

# On Wanting to Say, “All We Need Is a Paradigm.”

By Rupert Read

Can the *philosophy* of the social sciences do what it most centrally wants to do, namely, to specify how the social sciences ought to work? What normative recommendations, if any, can philosophers and methodologists actually make or state (coherently and groundedly) on the question of how to correctly *constitute* a social *science*? The great Cambridge (Harvard and MIT) philosopher, Thomas Kuhn, is widely thought to have offered or implied very particular answers to these questions—answers that have been taken up with enthusiasm by some social scientists and regarded with dismay by some philosophers. If we want to know more, the most profitable way to proceed initially may be by looking at those who have attempted to employ or ‘oppose’ Kuhn in this regard.

## The question of social scientists’ appropriation of Kuhn

WHEN ONE STARTS TO LOOK CLOSELY AT KUHN IN RELATION TO THE SOCIAL SCIENCES, a question—indeed a *conundrum*—strikes one. For, Kuhn is, without much doubt, the most apparently influential post-Popperian philosopher of the social sciences. And yet he wrote almost *nothing* directly on the topic. How come?

Kuhn has been enthusiastically taken up by some post-modernists, some apologists for the social sciences, and some major social theorists. They’ve read him as systematically licensing the thought that if only social scientists were able to agree on a ‘paradigm’ within which to focus their research, then that would be enough—the social sciences would genuinely *be* sciences.

Meanwhile, Kuhn has been viewed with alarm by Popper, Feyerabend, and mainstream Realist philosophers of science for just the same reason: he apparently legitimizes the pretensions of the social sciences, when what should be happening (according to these critics) is the remorseless exposure of the social sciences’ non-scientific status unless they adopt an acceptable methodology (such as, for the Popperians, Falsificationism).

The conundrum here is sharpened when we note that Kuhn himself wondered with irritation what all the fuss was about. He evidently felt that both his ‘fans’ and his ‘foes’ had gotten him wrong in supposing his views to have these drastic implications for the social sciences. I want to explore here the possibility that he might have been right about his own work—the possibility, that is, that observers of science tend to have the wrong image of (what Kuhn implied about) the scientificity of the non-natural sciences.

This standard but, I think, *wrong* image of Kuhn is, in outline, as follows:

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The way to scientificity is the *establishment* of the dominance of a *paradigm* in one's discipline. One needs to professionalize one's discipline by focusing on the doctrines of one of its schools, and that's all. The pro-Kuhn camp takes the message of this to be: "We just need a paradigm—if we enforce agreement on a paradigm, then we can be a proper science. There's nothing more to being a proper science than that." The anti-Kuhn camp takes the message to be: "Kuhn legitimates relativism and mob rule, because any group of people can 'get' a paradigm in that sense—the genuine sciences will not be able to distinguish themselves from pseudo-sciences if Kuhn's ideas are accepted."

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But has either camp caught on to what Kuhn was actually saying? What in fact *is* the nature and force of his 'paradigm' idea?

### **An approach to this question via the Feyerabendian critique of Kuhn**

IT HAS RECENTLY BEGUN TO BE NOTED THAT ALL OF THIS MAY INVOLVE A HUGE MISCONSTRUAL OF Kuhn.<sup>1</sup> But those who have so suggested, such as Fuller and Hollinger, have tended to just reverse the image of Kuhn that they find in his conventional foes and fans: Rather than finding Kuhn to be a quasi-political apologist for social science, they find him to be a quasi-political apologist for *natural* science.

I wish to see if there is a reading of Kuhn available on which he is neither of these but is simply a philosopher of science trying to put the history and sociology of science on a secure footing, trying to free it from the fetters of particular normative methodologies or political agendas. I can think of no better way of beginning this task in earnest than by looking at the strong words said to Kuhn by someone often thought to be one of his allies: Paul Feyerabend.

Here is Feyerabend, in a letter to Kuhn that sounds unmistakable notes of warning and of negativity:

I more than ever think your essay [*Structure*] is quite unique as regards the contribution it makes both to the history and to the philosophy of science. On the other hand my impression of danger, and my misgivings[,] have been very much increased. What you are writing is not just history. It is

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*ideology covered up as history.* Now please, do not misunderstand me.... [I do not] pretend that in history a nice distinction can be drawn between what is regarded as a factual report, and what is regarded as an interpretation according to some point of view. But points of view *can* be made explicit.... Nobody will think that the history of crime justifies crime, or shows that crime possesses an inherent 'reason' or an inherent morality of its own. In the case of the sciences or of other disciplines [for] which we have respect the situation is much more difficult and the distinction cannot be drawn with equal ease. But in these cases it is of paramount importance *to make the reader realize that it still exists.* You have not done so. Quite on the contrary, you use a kind of double-talk where every assertion may be read in two ways, as the report of a historical fact, and as a methodological rule.<sup>2</sup> You thereby take your readers in.... I do not object to your [having the] belief that once a paradigm has been found a scientist should not waste his time looking for alternatives but try working it out.... What I do object to most emphatically is the way you present this belief of yours; you present it not as a *demand*, but as something that is an obvious consequence of historical facts. Or rather, you do not even talk about this belief, you let it as it were emerge from history as if history could tell you anything about the way you *should* run science.<sup>3</sup>

And here is a more indirect but nevertheless significantly anti-Kuhnian remark, also from the early 60s:

It is very important nowadays to defend...a normative interpretation of scientific method...even if actual scientific practice should proceed along completely different lines. It is important because many contemporary philosophers of science seem to see their task in a very different light. For them actual scientific practice is the material from which they start, and a methodology is considered reasonable only to the extent to which it mirrors such practice.<sup>4</sup>

For "actual scientific practice," one can (I think) read Kuhn's term 'normal science' without much disturbing the sense.

Now I wish to consider in some detail whether the kind of critique being made here *actually and fully* applies to Kuhn, as an effective means of considering the more general question of whether the widespread—indeed almost *standard*—reading of Kuhn, as meaning to legitimate or at least as in fact legitimating (whether he explicitly meant to or not) the general recipe of the establishment of dominance of one paradigm as the road to scientificity, is itself a legitimate reading (of Kuhn). That is, I will endeavor to establish what Kuhn's text actually entails and supports—what it actually *can* and *wishes to* support—and I will distinguish this from the kinds of misreadings and over-dramatizations that Kuhn's *followers* (for example, social scientists wanting to be able to say, "We just need a paradigm!") and *foes* (for example, those who have worried that Kuhn is depicting science as saturated with power, as 'mob rule' or at best 'elite or institutional rule') alike have tended to impose upon him (albeit sometimes with Kuhn's unwitting help).

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## Are Feyerabend's criticisms valid?

SO THEN, *WHAT OF FEYERABEND'S CLAIMS HERE?* TO EVALUATE THEM WE WILL NEED to look in some detail at the relevant sections of Kuhn's work. Those are Kuhn's discussions of the 'road', if there is one, to normal science and in particular his comments about where on that road the social sciences might be.

Kuhn writes of 'pre-paradigmatic' sciences and of the emergence of a paradigm through the establishment of a dominance by one of the schools of thought that exist in 'pre-paradigmatic' disciplines. This can sound awfully similar to what Kuhn's followers and foes (mentioned above) find in his text. But does what Kuhn writes about this in fact imply a prescription for what social scientists ought to do? Let us focus on one of Kuhn's rare direct remarks on the social sciences in this context:

In parts of biology—the study of heredity, for example—the first universally received paradigms are [really quite] recent; and it remains an open question what parts of social science have yet acquired such paradigms at all. History suggests that the road to a firm research consensus is extraordinarily arduous.<sup>5</sup>

It is worth paying quite close attention to this quotation. Does it imply that social sciences must be on the road to a research consensus if they are to be doing anything worthwhile? Does it license the thought, "obvious" to positivists and also attractive to all who look to put their social science on a "secure" or "scientific" footing, that what is really required is a paradigm to bring the social science in question together "under one roof," to put it firmly on "the route to normal science"?

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*Kuhn describes for us the structure of normal science and of scientific revolutions—that is to say, in those disciplines that do in fact fit the description, in disciplines that "find themselves" with paradigms. He lays down no advice or prognostication for disciplines without a paradigm.*

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The use of the word "yet" might imply such a teleological vision. And likewise the term that Kuhn uses elsewhere, 'pre-paradigmatic'. But just because certain disciplines have become sciences surely cannot imply that *all* will. For example, here is one possibility: The social sciences will eventually come to appear to most of us as astrology appears to most of us now: as a pathetic attempt to ape science, failing because of its failure to have a genuine tradition of research, a genuine actionable set of problems and puzzles.<sup>6</sup> Here is another possibility: The social sciences might stay in what Kuhn describes (again, perhaps misleadingly) as the "early fact-gathering" stage. That is, they might remain "disciplines without a paradigm" (a phrase deliberately lacking the imaginable teleological consequences of 'pre-paradigmatic').

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Here is a third possibility: The social sciences might be best understood as already in some degree operating in a manner that is ill-captured by the formula of ‘fact-gathering’, but not in a manner akin to that of a science—for example, perhaps they have a systematically ‘hermeneutic’ structure.

I think there is reason to think that all of these three possibilities have *something* to be said for them. But the immediate point is this: I hope it will not be taken as a redundant vacuity if I remark that Kuhn describes for us the structure of normal science and of scientific revolutions—that is to say, *in those disciplines that do in fact fit the description*, in disciplines that “find themselves” with paradigms. He lays down no advice or prognostication for disciplines without a paradigm. There is *nowhere* in Kuhn—not in the quote given above, nor anywhere else—a *claim* that one can confidently predict that in a discipline with schools, the eventual victory of one school can be confidently predicted. I do not mean simply the victory of one particular named school. I mean the victory of any school, ever. Kuhn’s claim concerning the emergence of paradigms is purely a *retrospective* claim. *He is talking about the structure of the emergence of those disciplines that have become sciences*—not providing a manual for the creation of new sciences. He is, at least by implication and omission, pretty clear that that there can never be a guarantee that a discipline without a paradigm will acquire one and thus that there is no sense in which it can be obvious and perspicuous that, for example, it is right to conceive the social sciences as being on the road to normal science.

For a school to be victorious, it is perhaps necessary for certain types of institutions to be constructed—and it is certainly necessary after its victory. But this does not imply that it is a good idea to construct such institutions at any particular time nor that the construction of some will ever be *enough*. One needs to have sets of agreed-upon exemplars, common methods or ways of acting, and an absence of ongoing foundational disputes. There are strict limits to the extent to which any of these can be imposed upon others in a discipline unwilling to be imposed upon. One can try to suppress foundational disputes—for example, through hegemony in a professional association or in educational institutes in a discipline—but this is liable to be to some extent self-defeating, especially in any climate valuing academic freedom. Whichever way you cut the cake, it looks like scientificity just isn’t in the institutions (alone). And the struggle of schools for hegemony—the attempt to turn a school prematurely into a paradigm—can be the most self-defeating move of all.

### An example

THIS LAST POINT CAN BE NICELY ILLUSTRATED BY MEANS OF AN EXAMPLE FROM MARY Midgeley. She discusses the attempts to transform psychiatry, for example, into a scientific discipline, with normal problems, an agreed research agenda, and so forth. Now, if someone who had read Kuhn had come up with the popular thought that what she had to do would be to try to make the methods of her own school triumph as ‘*the scientific method*’ in psychiatry, she would presumably wage an essentially political campaign to demonstrate and enforce this, replete with accounts of how the beliefs of her school alone could enable psychiatry to understand itself, refine its methods, get lots of government money, be respectable within the medical community, and so on. The question is, would this be likely to be a productive thing to do? Would it be likely to hasten the advent of a truly scientific psychiatry?

Midgeley addresses these thoughts by means of querying in particular

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whether Materialist schools of psychiatric thought are pursuing their efforts to scientize their discipline in a way that ultimately makes much sense:

[T]he reduction of mind to body is now seen as a major factor in determining diagnoses and methods of treatment. As two concerned practitioners in this field have put it,

“Despite the ambiguity and complexity of psychiatry, it is striking that many students begin its study with the appearance of having solved its greatest mysteries. *They declare themselves champions of the mind or defenders of the brain....* The unfortunate result is that many of them become partisans—and needless casualties—in denominational conflicts that have gone on for generations and that they scarcely understand.” [Italics Midgeley’s] As the authors point out, this metaphysical issue cannot be ignored.... It is “more than a question of taste whether we think about schizophrenia as a clinical syndrome...as a set of maladaptive behaviors, a cluster of bad habits that must be unlearned, or as an ‘alternative life style’, the understandable response of a sensitive person to an ‘insane’ family or culture.

Each of these proposals makes different assumptions about the phenomenal world and its disorders, and each has different consequences for psychiatric practice and research.... The result of ignoring the fundamental differences between perspectives is not to diminish sectarianism but, in the end, to encourage it.”

Their quite long list...of possible ways in which schizophrenia might be seen shows plainly what tends to be wrong with reductive, exclusive approaches to large-scale problems. All these suggestions seem clearly worth taking seriously. One might reasonably expect that even the wildest of them might play some part in a proper understanding of this very obscure complaint. It seems reasonable to suggest that they would best be seen as viewpoints belonging to investigators encamped round the mountain of mental trouble. *Yet the temptation to choose one and to take sides is extremely strong for a profession that feels the ‘scientific’ imperative compelling it to choose only one approach.*’

Thus Midgeley is claiming that this idea is part of the *problem*, not part of the solution. The effort to make one’s discipline scientific may well encourage sectarianism rather than diminish it—and to no productive end. One might go so far as to say that in the human sciences, ironically, schools have tended to emerge and to firm themselves up—thus preempting perhaps the possibility of a unified account of their subject-matter sooner rather than later—precisely from and as a result of an effort to reach a point at which they could become ‘normal sciences’, in Kuhn’s term. That’s bad—even shocking—news, if it’s true. It would take us too far afield to consider the point further in detail, but the reader might think of the current state of literary theory or of sociological theory as potential illustrations. Doesn’t the advent of ‘theory’ imply a would-be *telos* of dominance for one or another theoretical school? And does this actually help at all in constituting the discipline of, for example, sociology as a science? Or does it rather stand in its way?

Now, I take this point that I have expanded upon from Midgeley to be *deeply consonant* with Kuhn’s analysis. The attempt to *force* a victory of one school by forcing the establishment of some dominant exemplars and thus the imposition

of a disciplinary matrix for the first time will normally result, not in a surer road to science, but in a surer continuation of the reign of 'schools'. That is, dogmatic efforts to transform a discipline into a science will normally have *just the opposite result* from what is intended. One will instead foster ongoing hostility, foundational disputes, and so forth.

A discipline will only become a science à la Kuhn if that is, as it were, its fate. And there is simply no telling in advance of the facts, in advance of the success of a new exemplar or some such, whether any particular discipline is destined to go down that road. After all, doesn't this make sense? After all, if we knew where economics or literary criticism or what-have-you were going, wouldn't we all be there already?

To paraphrase the sometime remark of a famous jazz musician: If we knew where 'pre-paradigms' were going, we'd be there already. This is surely what the sensible Kuhnian must say to the overzealous social studies-ists (or whoever) who search out a recipe in *Structure* for putting their conflicted discipline onto the true path of a science.<sup>8</sup>

### **Further assessment of Feyerabend's criticisms and consideration of certain other remarks by Feyerabend on Kuhn**

THUS WE MUST CONCLUDE PROVISIONALLY THAT FEYERABEND—A 'FOE' OF KUHN ON normal science and by extension on social science—has, like many others (including many apologists for social science, who hope for precisely what Feyerabend and the Critical Rationalists fear), importantly misread Kuhn on this point: on Kuhn's supposed recipe for the switch from 'pre-paradigmatic' activity to normal science. *Kuhn ought neither to be praised nor buried for having apparently given 'pre-paradigmatic' sciences a road or a menu toward normal science.* Because, appearances to the contrary, he simply did not do so. Careful reading indicates that he did not even attempt to provide such a road or recipe.<sup>9</sup>

If we now return for a moment to the quotation from Feyerabend with which I began this section, then his strong misreading of Kuhn becomes more obvious in retrospect. For Kuhn aims to record primarily not what scientists say they do, not what others say they do, not what they think they should do, not what others think they should do, but *what they actually do*. Thus it is off-target for Feyerabend to refer to Kuhn as having the "belief" that scientists "should not waste time looking for alternatives" to a working paradigm and for him to claim that Kuhn is saying that history can tell you the way science *should* be run.<sup>10</sup> Kuhn is simply trying to give an account of what history tells us about how science has actually been run and in consequence to question certain popular and philosophical normative philosophies of science.

*However*, we should also take care not to *over-emphasize* the extent to which Feyerabend's position on Kuhn on science is negative and thus to overemphasize the extent to which Feyerabend unequivocally held the (mis)reading of Kuhn in question. In particular, it should be borne in mind that:

(i) Feyerabend, much more than the orthodox Popperians, found Kuhn's concrete accounts of scientific *revolutions* impressive and highly suggestive—suggestive of the extent to which such events are 'non-rational'<sup>11</sup> and even also, very importantly, have an aspect worth calling 'incommensurable' about them.

(ii) Rather more surprisingly, there can also be found in Feyerabend, if

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one searches it out, a different strand—namely, a *limited* but real defense or explication of Kuhn's *normal science*. Here is Feyerabend again:

[In the analysis of science] historical research and not rationalist declarations must now determine the nature of the entities used, their relations and their employment in the face of problems and...a *general* theory of science *must make room* for these specific parameters. It must leave specific questions unanswered and it must refrain from premature and research-independent attempts to make concepts 'precise'. Kuhn's account perfectly agrees with these desiderata. His paradigms are 'obscure and opaque'<sup>12</sup> not because he has failed in his analysis but because the articulation changes from case to case. The relation between theories and paradigms remains unresolved because each research tradition resolves it in its own way, in accordance with the cosmological, normative, empirical elements it contains. There is little specific advice concerning the treatment of anomalies because each paradigm deals with these matters in its own way. [Larry] Laudan's accusation [against Kuhn] of incompleteness (which he takes over from a host of bewildered philosophers of science who have read a few logic books but have never seen science from nearby) shows that despite his severely historical posture he still shares the rationalists' dreams for clear, well-defined and history-independent conceptual schemes.<sup>13</sup>

So now, rather than seeing 'normal science' and 'paradigm' simply as concepts that Kuhn unjustifiably leaves under-exemplified in his account, we might try seeing them as *themselves* 'paradigms' (exemplars), which were necessarily general when Kuhn proposed them, waiting for more applied uses that will *change* them in the service of a revamped sociology, history, or philosophy of science that would *develop its own examples* (and simultaneously—crucially—in the service of displacing and overtaking a monstrous prior philosophy of science). Thus Kuhn's account can—thanks to Feyerabend's very insightful (and in this instance, in this insistence, very laudatory) perspective on it—perhaps be rendered compatible *with itself*. And, to repeat, this will involve a gestalt-switch to seeing the concept of 'paradigm' *as itself* a paradigm (an exemplar albeit an unusually abstract one).

This very sympathetic (as opposed to very negative) Feyerabendian interpretation of Kuhn *even on normal science* is to be found actually at a handful of important points in Feyerabend's work. For example, compare also the following:

Understanding a period of science [according to Kuhn] is similar to understanding a stylistic period in the history of the arts. There is an obvious unity, but it cannot be summarized in a few simple rules and the rules that guide it must be found by detailed historical studies (the philosophical background is explained by Wittgenstein...). The *general* notion of such a unity, or 'paradigm' will therefore be poor and it will state a problem rather than finding a solution: the problem of filling an elastic but ill-defined framework with an ever-changing historical content. It will also be imprecise. Unlike the sections of a theoretical tradition which all share basic concepts the sections of historical traditions are connected only by vague similarities. Philosophers interested in general accounts and yet demanding precision and lack of ambiguity...are therefore on the wrong

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track; there are no general and precise statements about paradigms.<sup>14</sup>

This seems a good reason for thinking that there will be an unjustified misreading going on if, without it being made plain that he is extending Kuhn's terms and adapting them for different purposes, someone takes Kuhn's terminology and outlines as abstract, tentative, necessarily imprecise, counter-hegemonic philosophical sociology to license a set of methodological rules<sup>15</sup> for transforming any discipline at any time from a set of studies to a science. What I have been trying to do is to question if Kuhn could be right in being angry about people reading him as legitimating the "We just need a paradigm" idea. The answer is, to a large extent, if not quite completely, "Yes."

Feyerabend then does two things with Kuhn: He alerts us cleverly to the 'dangers' of normal science *and* he makes clear how Kuhn's account of normal science is nevertheless useful if applied with a selective philosophical sensibility, rather than as a formula.

### **Do you need a paradigm to catch a paradigm?**

WHAT WERE FOLKS EXPECTING WHEN THEY EXPECTED THAT KUHN'S *STRUCTURE* should show them a way forward for the human sciences—for their own social discipline, along the sure road of science? These sociologists, political scientists, psychologists, anthropological linguists, and perhaps also literary and art critics, historians, and students of religion, were presumably expecting that Kuhn was pointing them toward something beyond his own practice—with the outline of how to get hold of

a paradigm *for their* use. But if they had looked at that practice harder or longer, they might not have been so expectant that it would provide them with something radically new.

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*Kuhn's own practice draws far more on the practices of the human sciences and the humanities than does that of any other major philosopher of science.*

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For one is struck, if one notes the listing of disciplines just made, that *Kuhn himself* borrows something from each of them. That is, one could well bear in mind his use of metaphors from

religious studies and politics, his analogies to art history, his use of methods from textual criticism, his borrowings from gestalt psychology and from Sapir-Whorf, not to mention his straight history (and sociology). Kuhn's own practice draws far more on the practices of the human sciences and the humanities than does that of any other major philosopher of science. Kuhn is to some considerable extent *himself* a human scientist. Kuhn is doing philosophy, and he is doing it after the fashion of interpretive human science far more than most philosophers of science (some of whom attempt to draw more on logic, or cognitive science, or to build their own [philosophical] 'theories' of science, for example).

And what does Kuhn's practice show us? By means of applying a historically 'based' approach to problems in the philosophy of science, it shows that para-

digms exist in natural science. It shows us the nature of normal science and of scientific revolutions. Thus, Kuhn, by looking at the behavior of certain human beings (principally scientists), finds in their practice paradigms—disciplinary matrices and exemplars (whatever exactly you want to call them). He attempts to capture how science works through employing the concept of ‘paradigm’. And one hopes that what Kuhn is finding is not wholly of his own invention. In other words, one hopes that ‘paradigms’ are actually already there in the social practices of the communities that Kuhn is talking about. That is, as Peter Winch would have it, that a reasonable scientist could actually be brought to formulate her own experience in the kinds of terms that Kuhn offers. Kuhn is keen to reflect some important aspects of how scientists conceptualize their own practice when they do so reflectively—presumably he would hope to do so never more than in this key respect. In other words, Kuhn encourages one to look at the sciences and to see if one finds paradigms there. Paradigms are already objects of Kuhn’s philosophical sociology. Exemplars and disciplinary matrices are both constitutive of the order of scientists’ practices in a normal science situation. And again, they predate Kuhn’s description of them, they are not just artifacts of Kuhn’s writing (unless Kuhn is quite wrong—in which case no one would need to worry about his lessons for the social sciences).

Now, what is it that human scientists more generally do? Is it not something very similar? For example, a sociologist looking at any set of practices, be they what they may, or an anthropologist looking at an ‘alien’ society: are they not in search of beliefs and methods of acting, tacitly agreed norms, and so forth, of the people they are looking at? Are they not in the business of describing these?

I am suggesting the following: Rather than taking inspiration from the *content* of Kuhn’s descriptive history of the natural sciences, rather than looking for a paradigm to guide and enforce limitations on their own practice *qua* social scientists (an enterprise liable to be useless or even counter-productive, as I argued earlier), social scientists could profitably look instead to Kuhn’s version of their practice *as he himself employs it*. For he himself employs much the same method in describing the history and sociology of science (sometimes fairly abstractly) and in teasing out their meaning for the philosophy of science. Social scientists could usefully think through the sense in which their own practice is *already* the searching out and describing of paradigms and their ilk. Do they *need* a separate paradigm of their own to do that properly? Might it not rather hinder their task? Take our sociologist, looking (say) at some set of religious practices and beliefs. Isn’t the Wittgensteinian philosopher of social science, Peter Winch, right in saying that “the sociologist of religion will be confronted with an answer to the question: Do these two acts belong to the same kind of activity? And this answer is given according to criteria that are not taken from sociology, but from religion itself.”<sup>16</sup> Why? Because:

The concepts and criteria according to which the sociologist judges that, in two situations, the same thing has happened, or the same action performed, must be understood *in relation to the rules governing sociological investigation*. But here we run against a difficulty: for whereas in the case of the natural scientist we have only to deal with one set of rules, namely those governing the scientist’s investigation itself here, *what the sociologist is studying*, as well as his study of it, is a human activity and is therefore carried on according to rules. And it is these rules, rather than those which govern the sociologist’s investigation, which specify what is to count as

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‘doing the same kind of thing’ in relation to that kind of activity.

An example may make this clearer. Consider the parable of the Pharisee and the Publican (Luke 18:9). Was the Pharisee who said ‘God I thank thee that I am not as other men are’ doing the same kind of thing as the Publican who prayed ‘God be merciful unto a sinner’? To answer this question one would have to start by considering what is involved in the idea of prayer; and that is a religious question. In other words, the appropriate criteria for deciding whether the actions of these two men were of the same kind or not belong to religion itself.<sup>17</sup>

Similarly, the appropriate criteria for deciding whether two people are engaged in the same kind of activity (say, are both testing a particular hypothesis or not) belong to that activity (say, the specific science in question) itself. At times of paradigm-shift, there may suddenly be divergent decisions among scientists on such issues. That normally there are not is if you like a condition of possibility for the stability of the sciences as institutions at all. And that normally there are not is another way of saying there are paradigms *in* science.

Let us sum up the above. Kuhn’s ‘fans’ say: Social science needs a paradigm (that doesn’t exist yet). I say: Social science is about, among other things, finding paradigms *that already exist*, in social settings. (And what one surely needs to do in relation to such paradigms is to describe them—to be responsive to them as they already exist, not to impose an alien theory onto them.)

None of the above *proves* that social or human scientists are wrong to look to Kuhn for a blueprint. It *might* be that one needs a paradigm in order to look most effectively for paradigms, though that does sound rather like putting a cart in front of a horse.

But the above argument *does* strongly imply that students of society should not think of the practice of finding paradigms and exploring their nature descriptively as radically new. Kuhn did what they have *already* been doing to a considerable extent (and vice versa, now). And what they do looks very different from what natural scientists do—for the latter, unlike the former, do not have as their business *anything like* the description of paradigms. If they ‘explore’ paradigms, it is in an utterly different sense—through theorization, experimentation, and so forth. And the point is that we have no particular reason to think that the human sciences will profit from aping such methods. For they need essentially to explore something like a paradigm *that already exists*, rather than creating a new one. They need to effect a description of a set of human practices—a set of practices that do not necessarily need to have a paradigm, a set of categories, imposed upon them in order to become interpretable, for they already *embody* such a set of categories. They may even be argued to be correctly describable, in principle, in a sense unavailable to natural scientific inquiries, because there is no such thing as describing the non-human world in a way that the latter prefers. However, perhaps the human world truly can be ‘cut at its joints’—by a description that respects it and (simply) gets it right, in terms accurately reflecting participants’ self-understandings and ordered activities. Paradigms and their ilk already exist—*in* social action (for example, of scientists). No new terms, concepts, or theories are necessarily required. As I brought out above in connection with Peter Winch, one loses sight of this aspect of the human sciences at one’s peril.

The ‘Kuhnian’ apologists for social science may, in their desperation for

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respectability, be overlooking this possibility—the possibility that careful description of social action, paradigms, and so forth, may, as Mary Midgeley suggests, already be possible (‘even’ in disciplines without a paradigm), *provided* one doesn’t get sidetracked into fighting for the dominance of a would-be social science. And let us be quite clear about this: The wish of Kuhn’s cruder fans or appliers to ape natural science by means of getting a paradigm—finding their own Newton—is deeply ironic. These people, who think that Kuhn has proven a kind of Relativism to be true, are still—at the very same time—wanting to have the kudos of being recognized as scientists, by means of having a paradigm to unify them and the paraphernalia of professionalism to maintain and enforce the unification. But this shows that they are still vulnerable to the attractions of Positivism. If they really had the confidence of their Relativist convictions, they wouldn’t care about how the natural sciences conducted themselves. They wouldn’t try to ape science as described by Kuhn or anyone else. They would boldly strike out in their own direction. They would rest content with self-generated criteria for how, if at all, to distinguish between good and bad ways of structuring their discipline—good and bad work within their discipline. It is in fact a sign of deep disciplinary insecurity that one calls upon a philosopher of science to supposedly legitimate one’s own discipline as being ‘just as good’ as the natural sciences. It makes no difference whether one hopes to do that by pulling one’s own discipline up to their level or pulling their discipline down to one’s own level.

What I am saying can even be put thus: Those who feel a need to argue that their discipline is as good as the natural sciences (or at least could be, if only it had a paradigm) are *ipso facto* still utterly in thrall of the prestige of the natural sciences. That’s a very poor man’s relativism!

The alternative of course is for ‘the social sciences’ to regard themselves as truly *sui generis*—as not needing to look to methodological aspects of the sciences with paradigms in order to validate themselves. This can only be done if one more or less accepts the current state of one’s own discipline, ducks out of endless methodological debate (except insofar as it is necessary to puncture the aspirations of those, for example, who have been criticized above), and *gets on with* doing what good work can be effectively done within that discipline (in sociology, for example, good ethnographies of very diverse social practices).

### **Towards a conclusion**

THE ABOVE DISCUSSION PROVIDES A FINAL AND, I THINK, DECISIVE SENSE IN WHICH the message of Kuhn is utterly misunderstood if, for good or for ill, it is taken to be: “Social science can make itself just the same as natural science, if only it gets itself a paradigm.”

And so to sum up my discussion: The basic point of the concepts ‘normal science’ and ‘paradigm’ is to demonstrate a distinction between disciplines in which many of the participants are always tearing everything up and starting again and disciplines in which that doesn’t happen. That distinction happens (and perhaps it is not mere happenstance) at this point in history to coincide roughly with the divide between the social and the natural sciences.

After spending some time at the Center for Advanced Studies in the behavioral sciences, while working on the ideas which would eventually become *Structure*, Kuhn wrote the following words, which are there for all to see on page

I was struck by the number and extent of the overt disagreements between social scientists about the nature of legitimate scientific problems and methods. Both history and acquaintance made me doubt that practitioners of the natural sciences possess firmer or more permanent answers to such questions than their colleagues in social science. [*Kuhn is saying that it's not as if natural scientists have a superior explicit grasp on proper scientific methodology than social scientists. That's not the clue to the difference between them.*] Yet, somehow, the practice of astronomy, physics, chemistry or biology normally fails to evoke the controversies over fundamentals that...often seem endemic among, say, psychologists or sociologists. Attempting to discover the source of that difference led me to recognize the role in scientific research of what I have since called paradigms. These I take to be universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners.

So in astronomy, physics, chemistry—or increasingly nowadays, in sub-specializations of those disciplines—scientists have a unifying idea *that comes from a certain impressive result or theory*—as, for example, Newton's laws were for a long time a universally agreed paradigm for physics.

Kuhn's concept of paradigm *comes*, then, from a distinction between disciplines like physics (disciplines with a paradigm) and disciplines like sociology (disciplines without a paradigm or, what comes to the same thing as not having a paradigm, disciplines with loads of competing paradigms, forming incompatible *schools* of thought). That our image of Kuhn may well have been quite wrong starts to become very clear when we realize that Kuhn *isn't* saying that enforcing the victory of one school via professionalization will do the trick of putting one's discipline on the secure road to being a science. The community of practitioners has to actually be impressed by—has to pretty much universally recognize a scientific achievement and take it for—a paradigm. If that doesn't happen, then too bad. *You don't have a science.*

Kuhn's main topic in his work is “mature” sciences—or, better, “disciplines with paradigms.” It's in *them* that we have actual examples of paradigms and paradigm-shifts. He doesn't dwell as much on transitions from ‘pre-paradigmatic’ to ‘paradigmatic’ states, and, when he does, it is only in cases where the transition has been made—in disciplines with paradigms, the natural sciences—which is not surprising for a historian of science.

Nor does Kuhn dwell very much on borderline cases of (mature) sciences. One extrapolates to these cases at one's peril. The interpretation given here of the true (and small, if you like) impact that Kuhn should have on the philosophy of the social sciences is a far less perilous way to go. We have been clear that Kuhn was not in the business of laying down norms for how to *get* paradigms, nor of saying that ‘getting’ a paradigm is always possible.

This is especially apparent in light of the fact that Kuhn was very mad at the appropriation by him of relativists, post-modernists, and apologists for the social sciences. My policy is to at least *try* to read Kuhn such as he understood himself—understood his own work—as *not* badly confused or stupid. In this, I follow Kuhn's own hermeneutic, famously applied by him to cases like those of Aristotle and

Carnot. On the topic of this paper at least, I think that that following this hermeneutic approach is in fact not too difficult to do and that Kuhn's 'foes' and 'fans' alike have more or less demonstrably got it wrong. I think that Kuhn could quite reasonably be said to be mostly in the right in his anger at those who saw him as trying to legitimate the "All that social sciences need to be real sciences is to get themselves a paradigm" idea. To think that 'getting' a paradigm is sufficient to guarantee one's discipline the status of a real—normal—science is to put the cart before the horse again. For if we have a paradigm, then we have normal science. But a paradigm is something one has only if one actually *has* it—not if one fantasizes it or attempts to impose it. Again, for Kuhn, a paradigm can't be forced on an area of investigation: it must emerge 'naturally'.

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*For Kuhn, a paradigm can't be forced on an area of investigation: it must emerge 'naturally'.*

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The clinching piece of evidence for my interpretation is from Kuhn's longest, if still fairly brief, consideration of an example from the social sciences in *Structure*. (This passage develops the thoughts that were so influential in the very development of his ideas, as expressed in the quotation given above from the preface to *Structure*). Here Kuhn addresses explicitly the question of what, if anything, the social sciences can learn from methodological reflection concerning the scientificity of their own disciplines:

Why should the [scientific] enterprise...move steadily ahead in ways that, say, art, political theory or philosophy does not? Why is progress a perquisite reserved almost exclusively for the activities that we call science?

Notice immediately that part of the question is entirely semantic. To a very great extent the term 'science' is reserved for fields that do progress in obvious ways. Nowhere does this show more clearly than in the recurrent debates about whether one or another of the contemporary social sciences is really a science.... Men argue that psychology, for example, is a science because it possesses such and such characteristics. Others counter that those characteristics are either unnecessary or not sufficient to make a field a science. Often great energy is invested, great passion aroused, and the outsider is at a loss to know why. Can very much depend upon a *definition* of 'science'? Can a definition tell a man whether he is a scientist or not? If so, why do not natural scientists or artists worry about the definition of the term? Inevitably one suspects that the issue is more fundamental. Probably questions like the following are really being asked: Why does my field fail to move ahead in the way that, say, physics does? What changes in technique or method or ideology would enable it to do so? These are not, however, questions that could respond to an agreement in definition. Furthermore, if precedent from the natural sciences serves, they will cease to be a source of concern not when a definition is found, but when the groups that now doubt their own status achieve consensus about their past and present achievements. *It may, for example, be significant that economists argue less about whether their field is a science than do*

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*practitioners of some other fields of social science. Is that because economists know what science is? Or is it rather economics about which they agree?*<sup>18</sup>

In this passage, Kuhn, unlike his 'foes' and his 'fans', is proclaiming the uselessness of *defining* science.

Now, one could take issue with Kuhn's characterization of economics here; it might be quite interesting to do so. There are, I think, serious questions about whether economics is actually substantially different from other social sciences in the regard mentioned by Kuhn.<sup>19</sup> But the passage is pretty decisive so far as *interpretation* of Kuhn goes. For Kuhn's claim about economics is *hypothetical*; what he plainly implies here is that *if* economics has managed to take on some of the characteristics that we find in the natural sciences, *then* it is not because of good philosophy or methodology of science. It's not because of 'following' Kuhn's philosophy of science, for instance, and 'getting' a paradigm. Rather, it is simply that economists have been fortunate enough to be able to *agree*—to achieve a research consensus *on matters of economics*. Economists, Kuhn is tentatively surmising, don't worry about fundamentals, but neither have they all explicitly agreed about methodology. They just get on with it.

Here is the crucial difference—the difference, which perhaps I have labored, but only because it is so crucial for understanding Kuhn aright, between *naturally acquiring* something like a paradigm and merely *striving deliberately* to get the *trappings* of one. Kuhn's 'followers' and his 'antagonists' alike confuse the latter with the former.

Returning then to the questions with which we began, we can note that Kuhn actually shows us that it's very questionable whether there can be anything that a normative philosophy of science can succeed in stating. In particular, the philosophy of social science wants to say things like, "All you need is a paradigm, so let us specify how to get one"—but none of these prescriptions can be given any basis. Philosophers up to now have wanted to *change* science; however, according to Kuhn, the point is to *interpret* it, to understand it.

The positive point of all this, perhaps, is as follows: We can avoid confusion, avoid wasting time, and *see the social sciences as they actually are* more clearly, if, rather than taking the standard prescriptivist approaches, we take this genuinely Kuhnian approach. And we need to be quite clear that that approach has very little to do with the hopeless hopes of Kuhn's purported followers or with the wildly exaggerated fears of his purported foes. The conundrum presented at the opening of this paper is resolved when we realize just how systematically Kuhn has been misread by both sides here—when we see that the popular conception of him as having grand consequences for social science is a castle built on just a few handfuls of sand. The completely discrepant accounts of the nature of the castle are a consequence of how small and loose its foundations are.

Kuhn's philosophy of science is, in actuality, fundamentally descriptive<sup>20</sup>—and prescriptive philosophy of science, by contrast, has little if anything to say.

### Conclusion

THE DRASTIC CONSEQUENCES FOR THE SOCIAL SCIENCES—POSITIVE OR NEGATIVE, depending on whether you are an 'advocate' for the 'coming' scientificity of the social scientists or a traditionalistic defender of the uniqueness of the natural sci-

ences (in which category at present we can place the Popperians)—which are supposed to follow from Kuhn's philosophy of science just do not follow, *on a correct reading of Kuhn*. The whole debate about the 'implications' of Kuhn for the theory and practice of social science has been misconceived from the start.<sup>21</sup> φ

## Notes

1. For example, in David Hollinger's "Free Enterprise and Free Inquiry: The Emergence of Laissez-Faire Communitarianism in the Ideology of Science in the United States," *New Literary History* 21 (1990): 897-919 and Steve Fuller's *Thomas Kuhn: A Philosophical History for Our Times* (Chicago: University of Chicago Press, 2000). Fuller and Hollinger argue that Kuhn's emphasis on 'the scientific community' was founded on the identification of the natural sciences as perhaps-inimitable paradigms of science and had the result that science was more, not less, isolated from social factors than before Kuhn wrote. Thus they suggest, in particular, that it is a huge irony (v. Hollinger, 914) that Kuhn's *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press, 1970) has been used as a manifesto by those of his fans who have sought to have exactly the opposite effect.
2. Cf. Kuhn's 'reply' to Feyerabend in his "Reflections on My Critics" *Criticism and the Growth of Scientific Knowledge*, eds. Lakatos and Musgrave (Cambridge, UK: Cambridge University Press, 1970), 237.
3. Paul Feyerabend, "Two Letters of Paul Feyerabend to Thomas S. Kuhn on Draft of *Structure*," edited by Paul Hoyningen-Huene, *Stud. Hist. Phil. Sci.* 26,3 (1995): 354-5. Feyerabend's quasi-Popperian commitment to normative philosophy of science is explicit here; the contrast with his later views on the normative inefficacy of methodological reflections on science should be evident (not to say blatant)—see the notes below.
4. Feyerabend, "Explanation, Reduction and Empiricism," *Scientific Explanation, Space and Time: Minnesota Studies in the Philosophy of Science*, vol. 3, eds. Feigl and Maxwell (Minneapolis: University Of Minnesota Press, 1962), 60. (Of course, Feyerabend will later change his "view" on this point—he will countenance an opposition to normative methodological precepts greater even than Kuhn's. See the early chapters of the 3rd edition of Feyerabend *Against Method* (London: Verso, 1993).)
5. Kuhn *Structure* 15.
6. At least, such is *Kuhn's* account of the failure of astrology to be a science, even in the days of its intellectual respectability. See Kuhn "Logic of discovery or psychology of research?" *Criticism and the Growth of Scientific Knowledge*, 7-11.
7. *The Ethical Animal* (London: Routledge, 1994), 51, final italics mine. Nested quotes from Phillip Slavney and Paul McHugh, *Psychiatric Polarities: Methodology and Practice* (Baltimore: Johns Hopkins University Press, 1987), 8, 123.
8. Cf. Kuhn, *The Essential Tension* (Chicago: University of Chicago Press, 1977), 222, 229
9. Though, having said that, I would, if space permitted, detail also why I think this position, this misreading, was so easy and attractive to take, to make and how Kuhn's invocation of terms such as 'pre-paradigm' guarded quite insufficiently against it. I think that Kuhn himself acknowledged these points in his later work and in the shifted terminology that was involved in that work.
10. Feyerabend "Two Letters," 355.
11. Feyerabend on this score goes further than Kuhn does. Compare his remarkable words on the Copernican Revolution in *Science in a Free Society* (London: Verso, 1978), 65: "In the beginning [Copernicus's] view was as unreasonable as the idea of the unmoved earth must have been in 1700. But it led to developments we now want to accept. Hence, it was reasonable to introduce it and try

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to keep it alive. Hence, it is always reasonable to introduce and to try to keep alive unreasonable views.”

12. Common terms of criticism from analytic philosophers of Kuhn; in this instance, Feyerabend is quoting the words of Larry Laudan, from whom he is defending Kuhn and others in this essay.

13. Feyerabend “More Clothes from the Emperor’s Bargain Basement; A Review of Larry Laudan’s ‘Progress and its Problems,’” *Problems of Empiricism* (Cambridge: Cambridge University Press, 1981), 236-7. Below Feyerabend adds, “Kuhn makes the highly interesting and revolutionary suggestion that *physics is a historical tradition and therefore as much in need of Verstehen [interpretive understanding] as history proper*. Laudan does not notice this feature of Kuhn’s theory (*sic*),” 237.

14. Feyerabend “Some observations on the decay of philosophy of science,” *Problems of Empiricism*, 24-5.

15. Notice that on the surface I appear on this aspect of the point to find Feyerabend directly contradicting himself, because this appears to let Kuhn off the hook of “writing an ideological history where historical facts could be read as methodological rules without any difficulty”—the very hook on which Feyerabend hangs him in the quotations with which I began this paper. It would take us too far afield to resolve this here: points to be noted include that (i) Feyerabend is quite happy, unlike most philosophers, to contradict himself, and (ii) Feyerabend would stress that a fair appraisal of Kuhn involves looking at his work in its totality, not looking only at those parts of it that suit one’s immediate purposes, and that this latter is what his grosser appropriators are doing: they don’t even take the time to notice that Kuhn’s account of paradigms and of normal science as the working out of a paradigm is *deliberately* imprecise and ambiguous. (Much like, I would add, Wittgenstein’s “parent” concepts of language-game, form of life, and family-resemblance, which are not true “technical terms” either.)

16. Kuhn, *The Idea of a Social Science* (London: Routledge, 1958), 87.

17. Kuhn *The Idea of Social Science*, 87.

18. Kuhn *Structure*, 160-1. Italics mine.

19. Furthermore, it is my view that attempts by economics to scientitize itself, to jump ahead of its clock, have often had nefarious consequences. A nice example is examined by R. Cooter and P. Rappoport in their paper, “Were the Ordinalists Wrong about Welfare Economics?” *Journal of Economic Literature* XXII (June 1984): 507-530. Consider, for example, the following quotations: “[T]he elements of the older framework...are now viewed through the distorting lens of the ordinalist framework,” (508). “The received view is that ordinalism represents scientific progress relative to the material welfare school, but one can talk unequivocally about the progress of a science only when it continues to address the same questions,” (528).

Here we have, one might argue, the trappings of Kuhnian (natural) science methodology. But the fundamental suspicion of Cooter and Rappoport is that, in *this* case, there were useful things that the abandoned research tradition (old-style Welfare Economics) had and perhaps still has to offer that are ruled out by the ‘paradigm’ that won the day, to our political as well as economic detriment: “The aim of this paper is to demonstrate that the arguments developed some fifty years ago to criticize the material welfare school do not in fact address the claims of that school, whose scientific integrity remains intact. This suggests that it may be fruitful to draw on the material welfare perspective in the analysis of present-day welfare problems, and perhaps warrants a comparison of the older view with the achievements of modern welfare economics,” (510-1). Arguably, economists still need to worry about fundamentals in a way that detracts from any claim that economics has a paradigm. The likes of Cooter and Rappoport are themselves implicitly suggesting that any such claim is quite premature.

20. Here is another link with Wittgenstein. I believe that Kuhn was quite largely a descriptive and indeed therapeutic philosopher—he wanted to cure people of the philosophy of science, unless by ‘philosophy’ one was ready to mean something quite non-standard. Though it would take us far

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too far afield to investigate here, I think that there are instructive affinities not only between Winch and Kuhn, but more generally between Stanley Cavell, James Conant, and others collected in *The New Wittgenstein*, edited by Alice Crary and Rupert Read (London: Routledge, 2000) on the one hand, and the kind of perspective on philosophy that we find for example in Kuhn, *The Road Since Structure*, edited by Conant and Haugeland (Chicago: University of Chicago Press, 2000), especially in some of the remarks of Kuhn's in the interview that closes that book, on the other.

21. My overwhelming and ineffable debt in the writing of this paper is to Wes Sharrock, who has immensely enriched my understanding of both Kuhn and Winch and with whom I am writing a book (that gives a 'Wittgensteinian' reading of Kuhn), entitled *Thomas Kuhn* (Oxford: Polity, forthcoming), from which this paper is an adapted excerpt.

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