Part II

EPISTEMOLOGY AND THE DYNAMICS OF SCIENCE

On The Intertheoretic Competition Hypothesis

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1. Introduction

A commonplace thesis of the "new philosophy of science" is the view that the testing of a given theory is not a simple comparison of the theory with nature but also requires the comparison of the theory with competing theories. More specifically the version of the intertheoretic competition thesis that shall be examined is as follows:

(ITC) In addition to comparing a theory with nature, a necessary condition for the rejection of a theory is the acceptance of an alternative theory.

It is Thomas Kuhn's *The Structure of Scientific Revolutions* that is responsible for the widespread popularity of (ITC).[1,2]

...a scientific theory is declared invalid only if an alternative candidate is available to take its place. No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature. ...the act of judgment that leads scientists to reject a previously accepted theory is always based upon more than a comparison of that theory with the world. The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgment leading to that decision involves the comparison of both paradigms with nature *and* with each other. (Kuhn 1962, p. 77).

(ITC) currently is the received view. It is assumed by nearly all recent philosophical treatments of testing. A vivid illustration of this is Larry Laudan's *Progress and Its Problems*. The book is a qualitative discussion of the acceptance and rejection of scientific theories. In the summary of the main chapter Laudan pulls together various central strands of his argument. The fifth summary principle is the following:

All evaluations of research traditions and theories must be *within a comparative context*. What matters is not, in some absolute sense, how effective or progressive a tradition or theory is, but, rather, how its effectiveness or progressiveness compares with its competitors. (Laudan 1977, p. 120).

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Curiously, nowhere in *Progress and Its Problems* is there an explicit argument for the above principle. This is not to fault Laudan, but to show the received status of (ITC). But even more suprisingly it is difficult to find a sustained argument for the principle anywhere in the literature. (ITC) is not without systematic significance. It has been used to support anti-realism and various species of Hegelianism.

Despite its popularity, I believe (ITC) to be false. A critique of the thesis requires two parts: i) to explore the argument (in so far as it can be reconstructed) for (ITC) and, ii) to consider the historical and scientific details of several counterexamples. This paper will be concerned with the former task though appropriate counterexamples will be briefly mentioned.

2. The Kuhnian Argument for (ITC)

A major consideration offered in support of (ITC) is the inductive evidence provided by a look at the history of science. Kuhn examines cases and finds the competition thesis instantiated. "In part this generalization (ITC) is simply a statement of historical fact ..." (Kuhn, p. 77). For the moment let us bracket this consideration. We shall have occasion to return to the "historical argument."

Two additional preliminary considerations are in order. It will be helpful to have a sketch of Kuhn's positive view before us. Suppose theory, **T**, entails observation, **O**, and **not O.** is observed. Kuhn regards **not O** as an anomaly or a discrepancy. There are several categories of anomalies. Suppose that a scientist or the scientific community was initially presented with **not O** at time t^1. At some later time, t^2, **not O** can be regarded in three ways. It may be a *solved anomaly*–a puzzle whose solution has been found. It may be regarded as an *unsolved anomaly*–a puzzle whose solution has not yet been found. Lastly, it may be a *precipitating anomaly*–a puzzle for which the failure to provide a solution has generated a crisis of confidence in $\mathbf{T}.^3$ (This is admittedly simplistic. There is no sharp distinction between unsolved anomalies and precipitating anomalies. Furthermore, in many cases it is not a single anomaly but a cluster of anomalies that are precipitating. Nevertheless these rough categories will serve to bring out the relevant distinctions.)

Normal science does not look for precipitating anomalies and for the most part does not find them. A major goal of normal science is to convert unsolved anomalies into solved ones. Kuhn is not denying that scientists reject views nor that experience plays a major role in the rejection process. Sometimes anomalies become precipitating anomalies and lead to the rejection of a theory. But this never occurs without the existence of an alternative theory, i.e., (ITC) is true.

The second preliminary point concerns the intended scope of (ITC). If it is intended to include *hypothesis testing* it is clearly false. This can be seen by recalling Carl Hempel's (1966) well known discussion of Semmelweis' work on puerperal fever. Semmelweis rejected numerous hypotheses without accepting alternatives. Counter-examples abound even where the hypotheses have some explanatory promise. Consider a simple case. Since about 1820 it was generally known that dark or Fraunhofer lines occur in the solar spectrum. Robert Kirchoff made satisfactory sense of the phenomenon in 1859. Prior to Kirchoff there was a number of unsuccessful attempts to explain the Fraunhofer lines. I wish to consider one of these for it provides a vivid counter-example to (ITC) as applied to hypotheses.[4]

John Herschel the English astronomer and philosopher of science and David Brewster the famous Scottish scientist shared the view that the Fraunhofer lines should be understood as absorptive phenomena. Quoting from Brewster, "...the deficient rays in the light of the sun and stars may be absorbed in passing through their own

atmospheres" (McGucken, p. 15). Brewster thought that throughout the spectrum the "original" light was continuous. Gases, that were generated by the light producing solar combustion, absorbed the deficient rays.

Brewster continued to pursue topics under the guidance of his absorption account. He produced a more detailed picture of the Fraunhofer lines. He explored how his account stood with respect to the intense particle wave debate about the nature of light. He also argued on the basis of a comparative spectral analysis that the same absorptive materials exist both in the gaseous form of nitrous acid and the sun's atmosphere. His reasoning here is both complex and flawed. The details need not concern us. The crucial point is that Brewster was doing work which he took to be within the bounds of his account.

In 1835 James Forbes, an Edinburgh physicist, derived an observational consequence from the absorption account. If the sun has an atmosphere that has an absorptive effect on light emitted from the sun, then one would expect this effect to be greater for light from the "edges" of the solar disc than light from other locations on the sun's surface. Light from the edges would travel a greater distance through the atmosphere, given the roughly spherical shape of the sun. Forbes expected "more numerous and broader" dark lines from the light on the sun's edges.

In 1836 an eclipse provided the opportunity for a test. Forbes noted no difference among the lines from different solar locations. He concluded that the absorption theory was mistaken. The scientific community agreed. Brewster himself repeated Forbes work and agreed not only that the absorption account was unsuccessful but that there was no satisfactory account of the origin of the Fraunhofer lines. The crucial point is that the absorptive account was abandoned without an alternative account in the wings.

Being sympathetic with Kuhn, what the Semmelweis and Forbes cases show is that (ITC) is not meant to apply to hypotheses. Kuhn himself usually speaks of (ITC) as applying to theories but often the reference is paradigms. The crucial point here is that (ITC) is intended as a criticism of positivistic philosophy of science. If it is to engage that tradition at all, it better apply to *theories* since it fails for hypotheses, and paradigms are a post-positivistic invention.

Turning to the argument for (ITC), a rare occasion where Kuhn does more than hint at the rationale is the following:

> Clearly, the role thus attributed to falsification is much like the one this essay assigns to anomalous experiences, i.e., to experiences that, by evoking crisis, prepare the way for a new theory. Nevertheless, anomalous experiences may not be identified with falsifying ones. Indeed, I doubt that the latter exist. As has repeatedly been emphasized before, no theory ever solves all the puzzles with which it is confronted at a given time; nor are the solutions already achieved often perfect. ...If any and every failure to fit were ground for theory rejection, all theories ought to be rejected at all times. On the other hand, if only severe failure to fit justifies theory rejection, then the Popperians will require some criterion of "improbability" or of "degree of falsification." In developing one they will almost certainly encounter the same network of difficulties that has haunted the advocates of the various probabilistic verification theories. (Kuhn 1962, pp. 146-147).[5]

I want to reserve more general and pressing criticisms for the following section. Nevertheless, the above has some glaring problems which need to be addressed in order to provide a sympathetic and accurate reading of Kuhn.

The argument quoted is a destructive dilemma against falsificationism. The background assumption is that all theories face anomalies. This is a point Kuhn makes often (1962, pp. 79, 81) and clearly takes to be central to his argument. The first horn suggests that every anomaly be treated as a falsifying instance. This leads to the absurd conclusion that all theories ought to be rejected.

Obviously a defender of falsificationism will insist that there are anomalies and there are *anomalies*. Some are admittedly unsolved problems but some are falsifying instances. Hence the *reductio* does not go through. The second horn of Kuhn's dilemma is designed to block our imagined falsificationist. The distinction between anomalies and "severe" anomalies (falsifying instances) can not be made out. It will require solving problems similar to those faced by probabilistic verification theories. This cannot be done.

For the argument to stand, two key and contentious assumptions need to hold: (1) that the *only* way to make the anomaly/severe anomaly distinction is to defend a "degree of falsification" account and (2) that a "degree of falsification" account necessarily leads to unsolvable problems. Both assumptions need to be justified. But Kuhn gives us very little in this regard (pp. 145-146).[6]

I propose that more abstract considerations provide the basis for Kuhn's belief that the anomaly/severe anomaly distinction cannot be made, namely, the now familiar Duhemian Thesis (1962, pp. 77-78). Put roughly, the idea is that theories have observational implications only when conjoined with an indefinitely large number of auxiliary assumptions (boundary conditions, other theories, etc.). From not O, where O is expected, it does not follow that T is false. It follows that T or one or another of the auxiliary assumptions are false. The testing situation is inherently ambiguous. One can not tell whether an anomaly is merely cause for revision of the auxiliary assumptions or is severe in the sense of falsifying the proposed theory. For the Kuhnian argument to go through the anomaly/severe anomaly distinction must be blunted. The exegetical claim is that Kuhn regards the Duhemian Thesis as sufficient for the task.

3. Critique of (ITC)

Let us make a distinction between non-relational and relational accounts of theory rejection. On a non-relational view the rejection relation holds in terms of a given theory and the world, alternative theories are irrelevant. Relational accounts require not only the world-theory relations but also specific intertheoretic relations to hold in a case of rejection. (ITC) is the claim that non-relational accounts of rejection are mistaken.

The argument for (ITC) assumes that every theory faces numerous anomalies. I am not convinced that there is a non-trivial reading of this claim but let us allow it to pass. The guts of the argument is the Duhemian Thesis. There is a considerable body of literature on the thesis. I want to skirt most of the issues. Only one point need be emphasized here.

It is tempting to think that a particular theory can be falsified, since at least in some cases, the auxiliary assumptions can be established. In a well known paper, Adolf Grunbaum (1960) tried to make just this point. He explored the falsifiability of Euclidean physical geometry and claimed that the case fit the following schema:

$$(((T.A) \to O) . -O.A) \to -H$$

We do not need to consider the scientific details of Grunbaum's example. A response to the example by a defender of Duhem and (ITC) is very interesting.

> The ... serious flaw in Grunbaum's counter-example ... is that A, though probable, is not known to be true: Despite A's high likelihood, a scientist is not forced to relinquish H unless A is known to be true. Since A is subject to some doubt, we cannot necessarily blame the failure of the prediction, 0, on H rather than A. To give up H might be more prudent, but the demands of prudence do not carry logical weight. It is perhaps correct to remark that Grunbaum's experiment would cause a rational person to cease to expound H, but the experiment does not provide an unambiguous falsification of H. (Laudan 1965, pp. 160).

What we need to mark is that defending the irresolveable ambiguity of the testing situation or the impossibility of refuting a hypothesis requires very strict epistemological constraints. Apparently to avoid Duhem the auxiliary assumptions must be beyond any doubt. This Cartesian-like epistemology will likely lead to skepticism but let us bracket that. In fact, let us grant Laudan a point: Duhem (and others) have shown that hypotheses cannot be *conclusively* falsified.

Returning to the Kuhnian argument for (ITC), what has been shown is the following: if a non-relational account of rejection requires that theory rejection be defined as conclusive falsification, then non-relational accounts of rejection are untenable. But the logical lacuna in the Kuhnian argument is now transparent. Put differently, that the argument is a *non sequitor* is now obvious. Why assume that all non-relational accounts of rejection must understand rejection as conclusive falsification? Suppose we define a weaker notion of falsification, e.g., one that only requires that the auxiliary assumptions be shown to be extremely probable. Why can not this notion be grounds for an account of theory rejection?

Nowhere does Kuhn or anyone else argue that a non-relational account requires conclusive falsification. There are several conditions that may have led to complacency on this point.

1) Kuhn may have thought that distinctions among degrees of probability cannot be drawn. He hints at this at the end of his quotation above. But he does not seriously argue for it. In fact it is false. The point can not be argued here but notice Laudan admits as much when discussing Grunbaum's example. In that case the auxiliary assumptions are such that a rational person should believe them and stop expounding H.

2) Kuhn may believe that if S rejects T on a non-relational understanding of rejection, that precludes the possibility of returning to T and that only conclusive falsification precludes such a possibility. But this is an unreasonable constraint. On Kuhn's own account of rejection it does not follow that a theory once rejected is necessarily permanently rejected. T can be rejected at t^1 and accepted at t^2. Such a possibility should not be incompatible with non-relational accounts.

3) There is a deeper reason behind Kuhn's complacency. Part of the rationale for his own theory of rejection seems to require conclusive falsification in the case of a non-relational account. For Kuhn to reject a theory is to no longer use (or have to use) that theory to solve problems or achieve empirical fit. Remember that Kuhn replaces the rejected theory with a superior problem solver. Perhaps Kuhn is assuming that if to reject a view is to stop using it then the only non-relational account that would provide warrant to stop using a theory is conclusive falsification. Though this may be the thinking behind the requirement, it will not do. First, even conclusive falsification does not prohibit the use of a theory. It is perfectly possible for one to know that a view is false *and* that a view solves certain problems. Hence it makes sense to use the view. Secondly, not using a view should not be taken as part of the analysis of rejecting a

view. Classical mechanics and the conservation of parity have been rejected but are nevertheless used more often than than their epistemological superiors.

The upshot of this is that the argument for (ITC) fails. What is surprising is that the considerations have been very abstract and *a priori*. This is odd given the historical orientation of the proponents of (ITC). Actually very little by way of careful historical analysis or linguistic usage is put forward on behalf of (ITC). Yet that is what is needed. Persuasive definitions of 'rejection' are of no use.

I think that there are numerous cases of theory rejection where no alternative is simultaneously accepted (in some cases not even contemplated). In these cases rejection involves believing that the respective theory is false and beyond repair. Obviously we need to look closely at cases. For the present I can only mention two. Even though I do not hold that the Duhemian Thesis rules out non-relational accounts of testing, I do hold that the Duhemian Thesis poses a serious problem for actual non-relational tests. We would expect theory rejection to be sensitive to this problem and that is exactly what we find in historical examples.

The first example is Newton's refutation of "the received laws of refraction." The case is developed in detail by Ronald Laymon (1978). According to the going theory, light refracted through a prism should form a circular image. It actually forms an oblong image. Newton considered assumptions which if added to the received view would explain the oblong shape. One is of particular significance. He showed that *improvements* in the experimental measurements and assumptions which were part of the refutation experiment *would not* allow the theory to gain a better experimental fit.

Clerk Maxwell provides a methodologically similar example. Kinetic theory does not make the correct predictions about the known ratios for the specific heats of all gases. The important point is that Maxwell considers ways in which the theory could be improved or modified. He postulated more and more complex internal structure for "atoms" as a way to account for the phenomena. Maxwell then attempts to prove that given the confines of the theory no internal mechanism will achieve the task. He concludes that the kinetic theory is mistaken and beyond repair. (Maxwell was in fact quite discouraged for he had no idea what account would prove correct.)

I have shown that the positive argument for (ITC) is not successful. Two examples which run counter to the spirit of (ITC) have been given. These are cases were theories have been rejected without simultaneously adopting an alternative theory. They are not cases which show how a Cartesian/Duhemian skeptic can be answered--for, of course, he can not be. But they are examples which should be studied to see how scientists actually handle the significant ambiguity raised by the Duhem problem.[7]

Notes

[1]There is some dispute about the "discovery" of (ITC). Putnam (1974, p. 229) claims credit. Interestingly, in response to Putnam, Karl Popper (1974, p. 995) claims to have scooped both Putnam and Kuhn. Isn't Popper supposed to represent the antithesis of (ITC)? For additional references from Kuhn see pages 8 and 145. Paul Feyerabend (1965, pp. 249-250) gives an early treatment of the thesis. See note 5.

[2]Kuhn regards (ITC) as a "central point" of his work.

[3]Kuhn's term for what I am calling "precipitating anomalies" is "counterinstances" (1962, p. 79) though he is not always consistent in his usage.

[4]The example follows the description in McGucken (1969).

[5]Paul Feyerabend (1965, p. 250) gives an argument similar to Kuhn's. " ... there does not exist a single interesting theory that is not in some kind of trouble. ... It is often better to wait and hope for the best than to throw up one's hands in despair and declare that the theory has been refuted. After all, the inconsistency might also have been due to faulty calculation, or else to incorrect observational results. This being the case, troublesome facts, taken by themselves, are almost never sufficient to eliminate the theory. What is needed is an alternative that "elevates the difficulty into a principle," fares well, both in the domain where the correctness of the original theory is without doubt and in new domains, and which moreover, possesses some intrinsic advantages, such as greater simplicity, greater generality, etc. This particular way of *accounting for* a difficulty is needed in addition to the *existence* of the difficulty if a straightforward refutation is to be obtained."

Aside from not claiming that (ITC) always holds, Feyerabend differs in a significant way from Kuhn. Unlike Kuhn, Feyerabend maintains that the new theory is not only more empirically adequate but also illuminates the precise failure of the old theory.

[6]He does cite Nagel (1939) for support.

[7]This essay is for M.R.K.

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