

1 “... I probably would never have written *Structure*”

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1.1 Introduction

In 1995, less than a year before his death, Thomas Kuhn engaged in a long autobiographical interview (RSS 253–323). Among the topics covered was some of the then new literature, specifically “Carnap and Kuhn: Arch Enemies or Close Allies” (Irzik and Grünberg 1995), showing that on a wide variety of important topics, Rudolf Carnap and Kuhn had strikingly similar views. This was part of a wider reappraisal of Carnap that had been developing for about ten years. In the interview Kuhn made an astonishing claim: “You know this article that recently appeared.¹ It’s a very good article. I have confessed to a good deal of embarrassment that I didn’t know it [the Carnap]. On the other hand, it is also the case that if I’d known about it, if I’d been into the literature at that level, I probably would never have written *Structure*.” Kuhn was fully aware that his and Carnap’s views were not exactly the same. But he obviously believed that the similarities were enormously important and went to the heart of his motivating ideas in writing *The Structure of Scientific Revolutions* (SSR) in the first place.

Kuhn must have been astonished on reading Irzik and Grünberg (1995). It is difficult to appreciate now, some sixty plus years after the publication of SSR, what a sensation it had caused in the late 1960s and early 1970s. The book has sold over a million copies and was required reading in virtually every department in most major universities. But Kuhn had gotten a lot of abuse in those early years, especially from philosophers. And some of those wounds remained quite raw. Moreover, he must have thought that he was fighting an uphill battle against the very ideas that he thought Carnap represented. Of course, the similarities that Kuhn suddenly saw between himself and Carnap diminish neither the importance nor the originality of SSR. But the dislocation

¹ Kuhn cites G. Irzik and T. Grünberg, “Carnap and Kuhn: Arch Enemies or Close Allies?,” *British Journal for the Philosophy of Science* 46 (1995): 285–307 (RSS 306).

that those similarities provoked for the then-standard narrative of the recent history of the philosophy of science and Kuhn's place in it must have been shocking. And that Kuhn more or less instantaneously accepted that dislocation shows enormous largeness of mind and generosity of spirit.

First, in Section 1.2, I will highlight some of the similarities and differences that Irzik and Grünberg discuss. My aim is not to reargue the case that they made quite well but to pick out some themes for further discussion. Kuhn's remark quoted above shows that he took some of the similarities quite seriously. As it happens, Carnap saw and took them seriously as well. Carnap and Kuhn saw differences as well. I am not concerned to say whether the similarities or the differences are more important. Instead, in Section 1.3, I shall then argue that, given the similarities, the two views could be and were attacked in similar ways. My aim is not to give a detailed history here but to indicate, in broad terms, what two of those challenges were. Finally, in Section 1.4, I shall argue that, given their differences, the two views can be allies in an even deeper sense, that is, help each other meet their respective challenges.

1.2 Similarities and Differences

Let us begin with some obvious differences. Kuhn was writing as a historian of science focused on scientific change as a social phenomenon. And what he offered was more or less a theory of punctuated equilibrium in which periods of rapid change, revolutions, alternated with other periods in which the changes are more incremental and somehow more orderly, normal science. The difference between these two sorts of episodes is that in normal science a scientific community is governed by a single paradigm, whereas revolutions begin with a weakening of that paradigm, proceed through the strife of alternative paradigms, and end by the establishment of a new paradigm and a new period of normal science. The notion of "paradigm" is thus central, and it has a number of components² that were delineated more clearly as Kuhn's career progressed. Paradigms involved examples of important scientific achievements that could guide and thus help evaluate further research in the area. These evaluations express the epistemic values of the community

² Kuhn recognized that his talk of paradigms in SSR ran together a number of considerations that are usefully separated. I will use the term "paradigm" for the collection of factors without itemizing them. As long as one such collection dominates periods of normal science, what I need to say about them here is undisturbed (SSR-2/SSR-3, 174–210; RSS).

and affect the meanings of its entire vocabulary, including those terms used in the most basic of observational reports. During normal science, paradigms and the standards or values of evaluation that they include are conveyed to each new generation of researchers by an educational strategy of textbooks that stress examples. But paradigms are not eternal, for they change in revolutions. As a consequence, there would seem to be no theory-neutral observations that could be used to compare two paradigms and choose between them. Paradigms must therefore be a different kind of commitment and evaluated differently from the more usual kind of theory. Such themes as community, education, values, and, above all, change are at the center of Kuhn's approach.

Carnap, by contrast, was a logician and philosopher of science who explored formal and artificial languages as a way of understanding both language in general and its role in science considered as a body of knowledge, that is, a body of claims taken to be true. He developed detailed accounts of the logical structure of scientific claims, gave formal theories of confirmation, and rarely invoked notions of community or values and said little about how scientific languages were learned. Logic, broadly understood, was to be at the center of philosophy, and all that could be said about rational belief and theory choice was somehow to proceed from that. The objects that such formal systems, called linguistic frameworks, seemed to be about were abstract and thus not themselves subject to change. What language we use, however, can and does change, often under pressure from changes in what we believe about the world.

Even so, Carnap's particular concern was that there are alternative logics that are not just verbal variants of one another. Since the logics embody the standards for evaluating claims, the choice among them is problematic. A search for the uniquely correct one had no standards of a theoretical sort to guide it. So Carnap rejected the idea that there was such a uniquely correct logic and treated the choice as one between alternative languages or linguistic frameworks to which the notion of correctness does not apply. Once the linguistic framework was identified, there would be sentences, the analytic ones, the truth of which would be guaranteed by the linguistic framework alone. But these sentences do not describe the world but collectively give the meanings of all the expressions of the language and determine the rules for assessing the nonanalytic, that is, synthetic, sentences. Instead of looking for the correct linguistic framework or language, Carnap took the choice as a practical choice of a tool rather than a theoretical one. There were theoretical choices to be made, namely, among the synthetic sentences considered as genuine descriptions of the independent world.

Carnap and Kuhn seem therefore to be miles apart: in what problems they take up, in their manners of treating them, in the themes they stress,

and, it may appear, in the results they get. Given these differences of approach and vocabulary, it is all the more surprising that the similarities that Irzik and Grünberg point out are so central and so extensive. But it is not surprising that the similarities went unnoticed for a long time.

Kuhn had help in missing Carnap's point. Kuhn says in the Preface to SSR: "W. V. O. Quine opened for me the philosophical puzzles of the analytic-synthetic distinction" (SSR-4 xli). But Quine viewed Carnap's talk of analyticity as an attempt to find logical certainty, which Quine rendered as our never having to give up such sentences. This is misleading at best. Quine did, however, have important objections to the analytic/synthetic distinction, to some aspects of which we will return later. Quine was not the only one of Carnap's detractors who was eager to explain what was wrong with Carnap's views. And the technical aspects of Carnap's writing make it difficult for those not versed in the details of alternative logics to see for themselves the general thrust of what Carnap was doing.

I will not try to reargue the case that Irzik and Grünberg have made, but I do want to highlight a few important similarities. Perhaps the most important of these similarities is that, for both, our scientific commitments sort into two tiers. The commitments of Tier 1 are evaluated according to something like the common picture of how theories meet experience. I will call the Tier 1 commitments *ordinary theories*. By this I mean those commitments that can change without changing the meanings of their constituent terms. For Kuhn, these are claims that are not part of the paradigm itself, and for Carnap, these claims are synthetic. For example, when we change the maps showing magnetic declination because we have remeasured and have a new value, this does not change the meanings of any of the terms involved. The commitments of Tier 2 are evaluated in some other way. For Kuhn this tier is the paradigm, and it is to be evaluated in terms of how well it guides the normal science done under its aegis. For Carnap the second tier is the linguistic framework. And this is to be evaluated in terms of our convenience in using the language thus specified, for example, in furthering the simplicity, fruitfulness, or convenience of the theories that are confirmed according to its directions. Thus, for both writers the choice among alternatives at this level is a practical one.

The similarities do not end with just having two tiers of scientific commitments. Their accounts of what Tier 2 contributes are also strikingly similar. The commitments of this second level determine the meanings of all expressions in the language and the standards of evaluation of theories at the first level. It follows that changes in the linguistic framework or paradigm involve changes of meaning, that is, conceptual

change, throughout the language, including at not only the theoretical level but also the observational level, should we choose to distinguish those. There is thus no independent, theory-neutral (paradigm- or linguistic framework-independent) observation language by which to evaluate a linguistic framework or paradigm or even to compare theories drawn from different frameworks or paradigms. This in turn precludes a development-by-accumulation picture of scientific change across revolutions or changes in framework. Moreover, the long-run changes in science are not toward some predetermined truth that could even be expressed in our current language. Instead, such changes represent the refinement of our conceptual structure for current purposes of getting on with science in the most convenient and productive way as measured by our current understanding.

The remark by Kuhn as quoted at the start of this chapter shows that he took these similarities to be very important and as going to the very heart of his motivations in writing SSR. There is reason to think that Carnap took them seriously as well. As George Reisch (1991) pointed out, Carnap was the editor for SSR, and in this capacity, wrote Kuhn two revealing letters. In the first Carnap is commenting on some manuscripts that Kuhn had sent in preparation for writing SSR. Carnap says in part:

I am myself very much interested in the problems that you intend to deal with, even though my knowledge of the history of science is rather fragmentary. Among many other items I liked your emphasis on the new conceptual frameworks which are proposed in revolutions in science, and, on their basis, the posing of new questions, not only the answers to old problems. (Carnap 1960, quoted in Reisch 1991, 266)

In the second letter Carnap officially accepts SSR for publication but includes a long paragraph on what he likes about the book. It is worth quoting in full.

I am convinced that your ideas will be very stimulating for all those who are interested in the nature of scientific theories and especially the causes and forms of their changes. I found very illuminating the parallel you draw with Darwinian evolution: just as Darwin gave up the earlier idea that evolution was directed toward a predetermined goal, men as the perfect organism, and saw it as a process of improvement by natural selection, you emphasize that the development of theories is not directed toward the perfect true theory, but is a process of an improvement of an instrument. In my own work on inductive logic in recent years I have come to a similar idea: that my work and that of a few friends in the step for step solution of problems should not be regarded as leading to “the ideal system,” but rather as a step for step improvement of an instrument. Before I read your manuscript I would not have put it in just those words. But your formulations and clarifications by examples and also your analogy with Darwin’s theory helped me to see clearer what I had in mind. (Carnap 1962, quoted in *ibid.*, 266–267)

Interestingly, Kuhn says in the autobiographical interview, “I would now argue very strongly that the Darwinian metaphor at the end of the book is right, and should have been taken more seriously than it was; and *nobody* took it seriously. People passed it right by” (RSS 307).

Well, Carnap took it seriously, and he thought that the similarities between his own and Kuhn’s work were serious indeed. Of course, there is more to it than that. From “On Protocol Sentences” (1932/1987) onward Carnap held that the meanings of the most basic evidential reports changed as the language changed. In *The Logical Syntax of Language* (1934/1937) he held that (what we take to be) basic theoretical laws were best understood as P-rules and hence as among the determiners of meaning throughout the language. This continued right on through the end of his life. When challenged to provide the analytic sentences that would endow novel theoretical terms their meanings, Carnap suggested what is now sometimes called the “Carnap sentence” for the theory. The technical details do not matter here, but the upshot is that the statement of the theory itself is not analytic but is involved in the statement of the relevant analytic sentences in such a way that when the theory changes, so does the (analytic) Carnap sentence. This means that when basic theory changes, so do all meanings of the expressions of the language, whether those expressions are at the theoretical or observational level.

There is one of Kuhn’s themes, values, that it might seem at first that Carnap would resist. The values in question are epistemic ones governing what puzzles are most worth solving and what counts as a better solution or even an adequate one. Carnap, by contrast, says little about values in the early part of his career, and what he does say sounds negative: ethics is a branch of metaphysics and hence without cognitive content (cf. Carnap 1935). Set aside the issue of whether for Carnap there are other kinds of content – there are. Since Carnap is known to have strongly held moral and political views, his comments on this score can hardly amount to a rejection of ethics or evaluative topics tout court. In fact, what Carnap is really rejecting is the idea that there is a uniquely correct evaluative system and especially the idea that philosophers have a special nonempirical means of identifying that uniquely correct system of values. This, then, is just the same sort of pluralism that Carnap readily applied to logic combined with a denial that philosophers have some insight into a reality that is deeper than or lies behind what empirical scientists can know.

In his logical writings Carnap also resisted formulating logic as an account of how people *ought* to reason. This may seem to be a rejection of the evaluative. But this was because he thought the formulation in

terms of how people ought to reason did not add anything and hence was equivalent to his own usual formulation. So Carnap is not really rejecting those apparently evaluative formulations as wrong.

In this section we have highlighted several similarities and differences between Carnap and Kuhn. Both are important and were recognized as such by the two writers. As we shall see in the next section some of the similarities suggest that the same or similar criticisms can be lodged against both views. As we shall also see in the final section, some of the apparent differences can help each to be a deeper ally to the other by helping him to address the criticisms presented in the next section.

1.3 Two Criticisms

In the section that follows I shall consider two broad families of criticism that have been lodged against the views we have been considering. I will avoid here the detailed histories of specific texts. This is because there are many minor variations of the complaints and neither the criticisms nor the responses that can be made depend on the details.

The first criticism has been lodged more often and more vocally against Kuhn, so I will frame it that way first. But because the criticism depends only on features that Kuhn and Carnap share, it would apply equally well to the latter. The criticism comes in two parts:

Criticism 1.a: If paradigms are the bearers of the standards of rationality, of the standards of theory choice, then those standards cannot be invoked in that transition. Therefore, paradigm change cannot be rational. Moreover, we can never have any basis for saying that an ordinary theory from one paradigm is rationally better than another ordinary theory from another paradigm. (As indicated earlier, an ordinary theory is just a commitment that can change without affecting the meanings of its constituent terms.)

Admittedly, Kuhn’s talk that in paradigm change “the world has changed” and that the process is one of “conversion” seems to suggest just this outcome. But it does not have to. Those could be just phenomenological descriptions of what it feels like at the time of paradigm transition.

Criticism 1.b: Without neutral observational claims there is no hope of giving an objective answer to the question of whether a proposed new paradigm makes significant, or indeed any, empirical progress over its rivals. Since paradigms do not meet experience in anything like the traditional ways that ordinary theories are thought to, what else are we to think?

Kuhn speaks of observation as “theory bound,” and some have interpreted this to mean that theories in general simply generate data that

uniformly supports that theory. This, obviously, would not be a good thing. But presumably it is not what Kuhn had in mind. Instead, his idea is that paradigms (and not just any claims at all) will affect the meanings of observational claims. So I have framed Criticism 1.b in terms of whether a paradigm-induced change in the meanings of claims at the observational level precludes a reasonable assessment of the paradigms themselves. Kuhn does say that the new paradigm will offer striking new empirical successes and be chosen on that basis. But again, one does not have to say this. But one does have to say what makes one paradigm better than another.

I have framed this criticism as though it were directed toward Kuhn rather than against both him and Carnap. And indeed, Kuhn has more often been charged with irrationality. Perhaps this is because he emphasized more explicitly that observation is not paradigm neutral and possibly because his writing style is more dramatic. He is, after all, talking about revolutions, and his audience, especially in the 1960s and 1970s, might be tempted to view him as a radical. In some ways he was. Nonetheless, the features of Kuhn's view that the above criticism responds to are shared by Carnap. So the criticism, if it is valid, ought to be equally valid against Carnap. By the 1960s he was a grandfatherly figure who came across as ever the careful logician with a writing style that is measured and even ponderous. Carnap was a radical too, but people did not see it.

I shall argue in the third section that Carnap stresses the themes and distinctions that allow him to respond to this objection. And there is no reason why that machinery could not be mobilized by Kuhn as well.

The second criticism has been lodged more forcefully and more persistently against Carnap. This criticism is directed at Carnap's two-tier account of our scientific commitments. The linguistic framework, or language, by itself guarantees that certain sentences are true. Those are the analytic sentences. To abandon one or more of those sentences would be to change languages. The remaining sentences, the synthetic ones, are given their meaning by the linguistic framework, but their truth is to be adjudicated, according to the rules laid down by the framework, by comparing them with observational judgments. As is well known, W. V. Quine has challenged Carnap on this, saying that the distinction between analytic and synthetic sentences – that is, between the commitments of the linguistic framework and other more ordinary commitments – simply cannot be drawn. The argument is roughly this:

Criticism 2: Any two-tier account must have a clear way to distinguish the tiers and to apply that distinction empirically to natural languages and to real

languages used in science. Such an empirical distinction would amount in Carnap's case to clear behavioral criteria that would mark the difference between analytic and synthetic sentences. According to Quine, such behavioral criteria have not been found and thus probably cannot be. If so, a two-tier account must be defective.

Carnap would like to sidestep Quine's demand for empirical/behavioral criteria (Quine 1951) on the grounds that he is not concerned with natural languages but considering only abstract proposals for constructing the language of science in a certain way. Quine can argue, however, that unless a clear behavioral difference can be specified there can be no way to tell whether one of Carnap's proposals has been adopted, and hence, there is no clear sense to 'making a proposal' (cf. Creath 2004 and Creath 2007). Moreover, without such criteria there would be no way to compare proposals for practical utility. Thus, it seems that Carnap cannot dodge Quine's demand. The question is whether a demand such as Quine's can be met even in principle. Carnap would not have to show that current scientific languages or current natural languages have the structure he proposes, only that we can tell empirically whether they have it or not.

It may not be obvious, but Kuhn faces a version of this objection as well. We saw above that he does distinguish two tiers of scientific commitments: paradigms and more ordinary theories. And he uses that distinction to formulate an account of the development of advanced sciences according to which a field oscillates between periods dominated by a single paradigm, normal science, and other periods, revolutions, in which no one paradigm is dominant or in which the dominant paradigm is replaced by another.

Kuhn's critics have often wanted to deny, say, that any period is governed by a single paradigm. The problem is not that Kuhn may be wrong and his critics right. Rather, the worry in this second criticism is that without a clear empirical criterion of what it is to be a paradigm and what it is for work to be done under the aegis of a paradigm, there is nothing for Kuhn or his critics to be right or wrong about. If Kuhn does have such a criterion, then it should be a fairly straightforward empirical matter to sort this out.

What I shall argue in the next section of this chapter is that the themes that Kuhn stresses do allow for a criterion that can turn Kuhn's model into an empirically decidable historical claim. I am not claiming that Kuhn has given an explicitly formalized and idealized statement of these behavioral criteria or that he should. Rather, it is that his themes and practices suggest that this *can be done* to the required degree of specificity. They suggest, moreover, where to find them. I shall suggest in the next

section that these same Kuhnian themes and practices will show that the demands that Quine raises against the analytic/synthetic distinction can be met as well.

1.4 **Allies in Meeting the Criticisms**

In this section I want to reflect on the criticisms just outlined and consider the prospects for avoiding them. I shall argue that, while much remains to be done, there is reason to think that the criticisms can be deflected and that it is the dissimilarities in their views and approaches that will allow Carnap and Kuhn to help each other do this.

Criticism 1.a is lodged primarily at Kuhn, though it can readily be transformed into a criticism directed at Carnap. Roughly, the question is: If paradigms provide the standards of rational theory choice in science, how can they be appealed to when the choice is between competing paradigms? Such choices must be irrational.

We said that Carnap's background is different from Kuhn's. Carnap had a long and deep involvement with and developed detailed accounts of logic, language, and meaning. From the 1930s on, he treated philosophical "theories" that seem to be about the world, for example, 'There are numbers' and 'Theoretical entities are real,' as better understood as proposals for structuring the language of science. Because he is a pluralist even about logic, he has explored not only the details of alternative logics and conceptual systems but also what kinds of reasons are available for choosing among them. Logics are to be construed as languages, and languages are not true or false, correct or incorrect. Some may be easier to use or allow for simpler descriptions of the world. The choice is a practical rather than a theoretical one. Carnap explored, as well, the options we have for fundamental concepts and how we might choose among them. Shortly, we will discuss some specific examples, namely, choices among time metrics and among alternative logics.

In any case, this long experience exploring logic, language, and meaning puts Carnap in a good position to respond to Criticism 1.a. Carnap did not tend to use the phrase 'standards of rational theory choice,' but it is plain that he would consider them as embedded in the inference and confirmation rules that constitute languages. So the choice among such standards, like a choice among languages, is a practical rather than a theoretical choice; it is a choice among tools, as he suggested in the second letter to Kuhn quoted above. So what do we need to know to make that choice? We need to know what the tools are like and what the effects of using them would be. This is a straightforward empirical question (insofar as any such questions are straightforward), and Kuhn

would treat it as such. And we would have to evaluate those outcomes to decide which we would prefer. There is absolutely no problem, absolutely no question begging, in using our current standards of rational theory choice in these deliberations. Besides, there is no danger of choosing falsely and no contradiction if you choose differently than I. Of course, this does not eliminate the possibility of bias and error in scientific deliberations. It is not intended to. But it does show that one can use one's current standards of rational theory choice in these deliberations and on that basis quite possibly decide to change those standards.

Carnap gives many examples. One is in the choice of basic units of temporal length. One needs for this a periodic process that one then declares by definition to measure out units of equal length. There is a great deal of freedom in this. I could choose my own heartbeats, and by this definition they would be of equal length. This would have the undesirable practical consequence that when I am resting, the world speeds up, and when I drink strong coffee that world slows down. And that would then have to be built into temporal laws of nature, making them complex indeed. There is no contradiction here, just an unwise practical choice. Or I could choose the ticks of a mechanical clock as my definition of equal temporal length. This would be better, but the world would still speed up as the wheels and cogs wore down. Suppose this were my current choice. I could still deliberate about whether to use this system and decide to change it by adopting the oscillations of a cesium atom as my unit of choice (Carnap 1966, 78–85).

The second example is that of a choice between our now standard elementary logic and a more limited one in which our current quantifiers cannot be expressed. In the former we can express classical mathematics, and that is very convenient for expressing powerful physical theories. The more limited logic cannot do this and so is less convenient. But our standard logic is more likely to be inconsistent than its weaker sibling. As a result, we have to choose between one logic that is convenient and another that is safer. The choice you make will depend on what your values are. Criticism 1.a provides no argument against the rationality of such a choice.

These Carnapian considerations generalize to a defense of Kuhn against this criticism as well. Carnap's very different starting point allows him to be Kuhn's protector as well as his own.

Carnap's careful discussions of languages and meaning are also helpful in dealing with Criticism 1.b. The issue is this: Kuhn's claim is that changes of paradigm induce changes throughout the language, including at the observational level. This implies that observation is not utterly theory neutral. Does this preclude the possibility of a new paradigm

being a dramatic empirical success in any interesting sense or even an ordinary theory being tested observationally? Does it render them untestable? No.

Of course, it is possible to invent a nontestable theory, one that no claims at the observational level could ever contradict or even disconfirm. But such theories would be empty, telling us nothing about the world around us. But having observational claims that do not change their meanings as high-level commitments change will not prevent that. It is true, of course, that people sometimes see what they want to see or fail to notice something that threatens a cherished belief or fail to recognize its relevance even when they do notice. Again, meaning change has little or nothing to do with this.

The mere fact that observational claims change their meanings with a new paradigm does not say what those changes are or whether those changes have the consequences that many of Kuhn's critics seemed to fear, namely, that, once the meanings of observational claims can change, those observational claims will always agree with the theory that they are used to test.

The way to defend Kuhn here against such fears is to spell out *how* changes at one level induce changes at another. This is precisely what Carnap's delineation of the operative inference rules is designed to do. The inference rules spell out what is logically and evidentially relevant to what is within the body of scientific claims. And by doing so those rules spell out what the public norms are for challenging or defending those scientific claims. We will return to this idea of public norms shortly.

When we use the explicit detailed inference rules to examine specific examples, the fears of the critic have not been realized. The historical evidence is that meaning change has not held theories immune from disconfirmation. And Carnap's abstract examples of changing definitions of scientific terms has not had that result either. If the critic is to make a serious case along the lines of Criticism 1.b, that critic will have to show that both the historical and abstract cases that Kuhn and Carnap discuss are exceptions rather than the general rule. So far, no such general argument has been forthcoming, and it is doubtful that it ever could be given.

For Kuhn much of the virtue of a given paradigm is indirect. It consists in its ability to guide research and successful puzzle-solving. But this does not change the situation regarding Criticism 1.b.

Let us then turn to the second criticism. This is lodged primarily at Carnap and stems most famously from Quine. Carnap had distinguished analytic from synthetic sentences, and drawing this distinction was essential for giving a special status to linguistic frameworks and for talking in a

precise way about meaning, synonymy, logical implication, and the like. Quine challenged Carnap to provide behavioral criteria that would allow the distinction to be applied to natural or even actual scientific languages. This is not a problem just for Carnap, as it would seem that if it is essential to give special status to linguistic frameworks, it would be equally necessary to do that for paradigms.

Here I think that the themes and historical practices that Kuhn has stressed can be useful in deflecting Criticism 2 from himself but also from Carnap. Kuhn can thus be an important ally to Carnap. There is no intent on my part to state a precise set of behavioral criteria or to find in Kuhn's writings any such precise statement. But I do think that Kuhn's writings help us understand the empirical factors involved and where the relevant empirical data is to be found. This is not entirely surprising, for history is an empirical discipline.

The Kuhnian themes that I want to highlight are *education*, *community*, and *values*. Education, by which I mean in this context scientific education, is for Kuhn the process by which the paradigm is conveyed from one generation to the next. Kuhn's discussion of education in SSR is designed to explain why paradigms are largely invisible. His explanation, using a Wittgensteinian distinction, argues that paradigms are not stated but shown in examples. The textbooks give the examples that embody the paradigm. And then at the end of each chapter are problem sets where the student is expected to apply the paradigm in ever more complicated ways. At no point along the way is it necessary to explicitly state the paradigm. We learn our native languages initially without being given explicit rules. There is no reason to think that learning scientific languages could not proceed in the same way.

I have no wish to deny any of what I have just recounted. But I would add what I take to be a Kuhn-friendly addendum. In the cases of both textbook education and learning our native language there is more going on than just the structured series of examples with the hope that the student catches on. In response to the problem sets, the students try out answers, and those answers are evaluated and graded. Often those grades are accompanied by comments about what went wrong. In a natural language we also have more than examples to go on. We try to use the language for ourselves. And our attempts are rewarded with approval or else suppressed in some way. Of course, for any finite number of corrected examples it is logically possible to carry on in infinitely many different ways. While this is logically possible, after a remarkably short number of trials different students from different backgrounds will carry on in largely the same way. As social creatures we are remarkably good at recognizing the public norms and conforming to them. In this way each

paradigm serves as a set of public norms, as a *community wide set of values* about how to proceed in science, what the important problems are, how to evaluate scientific claims, and how one's own claims are properly evaluated. Corresponding statements can be made about the community-wide sets of values or norms that constitute a natural language. These public norms are empirically accessible. They have to be in order to be learned. Norms can and do change, and those changes in norms are empirically accessible as well. Spelling and grammar are usually in flux, but the norms are evident in every proofreading or copy-editing. That there can be disagreements about what the rules are is evidence for rather than against the idea that there are such rules.

Norms are not facts about universal behavior that everyone speaks or thinks in a certain way. They are, rather, second-order facts about how in a given community various kinds of behavior are widely evaluated. And as with most social phenomena, community-wide norms are likely to be vague. They might be made more precise and even codified as in the law. But their vagueness does not imply that there are not such norms, or that the norms have no structure, or that they are empirically inaccessible. Such norms can be conveyed by explicit instruction. But no doubt they are often conveyed by seeing examples and learning what gets praised or scorned.

Where would we find evidence of community-wide values or norms? In the scientific case they can be found in the process of education and grading, in referee reports, in grant applications, in promotion letters, in review articles, and in the journal articles themselves. Every scientific writer puts down on paper what they expect the reader to accept as evidence and argument. Many such papers begin with a brief literature review in which specific work is picked out for praise or blame. All this evidence is in the historical record. Sometimes the current norms and values are more available to historians. But they are also available to us as scientists in real time because we have had that education, had our work reviewed, and written our own evaluations, which often themselves will be reviewed. I won't say that all historians of science try to reconstruct the community-wide norms of some former time or try to assess whether an earlier writer meant the same as we might mean by a given term. But Kuhn did. He aimed both (1) to reconstruct community-wide norms of former times and (2) to assess whether earlier writers (scientists) meant the same as we might by a given term. And it is that practice that gives reason to think that the various distinctions he needs are sufficiently clear and evidence based. Nothing in the issue at hand requires us to say that Kuhn was right in all his historical judgments. The issue is whether there is something to be right or wrong about on the basis of the empirical evidence. Clearly, there is.

Criticism 2 would have it that a distinction between the two tiers of the sort of accounts that Carnap and Kuhn give is unintelligible because there are no empirical criteria for drawing that distinction. I do not claim that Kuhn has explicitly stated any such criteria. He doesn't need to. But it is clear that there is an abundance of empirical evidence that is relevant to the kind of distinction he wants to draw between a paradigm and the other scientific commitments we make. His various themes and his historical practice point us in the direction of that evidence and reassure us that criteria can be found that will allow that practice to go forward.

Much the same can be said for Carnap. He is not a historian or sociologist. Nor does he need to be. What he does need is for the historian or sociologist to be able to determine empirically what the community-wide norms of evidence gathering, argumentation, and theory evaluation are in a particular time and place. That Kuhn can do what he does and that his themes and practices indicate where the appropriate empirical evidence is to be found is sufficient reassurance that there are in principle the behavioral criteria to apply Carnap's distinctions to natural and scientific languages. In this way Kuhn can be a particularly close ally to Carnap.

In Section 1.2 we highlighted various similarities and differences between Carnap and Kuhn. Among the similarities was a two-tiered system of scientific commitments. Even the characters of the two tiers were analogous. Importantly, the commitments of the broader tier could change, and such changes brought with them changes in meaning throughout the language, including at the observational level. This meant that there could be no fully development-by-accumulation picture of scientific development and no utterly theory-neutral observational basis for our theories. There were important differences too, especially in background. Kuhn was a historian who wanted to say how science did in fact develop. Carnap was a logician and philosopher of language whose abstract structures were more akin to mathematics than to history or sociology or even to empirical linguistics.

In Section 1.3 we explored some of the similarities and found that this meant that the same or similar objections could be raised against the two accounts. Criticism 1.a argued that changes from one set of commitments at the broader tier to another such set must be irrational. And Criticism 1.b argued that the lack of completely theory-neutral observational claims precluded rational theory choice even for ordinary theories. Criticism 2 argued that even drawing the distinction between the two tiers was likely to be impossible because of the lack of empirical/behavioral criteria for doing so.

In Section 1.4 we argued that the two criticisms above could be met and that it was the very difference between Carnap and Kuhn that allowed each to help the other to do so.

I do not know how much of this Irzik and Grünberg had in mind when they wrote “Carnap and Kuhn: Arch Enemies or Close Allies,” but they certainly saw the parallel between Carnap and Kuhn. That insight began with the then recent historical scholarship revealing the underappreciated complexity and depth of Carnap’s thought – indeed, its radical character (cf. Friedman 1987; Creath 1990; and Richardson 1998). Kuhn was a radical too and often in ways like Carnap. That they should have reached so nearly the same conclusions from such different points of departure is perhaps the second most surprising result of all. What is the most surprising? Surely, it is that their real differences in starting points allow each to help the other where they have often been thought to be most vulnerable.