating "problem-shifts" in a field of science. Although he didn't study the history of science as directly as Kuhn or others did, he drew attention in this way to an historical dimension conspicuously missing in the dominant logical empiricist (logicist) understanding of theory-appraisal. This was an understanding exemplified in what Lakatos termed the 'instant rationality' view of the justificationists and falsificationists of his day.

McMullin clarifies a second important connection between confirmation holism and the historical turn when he claims "The most significant of the contemporary virtues, in practice, are the diachronic ones, those that manifest themselves over time, as the career of the theory unfolds".[18] This dependence on diachronic or time-sensitive theory virtues contrasts with Popperian falsification, which is ahistorical or instantaneous. It follows that for the holists the history of science itself is more relevant to issues of scientific objectivity than it can be for those model scientific reasoning as application of the H-D method. Defenders of the relevance of progress in problem-solving, and of time-sensitive or diachronic theory virtues like *fertility*, *consilience,* and *durability* to theory selection, can easily maintain a lively understanding of the

[18] McMullin, (1995), 26.

importance of history of science to philosophy of science. The time-sensitive theory virtues could include those that are forward-looking or prospective, as well as retrospective. Thomas Nickles (2013) for example argues that what he terms "heuristic appraisal" and connects with *fecundity* or estimation of future fertility, can be even more important than retrospective evaluation within certain scientific contexts.

Thirdly, the historical turn as we will see further in Chapter 5, also led to wider recognition that norms of science including the norm of objectivity *themselves* have a history (Daston and Galison, 2007). The associations of objectivity, fertility, etc. change their meaning in relationship to the problems, aims, and methods of a given period in the history of science, and moderate historicists typically see none of these three elements of science as permanently set, but rather as continually re-adjusting over the course of time.

3.6 Incommensurability: Kuhn and his readers

Can the process of scientific change be reconstructed independently of ideological, social, psychological, political, and historical factors? Objectivists tend to think that it can be, while objectivism's critics deny this. It is interesting while reading Kuhn's *SSR* to pay attention to the many metaphors and analogies he employs. "Revolution" is a term drawn from politics; "conversion experience" from religion, "paradigm" from linguistics**.** Kuhn's reliance on such metaphors contributed greatly to the ambiguity of his text, and hence to the widely-diverging interpretations it received, both reactionary (sharply defensive) and celebratory. He extended, for instance, N.R. Hanson's point that seeing or observing is a theory-laden undertaking, to the hyperbolic claims that "proponents of competing paradigms practice their trades in different worlds" (150), and that to adopt a new paradigm in favor of an old one is more like a religious

conversion experience than a preference based solely in reason and evidence. Kuhn also employed language drawn from Aristotle's account of *phronesis* or practical reasoning, suggesting analogies between science and interpretive fields or value laden discourse from which positivists thought of science as being sharply demarcated.

For Kuhn to say that there are good reasons for theory choice, but that "such reasons function as values" (200), would for the positivists seem to be almost a contradiction in terms. Because of the centrality of the distinction between logic and psychology in the standard view, Kuhn's criticism of the notion of a "neutral algorithm for theory-choice," and his alternative characterization of paradigm-change during periods of scientific revolution in terms of "persuasion," were quickly jumped by his critics. The logic/psychology distinction insulated the 'messy' processes of scientific discovery and hypothesis formation from scrutiny, or even from relevance in gauging the rationality of science. The prevalence of this and related distinctions warped the perception and therefore the reception of Kuhn's claims, to the point that it has only been in hindsight that interpreters have been able to show the many continuities Kuhn's shared with the background of 20th century empiricism. Among Kuhn's vocal early critics were not just Popper, but Lakatos as well, who despite accepting many more aspects of Kuhn's holism, charged that Kuhn's account left paradigm-shifts a matter of 'mob psychology.'

To what extend does the structure of a language affect the perceptions of reality of its speakers? Can sensory data provide a completely objective basis for deciding among competing empirical theories? Can scientific language be purely observational in a sense of being neutral in respect to theoretical interpretation, and with respect to ontology? Much debate about the objectivity of science has revolved around these questions, and related difficulties especially concerning a different kind of holism, meaning holism.

Asserting the incommensurability of meanings is, by the root of the word, asserting that there is "no common measure" between them. *SSR,* one of Kuhn's earlier works, featured one of the more radical versions of meaning-incommensurability impacting the objectivity of theory-change in science. On Kuhn's account built around his distinction between normal and revolutionary periods of science, such problems of theory impacting one's observational language had philosophical implications. It supported the views both 1) that proponents of successive scientific paradigms in a non-trivial sense 'live in different worlds, and 2) that changing allegiance from the previous paradigm to its eventual successor is a matter of psychological "persuasion" rather than of logic.

Both of these claims spring from radically *holistic* conceptions of meaning. For the meaning holist neither the key scientific terms nor even the data is neutral between scientific research programs, and this calls into question the very idea of competition them. The burden of proof demanded from the defender of scientific objectivity is that they show that they employ a neutral observation language capable of mediating between theories. But the strong holist view of the theory dependence of facts and observations makes this impossible.

A related claim that critics of Kuhn's *SSR* fastened on to as entailing the non-rationality of scientific change was his analogy between seeing the world through one "paradigm" and seeing it through another as analogous to what psychologists call a "Gestalt switch." Like the duck/rabbit or vase/face images, you can see things the one way or the other, but not both at once. But if one likes this idea as a metaphor for alternative theories or paradigms, then the still stronger view asks, what common 'world' the contrasting perspectives are supposed to interpret. It starts to look like everything is a worldview, and there is simply no adjudicating between conflicting world-views since each misunderstands or rejects what the other presupposes. 'The'

world is thought lost when we take seeing as being always 'seeing as,' to the point that proponents of different paradigms or simply scientists of different time periods essentially experience a *different* world.

The study of natural language raises awareness that the way languages encode cultural and cognitive categories affect the way people think; speakers of different languages may think and behave differently depending on the language they use. Is there no reality unmediated by language? Does language strictly determines thought. Are the limits of our language, as Wittgenstein famously claimed, the limits of our world? Inter-theoretic communication and commensurability are serious concerns in science, and raise the possibility of losses of meaning, theoretical understanding, or problem-solving ability during a paradigm shift. Reflecting on Wittgenstein, Niels Bohr, one of the founders of quantum mechanics, wrote that "We are trapped by language to such a degree that every attempt to formulate insight is a play on words."

Duhem did not adopt this strong a view, but as a confirmation holist he was similarly concerned with how language functions in physical theory, and how this problem could deeply affect scientific practices and deepen a situation of local underdetermination. As it applies to scientific language and as a challenge to objectivity, the incommensurability problem seems to be based on the *possibilities* of misunderstandings between those who hold the old scientific theory or paradigm and the new. The *inevitability* of serious and deep miscommunication is a more doubtful claim. Since its force lies in meaning rather than confirmation holism, the challenge it presents will be to philosophical and theoretical disagreement of all sorts, and not to science's objectivity. Kuhn, anyway, directed the problem of incommensurability against a specific, logicists account of the objectivity (rationality) of science, and did not anticipate being considered a relativist or irrationalist by his readers. Kuhn's key claim in *SSR* that there was no

‘algorithm’ for theory-choice in science was not what his reactionary readers latched onto. What the objectivist critics took issue with was Kuhn’s description of the replacement of one paradigm by another during stages of revolutionary science as *a matter of persuasion rather than logic.*

In characterizing paradigm change this way Kuhn seems to have been leaning upon the logicist’s own distinction between logic (together with properly epistemic reasons) and psychology (and idiosyncratic and all that smacks of the non-rational persuasion). But he was using these categories *against* the logicist conception of scientific objectivity, by describing change during scientific revolutions as change through persuasion *rather than* logic. So it is easy to see why Kuhn’s account hit a nerve with holders of the standard view about scientific change. If science is objective, psychology and persuasion should play no part in it. Kuhn thus appeared to many of his readers as endorsing the irrationality of scientific change, since the logic / psychology and proof / persuasion distinctions to them were really inexorable *dichotomies:* there was presumed to be a complete division, and therefore no troublesome entanglement between the two sides.

This dependence upon the positivists’ own dichotomy (or at least the language of it) turned out to be most unfortunate for Kuhn, since almost at once after the publication of *SSR* he was at pains to dissociate himself from the way his reactionary readers understood his claims about “persuasion” leading to paradigm change. Kuhn adopted the quite different distinction between rational persuasion and force, in order to show that persuasion that involved theory virtues was not irrational. But the damage was done in terms of dividing his readers. After *SSR,* the several papers that Kuhn wrote bearing on bearing on objectivity, value judgment, and theory choice attempt to answer his *reactionary* readers while distancing himself from some of his *celebratory* readers—those who reveled in what *they* understood as the non-rationality of

paradigm-change in Kuhn's account. Kuhn backed away from the most radical, incommensurability-related claims of *SSR.* But he probably should have qualified his distinction between normal and revolutionary science at the same time as he shifted from *contrasting* logic with the persuasion that goes on in paradigm change, to speaking of the change as a matter of *rational persuasion* that goes considerably beyond strictly logical inference.[19]

Many of *SSR's* readers on both sides of the debate fail to notice how Kuhn's distinction between normal and revolutionary science (or "intra" and "inter" paradigmatic discourse) also mirrors an older positivist distinction, in this case the distinction between "valuation" and "valuing".[20] The positivists treated *using* a standard as rationally assessable in terms of the fitting of means to a *given* end; but *choosing* a standard, or actually engaging in value judgment by prioritizing one value above another they treated as beyond the scope of rational reflection and discourse. The close connection between this and what Lakatos complains most about in Kuhn's work, that Kuhn's distinction presents *normal* science as too orthodox and static, and *revolutionary* science as too much like 'mob psychology,' should have been apparent to readers familiar with logical empiricism. But it is often overlooked and this has certain costs: When Kuhn's reactionary readers accuse him of endorsing a merely decisionist or psychological account of scientific change they miss seeing that Kuhn was only showing how considerations strictly excluded from the "context of justification" in empiricist thought were in fact integral to scientific reasoning from a practice perspective. On the other hand, when Kuhn's celebratory readers interpret his account of scientific change as undermining all claims to scientific

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[19] Kuhn

[20] This distinction is sometimes alternatively cast by emotivists about value like Feigl as one between validation and vindication, with vindication being understood the non-rational or purely emotive side.

rationality and objectivity, they miss seeing that their own view is a boomerang relativism that actually endorses rather than distances from the pernicious false dichotomies of the standard view that sustained logicist objectivism: dichotomies between logic and psychology, valuation and valuing, and purely epistemic and purely pragmatic factors in judgment.

The revisions Kuhn made to his account of theory choice in the sciences is also interesting for revealing a substantially more conservative side to his thinking about the norms of science. For instance, for all his comparisons of the logicist conception of scientific change against the historical record of science, Kuhn actually assumed that "whatever their initial source, the criteria of values deployed in theory choice are fixed once and for all, unaffected by their participation in transitions from one theory to another." Even in his later work *The Essential Tension* (1975) he maintains that "such values as accuracy, scope, and fruitfulness are permanent attributes of science" (335). This doesn't look like the claim of an historicist, let alone a radical historicist. Yet while holding this fairly ahistorical and foundationalist view of science's cognitive values, Kuhn still distanced himself from logicism and its algorithmic account of theory-choice by arguing that there could be and often were individual differences and idiosyncrasies in the understanding, application, and weighing of science's recognized theory virtues. A specific theory virtue like fertility, Kuhn held, can mean different things to different scientists; scientists may apply it somewhat differently to extant theories; and scientists can also *weigh* it differently against other theory virtues in inferences to the best explanation. In this and other ways Kuhn continued in the holistic tradition of Duhem to insist on the importance of psychology and trained expertise for understanding scientific practice.

3.7 Scientific Realists and Constructive Empiricists

Some, but by no means all strong defenders of scientific objectivity are scientific realists, thinkers who hold that scientific theory aims at and either actually attains or progressively approaches a true model of the empirical world. Non-realists deny scientific realism, but while some non-realists are also critics of scientific objectivity, this is not necessarily so. Many self-described empiricists who are instrumentalists, thinkers whose conception of science's aims are closer to prediction and control than to producing true theories. The difference seems to be that scientific realists accept an account of scientific axiology that leads them to a robustly metaphysical conception of objectivity, whereas empiricists limit themselves to an epistemological conception that understands objectivity in terms of rationality, and rationality in terms of preferring only the theory that uniquely accounts for known data. Despite these commonalities, the realism/empiricism debate is an important one that most philosophers of science take a position on, so we will survey it briefly.

Realists emphasize the spirit of scientific objectivity as letting nature surprise us. Whenever technical progress has allowed new forms of evidence—a new window into the surrounding world—scientists have use the opportunity to look through this window, hoping to and often finding something unexpected. It is the independence of the natural world that allows these discoveries. Realists like Howard Sankey argue that "What best explains why scientific theories increasingly exhibit the epistemic virtues highlighted by methodological criteria is that such theories are increasingly close approximations to the truth" (2008, 107).

For the realist the epistemic dimensions of scientific objectivity support its metaphysical extension. The realist holds that scientific theories make claims about things presumed to exist independently of those theories, and believes that the process of observation and testing brings

the researcher into contact with these independent items. H.I. Brown thinks that "whatever claim to truth the results of science have derives from the role of objective procedures in science. These objective procedures are embodied in the observational testing of theories, and if scientific results have a special claim to truth, it is because these procedures are systematically employed in science to a greater degree than in other fields".[21] While we have reviewed discussion of the problem of securing the independence of the data from the theory it is used to test, the main point here is that "objective procedures" and the justification they confer reflect the epistemic sense of objectivity. But for realists it is used to support the further metaphysical claim that the theory indeed corresponds to the way the world is independent of mind.

One common strategy for the realist is to invoke inference to the best explanation, arguing that a convergence upon truth is the best explanation of the extraordinary success of science. Put in a stronger form, realists have argued that it is only the realist view that does not make the success of science a miracle. The "no miracle" argument holds that scientific realism provides a compelling explanation of the success of science, whereas empiricism leaves the reasons for its success unexplained or mysterious. Another strategy in the realist arsenal more attuned to its semantic dimension is to maintain that we ought to rationally believe in the existence of any entity which plays an indispensable (explanatory) role in our best scientific theories.

Critics of realism counter the no miracle argument with bountiful examples of theories that were highly successful as judged by the scientific communities of their day, but which were nevertheless eventually rejected in favor of different theories. Laudan provides a list of impressively successful theories from the past whose key terms did not refer: the phlogiston

[21] Brown, (1977), 205.

theory of combustion, the caloric theory of heat, the effluvial theory of static electricity, etc.[22] This kind of exchange appears to be a clash of two inductive arguments, one that correspondence with the way the world is explains the applicative success of our best theories, and the other that we should expect our present theories to eventually suffer the same fate as past theories.

One of the main competitors to scientific realism has been the constructive empiricism of Bas van Fraassen. Constructive empiricists are non-realists but many aspects of logicist objectivism in their views of theory confirmation and change. The empiricist understanding of the goals of science is typically associated with instrumentalism, the view that theories are instruments that we deploy in order to predict events and to systematize our observations.[23] For the constructive empiricist, "the belief involved in accepting a scientific theory is only that it 'saves the phenomena,' that is that it correctly describes what is observable."[24] While scientific realists and constructive empiricists are among today's strongest supporters of scientific objectivity, they disagree pretty fundamentally about the goal or purpose of the scientific enterprise. James Ladyman (2001) writes that, "the former thinks that it aims at truth with respect

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[22] See Laudan 1984.

[23] Some instrumentalists say that we accept theories because their predictions are accurate, and we can test the accuracy of predictions by seeing whether the observation statements derived from our theories are borne out in our experiments. This view leans pretty hard upon the theory/observation distinction and therefore inherits some of those problems discussed earlier.

[24] Van Fraassen, (1980), 4.

to the unobservable processes and entities that *explain* the observable phenomena; the latter thinks that the aim is merely to tell the truth about what is observable, and rejects the demand for explanation of all regularities in what we observe."

The status of unobservable processes and entities, Ladyman is pointing out, is a key dividing point. Realists see a continuity between those processes and entities we deem observable, and those we do not. They emphasize that the line between what is observable or unobservable for scientists changes with time and with the improvement of instruments and interpretive theories. This leads to thinking think that there is no great divide in terms of the epistemic status of observables and unobservables, and that we can apply roughly the same epistemology to both. Instrumentalists think that there is a fundamental divide, and in van Fraassen's constructive empiricism this is fortified by further sharp divisions between what is believed for purely epistemic and for merely purely pragmatic reasons. This in turn decides what is justifiably or rationally *believed*, marking it off from what can or should only pragmatically *accepted*. If this is a fair description, then one initial, broad worry about constructive empiricism is that its primary epistemic divisions essentially mirror the positivist dichotomies we have previously criticized between cognitive and noncognitive reasons, or the rational and the social.

The role in theory choice allotted to super – empirical theory virtues is also an important sounding board for differences between realists and constructive empiricists. This has been described as a fault line between two very different accounts of scientific objectivity and rationality. In contrast to realists who understand science as explanatory as well as descriptive, and who emphasize explanatory aims, constructive empiricists understand science as essentially a descriptive enterprise, and one whose goal is not knowledge of real structures but increasingly effective prediction and control. So van Fraassen distinguishes explanatory theory virtues from

internal consistency and empirical fit or adequacy, and strongly prioritizes the latter: Explanatory power is not a 'rock bottom virtue' of scientific theories; rather, consistency with the phenomena is.[25] Coherence and descriptive fit with observational data is the only genuine measure of a theory's epistemic status: its belief worthiness. Empirical adequacy is the measure of the epistemic component or status of a theory: "Science aims to give us theories that are empirically adequate; and acceptance of the theory involves as belief only that it is empirically adequate'.[26] So the empiricist thinks that acceptance of a theory on this basis entails no ontological commitments whatever beyond the observational level. Put more simply, one can consistently withhold belief in the truth of a theory while using its descriptions and theoretical language. With theories dependent upon any unobserved or unobservable processes or entities, this is the only appropriate attitude to take.

There are many things that could be said on both sides of the debate between scientific realists and constructive empiricists. We must also recognize that many non—realists are not constructive empiricists either, and take a stance critical of the objectivism presupposed in both of their accounts about scientific methodology and axiology. While this book takes no explicit stance on the issues of divide scientific realists and constructive empiricists, our study of confirmation holism and the importance of super – empirical theory virtues suggests that the quite narrow view of evidence held by constructive empiricists is implausible, even if their broader account of scientific axiology is not. It is always a good thing to be especially cautious in positing processes or entities that we cannot directly observe. There are many examples of this in

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[25] van Fraassen (1980), 94.

[26] van Fraassen (1980), 12.

long – abandoned theories of both the natural *and* human sciences. But too narrow (or too field-specific) a view of evidence conceals the importance of the super – empirical theory virtues in scientific reasoning, much as it did for the logicists. It strongly favors evaluating scientific research programs based only on a synchronic measure of empirical adequacy or fit, which in turn neglects the importance of the historical virtues of theories, and the need to evaluate them in terms of their progressiveness over time.

On the other hand, we have also noted above that it is doubtful that the role of super-empirical theory virtues can be used to prove the superiority of a realist perspective on scientific axiology. There may be a lesson here for those anti-realists who are skeptical of all evidence and epistemic standards because they hold an exaggerated view of the sort of holism that the underdetermination problem leads us to acknowledge. So what might be added is a warning that too *broad* a view of evidence is equally to be avoided. When confirmation holism is aligned with a radical form of holism about *meaning*, then we are deemed no longer able even to distinguish different kinds of claims as scientific or metaphysical, etc. Incommensurability concerns get raised in the vicious, 'encapsulating' way we saw, or even as a form of linguistic determinism. We are then saddled with one of Quine's most radical claims (from which he later backed away), that the unit of evaluation for philosophers of science expands *so* far from the isolated hypothesis of pre-holistic thinking that we can only say that that in every empirical test what we are actually testing is "the whole of science."

3.8 The Demise of the Demarcation Problem?

Like Popper, with whose name the demarcation problem is closely associated, Lakatos was quite interested in the question of whether there is clear demarcation criteria that distinguishes science

from pseudo-science. He wanted to ask, "If all scientific theories are equally unprovable, what distinguishes scientific knowledge from ignorance, science from pseudo-science?" (3). Popper thought that falsifiability was the defining feature of science that pseudosciences lack, for they are untestable and even try to insulate themselves from refutation by way of ad hocery. If I am prepared to hold a claim in an objective manner then I must also be prepared to subject it to the widest possible range of observational tests (203). But subsequent discussion of Popper's position seems to show that empirical falsification is both too narrow and too broad to serve this purpose of providing demarcation criteria. The things we call pseudoscientific are not necessarily unfalsifiable: often they are falsifiable, and in the eyes of everyone except their adherents are *in fact* falsified. The underdetermination problem and cases from fields such as scientific cosmology anyway already challenges the view that scientific theories are always directly falsifiable by tests.

Lakatos shared with Popper a commitment to the *importance* of the demarcation problem. "The demarcation between science and pseudoscience is not merely a problem of armchair philosophy: it is of vital social and political relevance".[27] Given the persistence of creation science, global warming denialism, HIV denialism, and the popularity of various conspiracy theories of the political left and right, one might well be inclined to agree. This problem is a social one, and one that philosophers can contribute to. But Lakatos did not agree with Popper's H-D model being a core rational method of science, and took a substantially different tact in responding to the demarcation problem. Criticism of theories, and in turn the question of demarcation criteria is far more objective on moderately holistic and historicist

[27] Lakatos (1978), 1.

grounds, Lakatos thought, than it can be on conventionalist accounts, among which he counted verificationism and falsificationism.

Kuhn introduced an element of pragmatism in holding that belief systems like astrology are pseudosciences not because unfalsifiable, but because they show no ability to sustain a normal-scientific puzzle-solving research tradition. Lakatos develops this more pragmatic, problem-solving approach further. The real question isn't just distinguishing science from pseudoscience, but progressive from degenerative research programs. Much as time-sensitive theory virtues are pertinent to answering the latter question, how supporters of a theory respond to objections and modify their theory over time settles scientific status as well. Theories are pseudo-scientific when they solve no open problems and can only criticize established theories but develop no alternative "positive heuristic" or problem-solving capacities of their own. It has been argued for instance that this approach indicates the pseudo-scientific status of Intelligent Design Theory more clearly than the falsifiability standard. So in contrast with Popper, it is history again that plays a lead role in Lakatos' approach: "It is a succession of theories and not one given theory which is appraised as scientific or pseudo-scientific" (47).

But other philosophers of science have been less inclined to think the demarcation problem is either as important as Popper and Lakatos maintained, or as tractable. Inspired by Kuhn's historical and social analysis of science, others like Paul Feyerabend were around the same time questioning to whole enterprise of trying to identify a core method of science, and saw this attempt as putting a lid on innovation. He called for a kind of "methodological anarchism" while questioning the view of science as the rational enterprise *par excellence*. Feyerabend may have been first among those sporting a view according to which science is just one tradition of thought among others, not one characterized by uniquely cognitive aims or uniquely rational

method. But here we should be careful to distinguish different question of demarcation: what differentiates science from pseudo-science, and what differentiates science from non-science. To search for a method of science as an answer to a question of what distinguishes science from non-science does not seem to have the practical importance that Lakatos attaches to the question of distinguishing science from pseudo-science. Its philosophical significance also appears doubtful. This concern arguably arises only in a scientistic account which identifies all knowledge and understanding with use of the scientific method. But many post-positivists would further insist on a more egalitarian conception of knowledge-producing practices than science-centric philosophies allow for, or at least argue that there is no single method of science that the social and natural sciences share.

These issues are explored further in Chapters 4 and 5, but still leave open the importance of the more narrowly-focused question of whether there exist clear criteria by which to demarcate science from pseudo-science, that is, theories or worldviews that *claim* scientific status but are only pretenders. Feyerabend's book *The Tyranny of Science* was arguably opposing a scientistic view which Lakatos' did not hold. Lakatos in the quote above clearly makes his claim be about the importance of demarcating science from pseudoscience, not science from non-science. There are many fields including philosophy, humanities, and the arts that are nonscientific, but not pseudoscientific, because they do not aspire to be recognized as scientific. The motivation for seeking to demarcate science from non-science is oftentimes a scientistic worldview in which the scientific worldview is privileged as the only source of knowledge. This view devalues non-scientific practices and sets up a hierarchy of knowledge-producers, whereas Feyerabend calls for a democratization of academic fields. Scientism can be a strong motivator of concern with the demarcation problem, but the specialness of science that scientism plies upon

will be reduced if one holds with Dewey, Pierce, and Haack that scientific patterns of reasoning are refinements and directed applications of everyday reasoning. Scientism seems incompatible with the view of Thomas Huxley that the person of science "uses with scrupulous exactness the methods which we all, habitually and at every minute, use carelessly."

There are others like Larry Laudan who while by no means agreeing with Feyerabend's view finds the demarcation problem even between science and pseudoscience less interesting that Lakatos does. Laudan thinks pseudoscientific and unscientific are basically "just hollow phrases" marking disapproval. Scientists are willing to consider anything and everything on its merits, providing it is open to repeatable experiments. We are better served to speak about scientific status as a matter of degree rather than of kind, and just to worry about distinguishing good science from bad science. Attempts to identify a small and exception-free set of epistemic or methodological features that all and only genuine sciences share have been less than successful. Given also that many views that were scientific in their day would be deemed unscientific today, Laudan thinks that there is no need to make pseudoscience into its own generalized category.

Let's examine this claim more closely. Are distinctions in degree enough? Others have thought that this approach leads to too much hair-splitting and isn't workable in the courts. Consider scientific fraud, which is not just erroneous or even incompetent science because it presupposes an intention to misrepresent or deceive. So Ladyman (2001) thinks that despite overlaps and hard cases we need to admit a third category between error and pseudoscience to properly classify it. Errors are a normal part of science, and scientific method is said to be self-correcting even if the possibility of human error remains ever-present. Simple errors in measurement or interpretation of evidence in science are typically no disgrace, but depending on

degree can smack of incompetence to the point of pseudoscience even when committed during research in established theories or fields. The taxonomy of scientific errors that Douglas Allchin (2004) provides are divided between Material, Observational, Conceptual, and Social. *Material* errors include improper procedure (wrong experimental protocol, poor technical skill) and improper materials (impure sample, contaminated culture). Common *Observational errors* include insufficient controls, reversing cause and effect, and relying on a small, unrepresentative sample. *Conceptual errors* can be reasoning errors (computational, logical fallacy, mistaking correlation for causation), exhibiting confirmation bias, or overgeneralizing the scope of the explanation. Finally, *Social errors* root in socio-cultural cognitive biases (gender, ethnicity, economic class, etc.) and are manifest in faulty peer review and other mistaken judgments of credibility.

Going beyond Allchin's basic but useful taxonomy of errors in science, sophisticated error-statistical methods are becoming an increasingly important tool of objectivity in the hard sciences. It is possible to discover the presence of bias and other kinds of error using statistical methods. This is a forensic study in the sense that it is generating statistical methods to identify errors in complex experiments and statistical reporting. Mayo and Spanos write that "Whereas the recognition that data are always fallible presents a challenge to traditional empiricist foundations, the cornerstone of statistical induction is the ability to move from less to more accurate data."[28] Error statistical methods offer ways to identify flaws in inference such as mistaking spurious for genuine correlations, mistaken directions of effects, mistaken values of parameters, etc. They can also clarify standards of evidence in legal contexts: The courts often

[28] Mayo and Spanos (2009), 9.

depend upon such methods when their task is to determine whether claims are erroneous, and whether the error is likely mere accident or a case of scientific fraud.

Methods like these infer intention only indirectly, from a study of the research methodology. But considered more directly, fraud is always motivated by intention to deceive or misrepresent, something that is certainly not true of scientific error, and is only sometimes true of pseudoscience. To use more standard examples, for-profit psychics, television psychic mediums, spoon-benders and etc. rely on persuasion through well-known tricks and manipulations which debunkers of pseudoscience have tried to catalogue. But some of the most debated targets of their criticism, the theories associated with Creation Science, Heartmath, and some forms of parapsychology have quite earnest supporters. This is especially the case where the theory has religious or spiritual roots or implications. People with ideological agendas can use the fallibility of science to undermine its authority and to justify standing against even a strong professional consensus: "The gap caused by uncertainty and fallibility can offer a powerful persuasive wedge for political ideologues" (Allchin, 2004). So for instance old and new forms of Creationism allege that evolution is 'just a theory,' and opponents of limitations on carbon emissions appeal to the incompleteness of science in arguing against the growing scientific consensus about global warming and climate change.

Cases such as these underscore the need for skills in critically assessing the potential for error, fraud, and pseudoscience, and also the need to distinguish these three. Pseudoscience is not just non-science, but neither is it well-described just as bad science. This returns us to our main question, the importance of the demarcation problem. For the reasons given I do not accept Laudan's view that the problem dissolves into matters of degree, nor his view that the term 'pseudoscience' is a hollow, merely emotive term of disapproval. But I still do take certain

lessons from his more general claim about the demise of the demarcation problem. Philosophers who believe in the unity of science are generally more inclined toward the search for universal demarcation criteria than those who argue for its disunity. Similarly for those who think that there is a single "scientific method," in contrast with those who endorse methodological pluralism in the sciences. Finally scientific realists sometimes unwisely tempt us to cast the demarcation problem in metaphysical terms, as a contrast of theories that correspond to the world and those that do not. Arguably, none of these make for sound approaches.

While there is some truth to the demise of the demarcation problem, this may be because the description of the problem has often conflated distinct issues, some more intellectual and some more practical. Nickles thinks that "What began as a logical or metaphysical issue ends up being a concern modulated by pragmatic reasons" (2013, 115). It is best to avoid the trap of thinking of demarcation criteria as an 'all or nothing' affair. Instead, the issues can be approached on multiple fronts. These claims by Nickles overlap with the approach of Massimo Pigliucci, who also finds Laudan's claim of the demise of the demarcation problem somewhat too quick. Laudan dismisses the importance of the demarcation problem too quickly. There are limits to proper tolerance, and Pigliucci thinks the problem is not just an important one socially, but one that philosophers of science should continue to play a key role in addressing. Yet a substantially new tact is needed if we are not to repeat the lessons of the past. Rather than looking for necessary and sufficient conditions for something to count as scientific or pseudoscientific, Pigliucci applies the view that both of these are what Wittgenstein called "family resemblance" concepts. This kind of concept is one where there are multiple identifying features, but no feature on the list may be necessary, and different *combinations* of features may suffice for identification as the kind of thing in questions. If science is a "cluster" or family

relations concept, then one needs a more varied set of features and an understanding that there are degrees of theoretical soundness and of empirical support. This means letting go, as Laudan demands, of the expectation that there are a small set of demarcating features of science that will fit all scientific endeavors, or for that matter, all pseudoscientific ones. Pigliucci contrasts the expectation to identify a universal criterion with his cluster approach. Even today's best known skeptics and debunkers of pseudo-science, like Michael Shermer, recognize the difficulty of identifying one universal criterion, and take the more guarded approach that "we can demarcate science from pseudoscience less by what science is, and more by what scientists do" (2013, 222). So he too has what might be termed a less strict, cluster approach to distinguishing science from pseudo-science, but thinks this can still be useful in science education and readily applicable in the courts.[29]

According to critics of the logicist account we have focused on in this chapter, the objectivity of science should never have been associated with completely value-free inquiry, nor presented as unique from all other forms of inquiry. But if in the end some of the issues logical empiricists associated with the demarcation problem have turned out to be less philosophically important than once thought, there still are issues of genuine concern. Establishing and knowing the markers of pseudo-science, especially in ways that comport to scientific literacy and the needs of the courts, remains an important concern even if it must be approached in the alternative way we have suggested. For somewhat *different* reasons, so does understanding the markers of scientific fraud, and the varied sources of scientific error.

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[29] All (2013) references and quotes are from M. Pigliucci and M. Boudry (eds.) *The Philosophy of Pseudoscience: Reconsidering the Demarcation Problem.*