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Alexander George

Opening the Door to Cloud-Cuckoo-Land: Hempel and Kuhn on Rationality

A reading is offered of Carl Hempel's and Thomas Kuhn's positions on, and disagreements about, rationality in science that relates these issues to the debate between W.V. Quine and Rudolf Carnap on the analytic/synthetic distinction.

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To my teachers Sidney Morgenbesser and Burton Dreben, who would not be satisfied.

It is difficult to deviate from an old line of thought just a little.
—Ludwig Wittgenstein

According to a common view, many fundamental aspects of Carl G. Hempel's early conceptions of science and of the philosophy of science were thrown into question by Thomas S. Kuhn's historical investigations. At the very least, it is commonly said, these case histories of scientific practice raise doubts about Hempel's approach at the time—more generally, about one kind of logical empiricist approach—to science and its philosophical study.

By contrast, I find that if one characterizes Hempel's and Kuhn's positions in a certain way, it is quite problematic to bring such case histories to bear on their disagreement about the source of normativity. This difficulty resides primarily in the surd clash that lies at the foundation of their dispute.

Once this bedrock disagreement has been brought to light, it will also be possible to appreciate the internal tensions in Kuhn's overall view, the troubled nature of Hempel's later interpretations of Carnap and Popper, and finally the tension within, or at least the tentativeness of, Hempel's last writings on the nature of norms.

1 A Distinction and the Data

Hempel and Kuhn have had much to say about many aspects of scientific inquiry. Here, I confine my attention to what appears to be a focal point of their disagreement. Hempel early on accepted as obvious the distinction between the context of discovery and the context of justification. These terms were first used by his teacher Hans Reichenbach in 1938 [Reichenbach, 1957, 6-7], although the idea must be traced back at least to Gottlob Frege[1] who insisted that psychology cannot be allowed to intrude into logic: how we actually reason is one thing, how we ought to is quite another.

The distinction is readily applicable to science. There is, on the one hand, the study of how scientists actually proceed, and, on the other, the study of how scientists ought to proceed if they are to be rational. Hempel himself explicitly draws the parallel in describing this view:

> The philosophy of science is regarded as concerned exclusively with the logical and systematic aspects of sound scientific theorizing and of the knowledge claims it yields. On this view, the psychological, sociological, and historical facets of science as a human enterprise are irrelevant to the philosophy of science, much as the genetic and psychological aspects of human reasoning are held to be irrelevant for pure logic, which is concerned only with questions of the deductive validity of inferences, logical truth and falsity, consistency, provability, definability, and the like [Hempel, 1988, 293].[2]

Scientific methodology, as elucidated in the philosophy of science so conceived, provides a normative ideal against which the actual, quotidian practice of scientists can be judged.

Kuhn's work is widely thought to have thrown this central distinction into question, and with it a very influential conception of science and of the philosophy of science. He himself declared that the distinction is "extraordinarily problematic" [Kuhn, 1970b, 9]. Its overthrow has been achieved through Kuhn's emphasis on "the subjective elements which [. . .] enter regularly into the actual theory choices made by individual scientists" [Kuhn, 1988, 281]. "My point," Kuhn explains, is "that every individual choice between competing theories depends on a mixture of objective and subjective factors, or of shared and individual criteria" [Kuhn, 1988, 281]. These elements might include such unshared factors as the influence of various metaphysical or religious doctrines; personality traits; even the contingencies of social or psychological circumstance. If we attend to the presence of these factors, Kuhn and others have argued, we will observe that the actual scientific practice of justification is not, in point of rationality and objectivity, sharply distinguishable from what transpires during theory generation. The distinction between discovery and justification "does not fit observations of scientific life" [Kuhn, 1988, 282].

None of this, however, should move anyone who shared the early Hempel's perspective. For he would be interested in how evaluation of theories should proceed if it is to accord with the canons of epistemology—and this normative inquiry is, from a Fregean point of view, distinct from the descriptive enterprise of recording how scientists actually do evaluate theories.

What such observations about scientific practice show is that Reichenbach's terminology is not felicitous. For no one who embraced the discovery/justification distinction would be persuaded to abandon it by the actual case histories Kuhn has presented—and not for doubting their factual accuracy. That historical instances of scientific evaluation involve what Kuhn calls "factors dependent on individual biography and personality" [Kuhn, 1988, 284] ought to be, for such a person, irrelevant to the assessment of his position. For the intended distinction between contexts of discovery and of justification is not that in practice the one is sensitive to subjective factors while the other is not. Historical case studies would indeed be germane—and no doubt inimical—to that claim. Rather, the intended contrast is between how scientists actually proceed and how they ought to proceed. Of course some aspects of how they actually proceed, for instance those surrounding a hypothesis' genesis, do not give rise to normative evaluation of their practice. But some aspects, most notably their judgments about evidence and justification, very much do. The real point of the distinction is to draw a sharp line between how scientists actually judge about evidence and justification and how they ought to, between the descriptive enterprise of recording how scientists in fact evaluate hypotheses and the normative one of articulating the canons of rationality in accordance with which such evaluations ideally take place. Thus, a better terminology would be "justification in practice" versus "justification in the ideal," though I shall hew to the standard one here.

Once the distinction is so understood, it seems clear that historical studies—however varied, numerous, or detailed—could not move someone who holds the distinction to reject it: for how could facts about our practice lead one to conclude that what one takes to be an independent normative perspective on aspects of that practice is not available? The factual, it might well seem to one who accepts the distinction between fact and norm, cannot speak to whether there is a stance beyond the factual; facts can only speak to claims about the facts.

This dismissal is not the conclusion of some argument: rather, one's doubts about the relevance of historical case studies for the tenability of the distinction between discovery and justification is really a mark of one's having accepted some such distinction. Put otherwise, that one takes historical case studies to be at all relevant to the tenability of the distinction between discovery and justification cannot so much be one's reason for rejecting the distinction as it is a criterion of one's having already rejected it.

Kuhn's rejection of this central distinction goes hand in hand with his inability to make sense of the normative vantage point on scientific practice that the early Hempel—more generally, the Fregean—finds himself able to occupy. Of course, Kuhn does believe that there are norms for theory choice, relative to which actual practice can be judged. But this is not yet to accept a Fregean conception of this distinction. The gap can be made clearer by looking at a couple of considerations that Kuhn offers with a view to undermining a confidence that such a perspective is available.

Thus, it would be reasonable, Kuhn grants, to take this kind of perspective to be at hand if one could articulate an objective decision procedure for evaluating theory choice in science: for the deliverances of such an "algorithm of objective choice" [Kuhn, 1988, 283] would indeed provide the normative basis for evaluation. But, Kuhn argues, we cannot describe such a decision procedure. Historical study reveals, he holds, that the evaluations of scientists fail to be fixed by shared ("objective") criteria and typically involve determination by idiosyncratic ("subjective") considerations as well. The ineliminable role of these unshared factors makes Kuhn doubtful that any kind of objective decision procedure is attainable. "Considerations relevant to the context of discovery are then relevant to justification as well," he says. "That is why it has been difficult to construct algorithms for theory choice" [Kuhn, 1988, 283]. And that is why, he concludes, we should not expect to be able to articulate a normative stance of the kind the Fregean takes himself to adopt.

Hempel agrees with Kuhn's conclusion about the unavailability of such algorithms.[3] He no doubt also agrees with the premises (descriptions of scientific practice) that lead Kuhn to this conclusion. And yet he would disagree with Kuhn's inference.[4] For Hempel must find the considerations that weigh heavily with Kuhn irrelevant, as they concern how scientists actually make their judgments: "these last considerations," Hempel says, "are psychological and sociological and cannot, of course, prove it impossible to formulate precise general criteria of theory choice embodying those desiderata" [Hempel, 1988, 298].[5] Hempel's "of course" signals his acceptance of a distinction between the factual and the normative that is sufficiently sharp as to bar him from taking the feasibility of any project in epistemology to be dependent upon features of scientific practice. After all, what comes with acceptance of such a distinction is the acknowledgment that it is in principle possible that, despite the fact that scientists have always proceeded in a certain way, they have been unjustified in doing so.

Instead, Hempel's reasons for being skeptical of the existence of "precise general criteria of theory choice" are rather that "the difficulties encountered by *analytic* efforts to explicate such notions as the simplicity of theories or the degree of variety of the empirical phenomena covered by a theory (and thus, perhaps, its scope) do not augur well for the attainability of those *analytic* objectives" [Hempel, 1988, 298] (emphases added). And just as Kuhn's reasons for skepticism are non-starters for Hempel, so Hempel's are for Kuhn: for Hempel's conception, or at least the logical empiricist's conception, of the perspective from which such "analytic objectives" are pursued is one which Kuhn cannot make full sense of.

Kuhn offers a second argument against the central distinction, again one designed to cast doubt on the availability of any perspective that is supposed both independent of actual practice and yet somehow to be relied on in evaluating it. The argument consists of two parts: first, a diagnosis of some considerations that might lead one to think there is such a perspective, and, second, the undercutting of those considerations. One might well acknowledge, Kuhn begins, that "subjective" factors will always be present in actual cases of theory choice, but still remain confident that there is an "objective" normative perspective that bears on actual practice *if* one were impressed by the formation of consensus, which can easily seem to signal the gradual diminishing of the effect of such factors. The judgments of scientists tend to coincide over time and this can naturally foster the impression that idiosyncratic factors will play an increasingly negligible role in evaluations. Consequently, the imagined situation in which such factors play no role at all might readily be viewed as the limit, so to speak, of a real-world process. As such, that situation has some claim to be an appropriate position of evaluation of actual practice. And yet, it is a position that will remain forever distinct from all actual ones.

Kuhn seeks to undercut this motivation, however, by suggesting that we may here be victims of an illusion: "If subjective factors are required to account for the decisions that initially divide the profession, they may still be present later when the profession agrees" [Kuhn, 1988, 283-4]. That is, convergence of evaluation could take place without convergence of the factors determining judgment. To infer the second from the first is simply, in Kuhn's view, a "non sequitur" [Kuhn, 1988, 283].

While Kuhn may be right that the inference is not logically valid, it is rather unlikely that anyone ever thought otherwise. It is more plausible to take the argument in question as claiming that a promising explanation for gradual convergence of judgment amongst scientists is an increasing similarity in the factors influencing their judgment. There is nothing inevitable about this explanation of convergence and it might in fact prove to be incorrect, but it remains a reasonable one nevertheless.

But there is a more important observation to be made about Kuhn's second argument, one that relates to the theme of this essay, and it concerns his diagnosis of why one might believe in the existence of a certain kind of normative stance in the first place. It is simply this: someone like the early Hempel could not find Kuhn's diagnosis in the least compelling. Kuhn can, because the only conception of an objective normative perspective available to him is that of some construction out of actual practice, for instance that toward which the actual practice of judgment in some sense tends. If one conceives of the normative in this way, then it is clear why the non-congruence of actual evaluation procedures over time would shake one's confidence in the existence of an objective evaluatory stance: there is nothing for such a stance to be beyond what actual practice tends toward over time (or beyond some construction out of actual practice), and the existence of this limit (or of any intelligible construction) is put into question if the evaluative procedures of actual scientists do not as a matter of fact exhibit sufficient internal similarities. This is precisely the conception of an objective normative stance that one would expect in the thought of one who does not strictly distinguish between contexts of discovery and of justification.

By sharp contrast, one who embraces this central distinction as a Fregean understands it will not conceive of a normative stance in this way, in particular will not conceive of it as contingent on the existence of any factual regularities or tendencies in the practice of scientific inquiry. For, a defender of the distinction would wish to claim, we find it intelligible to imagine that a given epistemic tendency on the part of scientists, however entrenched, is simply in error. Any convergence that might actually result in the evaluation procedures of scientists would not be viewed by the early Hempel as support for the intended distinction between the contexts of discovery and of justification. Correlatively, even if Kuhn were correct in his suspicion that individual evaluation procedures do not converge over time, Hempel should not be shaken from his belief in the existence of an objective normative perspective on actual scientific practice. Its availability, someone who embraced the Fregean distinction as intended would say, is independent of whatever patterns are discernible within scientific practice.

Again, one's judgment about the relevance of the considerations Kuhn points to will be determined by one's position on the very issue on which these considerations are meant to bear. Kuhn's work consists, then, not so much in a refutation of a certain Viennese approach to science as in a decision to proceed differently.

2 Quinean Congruences

The dialectic of the debate between Hempel and Kuhn bears a fascinating similarity to that operative in the dispute between Rudolf Carnap and W.V. Quine over the distinction between analytic and synthetic truths. I have argued (George [2000]) that the considerations that Quine advances against the notion of analyticity only count as such from a perspective that already rejects the analytic/synthetic distinction. Carnap never disputes what Quine points to; instead, he denies that these considerations bear on the tenability of the distinction. It is thus not helpful to say, as is commonly done, that Quine rejects analyticity because of the arguments he offers Carnap. If this were so, then, since Carnap actually accepts those arguments, one would have to conclude either that Quine's reasoning is much weaker than he realizes or that it is far stronger than Carnap appreciates. Hence, this common way of putting the matter allows for no interesting interpretation of their dispute. Better, then, to take Quine's drawing of skeptical conclusions about analyticity from the considerations he offers as a mark of his having already rejected analyticity, and to read Carnap's equanimity in the face of those very same considerations as a mark of his acquiescence in the distinction. The similarity to the dialectic between Kuhn and Hempel is plain.

This is not a coincidence. To recognize the analytic/synthetic distinction is to acknowledge that there are some stretches of our language which can be employed for evaluating claims about the natural world but which comprise statements that are not themselves adopted on the basis of empirical evidence. They fail to be so adopted because they do not hold in virtue of the way the world is, but rather because they reflect semantical or conceptual truths. And such an acknowledgment is effectively of a piece with an acceptance of a normative perspective on judgments about the world that is rationally independent of the vagaries or tendencies of actual practice.[6] Before proceeding, a little more needs to be said about this connection.

The analyticity of framework truths—for instance, of truths that provide an evaluative stance on scientific practice—consists in the fact that their selection is not justified or subject to deliberation in the way in which the making of synthetic judgments is. In particular, their choice is rationally unconstrained by the empirical facts; indeed, properly understood, their adoption is what makes possible a system of representation of the natural world and a way of rationally evaluating claims within that system. That scientific practice takes a particular form—that its history is so and so, that scientists find it rational to believe such and such—is just another fact about the world and as such cannot rationally constrain choice of the analytic truths used to evaluate that practice. On the view that accepts the analytic/synthetic distinction, then, choice of an evaluative perspective on scientific practice is not rationally determined by features of that practice.

Carnap holds that part of what one does when one articulates a linguistic framework is "to lay down explicit rules for the evaluation" of observations as "confirming or disconfirming evidence" [Carnap, 1956, 207]. These rules, which govern the assessment of claims, are components of the linguistic framework; settling on them is part of what we do when we settle on "the structure of our language" [Carnap, 1956, 207]. Now, like all such decisions, the laying down of normative rules of warrant is not rationally constrained by the facts. It is of course true that factual matters might incline us one way or another. But such inclining is not a form of rational suasion. Indeed, the very choice of certain rules over others is not really a cognitive judgment at all. Thus Carnap tells us that the opting for certain rules of evaluation over others will indeed "usually be influenced by theoretical knowledge, just like any other deliberate decision concerning the acceptance of linguistic or other rules," and "efficiency, fruitfulness, and simplicity . . . may be among the decisive factors" [Carnap, 1956, 208]. But this influence is not rational, for rationality is a framework-internal notion. Indeed, claims about the world could not confirm the choice of norms of evaluation, for to opt for certain norms of evaluation is not even to make a judgment with "cognitive content." The embrace of a particular framework, and thus of the norms of evaluation that partly constitute it, "cannot be judged as being either true or false because it is not an assertion" [Carnap, 1956, 214]. Thus, in particular, while facts about how scientists actually behave can "influence" our adoption of framework rules, like norms for what counts as "confirming or disconfirming evidence," they cannot rationally bear on the matter. Indeed, for Carnap, there is no matter with "cognitive content" for them to bear on.

As we have seen, it is just such a picture as Carnap's that Kuhn, in rejecting the discovery/justification distinction, objects to: a picture according to which our choice of the norms of evaluation is rationally unconstrained by what we find scientists actually doing. The supposition, Kuhn insists, "that we possess criteria of rationality which are independent of our understanding of the essentials of the scientific process is to open the door to cloud-cuckoo land." [Kuhn, 1970a, 264]. Any perspective that is prepared in principle (even if it granted that in practice it may not be useful) to declare that most scientists are not rational has, for Kuhn, *ipso facto* disqualified itself as a normative perspective.

We should expect, then, that Kuhn, by refusing to admit a potent discovery/justification distinction, would set himself against the notion of analyticity. And in fact, we do find that Kuhn agrees with Quine's rejection of the notion, often expressing his debt to Quine's writings on analyticity.[7] In the light of the dialectical similarity noted above, we now might even say that Kuhn is to the early Hempel as Quine is to Carnap (though we shall soon see reason to qualify this).

In fact, there is an interesting commonality to the form Quine's and Kuhn's rejections take. Quine often complains that the hypothesis that there are analytic truths has no empirical consequences: the world would look just as it does even if there were no distinction between analytic and synthetic truths. Carnap agrees. But they draw different consequences from this fact. Carnap concludes that the notion of analyticity and its cognates are not part of empirical science, but instead of philosophy in the non-pejorative sense of the term, that is, they are part of "logical analysis."[8] Since Quine—precisely by virtue of his rejection of analyticity—does not recognize a discipline of philosophy that is distinct in kind from natural science, the empirical emptiness of claims involving analyticity is seen by him as evidence for their emptiness period.

In like fashion, one finds Kuhn questioning what the empirical upshot is of the distinction between contexts of discovery and of justification: in elaborating this distinction and kindred notions, he claims, one could not but be articulating:

> parts of a theory and, by doing so, [one] subjects them to the same scrutiny regularly applied to theories in other fields. If they are to have more than pure abstraction as their content, then that content must be discovered by observing them in application to the data they are meant to elucidate. How could history of science fail to be a source of phenomena to which theories about knowledge may legitimately be asked to apply? [Kuhn, 1970b, 9][9]

But the discovery/justification distinction, as understood by a Fregean, is not an explanatory tool that is meant to organize facts about scientific practice in the way astronomy seeks to organize celestial data. Claims within the philosophy of science are not of the same kind as those of empirical science itself. Philosophy of science is not the science of science in the way biology is the science of living organisms. *Pace* Quine, the science of science is not science enough [Quine, 1976a, 151]. What goes missing is a logical analysis and normative assessment of scientific practice. Philosophy of science seeks not to capture that practice under some theory but instead to evaluate it in accordance with norms developed through philosophical reflection. And just for these reasons, Kuhn finds himself unable to accept the distinction, as understood by those who wield it. The fundamental kinship with Quine is clear.

3 Essential Tensions

Once this is appreciated, however, a serious problem in the interpretation of Kuhn's views arises. For, as a few commentators have noted, there is an important feature of Kuhn's work that is distinctly Carnapian.[10] And it is doubtful that it can peacefully coexist with Kuhn's Quinean bent. The feature in question is Kuhn's view about the nature of scientific theory choice in what he calls revolutionary periods in science. According to Kuhn, such choices are at the level of what he dubs paradigms, as opposed to more local, paradigm-internal judgments. A central feature of his view is that these choices are fundamentally different: they are not, and cannot be, determined merely by the evaluative procedures characteristic of what he calls normal science, for these depend in part upon the particular paradigm that is in question. "[I]n science," he writes, "there are two sorts of change" [Kuhn, 1970a, 250]. Unlike decisions in normal science, the "issue of paradigm choice can never be unequivocally settled by logic and experiment alone" [Kuhn, 1970b, 94]. A paradigm involves norms and standards by which competing scientific proposals are evaluated. Kuhn's idea is that when the proposal in question is a change of paradigm, then evaluation does not involve judgment in the normal sense and can only result in "a special sort of change" [Kuhn, 1970b, 181]. Paradigms are changed "not by deliberation and interpretation, but by a relatively sudden and unstructured event like the gestalt switch" [Kuhn, 1970b, 122]. In some ways, clearly, this resembles Carnap's claim that there is a difference between framework-internal and framework-external choices, the former being rational and properly a matter of deliberation, the latter being non-cognitive and subject merely to pragmatic pressures.

It is hard to see how this Carnapian strain in Kuhn's outlook can be reconciled with his rejection of the distinction between contexts of discovery and of justification. We saw earlier that the latter is closely connected to the rejection of the notion of analyticity. And yet it is difficult to see how Kuhn's Carnapian views do not ultimately demand embrace of something like that notion. For to say that choice between paradigms, and the norms for scientific inquiry they incorporate, is a different kind of choice is just to say that these norms do not relate to "logic and experiment" in the way that ordinary statements within normal science do. That is, choice of norms, of the bases of evaluation, is not evidentially dependent on how matters stand in the world. But since one of the ways in which matters stand is that scientific practice takes such and such form, it follows that the choice of norms is not rationally constrained by the nature of that practice. This is, however, precisely *not* in line with what one would expect from someone who rejects the discovery/justification distinction: such a rejection, we saw earlier, signals that choice of norms can be rationally shaped by the nature of the practice they serve to evaluate. In sum, an insistence on the Carnapian thought that some choices are radically different in kind from others is in tension with a rejection of the discovery/justification distinction.

The tension is just under the surface of Kuhn's writing. There are of course many passages in which the Carnapian thought is articulated. But there are also indications that he does not, after all, have the resources to draw a distinction in kind between choices of paradigm and choices within a paradigm.

When scientists engage in wholesale theory choice, Kuhn says, they are guided by values. These values (e.g., accuracy and scope) are shared. Yet different scientists may apply a given value differently (e.g., by arriving at different judgments about which theory has greater scope), and furthermore may weight values against one another differently (e.g., by differing about whether accuracy is more important than scope). Ultimately, these differences may be due to "factors dependent on individual biography and personality." In this way, Kuhn claims, these values are unlike rules or algorithms: two scientists can be committed to the same values, and yet, consistently with that commitment, apply them differently.[11]

I do not want to evaluate Kuhn's claim, but rather to point out that it does not seem to differ substantially from his view of decision-making *within normal science*. It is true that Kuhn often writes as if normal science were a very different kind of activity. In particular, his metaphor of normal science as puzzle-solving conjures up a picture of problems in normal science having exactly one correct solution—say as a chess problem has precisely one solution [Kuhn, 1970b, Ch. 4]. Additionally, this metaphor suggests that, while disagreements about how to proceed in normal science might arise, there can be no dispute about what constitutes the single correct solution: should several candidate solutions be presented, it seems there would be agreement regarding which, if any, to adopt. However, Kuhn never claims that in normal science there is exactly one solution to a given problem; rather, he writes merely that a criterion for something's being a puzzle is "the assured existence of a solution" [Kuhn, 1970b, 37]. Indeed, Kuhn regularly refers to the existence of a range of "admissible solutions to theoretical problems" [Kuhn, 1970b, 39]. Furthermore, though in general there will be widespread agreement regarding which of several proffered solutions is correct—after all, this is normal science[12]—it is still possible for two scientists to disagree about their choice and yet remain practitioners within the same paradigm. Why? "Normal science," Kuhn says, "is a highly determined activity, but it need not be entirely determined by rules"; he believes, in fact, that the practice of normal science can and should "be understood without recourse to hypothetical rules of the game" [Kuhn, 1970b, 42, 47].

What does govern the decisions of the normal scientist if not some kind of rule? Kuhn holds that it is a "strong network of commitments—conceptual, theoretical, instrumental, and methodological" [Kuhn, 1970b, 42]. In addition, just those same values (accuracy, scope, etc.) that play a role in situations of revolutionary choice also inform everyday scientific practice: "they function at all times," Kuhn says [Kuhn, 1970b, 184].[13] Surely, these commitments and values can be applied differently by different scientists working within normal science. Likewise, different scientists might well weight these commitments and values against one another in different ways, all the while remaining faithful to the same network of allegiances. Such differences could result in conflicting evaluations of the "admissible solutions" to some theoretical problem.

Are such conflicts substantively different from those arising in the context of theory choice? Is a choice made in the circumstances of normal science really of a fundamentally different kind from one made in revolutionary science? For instance, if choice in revolutionary circumstances is shaped by "individual biography and personality," then must not something similar hold for the scientist working within normal science, who applies and weights the same values (and yet other, more specific, commitments)? We do of course observe that some choice situations result in greater consensus than others. But this is a matter of degree, and so cannot provide evidence that two different kinds of choices are in play.[14]

In sum, it is hard to find in Kuhn's account the materials for fashioning a distinction in kind between normal and revolutionary science, and so his Carnapian theses fail to find a home within his actual views about theory choice.

4 *Reconciliation Resisted*

Hempel eventually shifted toward Kuhn's views. Perhaps wanting to show that this does not require traveling any great distance, Hempel offered a reconciling reading of Carnap. In Hempel's late interpretation of Carnap, however, we find him failing to appreciate the very same tension that is present in Kuhn.

I have just argued that Kuhn seeks both to reject the analytic/synthetic distinction and also to embrace a distinction (about kinds of theory choice) that is of a piece with it. Now according to the later Hempel, Carnap, when properly understood, accepts both the analytic/synthetic distinction and also the idea that normative claims about scientific practice are subject to rational evaluation in the light of empirical considerations. In trying to seek a rapprochement between Carnap and Kuhn—and hence ultimately between Hempel's earlier and later selves—by finding what he calls "normative" and "descriptive" elements in both their views, Hempel fascinatingly reads Carnap as an inversion of Kuhn. Ultimately, this is a fraught reading however, for (as I argued in section 2) the idea that claims about the logic of science are not subject to rational assessment in the light of empirical data is an idea that is cognate to the analytic/synthetic distinction itself. It will be instructive to examine in greater depth Hempel's reading of Carnap.

Hempel warms up his reader by arguing for a similar interpretation of Karl Popper. While acknowledging that Popper holds that methodological "principles are conventions which 'might be described as the rules of the game of empirical science'," [15] [Hempel, 2000a, 203] Hempel suggests that for Popper these conventions must rationally answer to empirical facts. Hempel writes:

> Indeed, Popper remarks that if, in accordance with his methodology, we stipulate that science should "aim at *better and better testable* theories, then we arrive at a methodological principle ... whose [unconscious] adoption in the past would rationally explain a great number of events in the history of science." "At the same time," he adds, the principle "gives us a statement of the task of science, telling us [w]hat *should* in science be regarded as *progress*".
>
> Thus, while Popper attributes to his methodological principles a normative character, he assigns to them, in effect, an empirical-explanatory role as well. [V]ery significantly, that many actual events in the history of science could be explained by the assumption that scientists in their professional research are disposed to conform to Popper's norms [Hempel, 2000a, 204-5]. [16]

But there is a slide here between the normative methodological principles, on the one hand, and, on the other, their adoption by working scientists. It is the assumption that these principles have been adopted by scientists (or the assumption that scientists for one reason or another are disposed to act in accordance with them) that explains the course of scientific inquiry. It is such an assumption that is rationally answerable to the actual nature of scientific practice. And this assumption is an empirical one—about the psychology or dispositions of working scientists—that is distinct from the normative claim of the methodological principles themselves. Thus, we do not have here a consideration that demonstrates that Popper understands his methodological conventions to be rationally sensitive to how matters happen to be in the world. [17]

Hempel says that Popper "intends [his methodological conventions] to meet certain justificatory requirements, and these have something to do with how scientists conceive and pursue the goal of their endeavors" [Hempel, 2000a, 204]. But the critical question will always be what "have something to do with" a range of empirical facts means, and in particular whether it means *are rationally answerable to* those empirical facts.

This issue becomes very clear when we turn to Hempel's reading of Carnap. I shall quote an illustrative passage in full:

> [...] Carnapian explications are intended to clarify and sharpen certain concepts — such as testability, rational credibility, and so on — *which are already in use and play an important role in the preanalytic discussion of scientific procedures*. Adequate explications must, therefore, conform to a reasonable extent to the preanalytic, vague use of the *explicandum* terms. Carnap, indeed, establishes this as a further requirement, in addition to the conditions of precise formulation, simplicity, and fruitfulness: "The explicatum must be *similar to the explicandum*" ... Thus, the similarity requirement imposes *empirical constraints* on explication while, at the same time, leaving room for a prescriptive conventional component when choosing some particular explicatory re-definition so as best to comply with the requirements of simplicity, precision of formulation, and fruitfulness [Hempel, 2000a, 207]. [18]

There is no doubt that Carnap held that many kinds of considerations were relevant when choosing a particular linguistic framework, that is, when settling upon a particular understanding of what terms like "testable," "evidence," "imply," and so on, mean. Some of these considerations may well involve facts about how speakers use these expressions. But for Carnap, as we have seen, such considerations can at best exert a non-rational influence on an agent who is choosing which particular framework to adopt. Talk of rational constraint only has its place within a framework, once a language and rules of reasoning and inquiry have been settled upon. For Carnap, this observation is critical in understanding why traditional philosophical disputes have proven to be so frustratingly irresolvable and so different from scientific disagreements: philosophers, unlike scientists, typically dispute about which framework to adopt, which language to speak, and no facts about the world can rationally bear on such disagreements. The empirical facts only come into focus, and talk of rational relevance only gets a grip, once a particular linguistic framework has been adopted. Of course, we might be inclined one way or another in our choice of frameworks. But for Carnap, the forces that so incline us are, as he puts it, "practical"; they make one choice of framework more useful than another [Carnap, 1956, 218]. The bearing such forces have on our choice of framework is of a different kind from the rational bearing empirical considerations have on hypotheses formulated within a framework. In fact, as we saw, Carnap is even inclined to withhold the term "judgment" to describe our embrace of a particular framework. Judgments are cognitive acts that presuppose the adoption of a framework. The adoption itself is not a judgment so much as a choice (about which language to speak, about what is to count as rational, etc.). "The acceptance" of a framework, Carnap says, "cannot be judged as being either true or false because it is not an assertion. It can only be judged as being more or less expedient, fruitful, conducive to the aim for which the language is intended" [Carnap, 1956, 214]. Thus, the fact that an analysis of some notion pertinent to the methodology of science (for instance one belonging to deductive logic, like the concept of valid inference) accords with the intuitions of working scientists might indeed make adoption of this analysis "expedient" relative to certain purposes. Nevertheless, it would be wrong to say that these intuitions rationally warrant adoption of the analysis; it would in no way be irrational to explore alternative analyses, including ones that are at odds with many scientists' intuitions (for instance, non-classical analyses of logical notions).[19] And the same holds for notions central to inductive logic, such as confirmation.

In connection with Carnap's work on rational inductive belief, Hempel quotes a passage that in his view:

> clearly comes close to claiming that an explicatory theory of rational credibility as conceived by Carnap should not just prescribe norms for rational research procedures but should also have the potential for providing at least an approximate descriptive and explanatory account of some aspects of actual scientific inquiry [Hempel, 2000a, 209].

But the passage from Carnap in fact falls short of that. It reads (in part):

> If sufficient data about decisions of this kind made by scientists were known, then it would be possible to determine whether a proposed system of inductive logic is in agreement with these decisions [Carnap, 1963a, 990].

It is critical to note that Carnap does not say that such agreement would rationally favor our adopting the proposed system (he does not even claim that it would incline us on pragmatic grounds). To be sure, such "data about decisions" would be evidentially germane to the hypothesis that scientists operate with such and such credibility function; but that is an empirical claim about the psychology or sociology of scientific activity.

Carnap expresses clearly the general conception of the positivists, which he always embraced:

> In line with Wittgenstein's basic conception, we agreed in Vienna that one of the main tasks of philosophy is clarification and explication. Usually, a philosophical insight does not say anything about the world, but is merely a clearer recognition of meanings or of meaning relations [Carnap, 1963b, 917].

Furthermore, this was Hempel's *own* conception of the position when he was younger and much closer to the logical positivists. In an essay from 1937, Hempel summarizes his view that "the validity of analytic propositions is based on the formal rules of the game, which consist of using a certain language; and we have compared these rules to those which govern the game of chess" [Hempel, 2000b, 68].[20] And he then considers the objection that, while the rules of a game are arbitrary and we can imagine changing them at will, this is not so for the rules of language. "In fact," he acknowledges, "it is incontestable that, in ordinary language, we generally conform to the principle of excluded middle and other syllogistic principles of classical logic to such a degree that it seems to us impossible to follow any other system of rules" [Hempel, 2000b, 69]. But he counters that this consideration, the fit with ordinary language, is rationally irrelevant. The "formal specifications" of alternative systems of reasoning "can no more be true or false than the rules of a game, but they can be more or less convenient in a certain context (e.g., more or less well adapted to the needs of a certain empirical science)" [Hempel, 2000b, 69]. Analytic propositions, he says, are such that "no experience and no phenomenon, however unexpected and improbable, can ever disconfirm them" [Hempel, 2000b, 70]. Our choices about which language to adopt "can be regulated not by truth criteria but only by practical considerations such as the question of whether the form of the language is convenient in relation to the context for which the language is intended" [Hempel, 2000b, 71].

In short, Hempel in 1937 warns us against precisely the kind of interpretation of his (and Carnap's) general view that he will promote, in a spirit of reconciliation, half a century later.

5 *Hempel's Dilemma*

Although Hempel originally embraced the analytic/synthetic distinction, he eventually let it go: "the idea of meaning, and related notions such as those of analyticity and synonymy," he says, "are by no means as clear as they have long been considered to be, and it will be better, therefore, to avoid them when this is possible" [Hempel, 1965b, 191]. It is surprising that when the later Hempel articulates his more positive views about the normative, about philosophical reflection on science and its relation to the descriptive, he never (to my knowledge) makes reference to Quine. Nevertheless, Hempel's late views, though often stated rather flatly and tersely, bear a strong affinity to Quine's. For instance, Hempel writes:

> The imposition of desiderata may be regarded, at least schematically, as the use of a set of means aimed at the improvement of scientific knowledge. But instead of viewing such improvement as a research goal that must be characterizable independently of the desiderata, we might plausibly conceive the goal of scientific inquiry to *be* the development of theories that ever better satisfy the desiderata [Hempel, 2001a, 356].

The thought is so compressed as to require some unraveling.

It can be approached by first considering a traditional positioning of the philosopher vis-à-vis the practice he is seeking to evaluate, viz. that of someone whose deliberations need not be rationally responsive to how matters actually stand. This stance is shared by philosophers of science as far apart on other matters as the Cartesian rationalist and the Carnapian positivist. In both cases, the philosopher takes his reflections and proposals about rational inquiry to be rationally unbeholden to the actual direction of scientific research: in the first case because the philosopher is somehow limning the ideal, and in the second because the philosopher is choosing which conventions to follow. From such a position, one takes oneself to possess a standard for judging actual scientific practice that one has arrived at independently of that practice. This standard can also serve to evaluate methodological maxims, or what Hempel calls "desiderata," for in so far as a maxim guides practice toward the ideal, it is justified.

Hempel's passage suggests a very different positioning on the part of the philosopher. For he urges there that the standard against which a methodological maxim is to be evaluated is actual scientific practice, a practice shaped (in very complicated ways) by the adherence to such methodological maxims. In other words, science, as we find it, already provides us with our best understanding of what successful rational inquiry looks like. If we wish to justify a maxim, then we can do so (and really, can only do so) by showing that its adoption promotes the choice of theories like the very best ones we presently have.

To some, this will seem a hopeless stance. For it cannot provide the philosopher with the justification he seeks. The reason is simply this. The stance does not permit us to ask whether the methodological maxims that have led to present science are really justified; for to do so would amount to asking the trivial question whether they have contributed to the development of those theories to whose development they have contributed. For instance, scientists (we are often told) opt to believe the simpler of two competing hypotheses. How can we justify this? Hempel would have us ask whether such a maxim would lead to the choices of theories of the kind we hold up as paragons of scientific achievement today. Well, we know the answer to *that* question, it might be objected. The stance, it might seem, is incapable of delivering anything but circular justifications.

Another manifestation of this is that the position Hempel describes is not one that would allow (even if only in principle) for the wholesale repudiation of the methodological maxims that actually guide (in some sense) scientific practice: to repudiate the lot of them would require concluding that our best theories of the natural world do not satisfy them, i.e., that the theories chosen on the basis of those maxims do not satisfy those maxims. Again, no need to spend any effort on *that* inquiry.

For those who embrace this view, however, these are limitations only in name. In the first place, there is still room for justificatory work. If a refinement or alteration of some methodological maxim is proposed, it can be tested: for we can check to see whether our best theories about the natural world satisfy it, that is, whether it is satisfied by theories in whose choice some related though distinct maxim figured. Even if the proposed maxim for rational inquiry differs substantially from those heretofore articulated, it can still be evaluated by seeing whether its adoption would indeed have led to those theories of nature now judged to be our best.

Secondly, the desire for a grander justificatory project is simply a Friar's Lantern. To think that we could evaluate a canon of inquiry in *another* fashion is to think that we have some access to what is a correct account of the world which is independent of that which contemporary scientific research provides us with and relative to which we can assess whether the canon in question contributes to the choice of such accounts. Put another way, it is to think that we have some way of discovering what rational empirical inquiry looks like that does not take off from our judgments about how we arrived at our best present theories of the world. But on this view, all such thoughts are illusory. We have no option but to "work from within," as Quine always insisted [Quine, 1976b, 252].[21]

There is a connection between abandonment of the analytic/synthetic distinction and the acknowledgment that we must always work from within our best theories of the world. To eschew that distinction is to forswear appeal to different kinds of truth. Any claim that is true is on a par with any other, at least as far as its truth is concerned. A claim within science and one about scientific justification are not claims to which different kinds of truth are appropriate; they are, in this respect, on the same level. They differ in their subject matter, though not in the kind of truth they might be. But a difference in subject matter does not signal rational insulation. A claim about silkworms might prove to be rationally sensitive to one about the benzene molecule. Likewise, how scientists actually judge in the course of their work might prove to be rationally germane to claims about the norms of scientific inquiry. At least, there seems no basis for confidence that the latter kinds of issues can in principle be settled without rational appeal to scientific judgments. That is, there is no basis for thinking that working from without is a real option. A rejection of the analytic/synthetic distinction strongly favors the idea that the philosopher cannot sit in detached judgment of all our ways of judging about the world.

Although Hempel articulates this idea with approval, his embrace of it is not as wholehearted as is Quine's. Thus he considers the above-mentioned worry, that this position "might be viewed as *justifying* in a near-trivial way the choosing of theories in conformity with whatever constraints are imposed by the desiderata," [Hempel, 2001b, 388] but he does not rebut the worry. Instead, he adds his own caution that:

> . . . this kind of justification does not address at all what would be the central concern of the classical problem of induction, namely, the question whether there are any reasons to expect that a theory which, as judged by the desiderata, is preferable to its competitor, at a given time will continue to prove superior when faced with further, hitherto unexamined, occurrences in its domain [Hempel, 2001b, 388].

In other words, he cautions, to point to the fact that a proposed theory best satisfies the methodological desiderata that are already satisfied by our most successful theories of the world is still not to have given "any reason" of the "classical" kind in favor of adopting that theory. But if we are truly working from within, then such facts are the only possible considerations anyone could offer in favor of a theory, and reasons of the "classical" kind are not so much an alternative to these considerations as they are spectral goals that elude full comprehension. That Hempel does not repudiate this worry, that he concurs that the stance he has articulated fails to address "the central concern," suggests that his embrace of this stance is less than total.

In this uncertain commitment, Hempel's position resembles Kuhn's. We saw earlier that Kuhn's rejection of the analytic-synthetic distinction (and along with it, a certain conception of the context of justification) does not sit well with his Carnapian insistence on there being a different kind of decision that takes place during revolutionary periods in science. For his part, the later Hempel also wants not to accept the analytic/synthetic distinction but does not fully follow suit with an unequivocal embrace of the view that philosophers too must work from within our developing system of beliefs about the world.

6 *Conclusion*

In sum, we find that Hempel does eventually move from his earlier views to ones that are closer to Kuhn's. I argued (in section 1) that if Hempel's earlier Carnapian position is correctly understood, this change is not rationally compelled by any of the considerations Kuhn musters. In fact, once one understands what lies at the root of the disagreement (see section 2), it is difficult to imagine what might constitute non-question-begging grounds for such a change. With this understanding in place one can furthermore appreciate a tension at the core of Kuhn's views (see section 3). In Hempel's later desire to effect a rapprochement between Carnap and Kuhn, we find him attributing something very like the dual of Kuhn's view to Carnap. Such a position has a correlative tension at its center and perhaps it is not surprising that a close examination of Hempel's evidence for this attribution finds it wanting (see section 4). Finally, I suggested (in section 5) that Hempel's late position on these matters is itself not a pure one and that he continues to harbor sympathies now irreconcilable with his core views on rationality.

Both Hempel and Kuhn can be viewed as looking for a position that is midway between Carnap's and Quine's: a position that eschews the analytic/synthetic distinction but at the same time plays with ideas that are in the same key, such as (in Kuhn's case) the thought that choices within scientific practice are not all rationally sensitive to the same kinds of considerations, or (in Hempel's) the related thought that it is conceivably not rational to select a theory that satisfies methodological norms that have led to the very best theories now available. It is a measure of the coherence of Carnap's and Quine's positions that it is far easier to reject either one than it is to find a middle ground between the two.

Alexander George
Department of Philosophy
Amherst College
Amherst, MA 01002
ageorge@amherst.edu

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Notes

[1]In Reichenbach's case, no doubt, via Rudolf Carnap. See for instance Frege [1959].

[2]In an earlier paper, Hempel insisted that there be no "confusion of logical and psychological considerations" [Hempel, 1965a, 9-10].

[3]Thus Hempel insists that there are "no objective logical criteria [that] determine uniquely what changes [in the total theory] should be made" to accommodate experimental evidence [Hempel, 1988, 299].

[4]Hempel would also disagree with Kuhn's thought that the absence of such procedures or criteria renders suspect the availability of an independent normative perspective on actual scientific practice.

[5]Along these lines, we find Israel Scheffler's insistence that to establish Kuhn's claim "it [would not] be sufficient to adduce examples from the history of science of particular debates conducted at cross-purposes," since "the objective availability of clear decisions is consistent with honest differences of judgment, not to mention plain misunderstandings" [Scheffler, 1967, 80].

[6]That a shunning of analyticity is tantamount to an eschewal of a potent distinction between discovery and justification is one way of reading the moral of Quine's "Epistemology Naturalized" (Quine [1969]).

[7]See for instance [Kuhn, 1970b, vi] and [Kuhn, 1977a, xxii]. Some have suggested that Kuhn's observations about scientific practice can be used to provide a decisive vindication of Quine's rejection of analyticity. For instance, Kelly Becker holds that "Kuhn's work can be exploited to show clearly that Quine is correct to deny analyticity" [Becker, 2002, 218] (see also [Becker, 2002, 223]). But if the picture presented here is correct, the facts about science that Kuhn points to should have as little suasive force for Carnap, as regards the tenability of the analytic/synthetic distinction, as they should for the early Hempel, as regards the discovery/justification distinction.

[8]"That part of the work of philosophers," Carnap says, "which may be held to be scientific in nature—excluding the empirical questions which can be referred to empirical science—consists of logical analysis" [Smeaton, 1937, xiii].

[9]See also [Kuhn, 1970b, 207-8].

[10]For instance, George A. Reisch observes that "[Carnap's] account of what is involved in scientific revolutions is remarkably similar to Kuhn's" [Reisch, 1991, 269]. See also Salmon [1999], especially pages 347-8 and note 25.

[11]See, for instance, [Kuhn, 1970a, 262].

[12]In conditions of normal science, "the solutions that satisfy [the scientist] may not be merely personal but must instead be accepted as solutions by many" [Kuhn, 1970b, 168].

[13]Towards the end of his career, using somewhat different terminology, Kuhn continued to hold that "these criteria [accuracy, scope, etc.], whose rejection would

be irrational, are the basis for the evaluation of work done during periods of lexical stability, and they are basic also to the response mechanisms that, at times of stress, produce speciation and lexical change" [Kuhn, 1993, 338].

[14]Kuhn says that the "learned perception of similarity" plays a role in normal science [Kuhn, 1977b, 318]. A scientist might deem one solution or approach to a problem better than another because he perceives the first to be more similar to certain exemplars than the second. The question now is whether this choice (one based on an option's seeming similar to an exemplar) is a different kind of choice from those made in revolutionary science. Certainly, no exemplars are in play during scientific revolutions. But Kuhn has described choices made during scientific revolutions as the result of perceptual Gestalt switches. Are choices based on this feature of our perceptual apparatus different in kind from those made on the basis of the "primitive perception of similarity and difference" [Kuhn, 1977b, 312]? Both cases can be described as one's coming to see the world in a certain way (in the one case through an experience akin to conversion, in the other through learning and training). It is true that not all scientists will agree on choices of theory during a period of scientific revolution: some scientists will come to see the world anew, and some will not. But the same holds for choices within normal science: some will perceive one option to be more similar to a given exemplar than another is, and some will not. Perhaps within normal science there will be much more perceptual agreement than there will be at times of scientific revolutions, but this is a matter of degree that does not mark a difference in kind. More needs to be said about how Kuhn might understand Gestalt switches and perceptions of similarity, but it is unclear to me that as yet we have here the makings of a difference in kind between choices made during scientific revolutions and those made in the course of normal science.

[15]The quotation from Popper is from [Popper, 1959, 53].

[16]The quotation from Popper is from [Popper, 1979, 356].

[17]Elsewhere, Hempel is quite clear that we ought to avoid "confusion" between "the methodological norm" and "the associated socio-psychological hypothesis [...] that the scientists are committed to that norm" [Hempel, 1988, 360].

[18]The quotation from Carnap is from [Carnap, 1950, 7].

[19] Recall Carnap's principle of tolerance articulated at the very end of Carnap [1956].

[20]This essay originally appeared as Hempel [1937]; the translation into English is by Hempel. (The text actually reads: "the validity of analytic propositions is not [sic] based on the formal rules of the game"—but it is clear from the essay that this is a slip.)

[21]See also George [2011]. This matter is also central to understanding Quine [1969].

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