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Abstract

The philosopher Richard Rorty counts himself among the supporters of Thomas Kuhn, who has done extensive and important work in the history and philosophy of science. Rorty bases his support on a number of similarities which he sees between his own work and that of Kuhn.

My intention in this thesis has been to demonstrate the distinct lack of similarity between the work of Rorty and that of Kuhn. It is my contention that Rorty's belief in the sympathy between their respective programmes is misguided, being grounded in Rorty's misunderstanding of a considerable number of elements in Kuhn's programme, as well as the intent with which it was offered.

The first chapter is an exegesis of Rorty's general philosophy of culture, moving into a more specific exegetical look at Rorty's references to Kuhn's work. The second chapter provides an extensive outline of Kuhn's historically sensitive examination of the philosophy of science. The third chapter engages in a discussion and analysis of comments which Rorty has made concerning Kuhn's work and the ways in which it relates to his own, encompassing both similarities and differences.

I believe that I have shown that Thomas Kuhn's work in the history and philosophy of science is not at all applicable to the sort of cultural programme which Rorty is offering, in terms of scope, intent, and fundamental content.
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Introduction

With some qualifications to which I will attend in the body of this work, Richard Rorty counts himself among Thomas Kuhn's supporters. In his *Philosophy and the Mirror of Nature*, Rorty describes Kuhn as one of the "heroes of this book". I wish to argue that Rorty's sentiments in this regard are grossly misplaced, and that his sense of alliance with Kuhn's project is achieved at the cost of his misunderstanding it.

The first chapter is intended as a strictly exegetical section, covering Richard Rorty's general philosophy of culture. The latter part of this chapter will look in more specific detail at Rorty's references to the work of Thomas Kuhn; I intend there to provide evidence for Rorty's attempt to enlist Kuhn as someone whose work promotes the direction of his own.

The second chapter provides an extensive outline of Kuhn's programme, which latter can be taken as an historically sensitive examination of the philosophy of science. This section is also strictly exegetical.

The third chapter engages in a discussion and analysis of comments which Rorty has made concerning Kuhn's work and the ways in which it relates to his own. This will encompass what Rorty sees as both similarities and differences.

It is important to understand that it is not my intention, in this work, to evaluate other writers whom Rorty invokes or directly quotes; they are merely included as vehicles for the element in Rorty which I wish to consider, viz., the relation of his philosophy to the work of Kuhn. I will therefore not be evaluating Rorty's work on the basis of internal consistency or the lack of it; nor will I be considering whether either Rorty's or Kuhn's programme provides a plausible explanation, in my view, of the way in which we ought to consider cultural and/or scientific activities.

I believe there to be few similarities between Rorty and Kuhn, and these are relatively superficial. The relationship between them is characterised primarily by divergence, although it is my contention that Rorty fails to see the degree to which he and Kuhn diverge, on account of his quite substantial misinterpretations of Kuhn.

Rorty understands his talk of cultural discourses to represent a generalisation of Kuhnian scientific paradigms. He sees Kuhn to be engaged in a compatible project to his own, by virtue of what Rorty takes to be:
1. their mutual rejection of correspondence theories;
2. their common desire to dispense with the notion of observation;

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1 Rorty, *PMN*, p. 382.
3. the credence which they give to non-rational factors influencing human behaviour;
4. their framing of rationality primarily in terms which describe it as the triumph of techniques of persuasion, rather than of force;
5. their rejection of the desire for and adherence to a framework of accepted conceptual and practical commitments, such as we find in traditional epistemology; Rorty describes this as their having questioned the need to identify a set of rules for directing rational agreement;
6. their orientation towards pragmatism;
7. their common intent to direct us to think of science as interwoven with other cultural activities: to this end, Rorty sees Kuhn as having "softened the distinction between science and nonscience";2
8. the importance which they place upon sensitivity to historical circumstance;
9. the fact that neither of their positions is relativist, despite the possibility of their being construed as such; and
10. their common proposal of hermeneutics as a way of "coping" with one's own and others' cultures.

Nevertheless, Rorty takes issue with Kuhn's relatively greater allegiance to traditional epistemology. Further, given what Rorty sees to be Kuhn's project, he believes Kuhn to have given undue credence to the idea that philosophy has a role in setting out the nature of knowledge. I wish to argue that Rorty has misread Kuhn as being, overall, inclined against traditional understandings of philosophy, epistemology, and rationality--as Rorty himself is. While Kuhn does push for a more historically sensitive philosophy of science, he does not intend to negate the function of rational thought, the possibility of some identifiable commonality which links the thought and experience of human beings, or the importance of developing a theory of knowledge. Kuhn does not concern himself with--either to accept or reject--the question of philosophy's functioning as an all-encompassing discipline which serves to ground and limit other disciplines.

Both Rorty and Kuhn reject correspondence theories of knowledge. However, Kuhn's retention of the Kantian notion of Nature as providing that which is "given" in perception leads him also to retain observation as an important feature of scientific work. While both Rorty and Kuhn eschew any ontological commitment, Kuhn's sense of the "given" prevents him from adhering to a strictly linguistic view of things; Rorty's outlook is linguistic in orientation.

Kuhn describes himself as in some sense a Kantian. In this light, he is not at all inclined towards the rejection of a conceptual and practical framework; in fact, the purpose of his scientific paradigms is precisely to describe the various frameworks by which scientists have operated. He refers to these paradigms as "moving, historically-situated, Archimedean platforms"3, which direct how acceptable scientific thought and practice are to be carried out. Rorty does not

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2 Rorty, ORT, p. 38.
3 Kuhn, RSS, pp. 6-7.
seem to be adequately aware of this structuralism which is inherent in Kuhn's programme.

Kuhn views science as primarily a pragmatic endeavour, but he spells this out more clearly than Rorty does for his own cultural "pragmatic holism". Finally, if what Kuhn has said serves to "[soften] the distinction between science and nonscience"\(^4\), it is clearly not his intent to dissolve scientific activities into those of the culture at large. Kuhn's work is specifically directed towards clarifying the way in which science works, and has worked throughout its history.

\(^4\) Rorty, *ORT*, p. 38.
Richard Rorty, albeit with some qualifications to which I will in due course attend, counts himself among Thomas Kuhn’s supporters, rather than among his critics. It is my purpose in the first section of this chapter to provide a general overview of Rorty’s programme and, later on in the chapter, to outline more specifically what Rorty has to say about the relationship of Kuhn’s work to his own. This chapter is intended to be exegetical, and not evaluative of either Rorty’s philosophical remarks or his understanding of Kuhn.

Rorty describes the central concern of philosophy as the instantiation of itself as a general theory of representation, such that its task is to divide the culture into several areas: those which represent reality well, those which represent it less well, and those which fail to represent it at all. In this model, where we have knowledge, we have accurate representation, which is made possible by special mental processes, and made intelligible through a general theory of representation.

This representational model, Rorty contends, was born out of the enclosure of mental activity in a special kind of mental substance, one which was to be understood as isolated from non-mental, or physical, substance. As the mind looks upon the world, it receives representations, which are in the mind, and which are therefore able to be viewed by the Eye of the Mind. The mental thus represents the non-mental, and the door is opened to scepticism and the haunting question, “How do we know that anything which is mental represents anything which is not mental?”

Rorty goes on to attack this notion of mind on the basis of what he sees to be its superfluity. He asks why it should make any difference whether or not we have minds. Behind this lies his view that mental states could sensibly be viewed merely as place-holders for talk of neurons. So, for example, in certain situations, such as contact with a hot stove, a brain-event occurs: we might call this a case of “stimulated C-fibres”. An expression of pain results, the communication of which being all we need to understand what has happened. There is no need to formulate a hypothesis which includes anything such as “raw feels”. Neither do we need to posit a special mental substance which would accommodate such

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1 Rorty, *PMN*, p. 46.
mental entities as these. Moreover, we have so far failed miserably in our attempts to identify any causal mechanisms which might serve to describe how particular raw feels could be linked with particular neural-stimulation events. It may simply be, that when we report what we misguidedly take to be raw feels, what we are, in effect, reporting is neural stimulations, the implication being that perhaps we ought just to do this directly, without inserting the misleading talk which promotes an unnecessary and confusing positing of the mind.\(^2\)

Amplifying this difficulty that we will never be able to explain the difference between *feeling* a pain and simply *reacting to*, say, a stimulated C-fibre, is the point, crucial for Rorty's philosophical view, that there is already no detectable difference between the two "from the outside". An observer of someone in pain will have no means of distinguishing whether the victim is experiencing a peculiarly mental entity or simply reacting to the physical stimulation of certain neurons.

It is in the examination of this latter point that we are confronted, once we set foot on epistemic ground, with the seductive appeal of metaphysics. If we take seriously the possibility of such private mental entities as raw feels, along with our ability to speak of these inner states in the absence of any talk of their behavioural accompaniments, then there is every reason to consider raw feels as serious candidates for ontological status. Then the door is open to the notion of a mental substance, of which these entities serve as modes. Moreover, an environment is created in which scepticism about the existence of other minds can flourish.

But Rorty believes that he can furnish us with a solution to this incommodious state of affairs. We should drop the notion that to have knowledge of something requires our being acquainted with its "special, felt, incommunicable qualities". To have knowledge of the presence of a pain is to understand the meaning of "pain", and this requires being taught to label a certain state (i.e., C-fibre stimulation) as a pain-state, and to behave in a certain manner with respect to that state. Unless there were such a thing as typical pain-behaviour, we would never be able to teach a child the meaning of the word "pain". But such behaviour belongs to the public domain. According to Rorty, our assumption that we learn what pain is by "casting linguistic garb over...our direct acquaintance with special, felt, incommunicable qualities"\(^3\) is in error. To deepen the problem, the notion that language is used to name that which is naturally already given (known by the mind's Inner Eye) leads us into scepticism: since knowledge of felt mental entities is not only privileged (i.e., incorrigible), but incommunicable, we end up being irremediably sceptical with respect to whether or not others are naming the same incommunicable quality that we are when speaking of pain. For the sceptic, facts about behaviour and environment are irrelevant to the *essence* of pain.

Since we cannot tell whether what we are accustomed to calling "raw feels" are truly experiences of mental objects or stimulations of certain neurons, then we ought, by a principle of philosophical economy, to abolish this excess explanatory

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\(^2\) Ibid., pp. 70-83.
\(^3\) Ibid., p. 110.
talk of raw feels (which are hopelessly private and incommunicable, anyway), and speak instead in materialist language, which is to say in terms of neural stimulation. But this does not mean that we should embrace eliminative materialistic mind-body identity theory, for the materialist is guilty of making *metaphysical* claims, and this is surely something which Rorty is concerned to avoid. Rorty’s claim is that it is pointless to pursue the issue of whether a sensory report signals something mental or neural, “not just because nobody has any idea how to resolve the issue, but because nothing turns upon it”. No predictive, explanatory, or descriptive power would be lost if we were to describe things like pains in terms of stimulations of particular neural fibres. What we are concerned with, after all, are these very powers, in their practical sense. What is more, we do not possess sufficiently clear ideas of what the mental and the physical are to be able to get much use out of a philosophical distinction between them. He contends that “[o]nly a philosopher with a lot invested in the notion of ‘ontological status’ would need to worry about whether a corrigibly reportable pain was ‘really’ a pain or rather a stimulated C-fibre”. Insisting on an ontological divide between mental states and neurons can only generate further unnecessary epistemic and metaphysical problems.\(^4\)

Rorty views our separation of mind and matter as ultimately an unfortunate linguistic development—one which has been made all the more unpleasant by the slide into metaphysical thinking which it facilitates. The terminologies of “sensations” and of “brain processes” are nothing more than two ways of talking about the same thing. Rorty here anticipates the question: “Two ways of talking about *what*?” But he advises us to resist our natural metaphysical urge, to “abandon argument and fall back on sarcasm, asking rhetorical questions like ‘What is this mental-physical contrast anyway? Whoever said that anything one mentioned had to fall into one or other of two (or half a dozen) ontological realms?’ ”\(^5\)

The most that I can make out of Rorty’s answer to this question “Two ways of talking about *what*?”, is that we are dealing with alternative vocabularies for talking about the world construed in something like an experiential sense, rather than about the world as metaphysically characterisable. Hence, it makes no difference *per se* which vocabulary we choose to employ, since we cannot tell whether what we are experiencing are mental entities or stimulations of neural fibres. However, given the unpalatable philosophical confusions and endlessly fruitless debates generated by the creation of a definite, ontologically conceived split between the mental and the physical, it would be best to dispense with one of these lexicons. Rorty opts for the abolishment of the mentalistic sort of vocabulary, on the grounds that it fosters the view of knowledge as accuracy of representation, an account which holds that certainty can only be had rationally about representations. He sees this sort of view as making the emergence of scepticism inevitable, which will lead us into the isolation of an inescapable

\(^4\) Ibid., p. 120.
\(^5\) Ibid., p. 122.
solipsism. For him, this is intolerable, as it must be for anyone whose utopian vision consists largely in what can be achieved through dialogue with others. Our "ability to say obvious things to ourselves alone" cannot therefore be a mark of philosophical triumph. So Rorty's solution entails ridding ourselves of the representational model of knowledge, thereby divesting ourselves of the image of mind as the mirror of Nature, which functions as the receptacle for these representations. This reinforces the fact that we can do without the notion of mind altogether. What would serve us better, in Rorty's estimation, would be a notion of "personhood", based not on philosophy's description of the human mind and its operations, but rather on our moral intuitions.

It follows from the image of mind as mirroring Nature—which rests upon the notion that the mind is naturally "given" to itself—that we can understand and improve knowledge by polishing the mirror: understanding as best we can the workings of the mind. The Rortian imperative is to set aside this offending image of mind as mirror of Nature.

In giving this model up, we give up the notion of philosophy as a discipline committed to the construction of a permanent, neutral framework for enquiry, and thus as a guiding light for the culture. This sort of discipline arose, according to Rorty, in the scientisation of philosophy which was promoted through the development of a methodology designed to achieve epistemic certainty. Thus, we can point to a traditional pattern: "an attempt to escape from history--an attempt to find non-historical conditions of any possible historical development". The natural quest for understanding which characterised pre-modern philosophy came to be run together with an unnatural quest for certainty.

For Rorty, this development signalled a misguided evolution of philosophy into epistemology. Rectifying it entails a re-conception of certainty as "a matter of conversation between persons, rather than a matter of interaction with non-human reality". In this, we see a prime issue for Rorty in his attack on the validity of correspondence theories of knowledge. Any distinction between necessary and contingent truths will thereby be erased, to be replaced by a simple differential in the ease with which interlocutors find themselves able to disagree with our assertions. This would place us in a similar position to the Sophists. "Rational certainty" then becomes a matter of victory or persuasiveness in argument, rather than relation to an object known. We would seek explanations for things among the parties to the discussion, rather than among putative faculties of the mind.

Rorty argues that the idea of foundations of knowledge—the desire for an epistemology—is merely the most recent product of a particular choice of perceptual metaphor. Alternatively, we could think of knowledge in terms of merely relations of propositions, with no reference being made to perception at all. Justification then becomes strictly a relation of inference between
propositions. He acknowledges that this could lead to an infinite regress of
propositions invoked to defend other propositions, but assures us that this is of no
consequence, since pragmatic conversation requires no more than that we
consider such a chain of justification only until all the participants in the discourse
are satisfied.

To think of knowledge as presenting a problem about which we ought to
have a theory is, according to Rorty, a product of our viewing knowledge as an
assemblage of representations. But if we see this way of thinking as optional, then
epistemology itself is optional, as is any conception of philosophy which is
grounded in it.

Rorty describes the process of justification within the epistemological
tradition as reductive and atomistic. This, because it attempts to isolate pared-
down foundations of knowledge within the individual; Rorty has described these
foundations as privileged representations. His preferred vision of human thought
is of something which he terms "holism", in which the justificatory process is
"conversational". Here, we find that justification, rather than being a matter of
the delineation of a special relation between ideas (or words) and objects, is
instead a matter of conversation as social practice. In this, we see pragmatism
confronting the unreality of traditional epistemological concerns. An
understanding of knowledge entails an understanding of the social justification of
belief.

Holism rejects the traditional quest for certainty in which philosophy has
involved itself, and turns away from such terms as "conceptual", "apodictic", or
"foundational". Epistemology becomes "naturalised", by virtue of becoming itself
a matter of empirical discovery. It is sufficient for us to learn about each other's
inner states from reports of them. We do not refrain from questioning pain-reports
on account of their primitive incorrigibility; rather, the certainty of a pain-report
reflects the fact that no one cares to question it. This attempt to explain rationality
and epistemic authority by means of reference to societally imposed limits on
linguistic practice illustrates an attitude of "epistemological behaviourism", a
species of holism. Understanding the rules of a language-game is the only
requirement which remains "to understand...why moves in that language-game
are made (all, that is, save for the extra understanding obtained from inquiries
nobody would call epistemological--into, for example, the history of the language,
the structure of the brain, the evolution of the species, and the political or cultural
ambiance of the players)". The shift into this version of understanding renders the
delineation of necessary and sufficient conditions for knowledge an impossible
task.

It seems that much of this emphasis on linguistic practice is motivated by
the difficulty of distinguishing between a response to language and a response to
experience. I presume this to mean that, while we can expect the evolution of

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10 Ibid., p. 170.
11 Ibid.
12 Ibid., p. 174.
response to experience to be aided by the development of a theory of knowledge, framing things in terms of a response to language does not require such a theory, but can get by with a treatment of the conventions of linguistic practice: a study of the workings of language-games.

The emphasis, in holism, upon collective social practice as the guide for what is to count as rational, and the correspondent refusal to allow for privileged acquaintance with sensory appearances or meanings, is an indication of the absence of any mediating element between the impact of the environment upon human beings and their reports about it. We can make no reference to "inner entities" as premises upon which to base our inferred knowledge of outer entities. Notions of "mind" or "stream of consciousness" are obviated. We remove the traditional epistemological bias towards grounding our practices of justification in "fact"; indeed, we have to ask whether or not the search for foundations of knowledge even makes sense, "whether the idea of epistemic or moral authority having a 'ground' in nature is a coherent one". From the pragmatic perspective, it is not possible to see what it might be like for social customs to be so grounded.\(^\text{13}\)

Since the epistemic concern with what is true, and the moral concern with what is right are more sensibly recognised to be matters of social practice than of any essential facts about humanity or Nature in general, the justificatory practice cannot operate outside reference to already accepted beliefs and language-practices. This renders coherence the test of what it is justifiable to assert. Rorty remarks that the latter point may look questionable, since we are used to conceiving of coherence as a by-product of philosophy's extraction of a permanent, neutral matrix which functions across the board as a foundation for all enquiry, and throughout all of history. The privileged status which this foundation, on this latter view, would accord to particular sets of scientific or moral views--allowing them to be seen as more "rational" than the alternatives--would support the notion that the more relativistic stance of holism must rule out coherence theories of justification. All that would be left to the holist engaged in discourse would be the ability to construct piecemeal and partial criticisms.

But Rorty objects that we cannot help ourselves to anything beyond justification based on holism, given that the traditional epistemological view rests upon appeal to behaviouristically unverifiable episodes (viz., raw feels, in which the mind recognises its own direct acquaintance with, for example, an instantiation of blueness). We saw this complaint earlier in Rorty's argument that we cannot distinguish between a report of pain as an expression of the presence of a genuine inner entity (a "raw feel"), present immediately to consciousness, and a report of pain as an expression of a stimulation of C-fibres. Because the holist is not driven by fear of the epistemological sceptic, as is the foundationalist philosopher, he is able to let go of a justificatory process which the foundationalist deems necessary, as the ground beneath our feet, in favour of a process which grants credence to the more fluid behaviourist holism which justification has truly

\(^{13}\) Ibid., p. 178.
always been, anyway. "Observation" becomes just what is intersubjectively agreeable these days.

An account of the nature of knowledge, then, can be no more than a description of human behaviour. Going back to our example of pain-reports, that we know what pain is merely reflects our ability to link the concept of pain with other concepts, in order that we be able to justify claims about pain; this, I take to be reliant on the framework of coherence described by the rules of the particular language-game in which the conversationalists are engaged. Knowing what pain feels like, in some sort of sense in which pain is immediately present to the mind, is irrelevant. Talk of sensory experience invokes insufficient and unnecessary causal conditions for knowing what sorts of things we are talking about when we make reports about pains, colours, etc.:

[They are] insufficient for the obvious reason that we can know what redness is like without knowing that it is different from blue, that it is a colour, and so on. [They are] unnecessary because we can know all that, and a great deal more, about redness while having been blind from birth, and thus not knowing what redness is like. It is just false that we cannot talk and know about what we do not have raw feels of, and equally false that if we cannot talk about them we may nevertheless have justified true beliefs about them.  

The traditional notions of "givenness" have confused the ability to discriminate with the having of raw feels. It is Rorty's contention that we can discriminate without having raw feels, and indeed only do discriminate in this way, by virtue of the linguistic community which is the sole true source of epistemic authority. In fact, it is on the basis of the individual's expected membership in the speech-community which prompts us to grant that she is the sort of being who is party to knowledge by raw feels in the first place.

Rorty observes that, while we may balk at the claim that the child begins to possess knowledge only at the time that he learns language (but not before), we are not similarly troubled by the claim that the adolescent begins to possess previously absent rights and responsibilities upon attaining adulthood. Rorty sees these claims as analogous: in neither situation has there been some essential shift from within the individual; rather there has been a shift in the individual's relations with others. What has changed is the social context.

The question of the attribution, to other beings, of knowledge of what things are like has, for Rorty, more to do with morality than with justified true belief:

[Nobody] except philosophers of mind cares whether the raw feel of pain or redness is different for koalas than for us, but...we all care quite a bit about a koala when we see it writhing about. This fact does not mean that either our or the koala's pain is "nothing but its behavior"; it just means that writhing is more important to our ability to imagine the koala asking us for help than what is going on inside the koala. Pigs rate much higher

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14 Ibid., pp. 184-5.
than koalas on intelligence tests, but pigs don't writhe in quite the right humanoid way, and the pig's face is the wrong shape for the facial expressions which go with ordinary conversation. So we send pigs to slaughter with equanimity, but form societies for the protection of koalas.¹⁵

From this, we can see that our moral sense is governed not by some sort of recognition of certain objective qualities in beings, but instead on the imagined possibility of conversation within a given speech-community. Our misguided attribution of feelings to other beings is nothing more than a reflection of this. The "inside" of living beings is thus to be explained by what goes on on the "outside", especially in relation to their place in the community. The interior state of the organism is merely something posited to explain its observed behaviour.

In this light, it is not irrational to "send pigs to slaughter with equanimity, but form societies for the protection of koalas". Since rationality derives from the dictates of the community and its language, and not from anything "internal" to a being, rational allotment of moral concern is based on our expected inclusion of a particular being in our community. This means that other animals who most plausibly resemble humans in some relevant way will inspire greater moral concern in us than those who resemble us less convincingly.

Rorty favours the rejection of the notion that language is the expression of something "inner", which must be discovered prior to our being able to tell what an utterance means. This entails the abandonment of concepts and meanings, including such things as beliefs and desires. Their dispensability derives from the lack of behaviouristic equivalents for them. If we view concepts and meanings as a special source of truth or authority, they become harmful; but they assume a more beneficent aspect when simply posited to explain our behaviour.

This beneficence is the reward of our recognition that explanatory power rightfully follows pragmatic dictates. Philosophy then drops out as a discipline which guards against "irresponsible reification" and which systematises "our scruples about what objects one may assume". The different vocabularies which we use to talk about things (e.g., about actions and beliefs, or about movements and neurons) is not a mark of the difference between the real and the ontologically disreputable, or between the factual and the mythical. Although these divergent vocabularies can be mixed in our utterances (e.g., in the sentence "If we had just stuck in an electrode in the right place in the cortex, he would never have decided he was Napoleon"), we cannot hope to develop them into laws which are parts of comprehensive theories. Yet this is not because they belong to different ontological realms, such as the mental and the physical. Neither can we reduce one vocabulary to another, in the interest of hitting upon a vocabulary which reflects the true and ultimate structure of reality. All that is open to us is to judge the various vocabularies we have in respect of their

¹⁵ Ibid., p. 190.
pragmatic or aesthetic virtues. There is nothing to be gained, then, from differentially valuing explanations as either “scientific” or “unscientific”.\footnote{Ibid., pp. 206, 208.}

So, to understand the superiority of the New Science to Aristotelian investigations, we need to turn “outward”, towards the social context of justification of the day, rather than seeking privileged items in consciousness which could serve as the touchstone for truth. The latter move is embodied in the mistake of the epistemic tradition: confusing the causal process of acquiring knowledge with the justificatory process. If we turn away from this mistake, we will find that there is nothing left for epistemology to be. We re-describe “truth” in terms of what it is good for us to believe, removing it as the gate-keeper of assertions illustrating genuine contact with reality. This opens the way for a culture in which there is no all-encompassing discipline which grounds and sets limitations on the claims of the others. Rorty believes that he finds support for this stance in Kuhn’s \textit{The Structure of Scientific Revolutions}.

In Rorty’s pragmatist vision, \textit{objectivity} (which he characterises as human beings trying to describe themselves in relation to a non-human reality) is to be reduced to \textit{solidarity}, an empathic recognition of community, imbued with reciprocal relations of responsibility. This shift obviates both metaphysics and epistemology. If we are determined to retain a notion of “truth”, then it must be relegated to the status of simply what it is good for us to believe. To say that something is “true” is nothing more fancy than an expression of commendation, and this term will have an identity of meaning in all cultures (viz., that on which intersubjective agreement has been achieved), but a diversity of denotation (i.e., different cultures will come to different intersubjective agreements from those in other cultures). To doubt the truth of what we believe at present is merely to expect that somebody may at some point come up with a better idea, or description.

On questions where agreement has been found to be relatively easy to attain, we claim knowledge; where agreement is more elusive, we can only claim to be expressing opinions. This focus on agreement renders truth and rationality dependent upon the familiar procedures of justification which a given society uses in an area of enquiry. In evaluating questions which arise within our culture, we must necessarily begin from what Rorty terms an “ethnocentric” position, where, in practice, we privilege the beliefs and values of our own group, even though there can be no non-circular justification for doing so. In both evaluating our own culture from within, and in testing our beliefs against those of another culture, all we can do is play off some beliefs against others, in the effort to generate more fruitful conversation, which is to say, conversation which aims towards a more successfully pragmatic picture. This brand of ethnocentrism is provisional: it provides a natural starting-point--the only one available to us--from which to expand our conversational efforts outwards, towards other cultures, casting the net of solidarity ever wider. As such, it is preferable to the unrecognised, or at least unacknowledged, ethnocentrism of the traditional philosopher, who
attempts to take refuge in buttressing his own perspective with fixed notions of correspondence to reality, or of a peculiarly human moral sense or set of cognitive faculties.

One of Rorty's chief aims is to suggest the establishment of a liberal utopia, characterised by a universal inclination towards "liberal irony". We are called upon to recognise that cruelty is the worst thing that we can do, and to face up to the contingency of even our most central beliefs and desires (thereby demonstrating "irony"). There is nothing in this vision which depends upon the recognition of genuine facts, to be gleaned from a process of proper enquiry. Instead, it speaks of human solidarity, which is fostered by imagination and empathy. Creation, and not discovery, is therefore the mode which we are trying to get at in this society. What this amounts to is that the final victory of poetry over philosophy will occur as the triumph of metaphors of self-creation over those of discovery. This focus on self-creation--and in turn, the creation of the culture by self-creating individuals--emphasises freedom as the goal of thought, at the same time that it banishes truth. Self-creating individuals are not merely to make do with an inherited world.

The aim is to achieve distance from the desire to bring particulars under general principles, and the attempt to find necessary truths. We need only judge "particular present situations and options as similar to or different from particular past actions or events". Anything which we encounter in life--"from the sound of a word through the color of a leaf to the feel of a piece of skin"--can "dramatize and crystallize a human being's sense of self-identity". Any such thing can play the role in individual life which philosophers have thought could--and should--only be played by things which were universal. But again, solidarity will require that reflective human beings give sense to their freely created individual lives by placing themselves into a larger context. They do so by telling the story of their contribution to their community. 17

After all of this, if we still insist on the relevance of the notion of internal representations, then we can only do so by restricting ourselves to something on the order of "'innatist' views common to Chomsky and Fodor" 18, where we encounter the notion of a "wired-in" language and meta-language of thought:

If one gives up the notion that empirical psychology is going to do what the British Empiricists failed to do--show how a tabula rasa gets changed into a complicated information-processing device by impacts upon peripheral sense-organs--then one will not be surprised that half of the adult's subroutines were wired into the infant's brain on instructions from the chromosomes. Further, it will not strike one as important to our understanding of the nature of man or his mind to discover just which were wired into them and which came along later. 19

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17 Rorty, CIS, p. 37.
18 Rorty, PMN, p. 251.
19 Ibid., p. 241.
Knowledge of how the human mind works is consequently put on a par with knowledge of how glands or molecules work. We need invoke no explanations to be uniquely applied to human beings, and not to things in the world. To grasp this sort of meta-linguistic vocabulary is not to grasp something general, but rather something particular. We are not engaged in the business of setting out the terms under which we can recognise someone to be a rational enquirer. Judgments of rationality are not to be related to elements in the mind, but rather to the ways in which means are adjusted to ends, as well as to the particular ends which are sought. This is to say that rationality attaches to particular behaviours, rather than to a general mental structure.

So any indulgence in the premise that there is a fixed language of thought should not be allowed to slide into the premise that our knowledge of the nature of that language is itself immune to correction on the basis of experience. On the assumption that there are a priori constraints in the form of inner, mental truths, we are led into the notion of foundations of knowledge and theories of representation, with all the attendant problems described thus far. On the other hand, Rorty quotes Fodor as claiming “that the discovery of the language of thought will be a long-drawn-out empirical process”, and that that “has a corollary that we may always be quite wrong about what this language is, and thus wrong about what is a priori”. Because of the uncertainty into which this places the nature of the a priori, and because its empirical character puts it on a par with the sorts of investigations we find in other disciplines, philosophy is ousted from its position of aggrandisement as the tribunal of reason which sits in judgment upon the activities of other human endeavours. “Accurate representation” should come to be seen as simply a belief which helps us to do what we want to do. Grounding “rationality” and “objectivity” in traditional notions of accurate representation “is a self-deceptive effort to eternalise the normal discourse of the day”, when all that discourse truly embodies is agreed-upon criteria for reaching further agreement.20

To sum up, Rorty’s is an extended attempt to “deconstruct the image of the Mirror of Nature”. It is essential to this effort that one see the human self as nothing more than a continually re-woven web of beliefs. There is nothing prior to socialisation or history which can serve to define the human. The desire for a permanent, neutral, ahistorical vocabulary is itself a historical phenomenon. In making this historicist turn, we resist the temptation to seek an escape from time and chance. The world and the self must become de-divinised, so that we can see both in terms of “an expanding repertoire of alternative descriptions rather than The One Right Description”. We no longer think of the world or the self as speaking to us. Epistemology is to be replaced by a new way of looking at things, which will count as “an expression of hope that the cultural space left by the demise of epistemology will not be filled—that our culture should become one in which the demand for constraint and confrontation is no longer felt”. This new hope is embodied in hermeneutics, and Rorty cautions us that we are not to view it

20 Ibid., pp. 11, 252.
as a discipline, a programme of research, or a method of achieving the same sort of results which epistemology failed to achieve.\textsuperscript{21}

I will return to this question of hermeneutics in Chapter Three. I wish now to turn to specific references which Rorty makes to Kuhn's work, in order to demonstrate Rorty's belief in the general similarity of Kuhn's programme to his own. But intelligibility of this account will be facilitated if I first give a few quite general statements elucidating Kuhn's programme for understanding the history and philosophy of science.

The standardly accepted model of science paints a picture of a fairly steady accumulation of individual discoveries and inventions, which accumulation constitutes the modern body of scientific knowledge. Periods when science does operate in this way are described, in Kuhnian terms, as exemplifying "normal science". Kuhn's contention is, however, that, if we take seriously an historical viewpoint of science, we see, rather than this cumulative picture, one in which not only scientific practice, but the entire way of envisioning the scientific field concerned, has undergone a series of variably radical transformations. As a result of such transformations, not only new answers to problems are generated, but the problems themselves are not the same after the shift as they were before.

The periods of upheaval between the dissolution of the old scientific order (for reasons I will explain in the next chapter) and the functional instantiation of the new one are characteristically conceptually and methodologically unusual periods, the work done in such a climate being referred to as "revolutionary science". These are times when the scientific vision which guides research is in transition, and hence somewhat unclear.

This situation reveals a need to pull back from conceptions of scientific truth and knowledge which entail the correspondence of belief to some discoverable absolute state of reality. Kuhn's programme is intended to describe, in the absence of such conceptions, how we might make sense of scientific activity.

Rorty states that "Wittgenstein, Heidegger, and Dewey have brought us into a period of 'revolutionary' philosophy (in the sense of Kuhn's 'revolutionary' science) by introducing new maps of the terrain (viz., of the whole panorama of human activities) which simply do not include those features which previously seemed to dominate.” In the interest of promoting this “revolutionary” outlook, his Philosophy and the Mirror of Nature "is a survey of some recent developments in philosophy, especially analytic philosophy, from that point of view of the anti-Cartesian and anti-Kantian revolution” described at the beginning of the introduction to the book. This implies that Rorty views his project as working to achieve much the same for philosophy as Kuhn's project did for science. Indeed, Rorty goes on to say that “most of the particular criticisms of the tradition which [he] offer[s] are borrowed from such systematic philosophers as Sellars, Quine, Davidson, Ryle, Malcolm, Kuhn, and Putnam.” Further, Rorty “hope[s] to

\textsuperscript{21} Rorty, PMN, pp. 315, 390; CIS, pp. 39-40.
convince the reader that the dialectic within analytic philosophy...has carried...philosophy of science from Carnap to Kuhn”. 22

The basis of what Rorty takes to be his common ground with Kuhn is the shift which he is advocating—and which I have outlined earlier in this chapter—from the more personally based ability to have knowledge which we see in traditional philosophy (whether it be grounded in the having of clear and distinct ideas or the having of raw feels), to knowledge based in the social sanctioning of belief. Hence, “to understand...the superiority of the New Science to Aristotle, the relations between this science and mathematics, common sense, theology, and morality—we need to turn outward, toward the social context of justification rather than to the relations between inner representations.” It is significant that Rorty notes that “this attitude has been encouraged” by, among other things, “Kuhn’s Structure of Scientific Revolutions”. 23

As evidence of this, Rorty says that so-called “observation”, according to Kuhn, is “just a matter of what we can agree on these days”. 24 It appears to me that he reads Kuhn as consequently preferring to drop the notion of observation altogether, 25 a move which would appeal to Rorty. In taking this stance, Kuhn has helped to “exhibit the sterility of attempts to give sense to phrases like ‘the way the world is’, or ‘fitting the facts’ ” 26.

Rorty remarks that there has been a temptation in traditional philosophy to see a new problematic as “the old one rightly seen” 27, which is to say that we get, through our contemplations, a progressively clearer picture of the old problematic, and gradually come to terms with it better; on this view, we find new ways to deal with old problems, and thus become increasingly adept at solving them. Rorty looks to Kuhnian history and philosophy of science as a parallel case which serves to illustrate the inaccuracy of this traditional general philosophical view. In his own field, Kuhn has helped to dispel “the notion of increasingly accurate representations of nature being found in its mirror” 28, viz. the mind. He has done so by essentially demonstrating, in his “criticism of the ‘textbook’ approach to the history of inquiry”, that the above “temptation should be resisted”. Importing this Kuhnian orientation into his discussion of philosophy in general, Rorty advises that, instead of the traditional, cumulative understanding of philosophy, we have “new philosophical paradigms nudging old paradigms aside”, such that “a new set of problems emerges and the old ones begin to fade away” 29.

So Kuhnian “revolutionary science” provides a model, for Rorty, for the fluid discourses which shape the sort of culture which he has advocated in his

22 PMN, pp. 6-7.
24 Ibid., p. 227.
25 Ibid. p. 225.
26 CIS, p. 20.
27 PMN, p. 264.
28 Ibid., p. 276.
29 Ibid., p. 264.
own philosophy. Hence, the Rortian distinction between "normal" and "abnormal" discourse is to be taken as generalising Kuhn's distinction between "normal" and "revolutionary" science. Rorty describes this parallel thus: in "the pragmatic approach to knowledge suggested by epistemological behaviourism," normal discourse is that which can be rendered commensurable (i.e., "able to be brought under a set of rules which will tell us how rational agreement can be reached on what would settle the issue on every point where statements seem to conflict")31. In contrast, abnormal discourse is that which cannot be rendered commensurable (i.e., it lacks rules through which rational agreement might be reached).

For Rorty, the question of the presence or absence of rules guiding rational agreement is not at all a crucial one for a culture. Rorty observes that Kuhn has a distrust of "the positivistic idea that rationality is a matter of applying criteria"32. Thus, Kuhn "gives us reason to say that there is no deeper difference" between the "patterns of argumentation" of science ("as the discovery of what is really out there in the world") and those of "discourses for which the notion of 'correspondence to reality' seems less apposite (e.g., politics and literary criticism)" than "that between what happens in 'normal' and in 'abnormal' discourse."33 In other words, the patterns of argumentation of science are not significantly different from those of any other area of the culture; neither need rely upon rules designed to guide rational agreement. We see this reflected in the following passage in Rorty:

As Kuhn argues in The Copernican Revolution, we did not decide on the basis of some telescopic observations, or on the basis of anything else, that the earth was not the center of the universe, that macroscopic behaviour could be explained on the basis of microstructural motion, and that prediction and control should be the principal aim of scientific theorizing. Rather, after a hundred years of inconclusive muddle, the Europeans found themselves speaking in a way which took these interlocking theses for granted. Cultural change of this magnitude does not result from applying criteria (or from "arbitrary decision") any more than individuals become theists or atheists, or shift from one spouse or circle of friends to another, as a result either of applying criteria or of actes gratuits. We should not look within ourselves for criteria of decision in such matters any more than we should look to the world.34

The similarity between the activities of science and those of other cultural endeavours is echoed in the similarity between philosophy and other cultural endeavours. But this latter understanding runs against traditional philosophy's view of itself as uniquely providing foundations for knowledge, via the mind's capacity to mirror Nature. In the traditionalist climate, "the explicit hope of pre-

30 Ibid., p. 320.
31 Ibid., p. 316.
32 ORT, p. 25.
33 PMN, pp. 332-3.
34 CIS, p. 6.
Kuhnian philosophers of science was to have an account of ‘the nature of science’ which no future scientific revolution could disturb”.\(^{35}\)

The Rortian understanding of hermeneutics (which I will flesh out in Chapter Three) is, roughly speaking, the relinquishment of the desire for an epistemology: the desire to unite people under a common rationality, the desire that there be “a special set of terms into which all contributions to the conversation should be put”.\(^{36}\) Rather, hermeneutics unites people through civility and the hope of agreement; other than this, there is neither common goal nor common ground. Rorty’s promotion of what he terms hermeneutics is another facet of his philosophy in which he sees himself as indebted to the work of Kuhn:

In recent years, debates about the possibility of epistemology as opposed to hermeneutics have gained a new concreteness as a result of the work of T.S. Kuhn....Since the Enlightenment, and in particular since Kant, the physical sciences had been viewed as a paradigm of knowledge, to which the rest of culture had to measure up. Kuhn’s lessons from the history of science suggested that controversy within the physical sciences was rather more like ordinary conversation (on the blameworthiness of an action, the qualifications of an office-seeker, the value of a poem, the desirability of legislation) than the Enlightenment had suggested. In particular, Kuhn questioned whether philosophy of science could construct an algorithm for choice among scientific theories. Doubt on this point made his readers doubly doubtful on the question of whether epistemology could, starting from science, work its way outward to the rest of culture by discovering the common ground of as much of human discourse as could be thought of as “cognitive” or “rational”.\(^{37}\)

Rorty goes on to say that Kuhn’s examples of revolutionary change in science are “cases of the sort which hermeneutics has always taken as its special assignment”. Hence, “Kuhn wished to oppose the traditional claim that ‘what changes with a paradigm is only the scientist’s interpretation of observations that themselves are fixed once and for all by the nature of the environment and of the perceptual apparatus’ ”. Kuhn’s embracing of the hermeneutic method is disturbing to those philosophers of science, oriented towards the epistemological tradition, who were looking for a “neutral scheme”, or neutral observation-language. They saw Kuhn as “endanger[ing] the notion of theory-choice in science”, since philosophers of science had “envisaged [themselves] as providing an algorithm for theory-choice”. But “Kuhn was right in saying that ‘a philosophical paradigm initiated by Descartes and developed at the same time as Newtonian dynamics’ needed to be overthrown”.\(^{38}\)

Rorty describes both himself and Kuhn as sharing the view of “pragmatism”: an anti-realist position which “gives up the attempt at a God’s-eye view of things, the attempt at contact with the nonhuman which [Rorty] has been

\(^{35}\) Ibid., p. 25n.
\(^{36}\) PMN, p. 318.
\(^{37}\) Ibid., p. 322.
\(^{38}\) Ibid., pp. 323-5.
calling 'the desire for objectivity'. He takes it that Kuhn would agree (as Rorty, himself, does) with Putnam's admonition that we "drop the notion of a God's-eye point of view". He describes Kuhn as espousing a "'pragmatist' holism", explaining that [w]hat [he is] calling 'pragmatism' might also be called 'left-wing Kuhnianism'. He states that the "general pragmatist claim" is that "there is no permanent ahistorical metaphysical framework into which everything can be fitted". A Kuhnian corollary of this Rortian thesis is that "there is no single commensurating language, known in advance, which will provide an idiom into which to translate any new theory, poetic idiom, or native culture". Referring to his *Philosophy and the Mirror of Nature*, Rorty observes that "Dewey, Wittgenstein, Sellars, Kuhn, and the other heroes of this book all have their own ways of debunking 'truthfulness to reality in the sense postulated by philosophical realism'".

Rorty observes that Kuhn's "pragmatist friends (such as [himself]) routinely congratulate him on having softened the distinction between science and non-science". In the time "[b]efore the arrival of Kuhn, Toulmin, Feyerabend, and Hanson, it was often thought that the physical sciences were...paradigmatically rational areas of culture". In the "pre-Kuhnian philosophy of science...rational enquiry was a matter of putting everything into a single, widely available, familiar context--translating everything into the vocabulary provided by a set of sentences which any rational inquirer would agree to be truth-value candidates".

I will move now to a detailed exegetical examination of the Kuhnian programme, taking as its source writings supplied by Kuhn, himself. This will prepare us for the analysis, in the third chapter, of how well Rorty's expectations of how Kuhn's work might aid his own project measure up against the ways in which Kuhn describes his own work.

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40 Ibid., p. 27.
41 Ibid., p. 65.
42 Ibid., p. 38.
43 Ibid., p. 215.
44 *PMN*, p. 382.
45 *ORT*, p. 38.
46 Ibid., p. 95.
In his groundbreaking *Structure of Scientific Revolutions*, Thomas Kuhn was concerned to replace what he saw to be a misguided conception of the scientific enterprise with one he believed more accurately to describe the way in which science actually proceeds. According to the standardly accepted model, science develops and progresses through a process of accumulation of individual discoveries and inventions. This accumulation constitutes the modern body of scientific knowledge. The history of science is thus presented as an account of “at what point in time each contemporary scientific fact, law, and theory was discovered or invented”, and by whom. Kuhn contends, however, that the “[c]umulative acquisition of unanticipated novelties proves to be an almost non-existent exception to the rule of scientific development.” If we take the historical viewpoint seriously, we see “that science does not tend toward the ideal that our image of its cumulativeness has suggested.” In fact, “most new discoveries and theories in the sciences are not merely additions to the existing stockpile of scientific knowledge.”

Kuhn was alerted to the inadequacy of the standard view when it struck him that, in a typical contemporary study of such theories as, for example, Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics—all now considered to be out of date—these discarded theories would be deemed incorrect or even unscientific, i.e., not yet science. But to do so is to ignore the presence and importance of the actual historical development of science. The mere fact of its having been replaced by a newer theory is insufficient evidence by which to hold an older theory to be unscientific. The key to understanding science lies not in our seeking to determine the contributions of an older science to our present science, but rather in our attempting to retain the integrity of that older science in respect of its own time. This means that a particular science of any age ought to be studied from a viewpoint wherein it possesses “the maximum internal coherence and the closest possible fit to nature.” Such a historical view entails the study of particular theories within the context in which they achieved prominence.

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1 Kuhn, *SSR*, pp. 2, 96, 140; *ET*, p. 226.
2 *SSR*, pp. 2-3.
That out-of-date theories cannot be accurately deemed simply unscientific points to the lack of a set of overarching conceptual and practical directives which might be used to dictate the conclusion to any scientific question. Instead, the framework of conceptualisation and practice has undergone a series of transformations throughout scientific history. A researcher at any point in time brings a certain scientific background to her investigations. Further, scientific conclusions are influenced by such factors as accidents and the individual make-up of the researcher. These are all temporal elements. So there is no neatly packaged knowledge of "what it is to be scientific" which can be applied to scientific problems. If there were, there would be neither need nor place for a specifically historical study of science.

Nevertheless, out-of-date theories can be recognised as having been produced by a roughly similar sort of activity to, and for the same sorts of reasons as, current scientific knowledge and research. The difficulty of the cumulativist view of science is underscored when we look more closely at the two possible ways in which we can consider out-of-date theories: as myths or as science. If they are to be relegated to the mythical, then their rough similarity to current science is puzzling. If we then decide that they are indeed to count as science, then we must conclude that science has, in the past, included bodies of belief which are strikingly incompatible with present beliefs.

That there is some contextual set of received beliefs which the researcher brings to his study of scientific problems indicates that, while observation and experience—the foundations of the empiricist view of investigation—must have a hand in directing the range of admissible scientific belief, they do not alone determine the body of that belief. Instead, "research [is] firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice". This "tradition-bound" conduct is the essence of science as it is generally practiced, i.e., during times of scientific stability, and it is what Kuhn has labelled "normal science."

Normal, or Ordinary, Science

The most obvious feature of Kuhnian normal science (as it is described in his earlier writings) is the presence of a "paradigm", which Kuhn defines generally as a situation in which accepted examples of scientific practice (including law, theory, and instrumentation) provide a model leading to a coherent tradition of scientific research. The paradigmatic nature of what is today familiar to us as science can be seen in sharper focus when we consider what Kuhn (in earlier writings) referred to as "pre-paradigm science". This is exemplified by the state of physical optics before the time of Newton. In these early stages of a science's

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3 Ibid., pp. 4, 6, 10.
development, one sees at most the existence of various "schools". Since, for Kuhn, the interpretation of data is only possible where there is at least some foundation of theoretical and methodological belief, each of these schools would have to possess its own foundation, and it was in the context of such a matric foundation that theory-selection, -evaluation, and -criticism would go on. Because such an underlying body of belief is thus implicit in the collection of facts, more than "mere facts" are at hand. Although there was, for each school, a grounding of agreed-upon scientific practice and vision within which work was carried on, "fact-gathering" in pre-paradigm science was still a more random activity than it has been in paradigm-based science (because of the plurality of research matrices in the former). Since there was no single standard conceptual and methodological corpus adopted by the scientific community either as a whole or in a particular area of study at any given time, a scientist's choices in terms of experimentation and observation were made relatively freely. In the absence of a universally accepted paradigm (at either the whole scientific or the disciplinary level), all of the facts that could possibly pertain to the development of a given science could seem equally relevant.

In the history of science, however, this initial divergence in schools of thought was eventually to disappear in the various scientific disciplines, as a result of the triumph, in each discipline, of one of the pre-paradigm schools. In order to accomplish this, the body of theory and practice of the successful school had to become accepted as a paradigm which was to guide the work of all researchers in a given field. It had therefore to seem better than its competitors, although it need not explain all the facts it was to encounter. The acquisition of a paradigm is a sign of maturity in the development of a particular scientific field; it is an indication of a new level of confidence which encourages researchers to undertake more detailed, precise, and esoteric work. This is because the constant re-examination of fundamentals which dogged pre-paradigm science is obviated by the more tightly directed fact-collection and theory-articulation demonstrated in paradigm-based science; energy previously directed towards the continual setting of foundations could be shifted into increased technical efficiency and more specialised problems. Scientific work could afford to become more esoteric and oriented towards puzzle-solving once its foundations could be taken for granted by experimenters. This signalled a new attitude wherein current theory came to be more exploited, and less criticised. The transition to a mature science is marked by the diminishment of critical discourse. Hence, as each discipline, or area of research, developed sufficiently, non-dominant schools gradually disappeared, through the conversion of seasoned practitioners, and the attraction of most of the next generation's practitioners, to the newly ascendant paradigm, as well as through those schools being ignored by the new scientific mainstream. Paradigmatic dominance is exemplified by Ptolemaic or Copernican astronomy, Aristotelian or Newtonian dynamics, and corpuscular or wave optics.

Ibid., pp. 10, 13, 15-17.
One of the basic characteristics of a paradigm, then, is that its achievements are sufficiently unprecedented so as to attract adherents away from competing models. But this novelty cannot be a continuing focus of researchers if the paradigm is to fulfill its essential role in the developmental progress of the field. Kuhn stresses that normal science does not aim to produce unexpected novelties, and scientists normally do not aim to invent new theories. When normal science is successful, novelties of fact or theory are not discovered; in fact, if any such novelties threaten to disturb paradigm stability, they are typically suppressed. The congealment of scientific attention into a successful paradigm is a result of that paradigm's ability to achieve anticipated research results in a new way, a way which constitutes an improvement over the collective abilities of the various schools of the pre-paradigm period, or, as we shall see later, over the abilities of the previously enshrined paradigm (in the case of an already-matured discipline).

As I have mentioned, it is in Kuhn's earlier writings that he refers to the science typified by the various schools of thought extant prior to the coming into dominance of a single disciplinary matrix as "pre-paradigm" science. Somewhat later, he sought to qualify this by observing that disciplinary matrices are indeed possessed by any scientific community, including these schools of the so-called pre-paradigm period: "a rigorous training in convergent thought has been intrinsic to the sciences almost from their origin."\

Paradigm-function in relation to research-problems is a crucial feature of science. Scientists are fundamentally "puzzle"-solvers, the dominant criterion for most of them being the ability to recognize and solve puzzles posed by Nature. Were it not for the stabilizing effect of the paradigm, the requisite security for the scientist's tackling problems, especially those of a more esoteric type, would be lacking. When engaged in a normal research problem, the scientist must premise current theory as the "rules of his game". The work of science is thus composed of these instrumental, conceptual, and mathematical puzzles, to which it must always be expected that there will eventually be a solution, else commitment to the scientific enterprise would wane. The corpus of accepted current theory bestows meaning on the problems of normal research, lending support to the assurance that a solution to the puzzle will be found. Problems which appear potentially insoluble are therefore not considered to be scientific problems: they may be rejected as metaphysical, as part of another discipline, or as being too problematic to be worth the effort.\

The paradigm, then, functions to provide criteria for choosing problems that can reasonably be assumed to have solutions. Within it, the range of anticipated and assimilable results is narrowed from the much larger field supplied by the experimenter's imagination. It serves to limit the nature of acceptable solutions and the steps by which they might be obtained. Research-outcomes which fail to fall within this normal range will, under the conditions of

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5 ET, pp. 228, 295.
6 SSR, pp. 35-7, 205; CGK, p. 4.
normal science, be deemed research-failures. So, what is being tested in normal research is the individual scientist's ability to solve his puzzle; what is not being tested is the corpus of current science, which is instead required to form the background against which puzzle-solving is done. Normal science thus "seems an attempt to force nature into the pre-formed and relatively inflexible box that the paradigm supplies", and new sorts of phenomena which do not fit into this box are often not seen at all. Paradigm procedures and applications, laws and theories serve to "restrict the phenomenological field accessible for scientific investigation at any given time", thus allowing the scientific endeavour to have a conceivable method of research. Nevertheless, the body of admissible procedure cannot be over-specified: the paradigm must remain sufficiently open-ended so as to leave problems for it practitioners to resolve. But behind the issue of problem-choice, the strictures of the paradigm provide guidance in respect of favoured conceptual, theoretical, and methodological commitments, as well as of preferred types of instrumentation and the ways in which accepted instruments may legitimately be employed; it may be necessary, for instance, that an experimental apparatus be redesigned in order that it produce results of a better fit with accepted theory.\footnote{SSR, pp. 10, 24, 35, 38-40, 42, 60; CGK, p. 5.}

The initial success of an adopted paradigm, however, is generally neither complete nor widespread. It is instead largely a promise of success in some areas. The settled regime of normal science is typified by the actualisation of a significant amount of what has been promised by the new paradigm. In this way, the paradigm becomes more accepted, but as it does so, it also becomes more elaborately articulated. Scientific problems are increasingly modelled upon previous achievements. Much scientific research within a tradition is an attempt to adjust existing theory and observation into progressively closer agreement. It also involves extending existing theory into areas which it is expected to cover, but to which it has not yet been applied. Some research will thus be directed towards finding new applications of the paradigm, or towards increasing the precision of applications that have already been introduced. In this process of elaboration, a paradigm may be somewhat reformulated. The significant point here is that none of these problems or adjustments would be considered worth undertaking by anyone who had basic doubts about the validity of existing theory, or who leaned towards a new theory.

As the discipline matures through the further articulation and success of the adopted paradigm, we see increasing professionalisation and specialisation, accompanied by a growing resistance to paradigm-change. Moreover, the development of an initial paradigm spells the end of any research done outside of a paradigm. All future research will be contained within one such matrix or another. Kuhn observes that the sciences acquired something like paradigms "at precisely the point when the field began to make rapid and systematic progress".\footnote{SSR, pp. 64, 79; ET, p. 230.}

It is crucial, in understanding Kuhn, to recognise that he regards science as an importantly communal enterprise. The paradigm achieves its success by virtue
of its acceptance by the relevant scientific community as a whole, the group of practitioners of a scientific specialty. Indeed, the paradigm may be said to govern not a subject-matter, but rather a group of practitioners. Paradigms are a demonstration of consensus.

Scientific communities, of course, exist at different levels. Within the overarching group of natural scientists, we can identify the divergent specialisations represented by astronomers, physicists, chemists, and zoologists, for example. At a level of further professional refinement, we can distinguish, say, solid-state from high-energy physicists, and organic from inorganic chemists. Yet further splitting up of scientific domains occurs as disciplines mature and their paradigms become ever more esoteric and narrowly focused. The main body of each group, at whatever level, is united around the subject of its members' highest educational degree, membership in professional societies, and publications in journals.

Consequently, paradigms will also exist at different levels. Some will guide the work of a broader group of researchers; others will be of relatively more narrow scope. For instance, within the large and diverse community of physical scientists, all will have learned the laws of quantum mechanics, but although they share the basic matrix, they do not all learn the same applications of these laws. Solid-state physicists are not working within quite the same paradigm as are chemists. Beyond this, even within the confines of the more precisely specified paradigm, two individual researchers may differ in the judgments which they draw from it. Scientists can agree on their identification of the paradigm in general without agreeing on their interpretation of it. Nevertheless, this disparity does not interfere with the paradigm’s ability to guide research: its guidance will just be manifested in slightly different ways.

In order to look with greater resolution at the paradigmatic framework of conceptualisation and practice in the sciences, we must turn to Kuhn’s later revisions of his own terminology, initiated in the wake of criticisms that his notion of a paradigm, as presented in *The Structure of Scientific Revolutions*, was not sufficiently clear. Kuhn’s later programme refines this notion by substituting the terms “disciplinary matrix” and “exemplars” for the term “paradigm”. The disciplinary matrix of a professional discipline is composed of three basic elements:

1. symbolic generalisations: the formal (or readily formalisable) components of the matrix;
2. models: components which provide the permissible and preferred analogies and metaphors, or even an ontology;
3. exemplars: concrete problem-solutions; the professional community’s standard examples, to which students are heavily exposed in their training (in the form of textbooks, lectures, and laboratory exercises); and
4. values: e.g., theories are expected to be accurate, precise, simple, plausible, and compatible with other current theories, as well as being self-consistent; they must permit puzzle-formation and -solution, and are ideally relatively
more quantitative than qualitative; unanimity within the community is a paramount value; the scope of a theory is also to be considered.

Symbolic generalisations allow the application of logic and mathematics to problems. An example would be the formal equation $f = ma$, or the verbal expressions “action equals reaction” and “elements combine in constant proportion by weight”. Less specifically interpreted, these belong to the broader scientific community. But different subgroups of that community will more specifically interpret and apply them in different particular ways.

Models which express analogies are of clearly heuristic usage, such as when one describes a gas as behaving like billiard balls in random motion. An example of a model intended to provide an ontology is found in the explanation of perceptible phenomena as being due to the motion and interaction of atoms in the void.

Exemplars provide a demonstration of and practice in problem-solving which help the student set the Gestalt which is typical of the disciplinary matrix. They allow the student to assimilate “a time-tested and group-licensed way of seeing”\textsuperscript{9}. The student “is learning the language of a theory and acquiring the knowledge of nature embedded in that language”\textsuperscript{10}. This will prove to be essential training for the prospective scientist, since researchers solve puzzles by modelling them on previous puzzle-solutions. Scientists “never learn concepts, laws, and theories in the abstract and by themselves...[i]nstead, these intellectual tools are from the start encountered in a historically and pedagogically prior unit that displays them with and through their applications”\textsuperscript{11}. Thus, we see that the cognitive content of science is not strictly embedded in theory and rules or criteria, with exemplary problem-sets serving as a means through which students can merely gain facility in the application of the theory. A great deal of this content is also carried in exemplary problem-sets.

Values tend to be more widely shared among various scientific communities than are symbolic generalisations and models. Although they may be shared generally throughout a given scientific community, they may not be applied in the same way within that community.

Kuhn insists that, rather than encountering the world in simplistic empiricist fashion, equipped only with a fresh tabula rasa, the science student is instead “programmed”, in her training, to recognise what her prospective community already knows. She learns that the meaning of her community’s endeavours inheres in its members’ ability to apply labels unequivocally and draw conclusions as to what might be further expected. The matrix which is learned in scientific education is then reinforced by subsequent life in the profession.\textsuperscript{12}

We might be tempted to characterise this structure as a collection of four fundamental types of rule-based activities. On this view, symbolic generalisations

\textsuperscript{9} SSR, p. 189.
\textsuperscript{10} CGK, p. 272.
\textsuperscript{11} SSR, p. 46.
\textsuperscript{12} Ibid., pp. 43, 46, 182-90; CGK, pp. 21, 241, 272; ET, pp. 227, 297, 312.
would provide one set of rules, models another, and so on. But Kuhn contends that the existence of a disciplinary matrix need not imply the presence of rules. Instead, scientists possess something more like an intuitive knowledge of what will count as research which fits the disciplinary matrix. Indeed, if the philosophers whose interest it is to pursue the abstract structure of science ask scientists to come up with the “correspondence rules” which they use in their business of problem-solving (allowing them to attach symbolic representations to Nature), scientists often deny the relevance of such rules, and if they do not, the “rules” that they do provide may well vary from one individual to another, and may, in any case, be defective. This is not to say that there is no place for rules, or criteria, in the explanation of how science works. It is just to say that, if we are able to identify such criteria, it is because they have been abstracted from an already functioning disciplinary matrix, rather than being the prior determinants of that matrix.

In Kuhn's estimation, we have no access to elements more minimal than sense-data reports, such as might be exemplified by “green there”. In some sense, we can view these as “the given”; in this respect, they are construed experientially. But understood theoretically, it is rather stimuli which rank as given (although we have access to these only indirectly, via scientific theory), since we can consider sensations to be the result of a vast amount of neural processing of stimuli.\(^\text{13}\)

How does this experiential fundamentality of sensations relate to science as non-criterial? Kuhn offers an analogy in which one is teaching a child to discriminate among ducks, swans, and geese. The child will come to be able to pick out swans as all similar to each other, for instance, but may well be unable to tell you what a swan is, which is to say that she will be unable to specify criteria for its identification and discrimination from ducks and geese. Her learning has been primarily by ostension. Similarly, learning by ostension is essential to the reconstruction of scientific knowledge: in both cases, such learning involves neural re-programming through reinforcement and correction. Elements within the disciplinary matrix are related to each other on the basis of their common familial characteristics, by the “network of overlapping and crisscross resemblances”. The prospective scientist undergoes a similar process to the child's, in learning to work with the exemplars of his prospective scientific community; “exposure to a series of exemplary problem-solutions teaches [him] to see different physical situations as like each other”. The practice of normal science depends partly upon this learned ability to group objects and situations into similarity-classes, without an answer to the question “Similar with respect to what?”\(^\text{14}\)

Seeking out criteria for picking out the similarities and differences on which any interpretation of data will depend is thus obviated by this focus on the acquisition of exemplars, which provides the student with training in seeing the

\(^{13}\) \textit{ET}, p. 308.  
Gestalt which is embodied in the disciplinary matrix. The science student’s basic qualification in solving exemplary problems “is a perception of similarity that is both logically and psychologically prior to any of the numerous criteria by which that same identification of similarity might have been made”. Further, “an acquired ability to see resemblances between apparently disparate problems plays in the sciences a significant part of the role usually attributed to correspondence rules.” Through her practice in working with exemplars, the science student will come to see new problems as analogous to previously solved problems. There is a form of correspondence here, in that she sees in the new problems the attachment to Nature of the symbolic consequences of old problems. But despite this emphasis on exemplars, Kuhn advises that it is not “likely that very much human knowledge is acquired and stored with so little recourse to verbal generalizations”; the effective ostension provided by exposure to exemplars must be combined with modelling and symbolic generalisations. Still, exemplars and models tend to be more effective determinants of community substructure than are symbolic generalisations, perhaps because the former are more completely specified, while the latter remain relatively more sparse. Overall, it is largely in the shared examples of the community that scientists assimilate and store knowledge.²⁵

A similarity-based scientific programme is preferable to one based on criteria partly because the stricter class-boundaries of the latter (that is, between familial groups identified and dealt with by the current theory) tend to foster a situation in which class-boundaries will have to be altered more frequently, in order to accommodate new data. The more “primitive” resemblance-programme handles cases of new phenomena more often unproblematically, and without continual adjustment. However, “[t]here are appropriate occasions for switching to the well-known strategy that relies upon boundaries and rules”: rules become advantageous when the need to resolve a particularly troublesome quandary arises.²⁶

It is primarily philosophers who tend to want to study scientific examples and derive correspondence rules from them. But this practice has the effect of distorting the view of the way in which the scientific community gathers knowledge, by substituting one means of data-processing (rule- or criterion-based) for another (similarity-based). Hence it generates an inaccurate picture of the way in which scientific knowledge is acquired and stored. Knowledge develops differently through disciplinary matrices in general, and exemplars in particular, from the way it develops through rules. If the cognitive process were truly to follow a fundamentally rule-based pattern, it would result in a considerably weaker endeavour. Manipulation of exemplars may be construed as practice in the application of rules, but if it is, then we must recognise it as an unconscious sort of practice, since no rules are made explicit in the passing on of these problem-sets to the student. These shared examples provide a sort of tacitly embedded knowledge. Rules are actually isolated after the fact, primarily as an activity of

²⁵ ET, pp. 306-8, 313.
²⁶ Ibid., pp. 316, 318.
philosophers wishing to erect an abstract procedural structure. They should not be viewed as the hallmark of objectivity, as concrete puzzle-solutions should not be viewed as "subjective" or "intuitive"; the latter are undeniably systematic and corrigible.

A typical and misguided way of understanding exemplars is as evidence for the theory of which they are applications. But, although "some of them were part of the evidence at the time actual decisions were being made...they represent only a fraction of the considerations relevant to the decision process." By the time many of these exemplary applications were devised, the decisions pertaining to theory-choice of which they may appear to be evidence have already been made. Supplying evidence is not their primary pedagogic function. If they were offered to science students with such an intent, the composers of textbooks would be guilty of "an extreme bias", through having failed to offer alternative theories for equal consideration, as well as any evidence against the accepted matrix. In addition, as anyone who has been a science student knows, students do not accept theories on the basis of having favourably weighed the evidence for them as presented in exemplars, but rather on the authority of teacher and text. Clearly, students have not the scientific competence to do otherwise. 17

Scientific Crisis, and the Transition into Revolutionary, or Extraordinary, Research

If normal science were all there were to the scientific endeavour, we would have the cumulative model which I have indicated above that Kuhn has laboured so strenuously to dispel. An accurate study of the history of science reveals that the stability of the matrix of many scientific disciplines has, from time to time, been upset, a new matrix subsequently eventually emerging. Such upsets are generally caused by the accumulation of a collection of anomalies, discoveries which are at odds with the current body of theory.

An anomaly is detected when it is recognised "that nature has violated the paradigm-induced expectations that govern normal science"; something has occurred which cannot "be aligned with professional expectation". This may take the form of a problem which ought to be solvable by known rules and procedures resisting solution, or of the failure of a piece of equipment. Initially in the period of stability, generally only the anticipated and the usual are experienced. Researchers at first tend to see only the types of phenomena for which previous experience within the matrix has equipped them; hence, the occasional discrepancy which crops up will not at this point be noticed. As the same discrepancies appear repeatedly, researchers will begin to become aware of them. Nevertheless, they are usually resolved by normal means. This may involve a minor adjustment of the matrix, so that the anomalous becomes the expected.

17 SSR, p. 80; ET, p. 327.
Some discrepancies will resist resolution, however. Such problems may be ignored, if the researcher deems it more scientifically profitable to move on to attempt to solve other problems, setting aside recognised counter-instances for later work.\textsuperscript{18}

If unsolved anomalies cannot reasonably be placed aside--for instance, if too many of them have accumulated--then a certain amount of energy within the scientific community concerned will be directed towards their being eventually resolved. At some point, however, the accumulation of discrepancies will reach its critical mass, so to speak, at which point it is fair to say that a state of crisis has hit the discipline. There is the reappearance of a significant amount of discourse of a critical flavour. Scientists begin to lose faith in the adequacy of their disciplinary matrix. Although they tentatively consider alternatives to it, they continue to resist its full-scale renunciation. Anomalies are still not treated even at this time as genuine counter-instances of the matrix. Increasing numbers of researchers rally to devote their professional attention to these difficulties; as a result, anomalies come to be isolated more precisely. Any rules which may have been abstracted and identified as exemplary of the matrix loom larger as its security begins to show strain; they provide a relatively more substantial handle which adherents to the old matrix are able to grasp, as they labour to stretch that matrix around persistent anomalies. If scientists demonstrate a tendency to venture into the terrain of philosophical analysis as a help with their problems--that analysis being, as mentioned previously, occupied largely with the abstraction of a criteriological skeleton for scientific research--they will do so particularly at this time. Kuhn states that

[i]t is no accident that the emergence of Newtonian physics in the seventeenth century and of relativity and quantum mechanics in the twentieth should have been both preceded and accompanied by fundamental philosophical analyses of the contemporary research tradition. Nor is it an accident that in both these periods the so-called thought experiment should have played so critical a role in the progress of research....the analytical thought experimentation that bulks so large in the writings of Galileo, Einstein, Bohr, and others is perfectly calculated to expose the old [matrix] to existing knowledge in ways that isolate the root of crisis with a clarity unattainable in the laboratory.\textsuperscript{19}

Looking, for example, at the Copernican Revolution, we may note that, as discrepancies were discovered in Ptolemy's astronomical system, astronomers were at first able to eliminate them as problems by making adjustments in the system. But as this process continued, the system had to become ever more complex, in order to encompass, as newly solved problems, what had been discrepancies within it. With time, the complexity of the system outstripped its accuracy. Further, manipulation of the system in one place often distorted it in

\textsuperscript{18} SSR, pp. 5-6, 52, 64, 81-2, 112-13, 186.
\textsuperscript{19} Ibid., p. 88.
another, giving rise to yet more new anomalies. Ptolemaic astronomy had reached a state of crisis.\textsuperscript{20}

This account of the increasing manifestation of anomaly as a prelude to crisis clearly depends upon an acceptance of the description of normal science offered above. Without this special apparatus (viz., normal science) which is constructed for the purpose of describing and dealing with anticipated functions, the results that lead ultimately to novelty would not be recognised as such. The detection of anomaly occurs \textit{because} the scientist knows, with precision, what to expect in his investigations. Anomaly must be highlighted against the background provided by the disciplinary matrix of normal science. Thus, the more precise the matrix, the more sensitive an indicator of anomaly it is. Paradoxically, then, normal science prepares the way for its own temporary demise, leading to change in the regime.

During the crisis-period, the disciplinary restrictions which are essential to normal science are naturally relaxed, as the previously accepted matrix functions increasingly less effectively. A typical symptom of matrix-crisis is the proliferation of a number of different "articulations and ad hoc versions" of the extant theory; these are offered as attempts to stretch the theory such that it might be capable of contributing a solution to intransigent anomalies. Otherwise, anomalies threaten to remain outside of any structure whatsoever, which would render scientific progress considerably more elusive. The result is increasing theoretical vagueness and decreasing theoretical utility, as the proliferations make it progressively more difficult to identify what the theory actually is. The decaying matrix guides research which more and more resembles that characteristic of the period prior to the dominance of any one matrix, what Kuhn had earlier called "pre-paradigm" science. Kuhn notes that "by proliferating versions of the [matrix], crisis loosens the rules of normal puzzle-solving in ways that ultimately permit a new [matrix] to emerge." This is the domain of "extraordinary", or "revolutionary" science which is clearly of a more turbulent nature than that produced in normal periods.\textsuperscript{21}

At this time, scientific attention is concentrated on a narrow area of trouble, and anomalies are more consciously recognised as such than they were before. Normal science, in fact, leads to revolutionary science much more surely than would pre-paradigm science, because it "isolates for continued and concentrated attention those loci of trouble or causes of crisis upon whose recognition the most fundamental advances in basic science depend".\textsuperscript{22} Still, the impending full breakdown of the matrix is only very rarely explicitly recognised as such. The atmosphere is rather one of at-times-overwhelming confusion: about how Nature is to be viewed, and about the conceptual framework and methodology which guide research. The relaxing of the old order allows new things to be seen, and/or old things to be seen in a new light. Commitment to the

\textsuperscript{20} Ibid., p. 68
\textsuperscript{21} Ibid., pp. 24, 71-2, 78, 80, 87.
\textsuperscript{22} ET, p. 234.
old paradigm has weakened, thus freeing up the vision of researchers, increasing its plasticity so that new visions may be forged. Progressively greater numbers of researchers become estranged from what has been traditional, and behave in an ever more eccentric manner in relation to that tradition. The speculative and less fully articulated theories of the crisis-period thus play an important role, in that they point the way to discovery, whether or not such discoveries are indeed the ones anticipated by the speculative theories. Sometimes what will ultimately win out as the new dominant matrix is foreshadowed fairly quickly and directly in the shreds of structure which scientists are attempting to build through their extraordinary research:

Often a new [matrix] emerges, at least in embryo, before a crisis has developed far or been explicitly recognized...one can say only that a minor breakdown of the [matrix] and the very first blurring of its rules for normal science were sufficient to induce in someone a new way of looking at the field. What intervened between the first sense of trouble and the recognition of an available alternate must have been largely unconscious.  

For that matter, a new matrix may be anticipated long before a crisis point even appears on the horizon. This was the case when Aristarchus proposed a heliocentric astronomical system in the Third Century B.C., during the period when Ptolemaic geocentric astronomy was the norm. Actual crisis and matrix-transformation did not occur until Copernicus' time. Generally, however, new theories are not glimpsed prior to the crumbling of the old, and further, the final stage of the emergence of the new way in which ordered data will be produced is inscrutable for quite some time even after the discipline has entered into crisis.

In general terms, what will eventually be revealed as a superior matrix to the old is that which exhibits the most coherent body of observation and theory, still being connected to the sensory input (the “given”) which forms part of the grounding of observations. In simple terms, we might say that the preferred theory is the one which best fits the facts, although this may be thought to put the case in an overly empirical way. I will have more to say on this, in relation to considerations of what constitutes truth and knowledge, later.

The new order must therefore promise to resolve problems left unresolved by the old matrix, while at the same time, promising to preserve to a significant degree the problem-solving ability that has accrued to science through previous theories. During this transition period, there will be a large, although incomplete, overlap between the problems that can be solved by the old and the new orders. Counter-instances to the old matrix help to permit the emergence of a new one in which they are no longer a source of trouble: within the new order, they may even appear tautologous. But although, to scientists, the ability of the new matrix to solve problems more effectively than the old is generally “the most significant and persuasive” factor, it may not itself be a compelling force in theory-change.

Sometimes, when it is first proposed, the new matrix has still not solved many of

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23 SSR, p. 86.
the problems confronting it, and it may not have dealt convincingly, or effectively, even with those it has solved. More decisive arguments for the new matrix tend to present themselves after it has had time to become more well-developed. These arguments will then pass into the body of normal-scientific theory.

Alternatively, values other than problem-solving ability may be applied, with variable amounts of weight. A shift may be facilitated if the new matrix displays a strikingly better quantitative precision than the old one, or if it is simply more quantitative. Again, in the initial stages of the shift, there may be benefits of the new theory which are not consciously appreciated. We see this, for instance, in general relativity's ability to account with precision for the anomaly which had emerged in the motion of Mercury's perihelion, but Einstein had not indeed anticipated this. Later revelations of this sort (which result from routine performance of research within the replacement-matrix) stimulate a somewhat delayed vote of confidence for that matrix.

Still another value which may be invoked is aesthetic appeal: the new matrix may be neater, simpler, or considered more suitable or appropriate than the old. This tends, at times, to be a rather weaker factor than some of the others, however, on account of the crudeness of the early versions of most matrices; aesthetic appeal is not initially particularly well-developed. By the time the aesthetic force of a theory is appreciated, other means of persuasion have already taken effect. This is not to say, though, that the power of this appeal ends up to be insignificant; it may still advocate strongly for the new theory, albeit with some delay, and it can be helped in this respect if scientists who respond positively to aesthetic advantages are themselves important within the scientific community.

Recourse to these and other values in judging the appropriateness of a new matrix are crucial to the progress of science. Kuhn states that "if a new candidate for a [matrix] had to be judged from the start by hard-headed people who examined only relative problem-solving ability, the sciences would experience very few major revolutions". Sometimes, it is "only personal and inarticulate aesthetic considerations" that can "make at least a few scientists feel that the new proposal is on the right track." Kuhn cites two examples: when first introduced, Copernicus' astronomical theory did not have many grounds of appeal other than this, and Einstein's general theory of relativity drew mathematicians principally on aesthetic grounds. So acceptance of a new matrix grows through the interplay of a number of values which, in any given case, will exhibit different strengths in persuasiveness.

A new interpretation of Nature, which will be offered up as a superior successor to the old and now-inadequate theory, emerges first in the mind(s) of one or a few individuals. Since new matrices are born from the soil of old ones, they do incorporate much of the vocabulary and apparatus of the old. At least part of the achievement of any matrix will thus prove to be more permanent.

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24 Ibid., pp. 78, 169.
Additionally, a revolutionary change may not affect the whole matrix, but only one application of it. Overall, though, new matrices wield the tools of the old in a new way, setting up novel relationships between the elements involved. In this way, the new matrix solves anomalies, revealing new regularities not previously known.

In order to appreciate the magnitude of change instituted by a matrix-shift, and thereby dissociate it from the traditional picture of simply cumulative science, we need to look more closely at the state of “incommensurability” which characterises the gulf between successive matrices. After a shift to a new theoretical and experimental structure, we can say, in some sense, at least, that scientists find themselves in a different world from the one in which they were working before. An example will illustrate this: Aristotelians saw a swinging suspended stone finally coming to rest according to their belief that a heavy body is moved by its own nature from a higher position to a state of natural rest at a lower one. The stone swung back and forth because it was falling with difficulty, being constrained by the string into this tortuous back-and-forth pathway. Galileo, however, saw in this arrangement a pendulum: a body that almost succeeded in repeating the same motion *ad infinitum*. Much of his new system of dynamics was constructed around the properties of this pendulum, and was governed by a considerably different way of seeing than that which Aristotle had possessed: where Aristotle had seen a falling stone, Galileo had seen a pendulum. The new Galilean order was born out of a “flash of intuition”, which revealed a novel way of seeing the world. The researcher who offers up a new matrix for consideration must then toil (along with any who choose to risk following him at the outset) to develop this programme to the point where it may function as a convincing instrument for scientific progress. Kuhn cautions, though, that the changes wrought through a matrix-shift, being less than total, ensure that the scientists after the shift are, to some degree, still looking at the same world as before. Much of the language and instrumentation remain the same, although they are often employed differently from before.\(^{26}\)

It should be clear from this example that the proponents of different matrices (e.g., Aristotle and Galileo) effectively set different standards for what is to count as proper science. We can achieve finer resolution of this point if we look at still further shifts in the evolution of dynamics. For example, under Aristotelian and Cartesian dynamics, a theory of motion was expected to explain the *cause* of the attractive forces between particles of matter. But Newton’s dynamics implied that this sort of explanation was unnecessary, since all the theory had to do was note the *existence* of such attractive forces. Hence, with the adoption of Newton’s theory, the question of the explanation of the attractive forces was essentially no longer considered to be a scientific concern.

The radical nature of the difference of these standards renders pre- and post-revolutionary normal-scientific traditions (i.e., the old and the new matrices) “incommensurable”, which is to say that one matrix cannot flatly be translated

\[^{26}\text{Ibid., pp. 118-9, 122, 129.}\]
into the other, for they carve up the world, as it were, in different ways. Although
the vocabularies of alternative matrices superficially consist largely of the same set
of terms, they apply many of those terms to Nature in remarkably different ways.
Similarity relations between phenomena change, such that objects are grouped
into similarity-sets (learned via exemplars) differently in the new matrix. At one
point, for example, metals were transferred from the set of compounds to that of
elements. Further,

the content of the Copernican statement, “planets travel around the sun” cannot be
expressed in a statement that invokes the celestial taxonomy of the Ptolemaic statement
“planets travel around the earth”. The difference between the two statements is not
simply one of fact. The term ‘planet’ is a kind term in both, and the two kinds overlap in
membership without either’s containing all the celestial bodies contained in the other. All
of which is to say that there are episodes in scientific development which involve
fundamental change in some taxonomic categories and which therefore confront later
observers with problems like those the ethnologist encounters when trying to break into
another culture.\footnote{RSS, p. 5.}

With these redistributions, researchers in competing matrices may respond to the
same stimuli in incompatible ways. These incompatibilities will “cluster most
densely about the phenomena upon which the choice of theory most centrally
depends”.\footnote{SSR, p. 201.} Because of this, “[c]ommunication across revolutionary divides is
inevitably partial”.\footnote{Ibid., p. 149.}

It is important to see that problems of incommensurability are not of a
merely linguistic nature. This means that inter-matrix translation cannot be
resolved through a simple re-stipulation of terms. The difference between
matrices, although reflected in their language, is actually prior to the application
of those languages. Kuhn believes that a language-metaphor for describing
alternative matrices would be too inclusive, given that we are really dealing with
what he calls a “lexical taxonomy”, which describes the meanings of a restricted
class of terms: natural kinds, artifactual kinds, social kinds. Kuhn cautions that we
might more appropriately think in terms of concepts, rather than of words. This
would make it something akin to a conceptual scheme, which is not a set of
beliefs, but “a particular operating mode of a mental module prerequisite to
having beliefs, a mode that at once supplies and bounds the set of beliefs it is
possible to conceive.” This taxonomy must therefore be in place before a
description of the world can begin, because unproblematic communication is only
made possible by shared taxonomic categories: “[i]n discussion between members
of communities with differently structured lexicons, assertability and evidence
play the same role for both only in areas (there are always a great many) where
the two lexicons are congruent”. It is these areas of overlap which provide the
necessary “bridgeheads” from which they endeavour to evaluate their differences,
and which permit the members of one group to attempt to acquire the lexicon of the other in order to assess its viability. Pinpointing areas of crisis, then, is tantamount to localising incommensurability in particular areas in which two lexical taxonomies diverge.  

That new matrices characteristically permit predictions different from those allowed by the old matrix demonstrates the logical incompatibility of two alternative matrices. While this is not to say that the logical inclusiveness of two matrices is, in principle, impossible, it is, according to Kuhn's observations, "a historical implausibility". Matrices at least appear to be logically exclusive. Their "incompleteness of logical contact" ensures that proponents of different matrices "talk through each other".  

The recovery, by the historian of science, of older meanings cannot therefore be a process of translation, since this is not possible where there is incommensurability. The shift cannot, as a result, be made one step at a time, but must occur all at once, a reflection, also, of the matrix as a fundamental unit for scientific research, one which cannot be fully reduced to logically atomic components. We see, for instance, pockets of inter-definition of terms—exhibiting "local holism"—which render it necessary that many of the terms of scientific lexicons be learned in clusters. Newtonian "force" and "mass" exemplify this; moreover, these terms must be learned in concert with Newton's Second Law of Motion, which guides how one is to pick out Newtonian forces and masses. This renders the shift a process more like a Gestalt-shift, the sort which produces "bilinguals", although not through translation-efforts.  

It is difficult for the proponents of alternative matrices to decipher how it is that they differ. They must first recognise each other as members of different lexical communities. In the initial stages of theory-conversion, they will attempt to translate each theory into the other. They will do so by first isolating the terms which are used unproblematically within each community, but which are troublesome between the groups. They will attempt to predict what the proponents of the competing theory would see and say when presented with a particular stimulus. They become more skilful at predicting each other's behaviour. During this period, it is important that they refrain from judging anomalous responses as incorrect. They will also attempt to demonstrate to each other the technical advantages of their own theory. The demonstration of impressive concrete results of the new theory will persuade some adherents to the old to try to ascertain how those results were achieved. But they will do so still operating in the mode of translation, attempting to foster the process by reading papers, talking with proponents of the new theory, and watching them and their students work. If the proponents of the old theory are unable to produce results as impressive as those of the new theory, the new theory begins to take hold, and those who had clung to the old theory find that, at some point, they have ceased

30 RSS, pp. 4-5, 9, 12.  
31 SSR, pp. 97, 110.  
32 Ibid., pp. 11, 85; RTC, p. 566; RSS, pp. 4-5.
to translate, and instead have begun to speak the new language. "Exploring an alternative theory by techniques [such as these], one is likely to find that one is already using [the new lexicon] (as one suddenly notes that one is thinking in, not translating out of, a foreign language)." The depth of the taxonomic divide means that, if proponents of alternative matrices fail in their discussions to achieve bilingualism, they are bound to end up talking through each other somewhat.\(^{33}\)

But a matrix-shift is not strictly similar to a Gestalt-shift, on account of the inability of the scientific enterprise to allow its practitioners to shift back and forth between Gestalten. We may see what appear to be reversals in the direction of shift, as in the greater resemblance of the explanation of gravitational attraction offered by Einstein to those of Newton's predecessors than to those of his successors. But this is, of course, not a genuine reversal, since the matrix promoted through Einstein's work was nevertheless a new one overall. Periods where this kind of instability does occur are notably periods of crisis (e.g., when light was sometimes believed to be a wave, and other times believed to be a particle). Further, the scientist does not see the matrix-shift in the same light as the psychological subject, for example, sees a Gestalt-shift. For instance, the scientist does not say that, looking at the moon, she once saw it as a planet, but now sees it as a satellite; her judgment is instead that, while she once saw the moon as a planet, she was mistaken. This speaks of a more solid commitment to a chosen matrix than the Gestalt-image would suggest.

That we see incommensurability at all is an indication of the absence of an overarching language—or, more accurately, in Kuhnian terms—lexical taxonomy "capable of expressing, in its entirety, the content of [all lexicons] or even of any pair". This entails the absence of a neutral observation language, as well as of external standards by which one might judge a matrix to be inherently unscientific: "what occurs [is] neither a decline nor raising of standards, but simply a change demanded by the adoption of a new [matrix]." Adherents to the old and the new differ with respect to the desired "institutional matrix", and there is no "supra-institutional framework for the adjudication of revolutionary difference". This lack of an overarching scientific standard entails that whole theories in competition with each other for preferred-matrix status cannot be evaluated using the same sort of criteria that we can isolate as being used in judging a single theory's individual research-applications. We might put this another way, by saying that there is no such thing as an algorithm for theory-choice—a set of rules through the application of which scientists might make a clear determination of a superior theory. If there were such a thing as an algorithm for theory-choice, it would "eliminate all need for recourse to group values, to judgments made by minds prepared in a particular way". The presence of an algorithm would thus contradict Kuhn's emphasis on exemplars as an important means of training in the collective practice of scientists through exposure to concrete scenarios.\(^{34}\)

\(^{33}\) SSR, pp. 148, 201-2; CGK, p. 277; ET, p. 339.

\(^{34}\) SSR, pp. 93-4, 200-1; CGK, pp. 12, 238.
In truth, there is an inaccuracy in describing theory-choice in terms of decision at all, since theories are research-programmes to be evaluated over time, and there is no "conscious deliberation on each issue prior to the assumption of a research stance". At no point is the scientist aware of having reached a decision or made a choice. Thus, the transfer of allegiance from one theory to another is better described as "conversion". Scientists come to evaluate the position in which they find themselves as a result of their having made the conversion. It is only after the fact of conversion to a new theory that terms such as "choice" and "decision" are applied; the true "nature of the change [is] disguised in later reports".35

Having asserted all of this, however, Kuhn insists that the lack of a neutral lexical taxonomy does not point us to the intrinsic irrationality of scientific theory. Against the charges of some of his critics, he is not "trying to make science rest on unanalyzable individual intuitions rather than on logic and law". He argues that one can still pick out good reasons which take part in the conversion from one theory to its rival.36

The lack of an algorithm for theory-choice means that arguments deployed by scientists in favour of particular theories are not logically compelling. Some of Kuhn's readers have sharply criticised this notion, seeing it, in one account, as leading to a situation governed by "mob psychology".37 This is a characteristically philosophical complaint, since "this mode of development [which] requires a decision process which permits rational men to disagree" (i.e., revolutionary science as Kuhn has described it) would be "barred by the shared algorithm which philosophers have generally sought". Kuhn has taken pains (more than should have been required) to emphasise that rationality is essential to the scientific enterprise, in times of both crisis and stability. For him, "rationality" and "justification" are inter-defined terms. Incommensurability is not a threat to the rational evaluation of truth-claims. The lack of logical compulsion which we see in inter-matrix arguments does not mean that scientists ever refrain from using logic and mathematics in these arguments: "[t]o say that, in matters of theory-choice, the force of logic and observation cannot in principle be compelling is neither to discard logic and observation nor to suggest that there are not good reasons for favouring one theory over another". There is no implication "that scientists may choose any theory they like so long as they agree in their choice and thereafter enforce it": "[o]ne scientific theory is not as good as another for doing what scientists normally do". It is perfectly possible for arguments to be logical, and yet uncompelling. There may be a disagreement over premises, or over the manner in which they are to be applied--both being a common feature among scientists debating theory-changes. At some point, one theory must assume dominance over the others; for scientists at times such as these, "their recourse is to persuasion as

35 CGK, pp. 238, 276-7; ET, p. 338.
36 SSR, p. 191; CGK, p. 235; ET, p. 328.
37 Lakatos, CGK, p. 178: "Thus, in Kuhn's view scientific revolution is irrational, a matter for mob psychology.

38
a prelude to the possibility of proof". Thus, decision-problems between alternative matrices cannot be resolved by recourse to proof—rather, they involve "techniques of persuasion, or...argument and counter-argument in a situation in which there can be no proof". This does not refer only to proofs of a logical sort; the competition between matrices cannot be solved through appeal to empirical proofs, either, since proponents of competing matrices are generally unwilling to grant the non-empirical assumptions of the others.\(^{38}\)

The arguments offered for the adoption of a matrix by its proponents are necessarily circular: each group must use the tools of its own matrix to argue in its defence:

The resulting circularity does not of course make the arguments wrong or even ineffectual. The man who premises a paradigm when arguing in its defense can nonetheless provide a clear exhibit of what scientific practice will be like for those who adopt the new view of nature. That exhibit can be immensely persuasive, often compellingly so. Yet, whatever its force, the status of the circular argument is only that of persuasion. It cannot be made logically or even probabilistically compelling for those who refuse to step into the circle. The premises and values shared by the two parties to a debate over [matrices] are not sufficiently extensive for that. As in political revolutions, so in [matrix] choice—there is no standard higher than the assent of the relevant community. To discover how scientific revolutions are effected, we shall have to study not only the impact of nature and of logic, but also the techniques of persuasive argumentation effective within the quite special groups that constitute the community of scientists.\(^{39}\)

The rationality involved in theory-choice can be seen in the struggle which scientists undergo when applying the values mentioned above to the question of theory-choice—e.g., accuracy, precision, simplicity, plausibility, broad scope, enhanced qualitative function, internal consistency, compatibility with other theories, and unanimous (or, at least, as nearly as possible) acceptance throughout the relevant scientific community. These values are important in that they provide the shared basis for theory-choice, in a community where a shared grounding is vital to its achievements. Because they are imprecise, however, individual scientists may legitimately differ about their application to concrete cases. Different scientists may disagree about the relative weights to be accorded to various values when several of them are to be deployed. Accuracy is the most nearly decisive of all the criteria: it is the least equivocal, and the predictive and explanatory powers so important to scientists depend upon it. But theories cannot always be discriminated in terms of accuracy. For example, Copernicus' system was not more accurate than Ptolemy's until Kepler later drastically revised it, and this only occurred because Kepler chose Copernicus' system for reasons other than accuracy. Individual differences in respect of relative emphasis on different values need violate no accepted scientific practice. So values may conflict: accuracy may

\(^{38}\) SSR, pp. 148, 158, 198-9; CGK, pp. 234, 261-2, 264; ET, pp. 320, 332; RTC, p. 569; RSS, p. 3.

\(^{39}\) SSR. p. 94.
dictate the choice of one theory, while other considerations--of scope, say--favour its competitor. Two different theories may each be more accurate or simple than the other, but in different areas of their operation or application. For instance, considering simplicity, the computational labour involved in predicting the position of a planet was pretty much the same within the Copernican and Ptolemaic theories, but Copernicus' system was much simpler in terms of the mathematical apparatus used to explain retrograde motion. No one value is sufficient on its own for theory-choice. So values function not as rules which determine theory-choice, but instead merely influence the choice. The revolutionary shift to a new matrix may involve a shift in the relative weighting and application of values. This constitutes a feedback loop.

As I mentioned before, Kuhn stresses that science is inherently a group activity. A matrix shift, then, will require the consensus of the scientific community affected by it. Nevertheless, it is absurd to expect that all members of the community will become convinced of the wisdom of the change at the same rate. So rather than a single group-conversion, there will be "an increasing shift in the distribution of professional allegiances". Gradually, the number of experiments, instruments, and publications based on the new order will multiply, and more researchers "will adopt the new mode of practicing normal science, until at last only a few elderly hold-outs will remain". But "after the last hold-outs have died, the whole profession will again be practicing under a single, but now a different, [matrix]." 40

This last statement reflects a thesis which has caused Kuhn much difficulty with critics. It is that, in both the pressure for matrix-transformation which brings about crisis and the theory-choice which is demanded of scientists in times of crisis, there are factors at play of which notice is not typically taken in traditional accounts of science. Returning to the crisis leading to the Copernican Revolution, we see that, although the breakdown in normal technical puzzle-solving activity was the primary stressor on the old Ptolemaic system, there were other significant factors, as well: social pressure for calendar reform, medieval criticism of Aristotle, and the rise of Renaissance Neo-Platonism--i.e., a number of historical elements, which could not be counted as scientific in themselves. Similarly, there are a number of unscientific influences on theory-choice by researchers. For instance, those who arrive at a candidate for a new matrix are almost always "either very young or very new to the field". They are less "committed by prior practice to the traditional rules of normal science", and more likely to see the inadequacy of those rules as it begins to appear. On the other hand, those scientists who are immersed, through time and practice, within the old matrix tend to resist its demise, demonstrating greater faith than the new-comers that their time-tested matrix will eventually solve all of its problems. Theirs is a necessary trait for the progress of normal science. But in times of crisis, and especially at the later stages of crisis, and when a new matrix-candidate has

40 Ibid., pp. 152, 158-9.
reached the point of serious consideration by the relevant community, this attitude may prove to be unreasonably stubborn.\textsuperscript{41}

Other non-logical and arguably unscientific influences may sit further afield from mere attachment to that to which one has become adjusted through thought and practice. In this regard, Kuhn refers to "the sun worship that helped make Kepler a Copernican". Concern about one's reputation within the scientific community may also influence the energy with which one pursues the promotion of a new matrix. Differences in the environment where science is practiced--e.g., different countries--can also have an effect on research and theory-choice.\textsuperscript{42}

Matrix-debates, then, are not strictly about relative problem-solving ability. The central issue concerns which matrix should guide future research on problems, and this question is raised at a time when neither competitor is capable of resolving all of its problems. The promise of future success of the matrix directs the choice somewhat more than does the past success of the old order. It is thus largely on faith that the scientist will embrace a new matrix. But this is not to say that such faith is blind: it requires a basis, although this basis "need be neither rational nor ultimately correct". At bottom, significant factors will be whatever scientists will allow to persuade them to change their minds. Certainly, as noted above, argumentation plays a considerable role in this process, but Kuhn's controversial point is that choices are not dictated by logic and experiment alone: socio-psychological imperatives are also involved. The Zeitgeist has a role in intellectual affairs. Considering the nature of the scientific enterprise and the attitude of its practitioners, however, one would not expect such factors to be merely frivolous in their nature. Understanding what scientific progress is requires an understanding of the value-system, or ideology, employed by scientists. This system is transmitted and reinforced through the institutions which characterise science, and so the explanation of progress will, "in the final analysis, be psychological or sociological". One of Kuhn's "irreducibly sociological principles" is that "[w]hatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains." This position is "intrinsically sociological", or "ideological", departing from "dogmatic and naive" "justificationism and falsificationism". Kuhn is not here intending to invoke an individual's psychological idiosyncrasies, but rather "the common elements induced by nurture and training in the psychological make-up of the licensed membership of a scientific group". These elements are, at least in principle, analysable. The "social psychology" to which Kuhn is referring is "quite different from individual psychology reiterated \( n \) times". But neither is it an idealised mind which practices normal science; there are, in Kuhn's estimation, no such minds--only shared ideals which affect behaviour.\textsuperscript{43}

\textsuperscript{41} Ibid., pp. 69, 90, 144, 151.
\textsuperscript{42} ibid., p. 152; \textit{ET}, p. 333.
\textsuperscript{43} SSR pp. 79, 152, 157-8, 191; CGK, pp. 21-2, 237, 240; RSS, p. 3.
Within this social-psychological net of influences offered to the scientist as part of his training, there is, nevertheless, room for variability based upon divergence in the individual emphasis placed upon particular factors. Some of the factors relevant to theory-choice are:

1. the individual's previous experience as a scientist, e.g., how successful he has been in his work thus far, in what particular field he was working when confronted by the need to choose, and how much of his work depended upon concepts and techniques affected by the crisis;

2. the individual's personality, e.g., the drive towards originality will be stronger in some researchers than in others, leading to both a relatively greater willingness to take risks and less ego-attachment to the theory; and

3. the individual's involvement in or influence by social movements, e.g., Kepler's immersion in Neo-Platonic and Hermetic movements, German Romanticism's predisposing effect towards the acceptance of energy conservation, and nineteenth-century British social thought affecting Darwin's concept of the struggle for existence.

Every individual choice will thus be the result of the application of a mixture of relatively more shared and relatively more individual values. But even the individual values are heavily tinged by the training which prepares the student for eventual acceptance into the scientific group, e.g., education and prior pattern of professional research. So, for instance, concern--within reason, that is--for the state of one's professional reputation could plausibly be seen as heavily influenced by these shared ideals, or common elements, since that reputation is an important key to the legitimation of the body of one's work, and therefore of one's contribution to the progress of the scientific endeavour. This progress is obviously an essential concern for scientists.

It would be a rather dramatic overstatement, in Kuhn's estimation, to label these socio-psychological influences as "irrational". He has insisted that "[n]o process essential to scientific development can be labelled 'irrational' without vast violence to the term". He prefers the milder term "arational" to describe these factors which, while not introducing a wild card into the scientific enterprise, nevertheless depart somewhat from strict logical considerations.\(^{44}\)

Considering the variety of influences which tug at the scientist confronted with a situation eventually demanding theory-choice, creative scientists must be able to tolerate living with an "essential tension", living "in a world out of joint". This tension exists between "convergent" and "divergent" thinking, and the ability to support it, even when it "can occasionally become unbearable is one of the prime requisites for the very best sort of scientific research". Convergent thinking typifies consensus-bound research, while the divergent tendency has to do with the flexibility and open-mindedness which allow the imagination to play with unlikely possibilities, refraining from becoming too attached to apparently self-evident facts or concepts. The judgment called for at this time of confusion mediates between the conservative retention of the old order and the riskier leap

\(^{44}\) CGK, p. 235; Horgan, p. 42.
to the new. The scientific group must simultaneously display the characteristics of both traditionalism and iconoclasm. Even if a new theory does begin to take hold --and most do not--much theoretical and experimental work is usually required "before the new theory can display sufficient accuracy and scope to generate widespread conviction". At this time, research guided by both the old and the new matrices will be conducted. The scientist will wish to preserve as many of her group's puzzle-solutions as possible (constituting a force which speaks in favour of guarding the integrity of the old matrix), but she will also wish to maximise the number of puzzles that can be solved, and this may be better served by the shift to a new matrix.\textsuperscript{45}

The individuality of scientific value-judgments within the community (meaning the relative weighting of the values listed above) is one of the tangled roots of the scientific crisis. It plays an important role in fomenting the essential tension which mediates theory-change, in terms of both openness to imaginative alternatives to old theories and moderation of the acceptance of new ones. An identification of the general characteristics which typify good scientific theories is not by itself sufficient to determine the decisions of individual scientists. Individual choice in respect of which canon to follow must also be taken into account: "[O]ne can explain, as the historian characteristically does, why particular men made particular choices at particular times". That the members of the group do not all apply the shared basic values of the community in the same way ensures that individual researchers respond to anomalies in different ways: some will detect the roots of crisis where others will see problems satisfactorily solvable by the old ways. Some will see crisis at a point at which others see "only evidence of a limited talent for research". If researchers were to respond in similar ways, they would, on the one hand, collectively see each anomaly as a source of crisis, which would impel them to embrace each new theory that came along. There would be none left to try to make the existing theory account for current apparent anomalies. Such fickleness would spell the end of normal science, having severe repercussions on scientific progress. Moreover, that most proposals for new theories prove to be unwise, and ultimately insupportable, would deepen the folly of too free and easy an attitude towards the taking of theoretical risks. On the other hand, it is necessary for the health of the scientific enterprise that somebody in the community react to anomalies and new theories in a higher-risk sort of way; otherwise, the revolutions which have proved essential to the developmental progress of science would be given no opening to occur. We might view this individual divergence as a "hedging of bets", "the community's way of distributing risk and assuring the long-term success of its enterprise". Individual variability thus functions as a strength: if a decision must be made under circumstances where there is a significant possibility of an error in judgment, it may be important that individuals judge in different ways.\textsuperscript{46}

\textsuperscript{45} SSR, p. 186; CGK, p. 21; ET, pp. 226-7, 332.
\textsuperscript{46} SSR, pp. 185-6; CGK, pp. 21, 241, 248, 262; ET, p. 226-7, 324-5, 332.
In the end, the highly important value of unanimity will require that the relevant scientific community will converge around a new matrix deemed acceptable as that which determines the future tone and direction of normal scientific research. Decisions as to the preferred matrix are made by the community of specialists, not by individuals. So the group behaviour with respect to theory-choice "will be affected decisively by shared commitments". 47

Empiricism, Kantianism, and the Spectre of Relativism

Normal science, which is what occurs most of the time, operates on the assumption that the scientific community knows what the world is like. This assumption is necessary for the success of the enterprise, being closely connected to the suppression of novelty which typifies normal science, since a sense of security in one's knowledge of the world requires a certain amount of stability.

The usual view of what occurs in a matrix-shift is that there is an alteration in the scientist's interpretation of observations which are fixed by both the nature of the environment and human perceptual abilities. On this view, an inherently empirical one, proponents of different matrices see the same world differently. But Kuhn argues that data are not simply stable in this way. Within a given matrix, the researcher is confident in his knowledge of what counts as a datum, what instrumentation is needed to reveal it, and what concepts are relevant to its interpretation. Therefore, while we are indeed justified in making reference to interpretation of data as being part of what is going on in science, we can only do so within the confines of a particular matrix. Aristotle interpreted his observations of the falling stones defined by his matrix; Galileo interpreted his observations of the pendula of which his matrix spoke. Interpretation can only provide an articulation of a matrix; it is incapable of correcting that matrix. Interpretation is a logical exercise, and as such, would be logically linked to the experience of a given normal scientific matrix. But in revolutionary science, the "flash of intuition" through which the beginnings of a new matrix are born does not embody such logical linkages. 48

In revolutionary science, then, we see a difference in what constitutes data between the old order and the new. (Again, this will not be total: at least some of the old matrix will be carried through the shift.) So, for instance, it is not the case that a scientific observer sees a swinging stone, and then interprets it as either an object in constrained fall or a pendulum. For the Galilean researcher, seeing a swinging stone is no more elementary than seeing a pendulum: the observation is dependent upon the matrix which acts as a prior definition of observations. The observation of a falling body is a vision through one matrix; that of a pendulum is a vision through another. So measurements or retinal imprints cannot be taken as

47 SSR, p. 200; CGK, p. 241.
48 SSR, pp. 120-3.
foundational, since they must already be part of a matrix in order to be intelligible at all. This is the only way that we can pose any questions about such measurements or imprints. If the matrix changes, so do the questions we ask: these questions "presuppose a world already perceptually and conceptually subdivided in a certain way". In order for a scientist even to enquire what measurements or retinal imprints make the pendulum what it is, she must first be able to recognise a pendulum. This connects to Kuhn's assertion that we do not learn to see the world item-by-item, since the recognition of something as a particular thing occurs within an accepted framework of similarity-relations (i.e., within a context).49

It must be understood, though, that the a priori structure, set out by the operative matrix, which defines the scope and meaning of normal research does not, for Kuhn, provide a criterial conceptual framework which would allow the scientist to specify in advance whether or not each imaginable observation will fit or falsify the theory. The necessary presence of a framework of intelligibility—one which allows the recognition and, therefore, continued observation of pendula, for instance—must not be construed as obviating amendment of the framework by observation. For example, one cannot seek a criterion which would specify, at the outset, that all swans are white, such that the discovery of a black swan falsifies that theory. The discovery of a black bird which strongly resembled swans in other respects would eventually cause a crisis for the theory. There would be an ensuing focus on research in this troublesome area, and this would include the search for more black birds of this type. In the end, either the theory will be amended to include black swans, or a new natural family made up of these birds will be introduced. Which occurs will depend largely upon the strength of the theoretical belief that colour is important for characterising natural families such as this. The point to note here is that both logical considerations and observation of concrete particulars will play a significant part in the outcome, whether this be the adjustment of the existing theory or the creation of a new taxonomic group.

The lack of a criterial basis for theory reconstruction dovetails with Kuhn's description, outlined previously, of the growth of theory through the building of similarity-relations, based on ostension. Some of these relations will be altered through revolution, so that objects and situations are grouped differently from how they were before.

So the old empirical picture of sensory experience that is fixed and neutral no longer functions effectively. The scientist only sees his data once "his research is well advanced and his attention focused"50. Making sense of fixed and neutral sensory experience probably would require the discovery of a pure observation-language, and this would depend upon the construction of a satisfactory theory of perception and of the mind, to which we are not even close. Very different stimuli can produce the same sensations in different individuals, the same stimulus can produce very different sensations, and the route from stimulus to sensation is

49 Ibid., pp. 127-9.
50 Ibid., p. 126.
conditioned, in part, by education and socialisation. "[S]timuli receive much neural processing before anything is seen or any data are given to the senses." If the members of two divergent research-groups have systematically different sensations upon receipt of the same stimuli, then, in some sense, they live in different worlds. These worlds not only diverge among communities, but also alter with time. Individuals of the same group share a common background in education, language, experience, and culture. Thus, we have good reason to suppose that their sensations are similar. They also exhibit coherently connected communication and behaviour. But, with the differentiation and specialisation of groups, we have no evidence for the immutability of sensation. Hence, it may be parochial to assume that the route from stimulus to sensation is the same for everyone. We do not even have direct access to stimuli, since these are only known, themselves, through elaborate theory. On pain of solipsism, though, we must regard the stimuli to which observers are exposed as the same for everyone.

It is along the pathway from stimulus to sensation that the various groupings of observations which distinguish alternative matrices will be learned. Kuhn says,

I think it likely myself that much or all of the clustering of stimuli into similarity sets takes place in the stimulus-to-sensation portion of our neural processing apparatus; that the educational programming of that apparatus takes place when we are presented with stimuli that we are told emanate from members of the same similarity class; and that, after programming has been completed, we recognize, say, cats and dogs (or pick out forces, masses, and constraints) because they (or the situations in which they appear) then do, for the first time, look like the examples we have seen before.

At any rate, Kuhn asserts that we should not over-emphasise the differences in experience between either individuals or communities. We do, after all, consider ourselves to share in a common sort of general neural apparatus, however divergent the "programming" which influences this biological structure may be: "[d]oubtless some aspects of that lexical structure are biologically determined, the products of a shared phylogeny." Beyond even the biological, much of the "programming" of individuals must also be similar, given their sharing of a significant amount of their history, perhaps of a language and an everyday world, as well as most of their scientific world. Really, even in great revolutions, the area under dispute is relatively narrow. Given what they share, and employing sufficient will, patience, and tolerance of the threat of ambiguity, scientists occupying divergent viewpoints can nevertheless come to find out much about how they differ. Their commonality provides a resource upon which they can rely when venturing into the uncertain territory wrought by a state of crisis.

51 CGK, p. 276.
52 SSR, pp. 126, 128, 193, 196; CGK, p. 276; ET, p. 308; RSS, p. 10.
53 CGK, p. 276.
54 Ibid., RSS, p. 10.
Even though the proponents of different matrices practice, in many respects, in different worlds, "that is not to say that they can see anything they please...or not at all". Kuhn insists that there is an integrity to perception, one which is born of the selection process which is driven by survival value; the experience and knowledge of Nature are embedded in the stimulus-to-sensation route. Thus, the neural process that transforms stimuli to sensations has a number of built-in characteristics:

1. it is transmitted through education;
2. it has been found, through trial-and-error, to be more effective than its historical competitors; and
3. it is subject to future change through further education, and through the discovery of a lack of fit with the environment.

Clearly, then, given his reference to stimuli and goodness-of-fit with the environment, Kuhn is not proposing an understanding of knowledge as strictly constructed by humans. The stimulus is that which is “given” by Nature, although it can only be rendered intelligible to us when we perceive it through a human template of some sort. Although the acts of judgment which feed into scientists’ rejection of a previously accepted theory and acceptance of a new one involve the comparison of theories with each other, they are always based upon more than this: they also entail a comparison of those theories with the world. Empirical observation is an essential part of the scientific enterprise: “[m]ost of the puzzles of normal science are directly presented by nature, and all involve nature indirectly”. Both normal and revolutionary science, in their respective ways, strive to bring theory and fact into closer agreement. While the nature of facts is, to some degree, determined by the matrix through which they are observed, they include some measure of the “given”. It is in this respect that observation is so important. Different matrices should then be understood as telling us different things about the entities of which Nature is composed, and how they behave.

“They are not simply about names or language but equally and inseparably about nature.” In scientific progress, then, it is fair to say that we see an increasingly detailed and refined understanding of Nature. The student’s contemplation of the exemplars of her prospective profession is an essential part of her learning how the currently accepted laws of science attach to Nature: in short, how the world behaves.\(^5\)

We must take care, then, in understanding the reconstructive nature of science to refrain from falling into the belief that the world is somehow mind-dependent, perhaps an invention or construction of the creatures which inhabit it, and in recent years such suggestions have been widely pursued. But the metaphors of invention, construction, and mind-dependence are in two respects grossly misleading. First, the world is not invented or constructed. The creatures to whom this responsibility is imputed, in fact, find the world already in place, its rudiments at their birth and its increasingly full actuality during their educational

\(^5\) SSR, pp. 77, 80, 125, 145, 150, 170, 195-6; CGK, p. 262, 274, 276; RSS, p. 10.
socialization, a socialization in which examples of the way the world is play an essential part. That world, furthermore, has been experientially given, in part to the new inhabitants directly, and in part indirectly, by inheritance, embodying the experience of their forebears. As such, it is entirely solid: not in the least respectful of an observer's wishes and desires; quite capable of providing decisive evidence against invented hypotheses which fail to match its behavior. Creatures born into it must take it as they find it. They can, of course, interact with it, altering both it and themselves in the process, and the populated world thus altered is the one that will be found in place by the generation which follows. The point closely parallels the one made earlier about the nature of evaluation seen from a developmental perspective: there, what required evaluation was not belief but change in some aspects of belief, the rest held fixed in the process; here, what people can effect or invent is not the world but changes in some aspect of it, the balance remaining as before. In both cases, too, the changes that can be made are not introduced at will. Most proposal[s] for change are rejected on the evidence; the nature of those that remain can rarely be foreseen; and the consequences of accepting one or another of them often prove to be undesired.\textsuperscript{56}

This is perhaps an overly vigourous attempt to defend the retention of a sense of there being a component of experience which is "given" by Nature. It should be clear from the discussion of Kuhn's work so far that he does not here mean to imply the absence of a further component of experience, supplied by the mind as a formal structure of some sort, one which organises the "given" into experience which is understood in a particular way.

This world, even though partially mentally constituted, and partially "given", can nevertheless rightfully be referred to as the "real world". After all, it provides the environment for life, placing constraints on that life, so that continued existence depends upon adaptation to those constraints. Science functions as an important contemporary tool for that adaptation. "What more can reasonably be asked of a real world?"\textsuperscript{57}

The partially mentally constituted, partially "given" nature of the experienced world is reflected common-sensically in that, when we view a community from the outside, we see it as adapting to its environment, while, from a vantage-point inside a community, our emphasis is on the mediation of our interactions with our environment through "something like a mental representation". In Kuhn's view, then, mental representations have, to some extent, a world-constituting role.\textsuperscript{58}

Under this scheme, fact and theory are not categorically separable. The assimilation of a new theory requires the reconstruction of both prior theory and fact. This is because a fact is the result of the integration of the "given", and the formal mental elements provided by the matrix. In a revolutionary re-formulation of the preceding scientific tradition, there is no piecemeal evolution of a theory to fit facts that were always there. We encounter no rules for inducing theories from

\textsuperscript{56} RSS, p. 10.
\textsuperscript{57} Ibid.
\textsuperscript{58} Ibid., p. 11.
facts, since theories are not induced at all, but are rather "imaginative posits". Theories "fit the facts" by transforming previously accessible information into facts that had not existed for the preceding matrix; they emerge together in a tapestry with the facts they fit. Similarly, as a result of a matrix-shift, some of the old problems may come to be designated as unscientific, and effectively cease to exist as problems, while others previously so labelled come to assume significance. The revolution changes the standards by which the profession determines what counts as admissible problems and legitimate solutions. The boundaries between science and metaphysical speculation change. The relationship between science and Nature is knowledge-mediated, changing as matrices die out and are replaced by new ones. The concept of "element", for instance, has not remained the same throughout history. Any concept gains its full significance only when related to other scientific concepts, procedures, and applications; it is thus context-dependent. In this sense, discovery is a complex process (as opposed to an event); it involves becoming aware of something new, but not fully understood, as well as further experimentation which defines the properties of that which has been newly found. It is a situation of not only finding that something is, but also what it is. Discovering that and what together only occurs within an established matrix.59

What is fundamentally at stake here is the correspondence theory of truth. The question at hand is whether or not theories correspond to an external, mind-independent world. Certainly, any Kantian brand of epistemological programme, i.e., one which specifies the presence of an a priori structure, acting in concert with a sensible "given", necessitates the rejection of a simple empirical correspondence theory. Kuhn's contention is that this latter notion of truth must vanish, along with foundationalism in general. Nevertheless, whatever replaces it will still require "a strong conception of truth":60 we must learn to get along without anything at all like a correspondence theory of truth. But something like a redundancy theory of truth is badly needed to replace it, something that will introduce minimal laws of logic (in particular, the law of non-contradiction) and make adhering to them a pre-condition for the rationality of evaluations...On this view, as I wish to employ it, the essential function of the concept of truth is to require choice between acceptance and rejection of a statement or a theory in the face of evidence shared by all.61

To declare a statement a candidate for a judgment of truth or falsity is to accept it as part of a "language-game", the rules of which forbid the assertion of both a statement and its contrary: viz., the law of non-contradiction. Any attempt to conduct discourse against this rule endangers the integrity of the scientific lexical community, contributing to the breakdown of discourse. Language-games conducted without the benefit of logical rules reduce to metaphor, poetry and

59 SSR, pp. 6-7, 55-7, 66, 103, 140-1; CGK, pp. 2, 12; ET, p. 338.
60 RSS, p. 6.
61 Ibid., p. 8.
mysticism. Adherence to logical rules is thus a presupposition of the sort of discourse demanded by normal science.

In order to decide if a statement is to qualify as scientific, one must first determine whether or not it is a truth-value candidate--whether or not it makes sense to attempt to judge it as either true or false. The answer will be determined by the constraints of the lexicon embodied in the disciplinary matrix. It is through this lexicon that one assesses the rational assertability of the statement. For this, "something like the normal rules of evidence" will be employed. A statement awarded truth-value candidacy in one matrix may therefore not enjoy the same status in another. What counts as evidence for an assertion in one case may not in the other. This will serve to deepen the sense of crisis at the time of impending matrix-change, since previously viable statements re-cast in the new lexicon may appear simply to be the expression of unscientific nonsense. This is why the multilingual historian of science must clearly take it upon herself to remember in which lexicon she is speaking at any one time, on pain of severely misunderstanding assertions made in historical contexts, and thus running the clear risk of caricaturing the scientists of the past. 62

What has been fashioned, then, in this account, is an "intra-theoretic" use of the notion of "truth": members of a scientific community "will generally agree which consequences of a shared theory sustain the test of experiment and are therefore true, which are false as theory is currently applied, and which are as yet untested". This understanding of truth eschews any sense of "metaphysical realism" which one might be tempted to attach to the assessment of theories when looking at them from the outside. The traditional questions of metaphysics are out of place here because there is no theory-independent way by which to determine what is "really there". We see this in moving historically from Aristotelian to Newtonian to Einsteinian mechanics: there is nothing identifiable as a coherent direction of ontological development. The vision of successive theories approximating ever closer to the truth is an ontological one. But justification, rather than aiming at a goal external to the historical situation, seeks to improve the available problem-solving tools within that situation. The way in which the historian ought to approach historical theories, then, is dictated by hermeneutics (an attitude and process to which I will return in Chapter Three), which invokes a similar pattern to that seen when the proponents of competing theories struggle to understand each other across a revolutionary divide. Looking inter-theoretically, the historian of science cannot judge either theory of an historical pair to be true: he cannot justifiably consider even the later, victorious, matrix to be a better approximation to the truth than the earlier, vanquished one. To do so would risk being ethnocentric, or Whiggish. This becomes clear when we remember the incommensurability which separates alternate orders: the lack of "a neutral language adequate to the comparison of...observation reports". Thus, looking back, we cannot say that Galileo's observation of a pendulum exhibited a more accurate or objective view than Aristotle's observation of a falling stone. In

62 Ibid., p. 9.
considering our sense of progress, "[w]e may...have to relinquish the notion, explicit or implicit, that changes of [matrix] carry scientists and those who learn from them closer and closer to the truth". It is thus not open to us to view science as an evolution towards anything, even though "[w]e are all deeply accustomed to that goal". 63

That historical disciplines require description in their own terms, as opposed to being imported into our current context, is underscored by the fact that modern disciplines have not evolved one-for-one from earlier disciplines. For example, even though we may say that, in Hellenic society, science and philosophy were one, there was no enterprise quite classifiable as either science or philosophy, which makes it very difficult for us to conceive of a single discipline represented by their union.

A rather stubborn adherence to an out-going matrix cannot, therefore, be deemed a failure to admit error in the face of a confrontation with proof. As I have already noted above, matrix-resistance or -change cannot be justified by an appeal to proof. Resistance to scientific change is neither illogical nor simply wrong, although it may prove to be unreasonable. A revisitation of Kuhn's description of matrix-shift illustrates this:

At the start, a new candidate for [matrix] may have few supporters, and on occasions the supporters' motives may be suspect. Nevertheless, if they are competent, they will improve it, explore its possibilities, and show what it would be like to belong to the community guided by it. And as that goes on, if the [matrix] is the one destined to win its fight, the number and strength of the persuasive arguments in its favor will increase. Most scientists will then be converted, and the exploration of the new [matrix] will go on. Gradually the number of experiments, instruments, articles, and books based upon the matrix will multiply. Still more men, convinced of the new view's fruitfulness, will adopt the new mode of practicing normal science, until at last only a few elderly hold-outs will remain. And even they, we cannot say, are wrong. Though the historian can always find men...who were unreasonable to resist for as long as they did, he will not find a point at which resistance becomes illogical or unscientific. At most he may wish to say that the man who continues to resist after his whole profession has been converted has ipso facto ceased to be a scientist. 64

In culture, Kuhn remarks that the recognition of parallelism in respect of the truth of different theoretical positions may lead to a sentiment of relativism. Indeed, it is precisely with manifesting this sentiment that many of Kuhn's critics have charged him. But he counters that, in science, the recognition of this parallelism, and the consequence that, in some sense, competing theoretical groups may all be correct, is not merely relativistic. Kuhn has attempted to describe science as a unidirectional and irreversible process, where later theories are necessarily judged to be practical and explanatory improvements over earlier ones (considering all the values referred to above: accuracy, simplicity, scope,

63 SSR, pp. 119, 170-1, 206; CGK, pp. 264-6, 277; RTC, p. 568; RSS pp. 7, 10.
64 SSR, p. 159.
number of problems solved, etc.). If he has succeeded in doing this, then he has also succeeded in salvaging some notion of scientific progress, even if this not entail the traditional notion of an evolution towards some ultimate truth.

During periods of normal science, the only viable way, in fact, for the scientific community to conceive of its work is as progressive. This is, in part, due to the fact that, in the absence of the competing schools of “pre-paradigm” science, researchers refrain from questioning each other’s aims and standards. Certainly, there is no call to do so: the common matrix has freed the community from constantly re-examining its fundamental principles. (We might similarly identify a sense of progress, however, within each individual school of the “pre-paradigm” period.) “In its normal state, then, a scientific community is an immensely efficient instrument for solving the problems or puzzles that its [matrices] define”: the results “must inevitably be progress”. Doubts about progress are in point only during times of crisis. Adherents to the old matrix view the prospective one as threatening to deter or set back scientific progress. In contrast, those who promote a new order cannot see their programme as heralding anything other than progress. The rewriting of scientific literature and the re-casting of earlier matrices either as incorrect or as special cases of the new programme help to paint the traditional picture of progress, which has led to the misreading of science as a cumulative process overall.65

Kuhn speaks of a “cognitive evolution”, which manifests in the discursive exchange of, on the one hand, the statements within a relatively stable community (as in normal science), and on the other, the statements of competing matrices (during the crisis period which spawns revolutionary science). He likens this to the exchange of genetic material which occurs in biological evolution. Kuhn’s is to some extent a Darwinian vision: the scientific community selects the fittest way in which future science is to be practiced. The increased refinement in theoretical articulation, and instrumental design and proficiency is punctuated by the relatively dramatic leaps of revolutionary selection. As Darwin proposed in his theory of biological evolution, this scientific epistemological process seems to have occurred without the benefit of a set goal (i.e., non-teleologically). But in this case, that means without “a permanent fixed scientific truth, of which each stage in the development of scientific knowledge is a better exemplar.” This points to his description of his position as “a sort of post-Darwinian Kantianism”. Like the Kantian Categories, the lexical taxonomy of which Kuhn speaks works to supply the pre-conditions of possible experience. But Kuhn’s lexical sort of Categories, unlike Kant’s Categories, transform with the passage from one community to another, whether that be through time (i.e., historical shifts of matrix) or across the divide separating contemporaneous disciplines. Because of the fundamental character of these Categories, their movability through revolution can result in huge effects on the conceptions, practice, and achievements of science, even in cases where only relatively small adjustments in matrix have been made.66

65 Ibid., pp. 163, 166.
66 Ibid., p. 172; RSS, pp. 11-2; Horgan, p. 44.
This shifting set of categories furnishes “a moving, historically-situated, Archimedean platform”, the shifting a priori grounding of science. It is this, and not single scientific knowledge-claims, which is being evaluated in theory-choice. The shared beliefs of a matrix already in place provide an Archimedean point for the comparative assessment of competing theories. Any overall truth or falsity of these shared beliefs is irrelevant to the rationality of the evaluation. For the historian of science, too, the set of beliefs which define a matrix provide a point from which she investigates that matrix. The details of actual scientific practice in historical cases are not as important for the historian’s understanding as is the perspective, or ideology, which she uncovers in examining these cases.

Summary of the Relationship Between Normal and Revolutionary Science.

In summation, the major point, perhaps, to be taken away from an encounter with Kuhn’s programme in general is that our usual notion of science is inaccurate. Revolutions are typically viewed in the light of simple additions to scientific knowledge, and the progression of science is seen as a much more smoothly cumulative process than it, in fact, is. Normal science is quite specifically the only phase during which the scientific enterprise could be properly described as cumulative, although it is true that normal science is what occurs most of the time. But the textbooks of science, and the philosophical and popular works modelled on them function as a source of authority which “systematically disguises...the existence and significance of scientific revolutions”. What these tools are truly doing is recording the “stable outcome of past revolutions”. They tend to supply a minimum of information as to how the current normal-scientific tradition came about. Textbooks are re-written, wholly or partially, after a scientific revolution, since they are “pedagogic vehicles for the perpetuation of normal science”. A prime influence here is the “omnipresent and perennial” temptation to “write history backward”. This distorted picture is fostered by the tendency of science to depreciate historical fact, as well as by the remarkable security enjoyed by scientific practitioners during the stable periods of normal science, when a deep commitment to tradition is highly important.

A further covering of the tracks, as it were, occurs because scientists operating within a later matrix (say, a Newtonian one) often re-interpret the work of scientists operating within an earlier matrix (e.g., a Galilean one) according to that later matrix. This further disguises the radical difference between their views. We see something of this sort going on in Einstein’s attempt to demonstrate that Newtonian dynamics was properly a special case of Einsteinian dynamics. This would have saved Newtonian dynamics, but would have done so by restricting it

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67 RSS, pp. 6-7.
68 SSR, pp. 52, 96, 136-8, 160; CGK, p. 2.
to applications that satisfied much narrower conditions than were claimed initially for the Newtonian programme. In order to view an out-of-date theory as a special case of its successor, the scientist must specifically transform it for that purpose. (Arguably, one could expect there to be a limit to the degree to which one matrix could be stretched to cover another, anyway.) But this view effectively denies the occurrence of revolutionary periods in the history of science; it promotes, instead, the cumulative model (against which Kuhn is arguing), wherein there is always only one matrix, which becomes progressively expanded and refined as science advances. Thus, work along lines such as this essentially prevents any established matrix from being challenged by a prospective one, since it would prohibit scientists from speaking “scientifically” about any phenomenon which failed to fit with the established matrix. A matrix could then provide no critical anomalies, and this would signal the end of the research through which science would develop. It would then appear to be the cumulative enterprise for which it has so commonly been mistaken. As such, it could no longer be considered a research-programme, and would have become merely a tool for engineering. But no active scientific-research matrix ever solves all of its problems. The radical and irreconcilable differences—which often show up in problem-solving ability—between successive matrices are necessary.

Revolutions need not be instigated by crisis, but they almost always are. That crisis, however, also need not affect the same professional group which provoked it: “[n]ew instruments like the electron microscope or new laws...may develop in one specialty and their assimilation create crisis in another”. Scientific revolutions need seem revolutionary only to those whose matrices are affected by them. The Copernican Revolution was one for everyone; the discovery of oxygen was one only for chemists. Upheavals may affect only a small community, several communities, or all of science. 69

Kuhn’s remarks about scientific development are intended to be read as both descriptions and prescriptions; the descriptive and the normative are inextricably mixed. In order to qualify recognisably as mature science, the research endeavour must and does demonstrate an alternating relationship (although not a regular or predictable one) between the stable periods of normal research and the upheaval of the revolutionary crisis out of which a new normal order is eventually born: “extended periods of convergent research are the necessary preliminary to” the relatively rare revolutionary shifts in scientific tradition. In the initial emergence of a mature scientific discipline, theory and technique coalesce into a dominant set of concepts, practices, and instruments which consistently work together to supply concrete problems and solutions for the practice of the field. There should be room left for refinement and increasing sophistication. The range and precision of existing theory are enhanced. At this time, it is important for researchers to focus on the smooth operation of their matrix, and not on all the anomalies or incompletely understood phenomena

69 SSR, pp. 92-3,181; CGK, p. 276.
which are lurking in the wings, in order that they be able to maintain some faith in their own work through their having made some headway in it.\footnote{CGK, pp. 233, 237, 245; ET, pp. 227, 236.}

Since no matrix ever sees the solution of all of its problems, there is never any research which does not generate counter-instances to the theory, or anomalies, i.e., a potential source of crisis. The difference between normal and extraordinary science is not, then, that the former does not come up against counter-instances, while the latter does. Normal science must encounter them in order to pass into the transformative revolutionary phase which accelerates progress in the discipline. Whether a problem is seen as a counter-instance to the theory or as merely a puzzle depends upon from which matrix the problem is being considered. What proponents of the normal matrix see as a puzzle can be seen, from the point of view of the prospective matrix as an anomaly. What Ptolemaic astronomers attempted to continue viewing as puzzles to be solved by their theory, Copernicus viewed as anomalies. There is no sharp dividing line between puzzles and counter-instances. This is what generates the vagueness at times of potential crisis as to whether the currently dominant theory will continue to hold sway, or give way to the emergence of a new research-structure.

A matrix-shift, once it occurs, accommodates, or assimilates, discoveries which had proven to be destructive to the old order. Through it, scientists become able--at least after a sufficient period of new-matrix-development has been allowed--to account for a wider range of phenomena than before, or to account for previously known phenomena with greater precision. It is crucial to understand that no scientific theory can be declared invalid in the absence of an available alternative. To reject a matrix without simultaneously substituting another is either to reject science altogether, or at least to make the inexplicable move of allowing it to return to a state of immaturity.

Revolution also often contributes to the growth of science by generating new cognitive specialties, or fields of knowledge. As a result of the crisis, there may be a narrowing of the scope of a particular community's professional concerns, increasing its extent of specialisation, and limiting its capacity for communication with other scientific groups, not to mention heightening its alienation from the laity. We might term this a "lexical divergence", leading to a lexical heterogeneity of science in general. This is fostered by the importance of the value of unanimity within the community: to maintain this, the specialty may have to be subdivided. This illustrates the supreme significance of the approval of the scientist's work within his specialist group. Specialisation and the narrowing of the range of expertise are the price of increasingly powerful cognitive tools. Clearly, this process works against the unity of knowledge; but the pursuit of such unity may place the growth of knowledge at risk. So specialisation permits the sciences collectively to solve the problems of a wider range of natural phenomena than would be possible with lexical homogeneity. Incommensurability is thus an important isolating mechanism, permitting overall scientific progress.\footnote{SSR, p. 170; CGK, p. 21; RSS, pp. 7-8.}
The Distinction Between Science and Non-science

Kuhn states that the arts and science cannot readily be distinguished “by the application of the classic dichotomies between, for example, the world of value and the world of fact, the subjective and the objective, or the intuitive and the inductive”. “Close analysis must again be enabled to display the obvious: that science and art are very different enterprises or at least have become so during the last century and a half.”

Both science and the arts face persistent technical problems which must be resolved in the pursuit of their endeavours. Both are guided by aesthetic considerations, and are governed by established modes of perception. But too much attention to these similarities obscures their important differences. According to Kuhn, the artist's goal is to produce aesthetic objects, and he resolves technical puzzles to do so. The scientist's goal is the solving of the technical puzzle, and the aesthetic can be one of the tools for doing this. In fact, in order to qualify as a science, a discipline must operate according to the function of puzzle-solving, and this requires, as we have seen, an accepted matrix. In the sciences, despite the importance of aesthetics as a scientific value which helps to guide theory-selection, it is seldom an end in itself, the way it often is in the arts; and if it were an end, it would not be the primary one. Aesthetic considerations must always be subsumed to the unlocking of the puzzle at hand: “only if the scientist's aesthetic turns out to coincide with nature’s, does it play a role in the development of science”. Otherwise, research-styles and aesthetic responses tend to be more private, or individual, and often end up being eliminated from the finished, public work.

Another difference is that, while artists generally have a public audience, the only true audience for a scientist is comprised of other scientists, and this audience becomes particularly narrow in the case of specialised sub-disciplines. A scientist generally seeks approval for her work strictly within her own specialised group.

Despite the fact that scientific development demonstrates, more clearly than does the development of any other field, a succession of tradition-bound periods, punctuated by non-cumulative breaks, Kuhn nevertheless says that he has borrowed this notion from his observation of other fields, such as literature, music, art and politics. These other fields also show periodisation, from revolutionary breaks in style, task, and institutional structure. They also produce products which are modelled upon one another, rather than being produced in conformity to rules; we see the same sort of process in science, which operates primarily in accordance with set, concrete exemplars.

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72 ET, pp. 340-1.
73 CGK, pp. 10-11; ET, pp. 342-3.
Thus, in both the arts and science, the historian can discover stable periods (during which practice conforms to a tradition) and periods of relatively rapid change (in which one tradition gives way to another). In the arts, however, unlike in science, the success of one artistic tradition does not render a previous tradition invalid. Artists can, and sometimes do, voluntarily undertake dramatic changes in style. Such changes in the career of an individual scientist are more rare, and are not voluntary, but rather are forced by acute internal difficulties within her tradition, or by a successful innovation produced by someone else. In science, unlike in art, to change one's "style" is "to confess that one's earlier products and that of one's masters are wrong". So art can support, far more than science, a number of simultaneous, incompatible traditions or schools. As a consequence, when controversy arises, it is solved far more rapidly in science than in art.\textsuperscript{74} This is particularly true of science at a mature stage, by which time a dominant research-matrix has emerged.

The past products of artistic activity continue to be vital parts of the artistic scene. Old scientific works, on the other hand, are generally only read by historians of science. As scientific orders are succeeded by subsequent ones, new breakthroughs initiate the removal of out-dated books and journals. "Unlike art, science destroys its past."\textsuperscript{75}

The internal crises of science will be much more intense than those in art. This is as we would expect, given the importance in science of allegiance to some sense of truth, the relative dependence upon a guiding matrix, and the pressure to achieve pragmatic results, i.e., to develop theories with sufficient explanatory power to solve puzzles. Crisis in science signals a real need for innovation, and directs the attention of scientists towards the area where fruitful innovation may arise. It must be remembered, however, that innovation is not in itself a prime value for scientists, and if pursued for its own sake, would be condemned by a scientific research community which is necessarily to some degree conservative. Innovation really only is admitted on the heels of crisis. A scientific avant-garde would threaten the steady work of science which is what occurs most of the time. So innovation is an often reluctant response to challenges to the dominant matrix. In contrast, artists do make innovation a primary value; even though the avant-garde may not immediately find institutional expression, it is still more appreciated than it is in the sciences. Artistic work is not required to progress in the same steady fashion which is crucial to most scientific research. So it is normal science which most clearly distinguishes science from other enterprises, since the stable periods of science are more markedly so than are any which appear in the arts. To say that science is cumulative and art is not is to mistake the developmental pattern in both fields, although the relatively greater cumulative nature of science does indicate the relatively lower value placed upon innovation in the sciences.

We can sum up the differences of science from other fields as follows:

\textsuperscript{74} ET, pp. 348-9.
\textsuperscript{75} Ibid., p. 345.
1. There is a relative scarcity of competing schools in the sciences, due to the fact that science has achieved a level of maturity which other fields have not attained; it is this which renders science more capable of regarding itself as ahistorical, and which strengthens its stable periods to the point where the prospect of change creates greater tension.

2. Science is more insular than other fields.

3. Puzzle-solving is a primary goal in science; aesthetics is secondary.

4. Science has its own set of specific values, as does any area of culture. Despite all of this, however, we do not generally identify necessary and sufficient conditions for any given discipline, including science. Instead, we recognise a group's activity as, for example, "scientific", partly by its resemblance to other fields which we already have come to recognise as scientific, and by its difference from the activities of other, non-scientific, disciplinary clusters. Partly, a particular discipline is identified by its position in the semantic field which contains all these disciplines.

The End of Science

Despite the facilitation of scientific progression granted by the pattern of mutually enhancing alternation between ordinary and extraordinary research, there is nothing to guarantee that the process will continue indefinitely. Speaking of science, Kuhn says,

There was a beginning to it...There are lots of societies that don't have it. It takes very special conditions to support it. These conditions are now getting harder to find. Of course it could end.\(^6\)

He indicates that science could also wind down if scientists, even given adequate resources, failed for whatever reason to make further headway. If we keep in mind Kuhn's rejection of a correspondence-based construal of truth, the door is left more open to a pragmatically oriented understanding of science. Kuhn enunciates this himself:

I think this way of talking and thinking that I am engaged in opens up a range of possibilities that can be investigated. But it, like any scientific construct, has to be evaluated simply for its utility--for what you can do with it.\(^7\)

Were the usefulness of the special activities which constitute science to prove exhausted at some point, then, the particular picture of knowledge to which we have become accustomed in this culture would presumably give way to

\(^6\) Horgan, pp. 44, 46.
\(^7\) Ibid.
something very different. This, in itself, would be a dramatic illustration of Kuhn's point: a radical shift in *a priori* structure, although of a more general order than the sort seen within science itself.
Allies or Adversaries: the Troubled Meeting of Rorty and Kuhn

Nature, Mind, and Correspondence Theories

The most obvious area of agreement linking Rorty and Kuhn lies in their rejection of the sort of "traditional" model typified by correspondence theories of knowledge. I have referred to this in point one of the list, included in my introduction, of similarities which Rorty sees between himself and Kuhn.¹ Neither Rorty nor Kuhn wants to promote the view wherein knowledge is gained through the mind's achieving an accurate representation of a mind-independent, external reality. Both would recognise that such representation would probably require the discovery of a pure observation-language, i.e., one free of significant perspectival influence, the existence of which both deny. Rorty observes that the traditional view entailed that procedures of justification, in order to be truly rational, must lead to the truth, to correspondence to reality. This picture is a straightforward metaphysical-epistemological one, antithetical to both Rorty's post-modern outlook of shifting, criterionless discourses and Kuhn's basically Kantian view wherein the world is, in part, constituted by an a priori template. What they are rejecting, in particular, is the mind as blank slate--Locke's tabula rasa--which receives ideas as sensations from a world outside the mind. But despite this commonality, they repudiate the correspondence model for strikingly different reasons. Rorty cannot entertain any framework which makes use of a notion of the mind, so long as he advocates the abolishment of this notion altogether. Since he takes himself to be removing all elements which might serve as a foundation for epistemology, he is also hostile to the notion of there being anything at all which might viably count as knowledge. Kuhn, on the other hand, shows no interest whatsoever in throwing out either the notion of mind or of knowledge.

I believe that Kuhn would see it as a plain violation of common sense to argue against mind and mental experience. This is reflected in his assertion that we have no elements more basic than sense-data reports, such as "green there". This is not to say that we need impute to Kuhn a metaphysical belief in mind as mental substance, or sense-data as mental entities. Kuhn--again, like Rorty, although for different reasons--eschews the making of metaphysical claims in epistemic talk. For Kuhn, metaphysical claims would reflect an expectation of our having knowledge of a mind-independent world. Since, for him, our knowledge is

¹ See pp. 1-2.
to some degree a world-constituting affair, metaphysical claims are totally out of place. There is no theory-independent way to determine what is really there. A world not understood through a particular mental template is an unintelligible one. But Kuhn does not want to abandon entirely the notion of a world “out there”; this is evidenced by his insistence that the “given”—however nebulous it may be without the application of a template—is an essential element of experience, and thus of knowledge (the template itself being the other essential element). He understands Nature as “giving” the stimulus, which is determined into a sensation by means of being perceived through a human template of some sort. This leaves discovery as part of what is involved in the acquisition of knowledge. Therefore, for Kuhn, the question cannot merely be one of language,

Rorty’s re-conception of certainty as a matter of conversation between persons, rather than a matter of interaction with non-human reality would be far too weak a thesis for Kuhn. Without some sense of certainty conferred by their research-matrix, scientists would be unlikely to develop the commitment to that matrix which is necessary for normal scientific research to proceed. The matrix helps them to formulate their understanding of Nature. During a revolution, that sense of security is shaken, but it will inevitably (for as long as science lasts) be restored by convergence of the community around a new dominant matrix in which scientists’ sense of certainty will come to be invested. Scientists must have some conviction where the most basic of their professional beliefs are concerned. But even though these procedures of what we might call social justification (within the scientific community) are employed to delineate what is to count as knowledge, they are not invoked in a climate devoid of input from the natural world. Further, to say that social justification is a matter of conversation implies that it is less structured—less grounded in a conceptual and methodological framework—than the way in which Kuhn believes it to operate in the matrix-based system of modern science. Even where conversation is understood to be guided by a set of conventions, those conventions are more easily subject to change (i.e., with less resistance and dependence upon a critical need for change, and with considerably less sense of crisis) than in matrix-changes in science. The scientific matrix is relatively more robust than are conversational conventions. This point renders Rorty’s belief as expressed in point five of my introductory list of putative similarities inaccurate.

In contrast, Rorty rejects the metaphysical on account of his ultimate rejection of any world at all beyond the various and specific linguistic patterns which make up the plurality of discourses; he takes the stance that the “given” is nothing but empty and misleading baggage, and that all attempts to make sense

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2 See p. 47.
of any sort of world "out there" have proven utterly fruitless. Consequently, and on the basis of Rorty's rejection of the input of any special mental processes, we arrive at the position where the world is nothing at all more than a linguistic creation. While for Kuhn, the world as we know it is the product of a process which includes both discovery and creation, Rorty's world (not something which can really be said to be known) is strictly a creation. Kuhn has remarked on the recent popularity of conceptions of this sort, placing them under the rubric of proposals advocating a mind-dependent world, although, for obvious reasons, the lexicon of Rorty's philosophy would have to be adjusted somewhat to accommodate the sort of view which Kuhn is driving at: a human- or language-dependent world, perhaps.

The question of scepticism which Rorty finds so troubling as a legacy of correspondence theories of knowledge, is not an irritant in his own programme, because mind, world, and all considerations of knowledge fall away, anyway. For Kuhn, the situation is somewhat different: we would not expect scepticism to appear as a problem haunting any programme which defined knowledge as being partially constituted by a priori mental components. This Kantian sort of picture is not conducive to worry about whether or not we can know the world as it really is, independent of our minds. Because the known world is not mind-independent, we do not have to be concerned about how we could possibly bridge a gap between it and our minds.

One might advance to the question of by what or whom this Rortian world is created. Clearly, it cannot be a creation of the mind, for the mind is barred from the Rortian picture. We seem to be left with some sort of materialistic neuro-psychological framework of understanding which Rorty insists that we refrain from viewing in ontological terms. We are instructed to treat this as a metaphor for our discourse on what remains of experience in the Rortian view. But one does not need to have ontological sentiments in order to have a working reference to mind, or to the ideas and raw feels connected with it. Even if we do not want to posit mind as an ontological entity, it would nevertheless probably strike Kuhn as counter-intuitive to avoid mentalistic talk completely, and resort to stimulus-oriented talk, which is set at a remove from experience, and which is the product of elaborate theory; we do not have direct access to stimuli. Kuhn's belief in the primitive nature of the phenomenal ("green there") supports this. He prefers to talk in terms of experience (and observations), rather than of the theoretical elements which stimuli are. Rorty's preferred discourse--centring on stimulated C-fibres, for instance, and in which phenomenal experience is denied altogether--operates at a strictly theoretical level, one which is at a remove from experience itself.

Consistent with his retention of the mind, then, and in clear opposition to Rorty, Kuhn preserves the notion of "something like a mental representation"\(^3\), which we understand from our vantage-point inside a community to mediate our interactions, and those of the rest of our community, with the environment. Kuhn

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\(^3\) See p. 48.
also speaks of the perspective from outside a community, from which we see it adapting to its environment. I would expect that mental representations would be involved in this perspective, as well, since observation is involved.

Rorty describes Willard van Orman Quine as rebuking such writers as Kuhn "for wanting to drop the notion of observation altogether". While Rorty disagrees with Quine's belief in the worth of retaining this notion, it does not appear that he takes any issue with Quine on the point of the latter's reading of Kuhn as wanting to drop it (assuming that Rorty has himself read Quine correctly). But if we can conclude from this that Rorty, like Quine, sees Kuhn as wanting to drop the notion of observation, then it appears that Rorty has not only overlooked an important feature of Kuhn's Kantian project, but also has rendered Kuhn far more distant from the traditional epistemological picture than he, in fact, is.

Overcoming Epistemology

Rorty re-casts the terms "knowledge" and "objective" (if they are to be retained at all) as applicable to areas of discourse where unforced agreement is relatively frequent and easy to obtain; the terms "matter of opinion" and "subjective", in contrast, are to be applied to areas where unforced agreement is relatively infrequent and achieved with greater difficulty. Therefore, it is not surprising that one of Rorty's few criticisms of Kuhn is that he should have discarded the epistemological project altogether, rather than seeking, as he did, an alternative epistemology to the traditional variety. But Kuhn's commitment to a programme which reserves due place for epistemic considerations—as opposed to Rorty's, which emphatically does not—reflects a sentiment that many of us would find it too jarring to abandon: that we want to be able to believe that we can know what the world is like, even if our beliefs in this regard are inescapably subject to change. Hence, for most of us—and apparently Kuhn, as well—the more traditional picture of selves inhabiting a world about which they justifiably struggle to learn something—such learning culminating in knowledge of that world—has greater viability than does the picture which Rorty paints. This more traditional view is also particularly important where the normal operation of science is concerned, since it forms part of the force of matrix-commitment which inspires and fuels research within a given conceptual framework.

Rorty complains that "Kuhn grants too much to the epistemological tradition", this being revealed in the latter's suggestion "that the philosophy of science has quite a distinct mission from the hermeneutic activities of the historian of science". Kuhn distinguishes the historical enterprise from that with which the philosophy of science typically concerns itself, in that "[u]nlike history...[the philosophy of science] is comparatively little concerned with the

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4 Rorty, PMN, p. 225.
5 Ibid. p. 340n20.
temporal development of theory, emphasizing instead the theory as a static structure, an example of sound knowledge at some particular, though unspecified, time and place....Philosophy’s business is with rational reconstruction, and it need preserve only those elements of its subject essential to science as sound knowledge”. This is to say that philosophy is generally concerned with what counts as knowledge, and not with how what counts as knowledge has changed, or even that it has changed. For Rorty, this has the unfortunate consequence of preserving “intact the myth that there is something called ‘the nature of sound knowledge’ for philosophers to describe, an activity quite distinct from what counts as justification within the various disciplinary matrices constituting the culture of the day”. But for Kuhn, although philosophy would do well to take some lessons from history, a historically enlightened philosophy of science would still occupy itself with the search for an account of what is to be accepted as sound knowledge. Kuhn’s observation that criteria of knowledge have transformed, sometimes dramatically, over time does not jeopardise the validity of an epistemic orientation per se.

This consideration highlights the fact that, while both Rorty and Kuhn regard sensitivity to historical circumstance as important, their respective understandings of the overall context in which one is to express this historical sensitivity are radically divergent. For Kuhn, this awareness ought to inform our approach to an understanding of what it is to have knowledge; for Rorty, such a project is misguided and counter-productive. We are also brought again to the inaccuracy inherent in Rorty’s belief as expressed in my introductory point five.

The Need for an A Priori Framework

Experience and the knowledge-acquisition which may follow from it reflect, for Kuhn, a process which is part discovery (taking account of the “given” from Nature) and part creativity (taking account of a priori matrices). The matrix is the movable template through which whatever is “given” is rendered intelligible experience. The learning of a matrix which determines sensations in this way is not just a matter of learning to assume postures—or make linguistic utterances—in response to stimuli. It is learning how to see or hear or feel stimuli as particular sensations. As Kuhn has advised, we do not just discover that something is (and later interpret what it is), but rather discover what something is from the start. The matrix, then, plays a crucial role in the generation of facts, as well as of beliefs about what sensations one has experienced.

One of Kuhn’s points with respect to the role of the matrix relates precisely to this situation. The matrix determines, and thus makes possible, the

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8 See point eight in my introductory list.
observations which scientists make. One of the facets of Kuhnian incommensurability is that alternative matrices will generate alternative sets of observations, not all of which will be commensurable. Observations will be a part of what is different between alternative discourses. Different matrices tell us different things about Nature. Evidence plays a considerable role in the acceptance or rejection of a theory. Kuhn suggests that, if we subtract accuracy-of-fit-to-Nature from the set of scientific values, “the enterprise that results may not resemble science at all”. So Kuhn is clearly not advocating that we dispense with observation-talk. Rorty may be surmising that Kuhn is so advocating, on account of the latter's rejection of a single, overarching, neutral observation-language.

Rorty's observation that we have so far failed to identify causal mechanisms linking raw feels to neural-stimulation events is echoed in Kuhn's remarks on the insufficiency of our understanding of how the mind works in perception. Still, Kuhn--unlike Rorty--takes the general position that there is a route from stimulus to sensation, and that the presence of such a pathway (acted upon as it is by the influences of genetic predisposition, education, and socialisation) is what allows us to make sense of the idea that different matrices can generate different facts or beliefs.

Rorty's dismissal of a generalised framework of thought and experience leads him to emphasise particulars--“from the sound of a word through the color of a leaf to the feel of a piece of skin”--which can “dramatize and crystallize a human being's sense of self-identity”. But the Kuhnian point which speaks against the ability to do this is that there can be no particulars without the presence of a general framework by means of which particulars are picked out in the first place. A given Gestalt is set by a matrix, and particulars are fragments of a specific Gestalt, which must be set before particulars can be picked out of it. If the Gestalt shifts, the character of identifiable particulars may also change, and possibly radically enough that what were distinct particulars before are no longer, while new ones emerge.

Rorty appeals to imagination as a force which generates a particular discourse, and habit as one which sustains it for a time as the status quo. In a Kuhnian scientific world, imagination and habit no doubt have a role of some magnitude or other, but the story runs much deeper than that. All the values which I have mentioned in Chapter Two--problem-solving abilities, scope, etc.--come into play here much more strongly than do these relatively superficial elements which Rorty employs to describe the birth and success of discourses.

Rorty asserts that the justificatory process cannot operate outside of reference to already-accepted beliefs and language-practices. So it would seem that the structure of the “web of beliefs” works to justify what further beliefs can

9 See quote, p. 49.
10 Kuhn, ET, p. 331.
11 See introductory point two.
12 See p. 13.
be incorporated into that web, while retaining coherence. But now it looks as though we are approaching being able to abstract a set of criteria of intelligibility from the body of the discourse, or at least to identify similarity-relations, upon which it seems to me that a "web" would have to depend. Specific sets of similarity-relations are what are required to set discourses off from each other. Indeed, Rorty wants to be able to judge "particular present situations and options as similar to or different from particular past actions or events". Further, he describes his version of pragmatism as geared towards replacing the notion of true beliefs with "successful rules for action" (my emphasis). But if this is what we end up with, then Rorty's system is beginning to look more like Kuhn's, making Rorty Kuhnian in a way which he would not want to be, and which he would not even recognise as Kuhnian. It is difficult, though, to pin down precisely to what degree Rorty is Kuhnian in this way, since he both wants to maintain something like an evaluative framework (albeit an unhelpfully vague one) through which assertions in a discourse can be justified, and yet disparages anything which might resemble criteria (presumably including the less-abstract similarity-relations), favouring a sort of lightly structured (if structured at all) poetic, self-creative urge. Rorty underlines this by describing himself as "auxiliary to the poet rather than to the physicist". All of this renders any similarity between Rorty and Kuhn on the issue of an accepted conceptual and practical framework considerably less likely.

Following this poetically oriented line, Rorty states that

[t]he craftsman typically knows what job he needs to do before picking or inventing tools with which to do it. By contrast, someone like Galileo, Yeats, or Hegel (a "poet" in my wide sense of the term--the sense of "one who makes things new") is typically unable to make clear exactly what it is that he wants to do before developing the language in which he succeeds in doing it. His new vocabulary makes possible, for the first time, a formulation of its own purpose. It is a tool for doing something which could not have been envisaged prior to the development of a particular set of descriptions, those which it itself helps to provide. But I shall, for the moment, ignore this disanalogy. I want simply to remark that the contrast between the jigsaw-puzzle and the "tool" models of alternative vocabularies reflects the contrast between...the will to truth and the will to self-overcoming. Both are expressions of the attempt to represent or express something that was already there and the attempt to make something that had never been dreamed of before.

But it is difficult to see what would motivate someone towards the construction of a new vocabulary if he had no idea what he was trying to do. Since new Kuhnian matrices arise out of the soil of the old, some guidance in the construction of a new order will be provided by the old framework. Also, the previously mentioned (Chapter Two) general values which guide theory-choice contribute to the

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13 Rorty ORT, p. 65.
14 Rorty, CIS, p. 8.
15 See again introductory point five.
scientist's sense of what needs to be done; foremost among these is the aim of problem-solving. While some of the problems up for discussion will disappear subsequent to a matrix-shift, and others not yet encountered will develop, many problems will subsist through the change. Leaving aside Yeats and Hegel, we can see that it may be somewhat premature to expect somebody such as Galileo to have a full picture of what normal scientific research under his new regime would look like, at the time that he proposed it. Still, Kuhn states that “[t]he man who premise a paradigm when arguing in its defense can nonetheless provide a clear exhibit of what scientific practice will be like for those who adopt the new view of nature”. Further research under the new order would serve to deepen, refine, and extend its “vocabulary”. One can at least say with some plausibility that Galileo promoted a heliocentric system to answer concerns of calendar-reform, and to deal with the build-up of anomalies under the geocentric Ptolemaic system. Both of these demands provided specific motivations which would have instilled in Galileo some idea of what his task was. This is not to say that imaginative input and “flashes of intuition” are alien to the birth of a new matrix; Kuhn has made us aware of their importance. But it is to say that there is greater structure informing even imagination and intuition, where scientific projects are the focus, than we see in the construction of poetry. If we must widen the meaning of the term “poet” to the extent which Rorty seems to be advocating, we do so at the expense of valuable clarity. Rorty's poetically oriented account of discourse-alteration offers no explanation of why motives change in the particular way that they do.

Rorty's contention that “[t]he application of such honorifics as ‘objective’ and ‘cognitive’ is never anything more than the presence of, or the hope for, agreement among inquirers” rings rather hollow when we begin to enquire why those areas of the culture which enjoy a greater degree of agreement--notably science, as Kuhn has explained--have gotten to that point. The Kuhnian argument is precisely that we have come to be able to expect to achieve relatively greater agreement within the sciences (at least, during the periods of normal stability which are by far the most prevalent periods) because of the presence of a commonly accepted a priori matrical structure. So the advantage is conferred not only by the existence of a matrical structure at all, but also by the fact that, in a mature science, there will be only one such structure operative at any one time, increasing scientists' sense of security in their own work. Scientists work towards the facilitation of agreement by first striving to determine an appropriate structure for research, and then by seeking to prevent the decay of that structure at times when it comes under stress (as in the recognition of an accumulation of anomalies). Hence, it is not merely a matter of chance, as it appears that it would have to be in Rorty's estimation, that agreement is achieved in specific areas of culture; rather, it is quite by design.

Rorty asserts that we can explain the observations which scientists make by reference to their “psychologies and sensibilities”; we can explain their

17 See p. 39.
18 Rorty, PMN, p. 335.
propensities to react with certain sentences to certain stimuli by reference to their upbringing. Scientists have been "programmed" so as to respond to certain retinal patterns with statements such as "[T]here goes a neutrino".\(^{19}\) There are two points which need be made here. One is that, for Kuhn, there is clearly more to the question of "programming" of scientists than what we might call mere "psychologies and sensibilities", although factors such as this would be expected to have their influence. What Kuhn is interested in elucidating, however, is that this "programming" is deliberately built in--and in a quite sophisticated manner--to the training of prospective scientists, in the form of the conceptual matrix which will define their future scientific research, and most especially in the form of the exemplars which are offered to the student as concrete cases which illustrate the application of matrical concepts. The stronger the presence of a collectively and deliberately designed framework, the less room for influence by individual psychological idiosyncrasies. The second point is the crucial one, that this "programming" of which Rorty speaks is, again, significantly reminiscent of Kuhnian matrices, and we are brought back round to the point that Rorty has not effectively escaped the structural tone which Kuhn has maintained throughout his own thesis, whether Rorty has detected it or not.

On the issue of the presence of matrical elements, one might take Rorty's and Kuhn's respective positions to be similar, in that we can substitute Rortian discourses for Kuhnian matrices, or vice versa. Rorty, himself, views his discussion of discourses to be a generalized account of the more specific Kuhnian "discourses". Support for this view might be assumed to be found in both Rorty's and Kuhn's denial of criteria, or rules, as being important in how we approach the world. But Rorty's discourses are more superficial than Kuhn's matrices. We see this in the miscibility of Rorty's discourses with each other, as in the sentence "If we had just stuck in an electrode in the right place in the cortex, he would never have decided he was Napoleon".\(^{20}\) Here, we have a neuro-psychological vocabulary side-by-side with a behavioural-psychological one. Kuhnian matrices, on the other hand, could not possibly co-exist in the same community at the same time, for they represent a conception of Nature, and alternative conceptions of Nature cannot receive simultaneous commitment from a homogeneous group. It is clear from their relation to a sense of the way in which the world is, that Kuhnian matrices must run deeper than Rortian vocabularies. As such, they will be more significant in guiding action. This leaves us, in a Rortian world, with a severely weakened framework for belief, and hence for action.

This weakening is reflected in the fact that Rorty's dismissal of criteria for judgment is far more radical--and more complete--than is Kuhn's. Kuhn diminishes the importance of criteria really only during times of matrical stability, when the normal investigative process occurs through the construction of similarity-relations between things and events in the world. Learning a particular way of carving up the world, so to speak, is achieved through particular instances

\(^{19}\) Rorty, \textit{ORT}, p. 56.

\(^{20}\) See p. 11.
of ostension more than through the learning of general rules. It is possible to
abstract rules from the accepted beliefs and procedures of the matrix, although
this is generally done after the fact of the construction of the matrix. The need for
abstracted rules becomes much more evident during times of matrix-instability.
This is clearly a far more subtle position than is Rorty's, where the acceptance or
rejection of criteria seems to be an all-or-nothing affair, and no allowance is made
for any sort of framework guiding the construction of even similarity-relations.
Again, this underscores the inaccuracy of Rorty's evaluation of the similarity of
Kuhn's programme to his own, as expressed in my introductory point five.

Rorty's allowance—albeit a begrudging one—for the remote possibility of
"innatist" views of a genetically "wired-in" language of thought reveals further
potential for an embarrassing slide into Kuhnian territory. Possibly such a
language of thought counts as part of that to which Kuhn is referring in his talk of
the genetic elements which people share, which includes the common ground
provided for through biological determination of lexical structure. This relates to
the perceptual integrity of which Kuhn speaks, which is born of the selection
process, driven by survival-pressure.21 Rorty's dismissal of the importance of our
determining which elements of human behaviour would be directed by an innate
genetic matrix, and which not, is further illustration of a regrettable lack of
subtlety in his programme. Distinguishing that which is innate from that which is
not can only help to advance our self-knowledge, and to yield improved
discriminating-power with respect to what it is possible for us to alter in our
thought and behaviour, and what remains more fixed. If we duly recognise the
significant function of some sort of matrix through which we necessarily
experience the world, then clarification as to which elements of that matrix are
more fluid and which more solid proves to be of epistemic importance. This
would be a matter of concern to Kuhn. Rorty's denial of the importance of the
distinction between fixed and movable elements has led him into some
unfortunate confusion. He warns us that allowance for any fixed language of
thought should not slide into the premise that that language is immune to
correction on the basis of experience. But we are not accustomed to viewing
genetically fixed elements as subject to correction on the basis of experience,
anyway, at least not directly. What will be experientially corrigeable are the
transformable elements of non-innate matrices, such as Kuhn describes for
science: his movable Archimedean platforms. If Rorty is allowing for these, this
again brings him closer to Kuhnian structure. Moreover, Rorty's reference to
Fodor is evidence of his own recognition of the identifiability of at least some
sort of a priori structure.

Rorty seems to be attempting to further his argument against the tendency
to formulate methodologies of any sort by objecting that the view that science
operates on the basis of our knowing in advance in what sort of discourse an
explanation of concept-formation shall be formulated "takes us around a rather

21 See p. 46.
tiny circle”.22 Certainly, Kuhn has recognised that the way in which science must operate is inherently circular: it cannot function without first delineating what is to count as acceptable scientific questions and answers. It sets out its own *a priori* parameters before proceeding according to them; this is precisely what it is to have a matrix. But Kuhn reminds us that not all circularities are vicious.

**Rationality and Solidarity**

One of the chasms on account of which it becomes evident that Rorty and Kuhn are talking past each other results from their strikingly different conceptions of rationality. This contrast seriously undermines the ways in which Rorty sees himself and Kuhn to be similar that I have noted in my introductory points three and especially four. This is yet another term which Rorty has undertaken to define non-standardly. We are offered a frustratingly loose description of what it is to be rational, in the form of a) the recognition of being part of one's own community, which derives from the common dictates of the community and its language, and b) victory or persuasiveness in argument. The latter rendering is something we would consider to be consistent with Kuhn, since he does acknowledge the importance of persuasiveness in theory-choice. But because Kuhn defines “rationality” in the standard manner, he does not come to *equate* it with persuasiveness, as does Rorty, and is therefore able to cite both rational and non-rational influences on the persuasiveness of attempts to promote a new theory. Again, his characterisation achieves greater subtlety than does Rorty’s. Kuhn also maintains a strong commitment to traditional reasoning, which both must constitute the rational elements of the consideration of new matrix-candidates, and must figure prominently in the internal logical workings of an accepted matrix during periods of normal stability. So for Kuhn, the social practice cannot entirely define what is to count as rational, because rationality, although to some degree defined specifically by the internal logic of a given matrix (*roughly* akin to a specialized case of Rorty’s social practice), is also to some degree an element which enters into the *choice* of a particular social practice, or, in more precise Kuhnian terms, theory-choice (although not in a form sufficiently complete to generate an algorithm). The scientist’s recourse to persuasion does not suggest a lack of very good reasons for choosing a theory.23 Kuhn comments that “[t]o suppose...that we possess criteria of rationality which are independent of our understanding of the essentials of the scientific process is to open the door to cloud-cuckoo land”.24 Presumably, the same goes for suppositions that we do not possess standard criteria of rationality at all. Adherence to logical rules is a presupposition of normal scientific discourse.

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22 Rorty, *ORT*, p. 57.
24 Ibid., p. 264.
Rorty asks, "Is the sort of community which is exemplified by scientific inquirers and by democratic political institutions a means to an end, or is the formation of such communities the only goal we need?" He considers debates about Kuhn's "irrationalism" to boil down to this question.\textsuperscript{25} It seems that Kuhn's "irrationalism" relates to this issue on account of the presence of standard rationality pervading the traditional adjustment of means to ends. Refusing to see something in terms of this adjustment would have the beneficial effect of obviating standard rationality. But I believe that Kuhn would see scientific communities not as ends in and of themselves, but rather as instruments functioning to further science as an epistemic organ. The community exists for a quite specific purpose: to promote growth of the body of scientific knowledge. The maturation of a science to the point where one dominant community has replaced a plurality of competing schools furthers the realisation of this end, since the unified matrix of the mature scientific community need no longer expend energy in disputes over the fundamentals of the research-matrix. It is not here just a matter of a single scientific school demonstrating greater solidarity than a diverse collection of such schools simply for the sake of increased comradeship, which we may imagine to be the case in other sorts of endeavours, very different from the scientific one. So the scientific community described by Kuhn is clearly a means to an end. In fact, to some degree, it functions as an expression of the matrix. This latter point seems not so far-fetched, if we consider that matrical shifts, in re-organising and re-defining what is to count as scientific, may result in established members of the community being edged-out, on account of what might be seen as an obstinate tendency to remain faithful to a dying matrix, while a group of researchers perhaps new to the field, having proposed the new matrix, gains ascendancy in the field as that matrix becomes dominant. In this respect, the matrix, as the tool exercised by the dominant majority of scientists within the community (dominant since the ascendancy of this new matrix), can exert an effect upon the constitution of that community, even though the primary influence is probably in the other direction--i.e., wherein a specific community of practitioners is united by their disciplinary matrix, which then becomes an expression of the community.

Rorty invokes Dewey as having argued (in speaking of morals) that we only know what we want after we have seen the results of our attempts to get what we once thought we wanted, and that the use of new means changes ends.\textsuperscript{26} Rorty sees the case of "post-positivistic philosophy of science" as analogous. It is probable that Kuhn would agree that Dewey's picture of morality corresponds plausibly with his own view of science. The fact that we are moved continually to adjust our ends--sometimes upon the introduction of previously unknown means--is perfectly consistent with the conception of science put forward by Kuhn. There are two processes going on here, in the statements which Rorty is attributing to Dewey. Firstly, a matrix may be required to shift as it becomes evident that its

\textsuperscript{25} Rorty, \textit{ORT}, p. 43.
\textsuperscript{26} Ibid., p. 68.
problem-solving ability is beginning to wan significantly—it is failing to yield what was desired, and we see that the results of our attempts to get what we once thought we wanted are less than satisfactory. A shift ushers in a new set of goals, at least to some degree, such that what we want has changed. The new matrix is expected to provide what is desired more effectively than the old one proved able to do, but it is clear that what is desired may exhibit a quite different form after the shift from before. The new regime may bring pressure to bear upon the scientific community to hasten the development of new instrumentation with which to gather evidence by means of which the new matrix may be refined and extended. But alternatively, and this is the second aspect of what Rorty is attributing to Dewey—new instrumentation may be devised which will so revolutionise the phenomena available for discovery that the ends of research will be altered to keep pace with it. The development of the electron microscope provides an example of this, and is cited by Kuhn as an instance where an occurrence in one scientific field has the effect of instigating a revolution in another. In this way, the introduction of novel means can transform the ends which are sought.

But this Dewey-Kuhn correspondence does not provide evidence in favour of Rorty's characterisation of science as more like a paradigm case of muddling through “than like a series of choices between alternative theories on the basis of observational results”27 Kuhn's account of what must go on in science is considerably more exact and structured than Rorty's view of discourses. For Kuhn, alternative theories become necessary, often because observational results begin to reveal a progressive build-up of anomalies. Even during a crisis-period, it does not seem appropriate to characterise what is going on as “muddling though”, given that the threads of the out-going matrix still provide some residual structure for conceptualisation, and that, at any rate, the way out of the state of relative chaos will be aided by the constancy of the various scientific values which I listed in Chapter Two. The further point might be made here that, even if there is some adjustment to be made in what we expect a theory to do for us, the adjustment of means—which really amounts to the adjustment of a methodology—is not equivalent to a lack of such methodology.

Rorty also cites Dewey as saying that intellectual progress usually occurs through the sheer abandonment of questions: rather than solving them, we get over them. Kuhn's position is again divergent: the very constant existence of some set of questions or other is vital in demarcating the matrix of scientific research—the matrix stipulates admissible questions. We can make no sense of the crucial motivation of problem-solving without acknowledging the need to solve questions. We might say that this very need is the engine which drives research. Even though revolutionary shifts in matrix lead to the “overcoming” of some questions, other questions will nevertheless survive the shift, and moreover, new questions will be generated as a result of it.

27 Ibid., p. 69.
Rorty states that what he calls the "objectivist tradition" seeks an ahistorical human nature; it is this nature, as shared common ground, which is supposed to bind humanity together. The traditional notion of rationality figures prominently in the general account of what it is to be human. But this "metaphor of inquiry and human activity generally" is one of convergence and unification; it is, for Rorty, inaccurate, because human activity demonstrates proliferative tendencies, and a movement towards diversification. We need "to throw out the last residues of the notion of 'transcultural rationality'." In this connection, he notes with approval that, in Kuhn, the philosophy of science becomes increasingly historicist and decreasingly logical. To him, this is beneficial, since our understanding of why the new science was superior to Aristotle's work, and of the relations between the new science and mathematics, common sense, theology, and morality is aided by our turning outwards, towards the social context of justification, instead of by our turning inwards, towards the relations between inner representations. Typically, the turn inwards has relied upon the employment of reason as a quintessentially human modality, one which unifies and defines all of humanity. In contrast, social contexts of justification as Rorty envisions them tend towards pluralism.

Still, Rorty argues for the preservation of the intellectual, social, and political "habits" which were nurtured by the Enlightenment, although he does not want to see these justified by traditional conceptions of rationality and truth, but rather by "a conception of rationality as criterionless muddling through, and by a pragmatist conception of truth". The sense of rationality which Rorty advises us to discard is that which directs us to be methodical, to have criteria for success laid down in advance. In Rortian terms, this is the stronger sense of rationality, of which science is generally taken to be the paradigm: we traditionally expect to have clear criteria for the success of scientific theories, by means of which their ability to predict is augmented. Strong rationality is associated with objective truth, correspondence to reality, method, and criteria. Rorty's re-envisioning of rationality leads us in a rather different direction: towards moral virtues, such as tolerance, respect for others' opinions, a willingness to listen, and reliance on persuasion in place of force. To be rational, then, is to be "sane" and "reasonable". Rationality is not the exercise of a faculty called "reason", a faculty which stands in some determinate relation to reality. We define the term more in relation to being "civilized" than to being "methodical": "We should avoid the idea that there is some special virtue in knowing in advance what criteria you are going to satisfy, in having standards by which to measure progress". Holistic pragmatism wants to hold on to the materialistic world-view that typically forms the background of contemporary liberal self-consciousness, while refraining from advancing the claim that this view has been established through a method. The formulation of methodological principles is considered to be generally a waste of time, anyway, since one often ends up only with "a string of platitudes, hooked up to look like an..."
algorithm”. In avoiding criteria, we avoid dogmatism, defensiveness, and righteous indignation.\(^{29}\)

There are a couple of elements here which I find problematic. Firstly, the type of picture which Rorty here paints, and which he implies is in concert with Kuhn’s vision, is quite strikingly antithetical to what Kuhn is attempting to show.\(^{30}\) While for Kuhn, criteria, or rules, are not the normal means by which prospective scientists are trained into their adopted scientific tradition, it is possible to abstract such criteria from the matrical framework after the fact of the laying-down of that framework. So, while criteria do not participate in the building or communication of the matrix, they can be made sense of, if we wish to focus attention on the skeleton of the matrix—a need which will become more apparent when the matrix appears in danger of crumbling, i.e., at times of crisis. At any rate, the manipulation of similarity-relations between things and events in the world which occurs as the matrix is refined and extended during periods of normal science is not a process which is clean of rational judgments. So Rorty’s statement that “Kuhn’s defenders [among which he counts himself]...typically draw the line between the rational and the non-rational sociologically (in terms of a distinction between persuasion and force) rather than methodologically (in terms of the distinction between possession and lack of explicit criteria)”\(^{31}\) reveals an irony: Kuhn himself would not espouse the position which his supposed defenders take. Secondly, Rorty appears to be directing his polemic against a sort of scientistic romanticism which regards scientific research somewhat as a show of virtue in the battle of knowledge and enlightenment versus ignorance and the chaos of irrationality. While Kuhn could certainly be expected similarly to turn away from this picture of science, it is clear, nevertheless, that he wants to maintain a definite distinction between the attitude and methodology of science, and the tone of other cultural endeavours.\(^{32}\) So working with an a priori matrical foundation, while not necessarily “virtuous”, is at least useful. Even an understanding of one’s research as contributing to an elucidation of Nature does not necessarily suggest the sort of moral righteousness in science that Rorty is holding up for scorn. The employment of rational methods does not entail, or even always reflect, the scientific tradition’s having made “the natural scientist into a new sort of priest”, even if we view her role as providing “a link between the human and the non-human”.\(^{33}\)

Considering the issue of whether human activity should properly be characterised as converging and unifying, or proliferating and diversifying, from Kuhn’s point of view, the question is not perhaps so simple. In the evolution of a scientific discipline into the more mature stage typified by work being done under

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\(^{29}\) Ibid., pp. 28, 36-7, 62, 65.

\(^{30}\) See again my introductory point five.

\(^{31}\) Rorty, ORT, p. 48.

\(^{32}\) See pp. 56-8, and the next section of this chapter; this issue also relates back to point seven of my introduction.

\(^{33}\) Rorty, ORT, p. 37.
a single dominant matrix, clearly we see a convergent pattern. But there is a corresponding proliferation, over time, as the matrices of scientific disciplines become more detailed, and as a result, more specialised and narrow in their focus; Kuhn has described how this tendency leads to single disciplines splitting up to produce new sub-disciplines, which eventually become quite separate scientific communities in their own right, complete with their own distinct defining matrices.

Rorty may be making the mistake of equating the traditional search for an ahistorical scientific conceptual and methodological framework with the acknowledgement of the presence of such a framework per se. But this would provide a rather crude analysis, as an examination of Kuhn's writings indicates. The movable a priori which is a fundamental feature of Kuhn's Kantian type of programme is clearly both sensitive to historical fluctuations and responsive to the need for a framework of some sort through which investigation is facilitated. Indeed, the use of a given framework, though it is stable only for periods of variable longevity, is, according to Kuhn, the norm.34

Returning to the notion of a transcultural rationality, we might identify a central feature of such a rationality as the process of induction. As we ought to expect, one of Rorty's complaints against Kuhn is that Kuhn “occasionally makes too large concessions to the tradition, particularly when he suggests that there is a serious and unresolved problem about why the scientific enterprise has been doing so nicely lately”.35 He quotes Kuhn as linking this to the problem of induction:

Even those who have followed me this far will want to know how a value-based enterprise of the sort I have described can develop as a science does, repeatedly producing powerful new techniques for prediction and control. To that question, unfortunately, I have no answer at all, but that is only another way of saying that I make no claim to have solved the problem of induction. If science did progress by virtue of some shared and binding algorithm of choice, I would be equally at a loss to explain its success. The lacuna is one I feel acutely, but its presence does not differentiate my position from the tradition.36

Rorty argues that the desire to provide such an explanation as Kuhn seeks “is one more result of hypostatizing the Platonic focus imaginarius--truth as disjoined from agreement--and allowing the gap between oneself and that unconditional ideal to make one feel that one does not yet understand the conditions of one's existence”.37 But the point, for Kuhn, is not to understand the conditions of one's existence, but to seek to understand the conditions of knowledge, and specifically, of scientific knowledge. The desire to explain the success of science, which, at its root, entails the desire to solve the problem of induction, does not entail a

34 See again point five in the Introduction.
35 Rorty, PMN, p. 340
37 Rorty, PMN, p. 340.
disjoining of truth from agreement. Indeed, it is the desire to explain how such agreement might reliably be reached at all. It appears, in this particular instance, that Rorty is importing a correspondence-view into the question, imputing to Kuhn a certain degree of slippage back into that territory from which he has moved away. But Kuhn need not be, and I think it is clear is not, influenced by correspondence-theory models, in his concerns about the problem of induction. 38

Rorty argues that Kuhn's worry about trying to explain how science as a value-based enterprise could evolve powerful techniques for prediction and control reflects Kuhn's desire to solve "the problem of fact and value". He contends that

[what we need, rather than a solution to the 'problem of induction', is the ability to think about science in such a way that its being a "value-based enterprise" occasions no surprise. All that hinders us from doing so is the ingrained notion that "values" are "inner" whereas "facts" are "outer." 39

But I do not think that it is Kuhn's purpose to lean very heavily on any inner-outer distinction on this issue. His concern here is to explain how it is that the level of agreement which science characteristically attains—and which permits its success—is achieved at all; it is to this end that he explores the vital importance of the matrical structure of normal science. Values manifest ultimately as facts, through their influence on theory-choice, given that particular sets of facts are generated by particular theories. Induction is one of the means through which this process happens. So exploring the problem of induction can offer a means by which we are enabled to see that it is not surprising that science operates according to certain values.

Science and Cultural Holism

In some moods, Rorty speaks favourably of the prospect of the term "science", as well as the distinctions between science and other aspects of the culture, gradually fading away. He claims that there is nothing to be gained from differentially valuing explanations as either "scientific" or "unscientific". There is just, rather incidentally, a greater degree of agreement among people in, say, scientific activities than among those involved in many other activities.

My rejection of traditional notions of rationality can be summed up by saying that the only sense in which science is exemplary is that it is a model of human solidarity. We should think of the institutions and practices which make up various scientific communities as providing suggestions about the way in which the rest of culture might

38 Rorty, in fact, does, in general, recognise that Kuhn's programme is not one which is compatible with correspondence-theorisation. See introductory point one.
39 Rorty, PMN, p. 341.
organize itself. When we say that our legislatures are "unrepresentative" or "dominated by special interests", or that the art world is dominated by "fashion", we are contrasting these areas of culture with areas which seem to be in better order. The natural sciences strike us as being such areas. But on this view, we shall not explain this better order by thinking of the scientists as having a "method" which the rest of us would do well to imitate, not as benefiting from the desirable hardness of their subjects compared with the undesirable softness of other subjects. If we say that sociology or literary criticism "is not a science", we shall mean merely that the amount of agreement among sociologists or literary critics on what counts as significant work, work which needs following up, is less than among, say, microbiologists. 

But it would appear that Rorty has failed to see that this stance is anything but Kuhnian. Much of Kuhn's point is that this state of being in better order is hardly accidental: it is achieved through the presence of the matrix which guides thinking and research. Pragmatic order does not appear ex nihilo, but rather is actively and deliberately sought. It is because of the convergently matrical nature of the mature natural sciences that they do enjoy the relatively heightened degree of agreement that Rorty cites as advantageous.

Rorty claims "open-mindedness" to be an optimal posture for the sciences to assume, an attitude which would, for instance, permit the Royal Society "to reinvent phlogiston if that happened to be what the next scientific revolution demanded". But this sort of move is proscribed by Kuhn's account of the history of science as demonstrating a unidirectional and irreversible progression of successive matrices. The most that we might expect to happen in the direction of re-invention of theories or their elements, if we rely on historical precedent to guide our expectations, would be the appearance of a theory which, if circumstances demanded it (and if someone thought of it), was somewhat more similar in some way(s) to the old phlogiston-theory than to the newer oxygen-theory. Kuhn mentions a situation similar to this in the history of science: "in some important respects, though by no means in all, Einstein's general theory of relativity is closer to Aristotle's [theory] than either of them is to Newton's." But a wholesale re-introduction of a previous matrix is not the pattern which science has demonstrated, according to Kuhn, and most likely for good reason. We would not expect a previously discarded matrix, be it phlogiston-chemistry, Aristotelian mechanics, or whatever, to be re-introduced, if only on the grounds that it has already proven itself inadequate to the tasks demanded of it, which was precisely why it came to be replaced on the heels of a period of scientific crisis.

Rorty sees Kuhn as having taken us further towards dismantling the grip which rationality had on human endeavour, by promoting an understanding of human activity which replaces a static and universal human nature with a historically situated and variable one, as well as by bringing our attention to the myriad of non-rational influences on human behaviour. Thus, Kuhn's "pragmatist

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40 Rorty, ORT, pp. 39-40.
41 Ibid., p. 59.
42 Kuhn, SSR, pp. 206-7.
friends (such as [Rorty]) routinely congratulate him on having softened the distinction between science and nonscience”. This leads pragmatists of Rorty’s stripe to try to enlist Kuhn in their campaign to drop the subjective-objective distinction altogether, substituting the idea of “unforced agreement” for that of “objectivity”. In fact, we ought to break down “all the old philosophical oppositions between mind and world, appearance and reality, subject and object, truth and pleasure”. He presumes that the resulting “rhetoric of culture” would be more Kuhnian, in the sense that it would mention particular concrete achievements—paradigms—more, and “method” less. There would be less talk about rigor and more about originality. The image of the great scientist would not be of somebody who got it right but of somebody who made it new. The new rhetoric would draw more on the vocabulary of Romantic poetry and socialist politics, and less on that of Greek metaphysics, religious morality, or Enlightenment scientism. A scientist would rely on a sense of solidarity with the rest of her profession, rather than a picture of herself as battling through the veils of illusion, guided by the light of reason.

Rationality will thus no longer be seen to be an intellectual virtue, leaving scientists with no general virtues other than reliance on persuasion (rather than force), respect for the opinions of their colleagues, and curiosity with respect to new data and ideas.

On this view there is no reason to praise scientists for being more “objective” or “logical” or “methodical” or “devoted to truth” than other people. But there is plenty of reason to praise the institutions they have developed and within which they work, and to use these as models for the rest of culture. For these institutions give concreteness and detail to the idea of “unforced agreement”. Reference to such institutions fleshes out the idea of “a free and open encounter”—the sort of encounter in which truth cannot fail to win. On this view, to say that truth will win such an encounter is not to make a metaphysical claim about the connection between human reason and the nature of things. It is merely to say that the best way to find out what to believe is to listen to as many suggestions and arguments as you can.

But Rorty is too eager to read Kuhn as an anti-rationalist, and to enlist him in the crusade to abolish all of the traditional dichotomies which Rorty lists. I would expect that Kuhn would favour some sense of an overall human commonality, based on his acknowledgment of general genetic similarity, for instance, as well, perhaps, as an overall similar environment. Moreover, even historical situation and variability in human nature do not of themselves speak against the employment of reason, traditionally conceived. Kuhn’s recognition of the impossibility of an algorithm for theory-choice, combined with his allegiance to the standard conception of rationality, is indicative of a position where

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43 Rorty, ORT, p. 38.
44 Ibid., p. 44.
traditional rationality can be employed in variable ways, depending upon the particular Archimedean platform considered scientifically appropriate at the time.\textsuperscript{46}

The presence of non-rational influences on theory-choice do not function to chase away all rational influences. What Kuhn outlines is a situation in which both rational \textit{and} non-rational factors affect the birth of a new matrical candidate as well as its eventual success in achieving dominance over competing candidates. This point qualifies the similarity which Rorty sees between himself and Kuhn, that I have noted in my Introductory point three.

None of this is intended by Kuhn to "[soften] the distinction between science and nonscience".\textsuperscript{47} In fact, as I illustrated in Chapter Two, Kuhn has certainly made efforts to explain in some detail how scientific disciplines are distinct from non-scientific ones. Some of these differences relate quite directly to the evolution of science towards ever greater efficiency and effectiveness in the problem-solving which is its chief aim. Furthermore, it would be quite peculiar indeed if Kuhn were to be interested in softening this distinction, given that his programme is specifically designed to explicate the workings of science; he does not offer his thesis as an elucidation of general human cultural activities. Rorty's interpretation of Kuhn as espousing "pragmatist holism"\textsuperscript{48} is therefore less than accurate.

Dissolving the distinctions between subject and object, mind and world, appearance and reality, and truth and pleasure are acts which I would expect Kuhn to find unhelpful, as far as scientific activity is concerned. Science, as Kuhn describes it, needs to be able to distinguish between what is to count as truth and reality, and what is deemed pleasure and appearance. After all, that is what a matrix is for. Rorty's tendency to view such distinctions as necessarily intertwined with fidelity to correspondence theories ignores the fact that one can make perfectly good use of these distinctions whilst adhering to a Kantian type of framework, as Kuhn does: repudiating correspondence theories of knowledge, and refraining from ontologising any part of these distinctions. To speak of observation, and of the subject's role in constituting objects (recalling the Kantian view of objects conforming to our knowledge of them, rather than the other way round) requires that some sense be made of a subject-object distinction.

While Rorty sees his new "rhetoric of culture" as "more Kuhnian" than the traditional view, his description of it reveals it to be decidedly \textit{un}-Kuhnian. He equates "particular concrete achievements" with paradigms, indicating that the emphasis on these signals a de-emphasis on "method". But Kuhnian "paradigms" (which Kuhn later broke down into the two categories of conceptual disciplinary matrices and concrete exemplars) are the very vehicle of methodology. To be sure, Kuhn was concerned to stress that typical accounts of science fail to acknowledge the importance of a recognition of concrete particulars; these are

\textsuperscript{46} See again point four in the Introduction.

\textsuperscript{47} See p. 19 and point seven in my Introduction.

\textsuperscript{48} Rorty, \textit{ORT}, p. 65.
embodied within the training of future scientists, in the form of exemplars. But these exemplars encompass both the concrete results achieved by science and the methodology which was employed in the generation of those results. Moreover, concrete achievements are important in demonstrating the character and worthiness of a particular conceptual matrix. Concrete achievements are only seen as achievements through their being sanctioned by the matrix of which they are representative. After a matrix changes, what were seen to be valuable scientific achievements at one time may indeed not even be considered scientific at all, which certainly entails their being rejected as achievements. The conceptual commitments and associated methodology which are prescribed by the matrix of a particular scientific discipline in a particular period are inextricably tied to the concrete achievements of that discipline in that period; the exemplars provide a voice through which conceptual commitments and their associated methodology may speak effectively. So one cannot celebrate the former and turn away from the latter.

Adherence to a matrix is, moreover, a sign of valuation of rigor in research. Originality is admittedly necessary for the emergence of new matrical candidates, something which becomes crucial at times of serious impending decay of an old matrix. But, during periods of normal science, an undue orientation towards originality and novelty would be a hindrance, rather than an aid, to scientific functioning. This is why Kuhn refers to the balance which must be kept between conservatism and originality within the scientific community as a whole. Such a balance will serve to maintain the stability of an old matrix until such time as the build-up of anomalies, and possibly various other pressures, will truly indicate the wisdom of its abandonment; thus, it ensures that matrix-change will not occur so easily as to impede the stability necessary for rigor in ordinary scientific research. On the other hand it will always ensure that new ideas are ready to move into the emerging vacuum created by a crumbling old order. So rigor—as well as reason—is important in promoting the necessary sense whereby science can see itself to be seeking truth and knowledge. Solidarity within the scientific community only exists for this purpose. Thus, it is inherently part of the scientific attitude to seek to “[get] it right”, as opposed to “[making] it new”. A thrust towards novelty would indeed be anathema to most scientists immersed in problems defined by the current matrix, at least without there being an undeniable state of crisis. Curiosity will play a role in both periods of normal science (propelling the refinement and extension of the extant matrix in regular problem-solving), and in periods of revolutionary science (fostering the genesis of new matrical Gestalten).

Without the notion of “battling through the veils of illusion” (although perhaps conceived with less dramatic flourish), the Kuhnian scientist’s commitment to her matrix will falter. Some sense of what is to count as truth must be present. Further, solidarity cannot forge a commitment to any particular matrix. We would not be able to make any sense out of an assertion that quantum physics, for example, exemplifies more (or less) solidarity within the community of physicists than does Newtonian physics.
Kuhn's statements regarding persuasion as an instrument for theory-change are not meant to advocate the opposition to traditional rationality which Rorty is promoting. Reason and intellectual thought will be significant virtues in this process. Kuhn's point is that reasoning will not be the only tactic used by proponents of a new matrix in their promotion of it; nor will it be the only factor helping to establish the routine acceptance of a fledgling matrix. This relates to the absence of an algorithm for theory-choice.

To a certain extent, praising the "institutions" which science has developed is tantamount to praising scientists for being more "objective", "logical", "methodical", or "devoted to truth" than other people. After all, among the institutions of science are just those methodologies and practices which direct research towards the goals of objectivity and the search for truth (aided by logical thought), even if these are not to be taken in some absolutist ontological sense. Rorty's bid to praise scientific institutions, while turning entirely away from objectivity, logic, method, and truth is therefore confusing: it is unclear just what he envisions scientific institutions to be. Even if these institutions "give concreteness and detail to the idea of 'unforced agreement'", this should not be meant to say that they do not contribute a significantly robust structure to the ways in which such agreement can be obtained. It is this structure which is exemplified in the methodology which marks the matrix. Finally, for a Kuhnian scientist, advocating that "the best way to find out what to believe is to listen to as many suggestions and arguments as you can" is to offer a hopelessly vague prescription: there must, in the end, be some way of making decisions on scientific matters which is directed towards achieving the particular goals which characterise the scientific endeavour. In normal science, it is the matrix which offers guidance as to what beliefs are to be considered plausible; in revolutionary science, the struggle to find a new scheme of guidance makes recourse to the scientific values detailed in Chapter Two. Matrices are crucially employed to place limits on precisely which suggestions and arguments will be deemed worthy of being listened to. Clearly, scientific models, specific as they are to the very specialised projects of science, are not adaptable to the activities of the culture at large. Quite explicitly, then, Kuhn does not share Rorty's brand of cultural "holism".

**Pragmatism**

I turn now to the question of pragmatism, a type of doctrine to which Rorty makes frequent reference. I have not encountered in Rorty much detail with respect to his concept of pragmatism; it remains a rather fuzzy notion, which may

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49 See again my introductory points three and four.
50 See again point five in the Introduction.
51 See again point seven in the Introduction.
indeed be the way which Rorty would prefer it to be, given the tenor of his philosophy. But what it serves to do is indicate that the motivation for discourse is not to reach something called “truth” in the style of traditional epistemology, but to adjust means to ends in an optimally practical manner, although against the background of a sentiment of tolerance. If we wish to make use of the term “truth” at all, we re-describe it in terms of what it is good for us to believe, transforming it into an expression of commendation. At some future time, if a better proposal for what it is good to believe were to appear, our judgment of commendation would shift to the new proposal, along with our assessment of what is true. This will be a collective affair, truth attaching to proposals upon which general cultural agreement has been bestowed.

Rorty’s pragmatic outlook is seated in a continuation of discourse only until its participants are “satisfied”. He envisions this as a settled state of cultural agreement. However, the details of what is to count as agreement are also missing, there being no indication as to whether it is to be based upon a sort of majority rule, consensus, or some other arrangement. To make even this decision of what sort of agreement is to be sought subject to the discursive approach which Rorty describes would be to enter into an unfruitful infinite regress.

Kuhn’s understanding of what satisfaction might entail bears a far harder edge: satisfaction, if we wish to call it that, is achieved through the smooth operation of a research-matrix. Hence, the researchers in Kuhn’s programme are working with a robust framework, one which would be intolerant of the kind of infinite regress of propositions in discourse which Rorty believes to be not at all problematic. Researchers working within a matrix must set some basic assumptions as inviolable, and as the grounding for all others; these constitute the most foundational elements of the matrix. One need consider the chain of justification only as far back as these fundamental elements, at least during periods of normalcy. It is the presence of this network of fundamental elements which sets off normal from revolutionary science; in the latter, much energy is devoted to the re-establishment of some such framework for agreement which will render future research coherent.

Kuhn does consider his model of science to be largely motivated by pragmatic considerations, given the importance of problem-solving, which is aided by such values as aesthetics, simplicity, etc. When Rorty speaks of pragmatism, however, his position is rather more enigmatic, since he refuses to allow for anything which would provide a solid ground for the evaluation of what is to count as pragmatic. Surely scientific judgments based upon pragmatic concern will achieve sufficient complexity to require some logical orientation or other which will help to sort out the more effective solutions to questions or problems from those of lesser effectiveness. Rorty’s “criterionless muddling through” fails adequately to acknowledge the importance of the learning process to such pragmatic endeavours. This learning process is to be found importantly in Kuhnian scientific exemplars. Pragmatism ought to be aided by a certain level of efficiency, which is, in turn, fostered by the learning of what works and what does not, and much of this learning involves the similarity-relations which form the
bulk of Kuhn's matrices. Rorty does want to acknowledge learned responses to stimuli; our assessment that an animal writhing around before us is in pain and may require help reflects our learned response of a judgment of "pain" upon the stimulus of our observation of this behaviour. But that responses should be learned at all—for instance, as part of a coherent framework of understanding in science—is more consistent with a model which incorporates the presence of matrices and exemplars which guide the learning process which leads to understanding and action than with the "fuzzier" articulations of Rorty's philosophy which he sometimes advances.

While Rorty recognises Kuhn as having encouraged a pragmatist position, he is dismayed at what he sees as the latter's having drawn away from it in desiring an explanation for why science works. Pragmatists of the Rortian school do not search for an explanation of the success of science.

This reveals some of the exceedingly puzzling features of Rorty's version of pragmatism. The suggestion that seeking an explanation of why science works betrays a drawing-away from pragmatic concern seems to me frankly unintelligible. Kuhn's description of science as strongly directed towards the development of problem-solving ability indicates a clearly pragmatic motivation, a point which Kuhn himself has emphasised. Thus, examining the workings of science is a paradigm example of what it is to be practically motivated.

It may be instructive in this connection to note that, in some moods, at least, Rorty appears to be more hostile to pragmatism than promoting of it, as when he asks

what is so special about prediction and control? Why should we think that explanations offered for this purpose are the "best" explanations?\(^{52}\)

And,

Nietzschean history of culture, and Davidsonian philosophy of language, see language as we now see evolution, as new forms of life constantly killing off old forms—not to accomplish a higher purpose, but blindly.\(^{53}\)

This attitude betrays a stance which has moved very far from the concerns which Kuhn addresses. Certainly, prediction and control figure prominently in the motivation and operation of science, by Kuhn's account, or any standard account, for that matter. Moreover, the removal of prediction and control from pragmatic concern makes it difficult to see what might be left. Also, discovering a Galilean vocabulary to be practically superior to an Aristotelian one would hardly be an example, for Kuhn, of a blind replacement.

Rorty describes the pragmatic view as one in which theory follows after, rather than being pre-supposed by, concrete achievement.\(^{54}\) As I have noted

\(^{52}\) Rorty \textit{ORT}, p. 58.

earlier, the particulars which are concrete achievements, in a Kuhnian system, can only exist by virtue of the general framework which contributes to the determination of those particulars in the first place. All observations are theory-dependent, which is at the root of why we are unable to isolate a neutral-observation language. It does not help to contend that concrete achievements are simply self-creating acts, for these, too, must be counted as observations in relation to any theory which incorporates them. If theory is to follow after the identification of particulars, then we have moved back in the direction of the correspondence theories which both Rorty and Kuhn have rejected.

Rortian pragmatists recommend that we worry only about the choice between two hypotheses, rather than about whether or not there is something which makes either true. This is supposed to rid us of questions of objectivity of value, rationality of science, and viability of language-games. We replace these with pragmatic questions about whether or not we ought to keep our present values, theories, and practices, or replace them with others. Pragmatists interpret the goal of enquiry as an appropriate mixture of unforced agreement and tolerant disagreement; what is appropriate is determined by trial-and-error.

For Kuhn, the question of truth is integral to the acceptance, by the relevant scientific community, of any particular matrix. The Kantian orientation of Kuhn's programme removes the notion of truth from the sphere of correspondence-theories. But that scientists envision their work as a search for truth is still an essential motivating factor, as I have mentioned in Chapter Two. Rorty seems to be missing the subtlety that, even if we reject the scientific realists' search for non-perspectival truth--attained upon our matching our theories with the way in which the world is, in some ontological sense--we need not reject entirely all notions of truth. Neither would Kuhn be prepared to give up the notions of the objectivity of value, the rationality of science, or the viability of language-games. Devoid of a grounding in correspondence-theorising, objectivity of value comes to look more like intersubjectivity of value: agreed-upon standards of thought and methodology, which are expected to be informed also, however, by the basic presence of the common "given". I have already discussed in this chapter the importance of reasoning in Kuhn's account of science. If "viability of language-games" is to mean how well various vocabularies function in facilitating communication, then this surely must be an aspect with which Kuhn is deeply concerned, given all that he has written on the question of the incommensurability of the vocabularies of different matrices, as well as the vital role played by the relatively static vocabularies of normal science. Even just in general terms, we would expect the fulfilment of pragmatic goals to be aided by effective communication.

Rorty's neglect of Kantian-type epistemological frameworks is also evident in the following passage:

54 Rorty, ORT, p. 79.
When the notion of "description of the world" is moved from the level of criterion-governed sentences within language-games to language-games as wholes, games which we do not choose between by reference to criteria, the idea that the world decides which descriptions are true can no longer be given a clear sense.\(^{55}\)

But "the world" does not have to be the source of criteria for there to be criteria (or better, similarity-relations) at all. In Kuhn's description of science, patterns of similarity-relations originate with the scientific community; they are not read off of Nature, as a scientific realist or empiricist would believe. Also, one can move the analysis to whole language-games (or whole matrices) from single sentences (or isolated observations), as Rorty observes that Kuhn has done, without jettisoning the notion that criteria—in the form of scientific values, for example—have something to do with theory-choice. Rorty seems to be unfortunately impeded by the notion that "[t]he temptation to look for criteria is a species of the more general temptation to think of the world, or the human self, as possessing an intrinsic nature, an essence".\(^{56}\) As we have seen with Kuhn's programme, this is not the only spirit in which we can conduct the search for paradigms for belief and action.

Also, in Kuhn's framework, the input of Nature as the "given" means that Nature does play some role in the viability of any theory, even if it does not play a decisive, or the sole, role.

I wish to consider one last passage in Rorty on the subject of pragmatism. This concerns the question of progress. Rorty says that

To say that we think we're heading in the right direction is just to say, with Kuhn, that we can, by hindsight, tell the story of the past as a story of progress. To say that we still have a long way to go, that our present views should not be cast in bronze, is too platitudinous to require support by positing limit-concepts.\(^{57}\)

But this is crucially under-specified, in respect of to what we might be heading in the right direction, or to what we might still have a long way to go. One wonders at what we are supposed to be looking, in this hindsight. Rorty has resisted spelling out anything which might furnish us with a sense of how one determines a direction for the culture—or for a specific cultural activity such as science. There are no goals; there is nothing for which to aim save the rather empty prescription of maximising unforced agreement and fostering a certain amount of tolerance for disagreement. But even this goal is not clearly defined. To make any sense, the claim that we are heading in the right direction, i.e., making progress, must be matched against the possibility, in principle, of our not being able to tell the story of the past as a story of progress: we ought to be able to identify what it would be for us not to progress. For Kuhn, progress is to be read, in the main, off of scientific puzzle-solving ability, which increases with successive matrices, not by

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\(^{55}\) Rorty, *CIS*, p. 5.

\(^{56}\) Ibid., p. 6.

\(^{57}\) Rorty, *ORT*, p. 27.
chance, but by a clear and conscious direction of purpose: “Newton’s mechanics improves on Aristotle’s and Einstein’s improves on Newton’s as instruments for puzzle-solving”.\(^{58}\) Judgment of progress involves our looking to our means to evaluate the effectiveness with which they have achieved our ends, a process, which will be facilitated, in part, by rational thought, traditionally conceived. So, Kuhn would not concur with Rorty that the notion that Newton’s vocabulary “[got]...at the truth about the heavens...is not an explanation of anything”, but “just an empty compliment—one traditionally paid to writers whose novel jargon we have found useful”.\(^{59}\) Such emptiness is antithetical to pragmatism.

The discussion of this section should indicate that Rorty’s belief, as expressed in point six of my introductory list, would, to be accurate, require such extensive qualification that we must regard it as fundamentally flawed; Rorty’s and Kuhn’s respective orientations towards pragmatism indeed provide little, if any, basis for compatibility between them.

**Truth and Relativism**

Both Rorty and Kuhn have strived to communicate to their readers the importance of keeping an eye to historical conditions and influences in our attempts to understand human endeavour. This has been an important factor on the basis of which Rorty sees his commonality with Kuhn.\(^{60}\) It is as a result of their taking historical elements as seriously as they do that both have had to deal with charges of relativism from their critics. Such criticism also focuses on their allowing a community to set standards for what is to count as true on the basis of considerations which are pragmatic in nature, rather than metaphysical (and hence more likely to be universal). But each denies that he is a relativist.\(^{61}\)

In Rorty’s defence of his philosophy as non-relativistic, he appeals to a sort of initial, or provisional, “ethnocentrism”. A community starts with the beliefs which it regards as acceptable: “[e]ither we attach a special privilege to our own community, or we pretend an impossible tolerance for every other group”.\(^{62}\) But the community must, at some point, subject those beliefs to scrutiny through discourse with other communities. As a result, the community’s web of beliefs may end up being re-woven: “[b]eliefs suggested by another culture must be tested by trying to weave them together with beliefs we already have”.\(^{63}\)

Rorty argues that it is this ethnocentrism—the community’s initial adherence to its own beliefs, coupled with its initial rejection of the beliefs of

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\(^{58}\) Kuhn SSR, p. 206.  
\(^{59}\) Rorty, CIS, p. 8.  
\(^{60}\) See again point eight in the Introduction.  
\(^{61}\) See introductory point nine.  
\(^{62}\) Rorty, ORT, p. 29.  
\(^{63}\) Ibid., p. 38.
other communities—which serves as evidence against his position being relativist. The only way I can make sense of this is to interpret Rorty as taking relativism to be a position wherein anything goes, so to speak, i.e., where any belief is as good as any other. But that is not the way in which relativism is typically defined. A relativistic position would be one which describes beliefs of what is true or false, or right or wrong, as relative to a particular community. But this is just the sort of picture with which Rorty presents us. Each culture would espouse its own ethnocentric point of view--shifts in its views notwithstanding--and there is no place for a God’s-eye view, as it were, from which the belief-webs of different communities might be evaluated. Therefore, from the point of view of any given community, its own beliefs are true.

Although ethnocentrism motivates us to employ our own present beliefs in our decisions of how to apply the term “true”, Rorty advises us that we must take care to refrain from defining “true” in terms of those beliefs64, for this is a definition which would only apply to our own culture, and it would be a mistake to universalise beliefs which are only typical of a particular group. I take Rorty to be offering an account of truth which relies on definitions which are qualified (in that any definition will be group-specific) as well as provisional (in that any definition will be subject to change over time, with exposure to new ideas).

There is a certain tension, clearly, between the concepts of ethnocentrism and of qualified, provisional truth. There will have to be some sense as to how strongly a culture ought to maintain its ethnocentricity before it comes seriously to entertain the potential truth of the beliefs of other cultures, upon which a re-weaving of the web of beliefs must be considered. (Incidentally, this is a parallel question to that of Kuhn’s balance between maintaining allegiance to a disciplinary matrix under stress, and seriously considering up-and-coming matrical candidates.)

Rorty states that, even if one drops the idea that a common ground for appeal in the question of truth is provided by evidence, this does not mean that one culture’s web of beliefs is as good as another’s.65 This seems a rather hollow statement. From within a given culture, by ethnocentrism, its own web of beliefs will be seen as superior to all others. But outside of the cocoon of mere intrasocietal agreement, the lack of any cross-cultural evidence to which appeal may be made removes all means of weighing beliefs from different webs against each other. Seen from this broader vantage-point, then, one web of beliefs must be considered to be as good as any other. Again, it is difficult to see how this is anything but a relativist position, since, if values are not to be universal, then they must be relative to something, i.e., the particular culture which holds them.

Kuhn’s approval of the authority of the accepted and time-proven matrix to sanction certain beliefs as true might be seen as at least superficially similar to Rorty’s relativism. That which is to be understood as true is relative to whatever the scientific community sets out as admissible thought and practice. But, as I

64 Ibid., p. 50.
65 Ibid., p. 67.
have noted in Chapter Two and above, Kuhn adheres to a thesis whereby some sort of scientific progress is achieved, evidenced by the increasing refinement, sophistication, and explanatory power of theories, as well as by the unidirectional and irreversible evolution of one matrix into the next. "One scientific theory is not as good as another for doing what scientists normally do." Later theories are progressively better as tools for the practice of normal science; earlier theories are later abandoned as false. Progress is not a feature of a relativistic programme; nor is it an intelligible feature of Rorty’s. The question then is whether or not Kuhn is correct in seeing science as progressive in this way. At any rate, as I have argued in the previous chapter, Kuhn’s description of the operation of different matrices is considerably more structured and complex than is Rorty’s portrayal of a plurality of discourses.

Rorty complains that critics of pragmatism charge that pragmatists “have defined ‘true’ as ‘satisfies the standards of our community’”. He objects that “we pragmatists do not hold this relativist view”. The most that I can make out of this is that Rorty believes his position and Kuhn’s to escape the shadow of relativism through what he estimates to be their mutual avoidance of standards, or criteria, against which a relativist conception of truth would be defined. Rorty attempts to further this by asserting that “true” should be neither analysed nor defined, anyway. So if we have neither standards against which to define truth nor a concept of truth to begin with, then we cannot be accused of occupying the relativist position which claims that truth is relative to whatever standards a society holds. Despite the fact that it appears that Rorty has misread Kuhn in suggesting that the latter does not indeed view truth as being measured against whatever the community has decided upon as a set of standards (viz., a matrix), there is still the obvious problem that Rorty simply cannot avoid the relativism of his own position in this way. One might viably re-cast the definition of relativism to suit Rorty’s programme rather admirably, as defining whatever it is good to believe as that discourse to which a given culture has habituated itself.

In this connection, Rorty’s protest that pragmatists “do not infer from ‘there is no way to step outside communities to a neutral standpoint’ that ‘there is no rational way to justify liberal communities over totalitarian communities’” sheds no light on how pragmatists might achieve such justification, given Rorty’s disdain for any standards at all, let alone neutral ones. Rorty seems unprepared to admit that even his programme requires standards of some sort, in order to get off the ground. He insists that “[w]hat [pragmatists] in fact infer is that there is no way to beat totalitarians in argument by appealing to shared common premises”. We are to “forget about being responsible to what is ‘out there’”, recognising instead “that human communities can only justify their existence by comparisons with

66 See especially p. 51.
67 Kuhn, CGK, p. 264.
68 Rorty, ORT, p. 42.
69 Ibid., pp. 25, 50.
70 Ibid., p. 42.
other actual and possible human communities".\textsuperscript{71} But comparisons can only be made by means of specified parameters. Moreover, it is difficult to envision on what persuasive power one could possibly draw, in discussions with totalitarians, if one is not to appeal to common human sentiments of some sort, such as empathy or justice. It is on the grounding of such sentiments that we enter into comparisons between other communities and our own. This need for standards of some sort in order to render Rorty's philosophy at least minimally intelligible again thrusts him back into Kuhnian territory, even though he recognises neither Kuhn nor himself as resorting to such standards. Again, this underscores the problematic nature of Rorty's belief as expressed in my introductory point five.

Rorty may be leaning on the notions of solidarity and tolerance, as means by which to justify liberal modes over totalitarian ones. In that case, Rorty's programme would indeed not be fully relativistic. But then he must clearly state this as a common, universal value, and his allowance for ethnocentrism would be correspondingly significantly diminished. If the idea of ethnocentrism is just that a culture begins from the beliefs which it already holds, and strives to achieve the ideal of greater tolerance from there, we appear still to have a universalist type of thesis, and not one which takes ethnocentrism very seriously.

While both Rorty and Kuhn take truth and reference to be relative to a conceptual scheme (although one may question the validity of Rorty's use of the concepts of reference here, given what he has said elsewhere), there is a basic difference in the way each of them conceives of a conceptual scheme. For Rorty, it is simply "what we believe now—the collection of views which make up our present-day culture".\textsuperscript{72} For Kuhn, it is what makes those beliefs possible in the first place, namely the \textit{a priori} matrical structure. We might consider this latter to compose a special category, or order, of beliefs.

\textbf{Alternative Discourses/Matrices}

The same lack of clarity which bedevils Rorty's view of single discourses plagues his account of a culture's re-weaving of its web of beliefs. He contends that, in holism,

[0]ne will do, in short, just what the "new fuzzies" in philosophy of science say scientists do when some relatively large-scale proposal to change the way nature (or part of nature) is pictured is up for discussion. One will muddle through, hoping that some reweaving will happen on both sides, and that some consensus may thus emerge.\textsuperscript{73}

\textsuperscript{71} Ibid.
\textsuperscript{72} Rorty, \textit{PMN}, p. 276.
\textsuperscript{73} Rorty, \textit{ORT}, p. 67.
It should be clear here that, although Rorty apparently views Kuhn as one of these “new fuzzies”, the Kuhnian account of matrix-transformation does not match this Rortian account of discourse-re-weaving. Any sense of “muddling through” will be displaced, in any endeavour recognisable as science, by the thrust to retrieve some sort of matrical structure. Also, adherents to an old matrix generally resist re-weaving, some for longer than others, in the attempt to stretch that matrix to accommodate anomalies or answer new demands. At any rate, in Kuhnian terms, as I shall explain in the next section, the question of some mutual re-weaving of matrices, in the interest of coming together on some median territory is moot, on account of the incommensurability of alternative matrices, or scientific discourses.

Rorty re-iterates his rejection of epistemology, this time, in the context of inter-discourse dialogue:

The advice to see if it might not pay to reweave your web of belief in the interests of a better ability to solve your problems is not the advice to formulate epistemic principles. The one piece of advice would only entail the other if experience had shown that having a conscious epistemological view were always an efficient instrument for readjusting old beliefs to new.

But for Kuhn, this is precisely what experience has shown. Science progresses through the refining and extension of matrices, followed by revolutionary shifts to new ones. One of the major reasons for the adoption of a new matrix is scientists’ commitment to a belief in its epistemic benefit.

In the same vein, Rorty asserts that the notions of criteria and choice (including that of ‘arbitrary’ choice) are no longer in point when it comes to changes from one language game to another. Europe did not decide to accept the idiom of Romantic poetry, or of socialist politics, or of Galilean mechanics. That sort of shift was no more an act of will than it was a result of argument. Rather, Europe gradually lost the habit of using certain words and gradually acquired the habit of using others.74

There is a certain degree of similarity to Kuhn here. As I have described in Chapter Two, Kuhn cites the inaccuracy committed in describing theory-choice in terms of conscious deliberation and decision, or choice. He finds the term “conversion” more accurate to peg the way in which the shift to a new theory occurs, what we might loosely term a Gestalt-shift. But he has also stressed that this does not point to the intrinsic irrationality of the process, arguing that one can still pick out good reasons which take part in the conversion from one theory to its rival. So argumentation is not alien to the process.75 Moreover, as I have mentioned several times, the Kuhnian account of science, in respect of both the matrices of normal science and the matrical shift which occurs in times of

74 Rorty, CIS, p. 6.
75 See pp. 38; this also relates back to the inadequacy of Rorty’s belief as expressed in my introductory point four.
revolution, does not completely eschew criteria (the diminished role he allows for them notwithstanding), and certainly does not discount the importance of a framework of similarity-relations.

There is also, as I have previously noted, more at stake than a mere shift in linguistic habit. If habituation to a discourse were as important a feature of its usage as it seems to be for Rorty, at least in some moods, then we must surely allow for a diminished focus on the pragmatic appeal of that discourse. A gradual change in habit of the sort which it would appear is being advocated in the above quote—neither willed nor argued for—would speak against there being anything like an assessment of the pragmatic advantage of a particular discourse.

Rorty also states that revolutionary achievements in the arts, sciences, and moral and political thought typically occur when somebody realises that two or more of our vocabularies are interfering with one another; a new vocabulary is invented to replace both. 76 This adds another dimension, in the form of a clash with a second vocabulary, beyond the consideration of the pragmatic effectiveness of a single discourse. It is un-Kuhnian—as Rorty would himself agree—in its omission of the expectation of some assessment of the goodness-of-fit of a theory with Nature, on account of the input of Nature into observations, via the “given”. It also departs from Kuhn in that, for the latter, there can be no question of revolution being fomented by a clash between two discourses (matrices), for only one discourse (matrix) can prevail in the pre-revolutionary period to guide research at any one time. At least that is what is true for science, which raises the further point that the account of science, for Kuhn, must be distinguished from that of the arts, and of moral and political thought. 77 In science, crisis is typically instigated when the extant theory’s goodness-of-fit to Nature begins to weaken.

The Question of Incommensurability

Rorty defines the term “commensurable” as “able to be brought under a set of rules which will tell us how rational agreement can be reached on what would settle the issue on every point where statements seem to conflict”. We are able, as a consequence of these rules, “to construct an ideal situation” for the reaching of agreement. All disagreements will then be seen as “‘noncognitive’, or merely verbal, or else merely temporary”. This structure “allows the interlocutors to agree to differ—being satisfied of each other’s rationality the while”. All of this illustrates “[t]he dominating notion of epistemology...that to be rational, to be fully human, to do what we ought, we need to be able to find agreement with other human beings.” He asserts that “[t]o construct an epistemology is to find the maximum amount of common ground with others”, with “[t]he assumption that an epistemology can be constructed” being “the assumption that such common

76 Rorty, CIS, p. 12.
77 See again point seven in the Introduction.
ground exists”. Common ground has been sought outside of us in such places as the Platonic Forms, or the realm of Being; within us, in the mind; and in language, where the search has been for a universal scheme for all possible content. But we are warned to ‘[n]ote that this sense of ‘commensurable’ is not the same as ‘assigning the same meaning to terms’ ”. This latter sense, “which is the one often used in discussing Kuhn—does not seem to [Rorty] a useful one, given the fragility of the notion of ‘sameness of meaning’ ”. So he asserts that “[t]o say that parties to a controversy ‘use terms in different ways’ seems to [him] an unenlightening way of describing the fact that they cannot find a way of agreeing on what would settle the issue”.

It is essential to notice here at the outset that Rorty’s definition of the term “commensurable” covers what he has elsewhere described as traditional epistemology, bolstered by equally traditional rational thought. This definition of the term is profoundly peculiar, since, as we have seen with other of Rorty’s terms, it is quite non-standard. It is unclear why he deemed such a re-definition necessary. The sense of “commensurable” which Rorty wants us to discount is not merely “the one often used in discussing Kuhn”, but is significantly close to the one which Kuhn himself employs. Incommensurable elements are not inter-translatable—a reflection of their bearing different meanings—because they belong to separate vocabularies which carve up the world, so to speak, in different ways. This understanding of commensurability encompasses, but goes further than, the linguistic one (assigning the same meaning to terms) which Rorty has rejected in favour of his own re-definition. Kuhn basically describes these vocabularies as exhibiting a fundamental difference in taxonomic categories (ways of “carving up the world”), rendering the situation deeper than a mere linguistic account of it would allow; a vocabulary is akin to a Kuhnian type of conceptual scheme, and must be in place before a description of the world can begin.

Rorty states that Kuhn has questioned the need for commensuration. But what this tells us, in Rortian terms, is that Kuhn has questioned the need for rules directing the attainment of rational agreement, the need for an epistemology, and the need for maximising common ground with others. As we have seen, it is not at all part of Kuhn’s programme to renounce epistemology, rational thought, or rules (or more accurately, similarity-relations) for directing thought and action. The maximisation of common ground among people is the only element here which seems at all correctly included, since Kuhn is only interested in the attainment of agreement among members of relevant scientific communities, and not among people in a more general context. Further, agreement between the proponents of Aristotelian mechanics and those of Galilean mechanics is not something which Kuhn would seek to maximise, since he is primarily interested in the ways in which they diverge. Kuhn’s exposition of science is largely directed towards elucidating just how little common ground there can be between proponents of

78 Rorty, PMN, p. 316 and footnote, 317.
79 See pp. 34-6.
80 Rorty, PMN, p. 317.
alternative matrices. In fact, it is the traditional cumulative picture of science which has over-estimated the amount of common ground among past scientific theories.

In Rorty's view, epistemology has traditionally viewed knowledge as having a logos, which can be given by a method of commensuration; this is needed for "genuine cognition". On the other hand, the pragmatic approach to knowledge which we see exemplified in the epistemological behaviourism which Rorty advocates divides commensurable discourses from those which are not. This is, for him, equivalent to dividing normal from abnormal discourse, which "generalizes Kuhn's distinction between normal and revolutionary science". It parallels also the traditional distinction between the search for "objective knowledge" and other, less privileged areas of human activity. Normal science solves problems against the background of a consensus about what counts as a good explanation of phenomena and what constitutes a problem-solution, while revolutionary science yields the introduction of a new paradigm of explanation and a new set of problems.

Rorty goes on to say that normal science is as close as real life gets to the epistemologist's notion of rationality. Abnormal discourse is born when someone who is ignorant of these conventions, or who sets them aside, joins in the discourse. The product of abnormal discourse can be anything from nonsense to intellectual revolution. There is no discipline which describes it: for Rorty, it is unpredictable and creative.

But, the practice of revolutionary science does not admit participants who are ignorant of scientific conventions; nor does it admit those who are willing simply to set them aside. Proponents of new matrices have already been trained in the dominant matrix; they would not be admitted to the process otherwise. Scientists do not invite non-scientists to propose new templates for their work. Accepted conventions are not simply "set aside". Some of them will remain intact, if they have not been thrown into question by the dissolution of the old matrix. But even if they do not survive the change, the old conventions are not "set aside" so easily: they begin seriously to crumble, and are reluctantly abandoned, under circumstances of great turmoil.

The discipline which existed in the time of normalcy (e.g., inorganic chemistry) will still be recognisable as a discipline after the shift, even if a new related discipline has been cleaved off of it as a result. There is no question of there being no discipline at all during abnormal periods.

Although a certain amount of unpredictability and creativity can be expected in the birth of a new research-matrix, the notion that the product of a period of abnormality could possibly be "nonsense" places what Rorty is describing at a considerable remove from what Kuhn is interested in elucidating. If a scientific revolution failed to produce a new order, then it would devolve into either the sort of pattern characteristic of pre-paradigm science (in which case,

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81 Rorty, PMN, pp. 11, 319-20.  
82 Ibid., p. 320.
there would be a plurality of parallel orders) or a situation where there was no
order at all. The first case would signal a transformation from a mature state to
one of immaturity, which I would expect Kuhn to view as an extremely unlikely
outcome, and one unprecedented in the history of science. The second case would
signal the end of science. Neither of these is a productive option when one is
concerned to describe the special process which is science (as Kuhn is), both
historically and in terms of what must be the parameters of the endeavour if it is
to continue to be called "science".

For Rorty, we will be epistemological in circumstances where we know
what is going on, and want to codify it, in order to extend, strengthen, ground, or
teach it. Thus, we do get epistemological commensuration, but only where we
already have agreed-upon practices of enquiry (or more generally, discourse).
When a practice has continued for long enough, its conventions are relatively easy
to isolate, which is what makes epistemological commensuration possible. 83

Rorty compares this stance towards epistemology to "Nelson Goodman's
pragmatist attitude toward logic", where we discover our rules of inductive and
deductive inference by discovering what inferences we habitually accept 84: "A rule
is amended if it yields an inference we are unwilling to accept; an inference is
rejected if it violates a rule we are unwilling to amend". 85

This particular version of the pragmatic approach to knowledge reflected in
epistemological behaviourism diverges radically from the prescription for the
treatment of standard epistemology which Rorty has given elsewhere. Hence, it
appears to be a contradiction internal to Rorty's philosophy. Generally, he offers
no option wherein we might "be epistemological", and hence be engaged in
"divid[ing] commensurable discourses from those which are not". The usual
admonishment is to dispense with standard epistemology--and therefore with
commensurable discourses--entirely. Rorty's allowance for epistemological
commensuration where a long-standing practice has made for well-engrained
conventions is at odds with his favourable (albeit inaccurate) assessment of Kuhn
as having questioned the need for commensuration at all, whether
"commensuration" be defined on Rorty's terms, or standardly. Additionally, the
"rules of inductive and deductive inference" which Rorty is here allowing, even if
they are only isolable after the fact of the laying-down of the order, had no place
at all in the Rortian cultural programme which I have described so far. The
codification of a discourse/matrix, "in order to extend, strengthen, ground, or
teach it" sounds suspiciously similar to the matrix-oriented picture of science
which we inherit from Kuhn. Again, Rorty seems to be ambiguous in what he is
advocating, and appears to be, at times, sliding into a Kuhnian picture which he
would not recognise as Kuhnian.

The rather skewed picture of Kuhn's programme which we receive from
Rorty is further evidenced in the following passage:

83 Ibid., p. 321.
84 Ibid.
Since the Enlightenment, and in particular since Kant, the physical sciences had been viewed as a paradigm of knowledge, to which the rest of culture had to measure up. Kuhn’s lessons from the history of science suggested that controversy within the physical sciences was rather more like ordinary conversation (on the blameworthiness of an action, the qualifications of an officeseeker, the value of a poem, the desirability of legislation) than the Enlightenment had suggested. In particular, Kuhn questioned whether philosophy of science could construct an algorithm for choice among scientific theories. Doubt on this point made his readers doubly doubtful on the question of whether epistemology could, starting from science, work its way outward to the rest of culture by discovering the common ground of as much of human discourse as could be thought of as “cognitive” or “rational”.

As we have seen, particularly in Chapter Two, Kuhn’s lessons from the history of science clearly do not suggest a greater similarity between the physical sciences and ordinary conversation than the Enlightenment had suggested. Kuhn, in fact, does not offer criticism of Enlightenment thinking in general. His criticism is primarily directed at the traditional, ahistorical picture of science as more or less smoothly cumulative. That he has denied the possibility of the construction of an algorithm for theory-choice is not tantamount to his denying a role for reason in matters scientific, and does not lead inevitably to a merging of the scientific endeavour with other cultural activities or ways of thinking. It seems safe to say that Kuhn would share with “his readers” a rejection of such an algorithm, but I would want to be more careful about suggesting that he would share with them a rejection of the notion that epistemology could work its way out from science to the rest of culture through the discovery of a common human rational grounding. Caution is required here not because Kuhn does see epistemology’s role in this light, but because his remarks on the genetic commonalities among human beings imply loyalty to the notion of at least some degree of fundamental common cognitive ground, upon which socio-psychological influences act. This particular point is at least part of the Enlightenment attitude which Rorty rejects. Also, while it is true that Kuhn is not advocating an epistemology which operates through science, to influence the rest of culture, neither is he advocating a melding of the projects of science with those of the rest of culture, as Rorty often does. Kuhn is clear on the divergence of structure found between the sciences and the non-sciences; his concern is to explore science as the distinct discipline which it has become.

Although Rorty is in agreement with Kuhn on the lack of a neutral observation-language, he complains that Kuhn has, on the basis of this issue, ventured too far into the territory of idealism. This relates to Kuhn’s view that the lack of such a language results from the situation wherein the proponents of different theories “see different things” or “live in different worlds”. He refers to remarks of Kuhn’s on this score as “incidental”. Rorty assesses that what Kuhn

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86 Rorty, PMN, p. 322.
87 See again introductory point seven.
wished to oppose was the traditional claim that “what changes with a paradigm is only the scientist’s interpretation of observations that themselves are fixed once and for all by the nature of the environment and of the perceptual apparatus”. Rorty contends that “this claim is innocuous if it means merely that the results of looking can always be phrased in terms acceptable to both sides (‘the fluid looked darker’, ‘the needle veered to the right’, or, in a pinch, ‘red here now!’); it would have been enough for Kuhn to show that phrasing things in terms acceptable to both sides is of no help in finding an algorithm for theory-choice.\(^88\)

The alleged foray into idealism probably relates to the point that different theory-dependent experiences signal divergent representations. For Kuhn, such different representations are the consequence of a difference in the \textit{a priori} template through which all experiences are had. This is the basis of his remarks concerning the way in which \textit{in some sense}, the proponents of different theories live in different worlds. As we have seen, this is not meant to be an ontological proposition. Since observations (or more broadly, experiences) are theory-dependent, different theories generate different experiences, at least in those areas where the theories are crucially divergent. We might expect Rorty to be clearer on this point, given his report that Kuhn espouses “post-positivistic philosophy of science”, followed by the explanation that the positivists held language and world, and theory and evidence, apart.\(^89\) Moreover, he states that “there is no question of taking an object out of its old context and examining it, all by itself, to see what new context might suit it....[A] belief is what it is only by virtue of its position in a web”. We must drop the traditional opposition between context and thing contextualised.\(^90\) It is curious that Rorty refers to Kuhn’s remarks as “incidental”, since an understanding of this issue is at the core of Kuhn’s programme: the theory-dependence of observations, and the resulting impossibility of a neutral observation-language, are the underpinning of his argument for the existence of scientific revolutions. A revolutionary history of science, as opposed to a relatively smooth cumulative one, is such precisely because of this fundamental divergence in experience (i.e., in the observations which are made, and in what thoughts it is scientifically intelligible to have).

The claim that it is only the scientist’s interpretation of fixed observations which changes with a change in matrix is not, for Kuhn, “innocuous”. This claim simply cannot be salvaged, because it does not go deep enough. Interpretation of results is something which goes on \textit{within} a given matrix, because it is something which is practiced on a given set of results. Only within a matrix can there be any such thing as a given set of results. Different matrices generate different sets of results, and so the way in which different propositions are made using different theories is not a result of interpretational differences. Again, this strikes at the heart of what Kuhn is trying to accomplish in introducing the notion of a history of science sensitive to the presence of revolutionary shifts. Rorty’s attempt to

\(^{88}\) Rorty, \textit{PMN}, p. 324; and Kuhn, \textit{SSR}, p. 120, as quoted in Rorty, \textit{PMN}, p. 324.

\(^{89}\) Rorty, \textit{ORT}, pp. 64-5.

\(^{90}\) Ibid., p. 98.
promote solidarity by suggesting that one can always phrase “the results of looking...in terms acceptable to both sides” is not possible on the Kuhnian view, and this, as we shall see shortly, is the basis of Kuhn’s description of alternative matrices as incommensurable. In light of all of this, it would not only not have been enough for Kuhn to show that phrasing things in terms acceptable to both sides is of no help in locating an algorithm for theory-choice, it would, in fact, be impossible for him to show this, since, if things could always be phrased in terms acceptable to both sides, this would indicate a persistent common template of experience, existing at a level overarching alternative matrices. Kuhn denies the presence of such a common template (which is not to say, however, that he denies all commonalities between proponents of alternative matrices).

Incidentally, Rorty’s examples of candidates for such mutually acceptable terms--perhaps most obviously “red here now!”--bear an ironic similarity to the phenomenal entities which Rorty has been so diligent elsewhere in attempting to purge from our understanding of things. It is an important point in Kuhn that such phenomenal sorts of reports are pre-theoretical; a scientist will make an observation of a needle veering to the right, for instance, in a way which goes beyond this pre-theoretical level: laden with a lot of background information concerning the apparatus and its function in his matrically-oriented research. So in science, observations will not occur in this sort of broadly acceptable form, but will diverge according to matrix-specificity.

Rorty complains that it is unfortunate that Kuhn used “romantic” notions such as that of scientists being presented with a new world, since this has contributed to the fears of his critics that he is promoting a created, rather than a discovered, world. For Rorty, “nothing deep turns on the choice between” discovery and creation of the world--“between the imagery of making and of finding”. He sees Kuhn as having invited trouble unnecessarily by using the notion of a new world: he could have avoided this by sticking “to the classic notion of ‘better describing what was already there’”. This is not meant to slide back into a metaphysical view: we stick to the classic notion because we speak of history from our own Whiggish standpoint, and we need to keep something constant throughout the story. “The forces of nature and the small bits of matter, as conceived by current physical theory, are good choices for this role”.

I have already explained why, for Kuhn, the notions of discovery and creation need be distinguished. They are radically different notions, and it is difficult to see how it could be that nothing deep turns upon the choice between them.

In citing the matrix-shift embodied in Kuhn’s example of Aristotle’s seeing the constrained fall of a stone where Galileo saw a pendulum, Rorty states that “we need make no more of the gestalt-switch in question than the fact that people became able to respond to sensory stimulations by remarks about pendulums,

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91 Rorty, PMN, p. 344.
92 See pp. 60-2; this also relates to my introductory point two.
without having to make an intervening inference”. Similarly, he notes that we have to be clear that the shift, for example, from Ptolemaic to Copernican astronomy was not brought about by “rational argument”. This is roughly in line with Kuhn’s view, although altered somewhat to reflect Rortian eliminative materialism. What we might approximate as Kuhnian Gestalt-switches are not brought about directly by inference, or rational argument. However, for Kuhn, reason does play some role in the process. Rorty’s admonishment that “we need make no more of” this switch than that it is not fully inferential downplays the dramatic significance that a scientific revolution can have. Also, that Rorty refers to these changes as Gestalt-switches betrays an understanding, or at least ought to, that things are experienced differently after the shift from before. Returning to Rorty's previously cited argument for solidarity's promoting the seeking of terms acceptable to both sides of a dispute, a Gestalt-switch precludes any possibility of there being a phrasing of things which is acceptable to both sides of a matrix-dispute. Using the familiar example, one cannot see a drawing simultaneously as a duck and a rabbit.

Rortian ethnocentrism prevents us from justifying our beliefs to everybody; justification extends only to those whose beliefs overlap our own to some appropriate extent. However, we are warned that this is not a theoretical problem about “untranslatability”, but simply a practical problem about the limitations of argument: it is not that we live in different worlds from the Nazis, say, or the Amazonians, but that conversions of point of view will not occur as a result of inferences from previously shared premises. Rorty’s point here is that inference from previously shared premises is not open to us, but that this nonetheless does not mean that we go so far in our divergence as to live in different worlds.

Rorty makes reference to Quine’s and Donald Davidson’s hypothetical examples of an anthropologist studying jungle tribes, and attempting to communicate with them. He states that when the natives’ and our behavior in response to certain situations is pretty much the same, we think of both of us as recognizing the plain facts of how things are—the noncontroversial objects of common sense. But when these patterns of behavior differ wildly, we shall say that we have different Weltanschauungen, or cultures, or theories, or that “we carve up the world differently”. But it would create fewer philosophical problems just to say that when these patterns differ, communication becomes harder and translation less helpful. Translation may become so awkwardly periphrastic, indeed, that it will save time simply to go bilingual.

It may be that Rorty is worried about the ramifications of Kuhn’s thesis of incommensurability among different scientific matrices (roughly extendible to different general discourses), given his remark that “[t]he strong point of Kuhn’s

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93 Rorty, *PMN*, p. 325.
94 Ibid., p. 332.
95 Rorty, *ORT*, p. 31n.
96 Ibid., p. 104.
critics was that incommensurability seemed to entail indiscussability”. Inability to
discuss things would be anathema to anyone whose thesis is built around the
promotion of solidarity and tolerance through discourse. He goes on to note that
“[t]he strong point of [Kuhn's] defenders was that...nobody could answer Kuhn's
challenge by explaining how commensuration was possible”. But according to
Rorty, now these “Kuhnian wars” seem to be drawing to a close, since “both sides
are coming to agree that untranslatability does not entail unlearnability, and that
learnability is all that is required to make discussability possible”.

I think that Kuhn would agree both that untranslatability does not entail
unlearnability, and that learnability makes discussability possible. This does
involve a process of becoming “bilingual”, and although the proponents of
entrenched theories will at first attempt strenuously to achieve this by means of
translation, they will, in the end, find that, in order to have come to understand
what the proponents of new theories are talking about, they have instead achieved
something more like a Gestalt-shift; in this, they come to realise that they have
ceded to attempt translation, and have instead begun just to speak the new
language.

But Rorty's conclusion that both Kuhn's critics and his defenders have come
to realise “that untranslatability does not entail unlearnability, and that
learnability is all that is required to make discussability possible” seems to be
acknowledging the presence of untranslatability. Otherwise, it is unclear why
Rorty would entertain the argument that untranslatability does not, after all,
preclude discussability. If untranslatability does not even enter into the question,
then it is superfluous to invoke it in any sort of argument which one deems
relevant. If his contention is that, even if alternative discourses were
untranslatable--which they are not, according to him--there could still be
discussion among their proponents, then this should be spelled out more clearly,
to head off any possible appearance of ambiguity, especially given other areas of
Rorty's philosophy which are more clearly plagued by ambiguity.

It looks, however, as though Rorty is not completely enough convinced of
his own thesis of translatability, since he makes the claim “that we are not talking
about the same thing if we say very different things about it”. Presumably this is
meant to apply to what is the case for alternative discourses. I think that this is
arguably reminiscent of the Kuhnian point concerning the way in which
alternative matrices carve up the world in different ways, so to speak, generating
(in some sense) different worlds. If we are not talking about the same things, then
translation must surely have become hopelessly elusive, and we have a situation
of incommensurability. Translation would only be possible if we were applying
different terms to the same things. This need not bear an ontological stamp: we
might all carve up the world conceptually in the same way (using a common

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97 Ibid.
98 See pp. 36-7.
99 Rorty, ORT, p. 105.
taxonomy), but simply do so employing different sets of terms (or lexicons). But
then this leads us to a language-world separation which Rorty disallows.

Referring back to the passage quoted above, I think it is similarly unclear, if
translation between alternative discourses is considered to be possible (as Rorty
considers it to be), why it would be “less helpful” in cases where patterns of
communication differ significantly. Translation ought to be either possible or not,
and if it is not—as Kuhn argues, in respect of alternative scientific matrices—then
the only remaining option is to resort to a Gestalt-shift in understanding. This is
not necessary if translation, however difficult, is still at least truly possible. (I am
assuming here that Rorty’s exhortation, in situations such as this, to “go bilingual”
involves not translation, but a sort of Gestalt-shift.) Moreover, surely what we are
concerned with here is not a question of saving time, but of determining what is
possible.

Hermeneutics

Before taking a look at the view of hermeneutics which Rorty espouses, and
subsequently at how that measures up to what I take to be Kuhn’s understanding
of the role of hermeneutics in his own programme, I wish to take a general look at
the portrayal of hermeneutics which we find in the work of Wilhelm Dilthey, to
serve as a background for Rorty and Kuhn on this issue. It is not my intention to
evaluate this at all, but merely to present it—albeit in fairly rough and truncated
form—as a basis from which to consider Rorty and Kuhn.

Dilthey’s exploration of hermeneutics was directed towards reaching an
understanding of written works based upon an imaginative consideration of the
creative process of the author. The intent of an interpreter of a text was thus to
include the purpose and meaning of the author in the consideration of the work
itself. Earlier interpretive methodologies had conceived of the process in logical-
rhetorical terms; there were attempts to reduce literary interpretation to a system
of rules. Friedrich Schleiermacher, whose work on the interpretation of biblical
texts inspired much of Dilthey’s work in hermeneutics, introduced new concepts
of receptivity and creativity into the process. The interpreter must become
receptive to the mentality of the author, and to whatever were the features of the
environment in which that individual was writing. For this, the interpreter must
invoke her own creative capacity in generating the necessary empathetic sense for
the development of this receptivity. She must empathetically identify with the
author.

The idea here is that the interpreter moves her own being in some way into
the relevant historical setting of the author, by momentarily emphasising some
mental processes, while allowing others to fade away. In this way, she is expected
effectively to reproduce the alien life of her author within herself. Dilthey termed
this process "transference".\textsuperscript{100} But we cannot understand what we have not experienced; we can understand of the psychic states of others only as much as we know in ourselves. Thus, the greater our own inner richness, the better-equipped we will be to come to an understanding of others. This is the foundation of the understanding required for interpretation. Since there is no immediate communication between individuals, such as the author and his interpreter, the latter must rely upon inference.

The interpreter attempts to judge the relation of a work to the mentality of its author. This is achieved through the workings of the hermeneutic circle, which, in general terms, entails a circular movement of interpretation between a whole and its parts: to quote H.P. Rickman,

[The hermeneutic circle] arises in the understanding of complex wholes and their parts, because a whole can only be comprehended in terms of its parts while the latter acquire their proper meaning within the whole. Words and sentences are the most obvious example. We understand "hand me my clubs" by grasping the meaning of the individual words; but we can only select the appropriate meaning of "club" or discard the use of "hand" as a noun when we have an idea of what the whole sentence means. In practice we solve this problem by a to-and-fro, or shuttlecock, movement, though in simple cases we are hardly aware of it.\textsuperscript{101}

So the interpreter strives to understand the author's whole work from the combination of its individual words and phrases, as well as to understand the individual words and phrases based upon a consideration of the work as a whole. For this, language offers a basic interpretive tool, one through which the inspired creativity of the interpreter can act.

The creativity of the interpreter and that of the author meet as one, such that the interpreter is enabled to re-create and re-live that which is past. But notwithstanding the relative fluidity of the creative aspects of the interpretive process, a developed historical consciousness must ground the personal inspiration of the interpreter. The hermeneutic "procedure of circular exegesis...promises to provide a means of averting what has been called the commonest error of the intellectual historian: to write about things he does not really understand--things he has not 'internalized' and thought through for himself".\textsuperscript{102}

One may object that an adequate degree of empathy cannot be generated between two individuals who may be quite significantly separated in time and circumstance. Certainly, the lack of immediate communication between individuals in such situations, and the resulting reliance on inference, renders the possibility for the required degree of empathetic understanding somewhat doubtful. The hermeneutic view of Schleiermacher takes the personalities of both author and interpreter to be adequately similar, in having been formed within a

\textsuperscript{100} Dilthey, \textit{SW}, p. 454.
\textsuperscript{101} Rickman's introduction to Dilthey, \textit{Swr}, p. 10.
\textsuperscript{102} Hughes, quoted in Ermarth, p. 10.
common human nature. On this view, differences among individuals are not really radical ones of a qualitative sort; they merely reflect differences of degree of or relative emphasis on mental traits which are common to us all as human beings.

It will be interesting to note in relation to Rorty that, for Dilthey, hermeneutics is committed to the notion of truth. Hence, it has an epistemic function: to promote knowledge of ourselves and of humanity, through an examination of personal and cultural creativity. We may extend this attitude towards truth to a consideration of the sciences, as H.P. Rickman has done:

An outstanding example [of this circularity] is the fact--already noted as crucially important for the human studies--that the thinking of individuals can only be understood by reference to the world of mind or the cultural sphere while comprehension of the latter involves knowing about the mental processes of individuals. This is, of course, logically unsatisfactory; but the scientist, concerned mainly with relationships between the general and the particular rather than the whole and its parts, is involved in an analogous circle. He can only have a general conception of what dogs are like by gaining knowledge of individual dogs, but could never recognize an animal as suitable for his study on dogs unless he had a general idea of what a dog is.  

I turn now to look at Rorty's plea for hermeneutics, and to examine how it relates to the above-described picture. Since Rorty sees the desire to develop a theory of knowledge as a desire for constraint, his advocating that we give up the desire for confrontation and constraint means that we relinquish the desire for an epistemology. He describes hermeneutics as an expression of this hope. He is careful to point out that it is not intended to be a successor to epistemology, a new discipline, or a programme for research.

Epistemology sees the hope of agreement as a token of the existence of common ground which, perhaps unbeknown to the speakers, unites them in a common rationality....For epistemology, to be rational is to find the proper set of terms into which all the contributions should be translated if agreement is to become possible.

Thus, epistemology unites people through "mutual interests in achieving a common end". On the other hand,

For hermeneutics, to be rational is to refrain from epistemology--from thinking that there is a special set of terms in which all contributions to the conversation should be put--and to be willing to pick up the jargon of the interlocutor rather than translating it into one's own.

Hermeneutics unites people through civility: there is no common goal, no common ground. The hermeneutical view sees the relations between various

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103 Rickman's introduction to Dilthey, Swr, p. 11.
104 Rorty, PMN, p. 315.
105 Ibid., p. 318.
106 Ibid.
discourses as those of strands in a possible conversation, and presupposes no
disciplinary matrix which unites the speakers. The hope of agreement is never
lost, so long as the conversation lasts.\textsuperscript{107}

While we get epistemic commensuration only where we already have
agreed-upon practices of discourse--where we know what is going on, and want to
codify it, in order to extend, strengthen, ground, or teach it--we must be
hermeneutic where we do not understand what is happening, and where we
admit that, rather than being \textit{blatantly} Whiggish about it. So this difference
between epistemology and hermeneutics boils down to a question of familiarity.
Rortian ethnocentrism dictates that we must operate within, at least at the start of
discussion, the belief-web of our own culture. Rortian hermeneutics works
consistently with this attitude: “that [it] inevitably takes some norm for granted
makes it, so far forth, ‘Whiggish’”. But it “proceeds nonreductively and in the
hope of picking up a new angle on things”; thus, it can “transcend its own
Whiggishness”, which apparently is to say, that its Whiggishness becomes less
blatant, and more provisional. It is likely that Rorty would oppose this mitigated
sort of Whiggishness to “the attempt (which has defined traditional philosophy) to
explicate ‘rationality’ and ‘objectivity’ in terms of conditions of accurate
representation”, which “is a self-deceptive effort to eternalize the normal
discourse of the day.”\textsuperscript{108}

According to Rorty, the hermeneutic notion of knowledge is of culture as a
conversation: we play back and forth between guesses about how to characterise
particular elements (the parts) and guesses about the point of the whole situation
(the whole). We cannot avoid the hermeneutic circle: we are unable to
understand the parts of a strange culture, practice, theory, language, etc. without
having an idea of how the whole thing works, and \textit{vice versa}. Thus, hermeneutics
is better seen as another way of \textit{coping}, rather than as another way of \textit{knowing}.
“Hermeneutics does not need a new epistemological paradigm....Hermeneutics,\textit{ rather,} is what we get when we are no longer epistemological”.\textsuperscript{109}

The typical Rortian emphasis, here, on the abandonment of epistemology is
not consonant with the view of hermeneutics which we see in the Diltheyan view
above. Hughes' reference to “the commonest error of the intellectual historian: to
write about things he does not really understand” implies that it is possible to
misunderstand historical writings, which further implies that it is possible to get
things wrong in history. This presents us with the distinction between truth and
falsity, a dichotomy which Rorty has rejected. Moreover, Dilthey was committed
to preserving an epistemic role for hermeneutics, such that its dedication to the
notion of truth served to promote knowledge of ourselves and of humanity.

It would appear that Rorty’s account of hermeneutics focuses more on the
aspect of the development and maximisation of solidarity among contemporaries,
than on its role in the interpretation of past historical works. But this latter

\textsuperscript{107} Ibid.
\textsuperscript{108} Ibid., pp. 11, 320-1.
\textsuperscript{109} Ibid., pp. 319, 325, 356.
function is of considerable importance in Dilthey's account, and I believe that it is this historical purpose which Kuhn has in mind. For Kuhn, as we have seen, the proper approach of the historian to historical theories--such as scientific ones--is dictated by hermeneutics. One must evaluate each theory on the basis of its own context. The lack of an overarching context for evaluation precludes the God's-eye view from which the judgment of truth might be applied to any particular theory, to the exclusion of all of the others. Hence, "Kuhn's claim that no algorithm [i]s possible save a post factum and a Whiggish one (one which construct[s] an epistemology on the basis of the vocabulary or assumptions of the winning side in a scientific dispute)."111 All of this relates to the incommensurability which imbues the relationship between alternative discourses, or scientific matrices, since it is this incommensurability which necessitates hermeneutics.

Relating this more precisely to the Diltheyan account, the way to evaluate Aristotelian mechanics, for instance, relies upon an attempt empathetically to enter, as much as possible, the context in which Aristotle was thinking and writing. Separate elements in Aristotelian theories would need to be understood in relation to this entire context, and in turn, we are aided in formulating our sense of this context by virtue of particular elements in Aristotelian thought. The question will be to what degree we are able to achieve this empathetic "transference"; we ought to expect there to be some limit to it, such that we will never quite be able to understand Aristotelian mechanics in the way a contemporary of Aristotle would have. The difficulty of trying to do so is consistent with the greater divide between matrices that we see in Kuhn's thesis of their incommensurability than we see between discourses in Rorty's thesis of their inter-translatability. Still, the hermeneutic system reminds us at least to attempt to do this as far as we are able, rather than "blatantly" interpreting Aristotle strictly according to our own contemporary norms.

In light of this last point, Rorty's claim that "Aristotle and Galileo both have to face the tribunal of our present beliefs before we shall call anything either said 'true' "112 is antithetical to the spirit of hermeneutics as presented both by Dilthey and his supporters, and by Kuhn. This claim makes it look as though Rorty is espousing a most blatant sort of Whiggishness, one which is resistant to "pick[ing] up the jargon of the interlocutor rather than translating it into one's own". It surely does not resemble the attitude of empathy which characterises Dilthey's and Kuhn's understanding of hermeneutics. It is difficult to imagine what Rorty could have in mind here, in light of the historical nature of the "discourses" being considered. His point, in speaking of his ethnocentrism and the periodic re-weaving of beliefs, is that we ought to begin with our own set of beliefs, and then re-evaluate them in light of the beliefs of others, which we encounter in discourse with them. Direct discourse with historical figures is obviously impossible. So, how do we come to understand historical figures, if we insist on a conscious

110 See pp. 50-1.  
111 Rorty, PMN, p. 324.  
112 Rorty, ORT, p. 51.
adherence—even if only initially—to our contemporary framework? Ethnocentrism (even the Rortian variety) is antithetical to hermeneutical empathy, as it applies to historical understanding.

Rorty equates epistemology with normal discourse (i.e., that which is able to be brought under a set of rules), and hermeneutics with abnormal discourse (i.e., not rule-bound). He labels hermeneutics as, roughly, a description of our study of the unfamiliar, while epistemology is, roughly, a description of our study of the familiar. This is not a dichotomy which we would find in Dilthey or Kuhn, since they both understand hermeneutics to be an epistemological tool, one which is essential for an historically sensitive account of epistemology. This sort of epistemological account embraces the study of the unfamiliar: specifically the study—in historical science—of unfamiliar epistemologies. Neither can hermeneutics be simply opposed to rule-bound discourse, since, in trying to develop an empathetic understanding of a historical figure, we attempt to “internalise” the context surrounding that figure—including, in the case of science, the rules of thought and practice which the scientist in question accepted. However, hermeneutics can more safely be identified with what we might call abnormal discourse as a generalisation of Kuhn’s revolutionary science: Kuhn has likened the hermeneutic process through which we attempt to understand previous normal discourses, such as Aristotelian mechanics, to the pattern seen when the proponents of competing theories struggle to understand each other across a revolutionary divide. Dilthey is similar to Kuhn, in viewing this process as somewhat less rule- and logic-bound than others have perhaps envisioned it to be, and more influenced by a fluidly creative energy. But this is not to erase all presence of structure, or reason.

Rorty states, however, that, if we draw the line between epistemology and hermeneutics as he has suggested, “then it seems clear that the two do not compete, but rather help each other out.” The reason given for this is that “nothing is so valuable for the hermeneutical inquirer into an exotic culture as the discovery of an epistemology written within that culture.” This is a view of things which is consonant with Kuhn’s, and it indicates the epistemic orientation of hermeneutics. But it is disharmonious with the urgings, which we often encounter in Rorty, to abandon the epistemological project altogether. As I have noted before, Rorty is ambiguous on this point.

He goes on to say that “nothing is so valuable for the determination of whether the possessors of [that exotic culture] uttered any interesting truths (by—what else?—the standards of the normal discourse of our own time and place [my emphasis]) than the hermeneutical discovery of how to translate them without making them sound like fools”. But the point of hermeneutics is to prevent an interpretation of historical figures which would make them sound like fools.

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113 Rorty, PMN, p. 346-7.
114 Ibid., p. 349.
115 Ibid., p. 346.
116 Ibid.
precisely by, as much as possible, not, evaluating them using the standards of the normal discourse of our own time and place. This is why it is so valuable for the hermeneutic enquirer to encounter an epistemology in historical works, since it will be this past epistemology which she will aim to internalise (attempting to leave her own behind), in trying to understand historical theories.

Rorty states that “Kuhn’s examples of ‘revolutionary’ change in science were, as he himself has remarked, cases of the sort which hermeneutics has always taken as its special assignment--cases in which a scientist has said things which sound so silly that it is hard to believe that we have understood him properly”. He offers this quotation from Kuhn’s The Essential Tension:

When reading the works of an important thinker, look for the apparent absurdities in the text and ask yourself how a sensible person could have written them. When you find an answer,...when these passages make sense, then you may find that more central passages, ones you previously thought you understood, have changed their meaning.

It should be clear that apparent absurdities in historical texts appear absurd when one attempts to do just what Rorty is advising us to do: to begin looking at them through the eyes of our own circumstances and ways of thinking. The process of “ask[ing] yourself how a sensible person could have written [such absurdities]” is the very process of making the Gestalt-shift into alien conceptual terrain; this is the process of hermeneutics, as construed by Dilthey and Kuhn.

Rorty reports that Kuhnians speak of cases in the history of science in which the description of the problem to be solved changes, thus changing the “observation language” used to describe the “evidence”. But he warns us that “[t]his is not to say that we cannot, retrospectively, describe the problems and the data of all earlier epochs in a single, up-to-date, commensurating vocabulary”. Rorty calls this “the ability to commensurate by hindsight--the ability to say that what Aristotle was looking for was what Newton found”. Nevertheless, this “should not mislead us into trying to describe our favorite ancestors as using ‘the hypothetico-deductive observational method’ (as Hook sometimes characterizes ‘scientific inquiry’).”

In Rortian terms, “commensuration by hindsight” would entail bringing a plurality of discourses under a set of rules which would indicate how we might achieve agreement among them. But, if our description of problems and our observation-language--our discourse, in short--change, then we ought not to be able retrospectively to describe earlier discourses using a single, commensurating vocabulary, for this latter would be a neutral observation language, the existence of which both Rorty and Kuhn have denied, and which is frankly contradictory to any change in observation language as a corollary to a change in discourse. It seems that we are to understand this “hindsight” as operating to re-contextualise

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117 Ibid., p. 323.
118 Kuhn, ET, p. xii, quoted in Rorty, PMN, p. 323.
119 Rorty, ORT, pp. 68-9.
all previous discourses into the context of our own. As I have explained, Kuhn has shown the folly of this manoeuver in a properly historical understanding of science. It is also antithetical to Diltheyan hermeneutics. It is difficult to see how we might be able to perform "commensuration by hindsight", since different webs of belief (particularly those of distant historical character) cannot be expected to fit into our contemporary context of belief without considerable warping, given that their natural contexts are different from our own. The incommensurability of different discourses for which Kuhn has argued in the context of scientific matrices thus speaks against our re-weaving our web of belief to accommodate beliefs from significantly alien sources. Hermeneutics, employed in the study of history, does not make use of such a re-weaving, but rather must find a way to leap across the chasm of incommensurability which separates different historical discourses. This chasm is what renders them separate discourses, after all.

It is peculiar, also, that Rorty states that, in pre-Kuhnian philosophy of science, rational enquiry involved putting everything into a single, familiar context, such that any rational enquirer would agree that the sentences of this vocabulary were truth-value candidates. Given Rorty's rejection of pre-Kuhnian philosophy of science, we might expect him, on the basis of this statement, similarly to reject such a unifying context. But what he describes here as pre-Kuhnian philosophy of science looks to be performing an identical function to his own "commensuration by hindsight".

It should be clear from the discussion in this section that Rorty's belief, as expressed in point ten of my introductory list, is inaccurate, given the radical divergence between Dilthey's construal of hermeneutics, which Kuhn arguably embraces, and Rorty's version of hermeneutics.

\[120\] Ibid, p. 95.
Conclusion

My intention has been to elucidate the many ways in which Richard Rorty finds similarity between Thomas Kuhn's work and his own, and further, to demonstrate what I see to be a distinct and extensive lack of similarity between them. It is my contention that Rorty has misunderstood a considerable number of elements in Kuhn's programme, as well as the intent with which it was offered. I wish to proceed now to provide a summary of these problems.

Despite their mutual rejection of correspondence theories, Rorty and Kuhn part ways in respect of what they see as their replacement. Rorty wishes to abolish all talk of mind and mental representations, while Kuhn shows no interest in doing so. In connection with this, Rorty disdains the notion of observation, while Kuhn retains a commitment to it, as the element of “discovery” (that which is “given” by Nature) which is an essential component of knowledge-acquisition.

A significant issue which drives a wedge between the respective theses of Rorty and Kuhn is that of the need (or lack thereof) for a conceptual framework which guides thought and action. For Kuhn, following as he does, a generally Kantian type of programme, such an a priori framework is essential for the intelligibility of the endeavours of science—which is the activity that he is concerned to discuss. Rorty, on the other hand, is not always clear about whether he thinks that all traces of a guiding framework should be abandoned, or that there will be some mutual understanding among the participants of a discussion regarding what is to count as acceptable, thus defining their particular culture’s web of beliefs. At any rate, Rorty fails to grasp the crucial importance, for Kuhn, of a strong a priori conceptual framework. Hence, he sees Kuhn as engaged in a crusade parallel to his own, of rejecting such a framework.

Importantly connected to this is the question of their respective attitudes towards science. The thrust of Kuhn’s work is clearly intended to clarify the way in which science works, and has worked throughout its history. He has made specific attempts to delineate ways in which science and other cultural activities can be distinguished from each other. Rorty appears not to have noticed this, having extrapolated what he sees as Kuhn’s programme to cover all cultural endeavours; he takes Kuhn to be doing this, as well.

For Rorty, this new way of conceiving of cultural activity (including science) illustrates a programme of “holistic pragmatism”, wherein the pattern of all cultural activities forms a sort of seamless whole, driven by pragmatic concern. I have tried to show that, not only is Rorty’s explanation of how he sees pragmatism to work hopelessly vague, but his writing often betrays an approach which is arguably antithetical to pragmatic concern. This is at odds with Kuhn’s much clearer explication of science as essentially a pragmatic activity.
Certainly, Kuhn's attempt to give credence to non-rational factors which have influenced the course of science throughout its history has been an important link, in Rorty's eyes, between himself and Kuhn. However, I believe that Rorty has over-estimated the degree to which Kuhn leans on non-rational factors in explaining both the historical course of science and its functioning at any given time. Kuhn wants to maintain that both rational and non-rational factors guide the work of scientific disciplines.

Moreover, Rorty confuses this issue by re-defining "rationality" in terms of solidarity and tolerance among the members of a culture, and of persuasiveness in discussion (as opposed to what he sees to be the more coercive attitude of traditional rationality in argument). He sees Kuhn as basically an ally in this new orientation towards rationality. I think it is clear, however, that Kuhn demonstrates no tendency to understand rationality in non-traditional terms.

Finally, while both Rorty and Kuhn advocate use of the hermeneutic method, Rorty's intent is for people to apply it to attempts to "cope" with their own and others' cultures; for Kuhn, hermeneutics more specifically provides a methodology for attempting to understand, by reference to their historical context, scientific conceptual and practical schemes which are now conventionally considered out-of-date and inaccurate. I have argued that Rorty's construal of hermeneutics is at odds with the more standard view of it, common to both Wilhelm Dilthey (who did extensive work in the development of hermeneutics) and Kuhn. Much of Rorty's difficulty here rests on his confusion about (and, again, idiosyncratic definition of) incommensurability between alternative views of the world.

I believe that I have shown that Thomas Kuhn's extensive work in the history and philosophy of science is not at all applicable to the sort of cultural programme which Rorty is offering, in terms of intent, scope, and fundamental content.