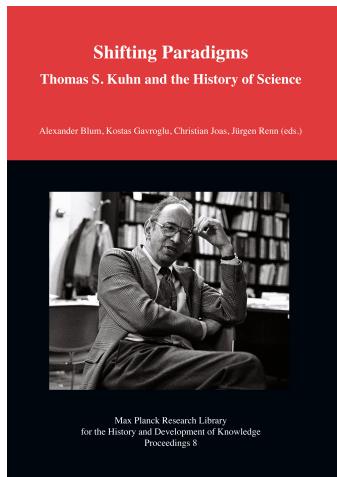


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David Bloor:

The Pendulum as a Social Institution: T. S. Kuhn and the Sociology of Science



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Chapter 17

The Pendulum as a Social Institution: T. S. Kuhn and the Sociology of Science

David Bloor

Introduction

In the fullness of time, when the ‘history of the history of science’ is written, there are three themes which will surely find a place in the narrative covering the latter part of the twentieth century. The first is the impact on the history of science of Kuhn’s *Structure of Scientific Revolutions* (Kuhn 1962, second, enlarged edition 1970). The second is the disputed relation between the history of science and two neighboring fields: the sociology of science and the philosophy of science. The third is the question of relativism and its relation to the history of science. The three themes are interconnected. Part of the problem of defining the impact of Kuhn’s *Structure* derives from the question of whether he challenged the *status quo* by virtue of introducing new sociological insights, or because he introduced new philosophical assumptions, or new historiographical methods, e.g. methods that led to relativist conclusions about the status of knowledge.

A complicating factor that must be confronted by any future historian of the field is that the historical actors, by which I mean Kuhn and his contemporaries, have themselves offered answers to some of these questions. This raises the question of whether the self-awareness of the historical actors was accurate. Were the contested tendencies of the recent past correctly understood by those who enacted them and argued about them? For example, in some quarters Kuhn’s book has been read as a work which forges a link between the history of science and the sociology of science, although Kuhn himself spoke out in the strongest terms against certain sociological accounts of science that were based on his work (Kuhn 1992, reprinted in Kuhn 2000, 105–120).

I shall come back to the question of relativism. For the moment I want to concentrate on the relation between historical and sociological approaches. To say that Kuhn’s work provides a link between the history and sociology of science is to say very little until the character of the link is identified. Unfortunately there are superficial ways to make the connection as well as deeper and more

penetrating links that might be discerned. The future historians of the history of science will be failing in their scholarly duty if they cannot sift the two and focus their analysis accordingly. This danger would arise if the future historians had a false stereotype of sociology in their minds (for example a stereotype in which a sociological explanation involved a denial of the role played by the material world). Such a stereotype would distort any answer then given to the question of whether Kuhn, as an historian who clearly respected science, was also engaged in a ‘sociological’ analysis of that enterprise.

In what follows I shall not try to anticipate the relevant parts of the future history of the history of science. I shall not try to write it in advance, but I shall offer some material that, I believe, should inform the thinking of those who, in future years, might chronicle today’s activities. My argument will be that, despite his own protestations, Kuhn developed some profound sociological insights which were integral to his analysis of scientific knowledge. These insights, however, may not be immediately evident to an eye that has not been sensitized to pick them out.

In order to prepare the ground I need to lay out some basic ideas about the nature of social phenomena. This will make it easier to detect the sociological themes in Kuhn’s thinking. In particular, I shall formulate a simple model of a social institution.¹ I shall then use this model to offer a reading of Kuhn’s important 1961 paper “The Function of Measurement in the Modern Physical Sciences.”² By making Kuhn’s use of the idea of a social institution explicit I shall show how it is possible to appreciate more fully the coherence of the paper and, ultimately, the coherence of *The Structure of Scientific Revolutions*.

In setting out my argument in this way I shall be following the work of the Edinburgh sociologist Barry Barnes as he developed it in his 1983 book *T.S. Kuhn and the Social Sciences* (Barnes 1982, 1983; 2003). This is not a book about ‘applying’ Kuhn to the social sciences, in the sense of fitting sociology, or its history, into some pre-given ‘Kuhnian’ pattern. Rather, the book is about the sociological insights to be found in Kuhn’s work itself, and the potential generality of those insights. Barnes’ book is not as well known as it should be, but it would be a grave injustice if it were overlooked by the future writers of the history of the history of science. As well as seeking to offset this possibility I also want to

¹An institution might be defined as a set of roles and statuses that are linked by conventions. The meaning of the words ‘institution,’ ‘role,’ ‘status’ and ‘convention’ differ from one another, but all of them designate aspects of social reality that exhibit an important, common feature. Furthermore, despite their differences, all of them describe a reality that is strangely intangible and elusive. For present purposes the differences between these terms is not of great importance. My aim is to explain the common feature they share and identify the source of the elusive quality of the social.

²Kuhn (1961), reprinted in: Kuhn (1977, 178–224). The page references are to the 1977 publication.

develop parts of Barnes' argument in more detail. I now move to a brief account of social institutions as a preliminary to the exposition of Kuhn.

Institutions

Sociologists, like anthropologists, use the word 'institution' in a broad sense. They talk of such things as the institution of marriage, the institution of law and the institution of money. The point of this usage is to draw attention to a range of important facts: for example that marriage involves, but goes beyond, the biological facts of co-habitation and child-birth; that criminality involves more than entering a person's house and exiting with the silver ware; and that money involves more than passing someone a metal disc when they pass you a loaf of bread. For simplicity let us stay with the latter example. Everyone knows that coins are more than metal discs. They are discs with a certain status and they operate within the broader institution of economic activity. But what is the added element? What makes a metal disc into a coin?

Stated in simplified terms the answer is that a disc becomes a coin by virtue of being regarded as a coin, treated as a coin, believed to be a coin, and being referred to as a coin. Let me summarize all these different orientations to the disc by saying that coins are coins because they are 'called' coins. The word 'called' can stand in for the rest. This analysis looks suspiciously circular but this is not really a defect; it will turn out to be a crucial feature of the model that is being developed.

In order to explore this circularity consider the question: When I refer to the disc as a coin, to what, exactly, am I referring? It will not be to the disc *qua* disc. Rather, I am referring to the status of the disc as a coin. But what is the status to which I am making reference? What sort of thing is a status? What reality corresponds to my words? We need an answer that articulates the informal idea that a coin is a coin if it is 'called' a coin. The answer is this: When I refer to a coin I am referring to other people's references to the disc as a coin. And what are these other people referring to? The answer must be that they too are referring to other people's references. This must culminate in a self-referential system. It follows that there is no reality independent of the discourse to which the discourse corresponds or refers. Each act of reference has an independent object, namely other people's acts of reference, but the system as a whole has no independent object. Rather, it has a self-referential character.

This analysis can be generalized to cover all other words that designate social reality. When a person follows a convention, what are they following? The answer is that they are following other peoples' following. They are doing this because other people do this, and they would not do it otherwise, but bringing into

the story the motives and calculations that are at work does not alter the basis of the account. One can still detect the self-referential character of the motives and calculations that may be involved. No wonder that the basis of social ontology is difficult to grasp. There is nothing to grasp but the grasping. The concepts involved do not have an independent object of reference. They furnish their own object through self-reference.

It is important to appreciate that this theory does not reduce society to mere talk about talk. Recall, first, that the verbal emphasis on ‘calling’ something a coin was just a simplification. It stands duty for a range of responses that include non-verbal reactions to objects and people. Secondly, these verbal and non-verbal classifications have material consequences. Lack of anything that can be ‘called’ money doesn’t just limit what can be said, it limits what can be eaten. The self-referential analysis is not, as it may at first appear, a thin and verbal account of social reality. Rather, it is an account which enables us to focus on the most basic processes that enter into the construction and maintenance of any form of social system.

To appreciate the potential of this analysis it may be of help if the process of self-reference is related to another mechanism which may be easier to grasp, namely, that of a self-fulfilling prophecy. The social reality associated with the use of what is called ‘a coin’ may be thought of as the product of a self-fulfilling prophecy. The existence of a currency is a reality brought about and maintained because, and in so far as, people believe in it. Similarly, to follow a convention is to add to the strength of the convention and increase the likelihood that others will conform.

The sociologist Robert Merton has drawn attention to the importance of self-fulfilling prophecies. He saw them at work in a range of phenomena such as racial, class and sexual prejudice. Belief in the inferiority of a sub-group will lead to its members being treated in hostile ways and this will result in further disadvantage and thus consolidate their inferior status. Coming closer to the example of the coin, Merton also illustrated the idea of self-fulfilling prophecies in the realm of finance. If a bank is believed to be unsound the depositors will withdraw their money and, whatever the initial state of affairs, the belief will be made true. The ‘prophecy’ of the bankruptcy is fulfilled (Merton 1957, 421–436).

Merton belonged to the school of American sociology called ‘functionalism.’ Functionalists try to explain social phenomena by identifying the ‘functions’ they perform. (Kuhn appears to have taken over their terminology in the title of some of his papers, including his paper on measurement.) Merton thought that the appeal to functions could explain the ‘norms’ and ‘values’ that, on this theory, move people to action. However, Merton disapproved of prejudice, and rumors that destroy banks, so he identified these phenomena as dysfunctional

rather than functional. He thus confined the appeal to self-fulfilling prophecies to the explanation of social pathology. As a result, Merton overlooked the fact that these mechanisms can work in the other direction. A correct analysis of why a bank is sound will also contain an important element of self-fulfilling prophecy. His concentration on cases with undesirable outcomes led him to miss the generality of his own insight.³

If one gives the mechanism of the self-fulfilling prophecy the generality it deserves we can use it to see how the process of self-reference works over time. Appreciating the role of self-reinforcing processes adds motion to an idea that might otherwise seem static. It provides the dynamics of the system. The appeal to self-fulfilling prophecy thus helps to make the simplified model of self-reference a little less abstract and easier to relate to concrete historical material, for example, the sort of material that Kuhn assembles in his paper on “The Function of Measurement in Modern Physical Science.”

The Criterion of Agreement

At the beginning of his paper on measurement Kuhn draws attention to the tables that often occur in scientific textbooks. These tables juxtapose a list of numerical predictions, derived from a scientific theory, with a list of measurements taken from experiments. Kuhn gives the (presumably hypothetical) example shown in table (17.1).

Theory	Experiment
1.414	1.418
1.732	1.725
2.236	2.237

Table 17.1: Kuhn’s example of a table of results.

He asks the question: What are these tables for? What function do they perform? The answer might seem obvious. Surely, they are meant to show the reader that the theory is true or, at least, to show that the theory agrees with the facts. Kuhn, however, gives a subtly different answer. He says that the tables are there to show us what is meant, in this context, by ‘agreement with the facts.’ Everyone knows that agreement can never be absolute so, Kuhn concludes, the function of the table is best expressed by saying that it shows what counts as

³For a further discussion of this example, and further references, see Barnes (1983, 536–537).

‘reasonable agreement.’ The table, therefore, does not simply reflect a truth about nature; it also embodies, and speaks to, the scientific community’s own response to nature. The table carries the message: ‘*This* is what the scientific community counts as reasonable agreement.’⁴

Kuhn’s argument now takes a striking turn. Suppose we enquire into the *criterion* of ‘reasonableness’ that is in play. What is this criterion? Kuhn says that the answer can only be found in the tables themselves. We are forced to look at the tables themselves, says Kuhn, because there are no “consistently applicable external criteria.” The criteria of agreement between fact and theory vary greatly from discipline to discipline and time to time. In addition, and crucially, these criteria are not external criteria, that is, external to the local practices of science itself. But, notes Kuhn, this has taken us around in a circle. As he puts it, “we have gone full circle” (p. 185). We look at the table and ask for the criterion that informed its construction, but to find that criterion we must refer back to the table itself. Kuhn’s own formulation is as follows:

I began by asking at least by implication, what characteristics the numbers of the table must exhibit if they are to be said to ‘agree.’ I now conclude that the only possible criterion is the mere fact that they appear, together with the theory from which they are derived, in a professionally accepted text. (p. 185)

Appearing as they do, in a table, he goes on, they cannot demonstrate anything but reasonable agreement.

And even that they demonstrate only by tautology, since they alone provide the definition of ‘reasonable agreement’ that has been accepted by the profession. (p. 185)

Kuhn speaks of tautology and circularity but he could have expressed his insight by speaking of self-reference. What is being referred to when reference is made to the reasonable agreement of theory and experiment? The reference is to the phenomenon made visible on the pages of the book in which a certain relation

⁴Kuhn does not cite any actual instances of text-book tables (or graphs) though there can be no doubt that he was speaking on the basis of first-hand experience. His hypothetical example is sufficient for his argument, but it is easy to assemble a few, real-life examples, e.g.: Born (1923, 27), a table comparing calculated and observed wave-lengths of the Balmer series; Richtmyer, Kennard and Lauritsen (1955, 131), graphs comparing spectral-energy distributions predicted by the Rayleigh-Jeans formula, Planck’s formula and Wien’s formula with experimental data; Bleaney and Bleaney (1957, 521), a table comparing observed and calculated values of specific heats of some metals and (p. 536) a table comparing observed and calculated values of the Hall effect; Reid (1932, 206), a table of calculated and experimental values of the angle of zero lift for a range of wing-sections.

between fact and theory is presented as (i.e. is ‘called’) ‘reasonable agreement.’ The readers of the textbook correctly refer here to reasonable agreement because authoritative members of the scientific profession, represented through the text, refer to it as that. In referring to it as reasonable agreement the members of the scientific profession *make it* reasonable agreement. They have self-referentially created a vital piece of social reality, namely, the professional standard and criterion of agreement between fact and theory.

Measurement in Normal Science

I now want to follow Kuhn’s argument a stage further. We have seen that he started his discussion with textbook science, but it is important not to miss the generality of the point he was making. The reader must not form the impression that the self-referential processes that he detected were a feature of textbooks alone—an artefact, perhaps, of the demands of pedagogy. For Kuhn, they are neither artefacts nor oddities: they are to be found in all the physical sciences that depend on refined measurement.

In his discussion of normal science Kuhn once again identifies the central role played by ‘reasonable agreement.’ The task of many, and perhaps most, scientists is that of refining and re-defining what may count as reasonable agreement. The objective of this work, he says:

is, on the one hand, to improve the measure of ‘reasonable agreement’ characteristic of the theory in a given application and, on the other, to open up new areas of application and establish new measures of ‘reasonable agreement’ applicable to them. (p. 192)

Questions of reasonable agreement are therefore not confined to text-books or to science in its completed state; they are ubiquitous.⁵

There are, however, profound difficulties confronting scientists when they seek to make measurements that are both precise and meaningful. Refined measurements only make sense, and are only possible, says Kuhn, against a back-

⁵In their papers and reports scientists and technologists frequently provide a running commentary of evaluation regarding the degree of agreement between their predictions and their measurements. They will declare this result a good one and that result disappointing or puzzling. What analysis can be given to these streams of individual judgments? They can be thought of as ‘performative utterances,’ see Austin (1961). What Austin called the ‘felicity’ or the ‘infelicity’ of the utterance depends, in this case, on whether the judgments are deemed appropriate by the relevant community. This fact about their broader reception is, of course, known to those who are making the judgments. They are aware that, not just the theory under test, but also their own reputation, is on the line as they issue their personal responses to the results.

ground in which a stable, theoretical tradition has been achieved.⁶ When such a tradition has been achieved, he argues, the general form of some of the basic laws will already have been guessed. Given these guesses, then refined measurements will start to make sense. This is because the guessed-at laws themselves are used in refining the instruments of measurement and in modifying the experimental techniques which generate the data. One again there is a sort of circularity at work.

Kuhn illustrates his claim by describing the difficulties encountered in finding the basic laws of chemical combination. Before Dalton, the empirical data about combining weights were complex and inconsistent. After Dalton's theory became known, instruments could be adjusted, and techniques pursued, with a sense of direction and purpose. This was possible because Dalton's theory was itself used to guide the adjustments and refinements. Kuhn sums up the situation and its resolution in an interesting way.

Before Dalton's theory was announced, measurements did not give the same results. There are self-fulfilling prophesies in the physical as well as in the social sciences. (p. 196)

The mechanism of the self-fulfilling prophecy is at work in the normal science that creates the shared standards that eventually find their way into text-books.

Kuhn occasionally made reference to Merton's work so perhaps he invoked the idea of the self-fulfilling prophecy because he had read Merton on this subject. But whatever the origin of the remark, what Kuhn said about self-fulfilling prophecy is right. It is a pity he did not follow up his insight. He could have corrected Merton's one-sided tendency to see self-fulfilling mechanisms at work only when 'dysfunctional' social phenomena are to be explained. Kuhn certainly did not take himself to be describing anything 'dysfunctional.' He was invoking self-fulfilling prophecies to illuminate one of the great achievements of science. If Kuhn had made the point explicit he could have restored the symmetry that was missing from Merton's analysis, but central to his own approach.⁷

⁶Kuhn warns the reader against the assumption that science is a purely theory-driven or 'theory-first' enterprise. His claim is that theory is a precondition of measurement, not of experiment or experience in general.

⁷Kuhn refers to Merton in a footnote on p. xxi of *The Essential Tension*. In the footnote Kuhn seems to take exception to the criticisms of Merton's work put forward by sociologists of science, such as Barnes, who do not subscribe to the functionalist approach. See Barnes and Dolby (1970). In the light of his comments, I suspect that Kuhn did not have a clear apprehension of the difficulties of Merton's view or the extent to which his own work might provide the basis for criticizing the explanatory appeal to norms and values.

Revolutions that Change the World

In *The Structure of Scientific Revolutions* Kuhn argued that, in the course of a scientific revolution, the world does not change but, after a revolution, the scientist works in a different world. As it stands this is a logical contradiction, though I do not think it was a mere lapse. I assume it was a deliberate device to draw attention to the difficulty of conveying what he wanted to convey (Kuhn 1970, 121).

Should we wish to do so, it is easy to remove the contradiction. All that is required to restore consistency is to attach a different meaning to the word ‘world’ on the two occasions on which it occurs. Thus it might be said that, though the ‘world-in-itself’ does not change, the ‘phenomenological world’ of the scientist might change during a revolution (Hoyningen-Huene 1993, 32 ff.). All strategies for removing the contradiction must have this general form. They can be differentiated from one another by their capacity, or lack of capacity, to carry forward the deeper aspects of Kuhn’s argument.

My suggestion is that the world that changes is the social world of the scientist and the world that does not change is the world of (non-social) nature. Recall- ing Kuhn’s paper on the function of measurement, notice that the ‘social world’ of the scientist contains things such as the standards of reasonable agreement between theory and experiment. We are therefore not, primarily, dealing with phenomenological changes but with procedural and behavioral changes. The changes concern the standards that are sustained by the profession as social institutions. These are the new things that come into existence and, because they are social things rather than material things, they can be said, roughly, to exist because they are believed to exist. Here, perhaps, we have the cause of Kuhn’s contradictions and hesitations. He was not equipped with the theory of social ontology that he needed to convey his meaning.⁸

Is this sociological reading consistent with the other things which Kuhn said about the changes that take place during a scientific revolution? For example, Kuhn described the change from Aristotle’s mechanics to Galileo’s mechanics by saying that, before this revolution, there were no pendulums—only swinging stones (Kuhn 1970, 121). How is this possible? On my sociological reading the change from a swinging stone to a pendulum must be understood as analogous to the change from a metal disc to a coin. A material object is accorded a certain status and is set in the context of an institutionalized pattern of activity. The swinging stone does not change but how the swinging stone is treated certainly does change. The change will be invisible if attention is focussed on the swinging stone itself. To see the change it is necessary to bring into the picture all the

⁸The sociological reading of ‘world’ does not rule out perceptual changes during a revolution, but I suspect that, if the phenomenology shifts, the shift will be a consequence of the prior social changes.

persons and activity around the swinging stone and attend to what these persons, collectively, do to, and with, the material object.

In the terms of the self-referential model of institutions it could be argued that, at the most basic level, a swinging stone becomes a pendulum because it is ‘called’ a pendulum. Can this be elaborated in a revealing way? It might be acceptable to call ‘reasonable agreement’ a social institution, but can it be acceptable to argue, by analogy, that a pendulum is an institution? The salient feature of the theory of social institutions that I have been using is that the discourse referring to social realities has a self-referring character. But references to pendulums are surely not self referential. They are examples of external reference, i.e. reference to an independent object. The analogy between the case of reasonable agreement and the pendulum, or between the coin and the pendulum, thus appears to break down. Is there any way to reconcile the pendulum as an institution and the pendulum as a material object? I think there is.

The Pendulum as an Institution

The place to begin is with the process of learning the meaning of the word ‘pendulum.’ It is plausible to assume that it is learned through ‘ostensive definition,’ that is, by a teacher pointing to examples and giving them their name. “*This* is what is called a pendulum.” A small number of exposures to a limited range of cases will generally suffice to evoke a reasonable competence in recognizing the more familiar kinds of pendulum, for example, those in grandfather clocks. By starting my account with ostensive definition I hope to ensure that the subsequent analysis will in no way compromise the material reality of the pendulum. The starting point, then, is independent reference, not self-reference.⁹

There is, however, a question that must be confronted. Has enough been said to ensure that the relation between the word ‘pendulum’ and the independent object, the swinging stone, is really one of ‘reference’? Given the story so far, do we have genuine reference, or merely some of the preconditions of reference? The answer is that we only have the preconditions. More is required. Genuine reference requires that it is possible to draw a distinction between correct and incorrect applications of a word and for the users of the word to be responsive to this distinction. This requirement may be expressed by saying that the ‘normativity’ of the concept must be explained. So far, in the sketch I have given, the pupils learning the word ‘pendulum’ have been shown examples and then (it seems) left to their own devices. Their future applications of the word would have

⁹Kuhn’s own discussion of the priority of ostensive definition over verbally formulated definitions and statements of natural regularities is to be found in his “Second Thoughts on Paradigms,” reprinted in *The Essential Tension*. The discussion occurs on pages 309–318.

to be guided by nothing but their subjective sense of similarity. Whatever seems to them to be a pendulum will be called a ‘pendulum.’ But ‘subjective standards’ are not really standards at all. Standards must be objective and external to the mind of the individual language user.

The answer to the problem of how to make provision for norms of correct usage, and hence make genuine reference possible, is implicit in the scenario of the teacher and pupil. The teacher provides the standards by correcting the pupil’s subsequent attempts to apply the word. Of course, teachers here only stand in as the representatives of the society whose language they are transmitting. By precept and example, step-by-step, the teacher shapes the sense of similarity needed to confront new cases and to confront them in a way that co-ordinates usage with that of other persons. When the teachers have finished their work the sources of correction will come from other users in the course of subsequent interaction. But whether corrections come from teachers or other users there are limits to what can be conveyed. No two pendulums will be identical. All that teachers, or anyone else, can do is to sustain a shared sense of what counts as being reasonably similar to the ‘paradigm cases’ used when the word is taught.¹⁰

It will be clear that the present discussion is now proceeding along similar lines to the discussion of measurement in Kuhn’s paper. Both deal with a form of ‘reasonable agreement.’ We can ask of the language teacher the question that Kuhn asked of the text-book writer: What is the criterion that is in play? By what criterion is the word ‘pendulum’ rightly applied? What are we speaking of when we speak of its rightness?

The answer that must be given is the one that Kuhn gave. There is no external criterion other than the authoritative practice of the community itself. The point can be conveyed by using the same words that Kuhn used, merely inserting ‘pendulum’ in the appropriate place. Thus: “I began by asking what characteristics the things called ‘pendulums’ must exhibit if they are to be said to ‘agree’ with the concept of pendulum. I now conclude that the only possible criterion is the mere fact that they appear, together with the word ‘pendulum,’ in a pro-

¹⁰I have used the expression ‘paradigm cases.’ This terminology was commonplace in philosophy before Kuhn introduced the word to refer to an exemplary scientific achievement. Oxford philosophers of the ordinary-language school developed what was called ‘the paradigm-case argument.’ The aim was to combat scepticism and relativism. If a sceptic doubted the existence of, say, free-will the response was to point to examples of behavior of the kind that could be used to define the concept of ‘acting freely.’ The claim was that the reality of free-will could not be doubted in any coherent way because these examples furnished the very meaning of the concept whose application was in question. Though not without interest, as an argument against relativism it is powerless. Different cultures can all employ the paradigm-case argument to suit their own ends, i.e. to introduce the concepts central to their own view of the world and (using the methods of Oxford philosophy) thereby ‘prove’ them to be unassailable. For criticisms of the paradigm-case argument see Ernest Gellner (1979, 52–59) and John Passmore (1961, 113ff.).

fessionally competent lesson for learning the language.” Following Kuhn, the conclusion must be that “the examples alone provide the definition of ‘reasonable similarity’ that has been accepted by the community.” Like Kuhn, when we try to locate the reality that embodies the crucial, normative component of discourse, we find “we have gone full circle.” As before, I want to locate the reason for this circularity in the role played by self-reference. The thing to which we are referring, that is, the ‘rightness’ of any act of concept application, resides (like ‘reasonable agreement’) in the totality of other references, implicit and explicit, to this rightness.

It is now possible to reconcile the self-reference of the words that apply to social reality with the external-reference of words such as ‘pendulum.’ The link between the two is this: External reference requires that the meaning of a word incorporates a normative component. That normative component is a social component and, given its social nature, it must be sustained by, and consist in, processes of a self-referential kind. The apparent problem of ‘reconciling’ these two things was therefore an illusion. In reality, the full requirements of external reference depend on mechanisms of self-reference and could have no existence without them. But before I can go on to draw the conclusion I want to, a little more needs to be said about the normative apparatus surrounding the word ‘pendulum.’¹¹

The discriminations that differentiate right from wrong applications of ‘pendulum’ are not static. They do not reside solely in the guidance given by teachers to children on their early encounters with this class of worldly object. Historically the concept of the pendulum changed as these objects became the focus of scientific interest. The law of the pendulum was first guessed and then refined, and the behavior of the pendulum became the subject of increasingly precise measurement. Here again the story picks up the account given by Kuhn in his references to the history of mechanics. The norms governing the correct application

¹¹I have argued that external reference depends on self-reference, but there is also dependence in the opposite direction, that is, self-reference depends on external-reference. If I call a disc a ‘coin’ because you call it a ‘coin,’ I must be able to respond to the physical phenomenon that constitutes the sound of the word ‘coin.’ But can both directions of dependency be real? Can this be possible without creating a problematic form of circularity? I think this apparent problem is no more than an artefact of the demands of exposition. It looks as if a temporal sequence is being identified where A must happen before B whilst also requiring that B must happen before A. In reality there is no such temporal sequence. It is not necessary to get certain self-referential processes up and running as a precondition for external reference while also demanding that external-reference is already in operation as a precondition of achieving the requisite degree of self-reference. The two forms of reference can and do arise alongside one another. Both are pre-figured in patterns of causal interaction and dependence i.e. the responsiveness of actors to the material environment and a simultaneous responsiveness of actors to one another. Both forms of responsiveness operate in unison and their interaction gives rise to the patterns that are later accorded the full, normatively informed, status of ‘reference to’ this or that aspect of the overall environment.

of the word ‘pendulum’ ceased to be confined to ones that could be followed on the basis of visual inspection alone. They come to include the accepted ways in which the object can be subject to mathematical analysis. Thus the correct application of ‘pendulum’ was now related to mechanical concepts such as ‘force’ and ‘acceleration’ as well as geometrical ideas such as ‘circle’ and ‘cycloid’ and, of course, engineering concepts such as ‘escapement.’ As Kuhn explains, the further elaboration of the concept also included an awareness of the all-important techniques of approximation that are applicable to it. There must be a shared understanding of the correct response to the ever-present discrepancies between the predicted behavior of an idealized, ‘mathematical’ pendulum, with its massless string, frictionless surroundings and constant temperature, and the real thing in the laboratory or the clock-smith’s workshop.

I have now filled out the process of ‘calling’ something a pendulum and have arrived at an account that does more justice to the pendulum as an object of high scientific significance. I have also shown that the route to that status was exactly the one set out by Kuhn in his paper on the function of measurement. The pendulum is now an object that calls for immensely sophisticated behavior on the part of those who surround it, orient to it, observe it, refer to it, adjust it, modify it, measure it, experiment on it, complicate it, and use it as a resource for understanding other phenomena. Now we have ‘the pendulum’ as a veritable institution. And this designation in no way compromises the material reality of the object at the center of all this co-ordinated and conventionalized social activity.

The Vacuity of Absolute Truth

It would be possible to go further in teasing out the sociological thread that runs through Kuhn’s argument in his paper about measurement. I have mainly concentrated on the theme of ‘reasonable agreement’ but further elements of self-reference can be identified which link what Kuhn said about reasonable agreement to: (i) the mechanisms which sustain the trade-offs that scientist must make between desirable but competing characteristics of a theory, for example simplicity and accuracy (p. 212), (ii) the patterns of relevance linking different phenomena, e.g. linking the inclined plane with free fall and trajectories (p. 115), (iii) the selective use of intellectual traditions, as shown by Galileo’s use, at various times, of both Aristotle and Archimedes (p. 215). In all these cases the community of scientists has to make collective choices and then sustain the choice as a convention. Without the capacity to sustain conventions, or to institutionalize preferences, scientific practice would fragment. The cognitive order of science would be replaced by subjectivism, individualism and cognitive anarchy.

Kuhn spoke of reasonable agreement, but a theory that ‘agrees’ with the facts is a theory that ‘corresponds’ with the facts. The table in the textbook, showing the reasonable agreement of theoretical predictions and experimental measurements, exhibits their ‘correspondence’ to one another. Kuhn’s analysis is therefore relevant to the famous correspondence theory of truth. In effect Kuhn posed the question of what ‘corresponding with the facts’ amounts to in the context of experimental science. It is illuminating to compare Kuhn’s position with that of Karl Popper, a determined advocate of the correspondence theory of truth.

Popper places great emphasis on the Tarski-style formula that ‘Snow is white’ is true if and only if snow is white, or, more generally, ‘P’ is true if and only if P. Popper says that this formula provides a valuable clarification of what is meant by correspondence with the truth. He believes that it captures common-sense intuitions about truth and furnishes a justification for the confident appeal to ‘correspondence’ in discussions about the methodology of science. In particular, Popper thinks that this approach provides a weapon against relativism because the Tarski formula is said to embody an ‘absolute’ theory of truth. In his intellectual autobiography Popper (1976, 141–143) said:

The correspondence theory of truth which Tarski rescued is a theory which regards truth as objective: as a property of theories, rather than an experience or belief or something subjective like that. It is also absolute, rather than relative to some set of assumptions (or beliefs)
[...] (Popper 1976, 143)

Suppose that, for purposes of comparing Popper and Kuhn, the Tarski-style correspondence formula ‘P’ is true if and only if P, is re-cast into the form of Kuhn’s text-book table. The table, which might be called a Tarski table, would look like table 17.2.

Predicted Value	Measured Value
n_1	n_1
n_2	n_2
n_3	n_3
.	.
.	.
.	.

Table 17.2: A Tarski table.

Just as the symbol ‘P’ occurs twice in the Tarski formula, so each symbol ‘ n_i ’ ($i = 1, 2, 3$, etc) occurs twice in the table, once as a predicted value and once as a measured value. Thus the predicted values of the quantity n_i and the measured values of n_i will be identical.

What are we to make of the Tarski table? For example, could it be used to perform the function that Kuhn identified when he reflected on the real tables found in real textbooks and research papers? The answer is: No. As Kuhn explained, the real table was of value because it conveyed information *both* about nature *and* about the scientific community. A Tarski table cannot be used in this way. In particular, it cannot be used to convey what the relevant scientific community count as reasonable agreement. The real table could be used in this way precisely because the numbers in the left and right-hand columns were *not* the same. Unlike the real tables, the Tarski table is doomed to be an idle cog-wheel in the machinery of science.

Popper sometimes speaks of truth as a ‘regulative ideal.’ Could it be said in defence of Popper’s position that the Tarski table functions as a regulative ideal? Perhaps the table can be understood as giving the scientist a goal towards which to work. Kuhn would certainly agree that the column of predicted values, on the left-hand side of the table, constitutes a goal of some kind. He identified theoretical predictions playing such a role when he described how scientists used a theory to guide them as they refined their instruments and techniques. It was in this connection that he spoke of self-fulfilling prophecies. But to use the left-hand column as an ideal is not the same as using the table as such, that is, both columns, as some sort of goal. Indeed, Kuhn had some caustic comments to make about results that are too close to the predicted values, that is, tables of results which begin to look like the Tarski table. He noted that, “at least on a student lab report overly close agreement is usually taken as presumptive evidence of data manipulation” (p. 184). And the same applies to suspiciously good research results published in scientific journals. One could therefore invert Popper’s argument, about absolute agreement as a regulative ideal, and present the Tarski table as a symbol of shameless and extravagant fraud.

Popper was equally off-target, in the remarks quoted above, when he said that Tarski’s analysis lent support to the idea that truth was a property of theories and could be abstracted from the ‘subjective’ assumptions and beliefs of those who employ the theory. It is right to demand objectivity but, where Popper sought objectivity in the Tarski table, Kuhn sought it in the collective use of real tables. Kuhn knew that the theory and the table (the real table) must be linked—just as the examples in an ostensive definition must be linked to the agreed use of the term in a social group. In both cases those links embody and assert the normative

dimension, a dimension that can only exist in the form of a social institution. As Kuhn put it:

Without the tables, the theory would be essentially incomplete.
With respect to measurement, it would not be so much untested as
untestable. (p. 186)

The theory would be untestable because there would be no conventions about how close the experimental results had to be to the predictions in order to be deemed to ‘correspond.’ In the absence of these conventions there could be no objectivity in the assertion that theory and experiment ‘correspond’ or ‘do not correspond’ with one another. Nothing in reality will ever exemplify the absolute identity of the Tarski table and the table itself provides no guide to action in the real world outside the ideal table. Equipped with Kuhn’s argument we thus reach a striking result. Despite all his emphasis on testability and objectivity, Popper failed to detect the real-world preconditions of testability and mistook their presence for mere ‘subjectivity.’

Summary and Conclusion

I began with an account of social reality as something created through a process of self-reference. It will be evident that Kuhn’s most famous analytical concept, the paradigm, has this character. An achievement becomes a paradigm in virtue of being regarded as a paradigm, treated as a paradigm and known as a paradigm (though the word ‘paradigm,’ as such, may not feature in the process). What may be less evident is that these forms of interaction are also at work on other levels of Kuhn’s analysis. The significance of the paper on measurement is that the same self-referential and self-reinforcing processes that sustain a paradigm can be detected in the detailed construction of the standards of reasonable agreement between theory and experiment. Kuhn’s message was that this vital relation cannot be understood without bringing in the scientific community and he traced the self-referential character of that process.

I have tried to make Kuhn’s sociological insights explicit and use them to understand some of the puzzling things that he said about scientific revolutions, for example, that after a revolution scientists work in a different world and that new objects, such as the pendulum, appear to spring into existence. I have also tried to show that Kuhn’s analysis of the self-reference involved in scientific measurement has important implications for the correspondence theory of truth. His account of the relation of ‘reasonable agreement’ between fact and theory exposes the embarrassing lack of *empirical* curiosity that so often disfigures the

appeal to the correspondence theory of truth when it is used as a weapon against relativism.¹²

In regard to historiography, my aim has been to expose the intimate connection between the history of science, at its best, and the sociological analysis of knowledge. This intimacy is exemplified by Kuhn's work—but so is something else, namely, the difficulty of perceiving and acknowledging that bond. Kuhn succeeded in one of these respects and failed in the other. He displayed a strange mixture of awareness and lack of awareness. If an historian of science of Kuhn's calibre can make this mistake, one can only wonder what will happen when the history of the history of science comes to be written.¹³

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¹²Lack of empirical curiosity about the actual attribution of truth, in favor of formal specifications and definitions, is not confined to Popper and Tarski. These defects are to be detected in the work of many other philosophers. For example, John Searle also uses a form of correspondence theory to justify the rejection of relativism. Like Popper, Searle ignores the irreducible contingencies of human judgment that surround every act of concept application and treats the relation of ‘satisfaction,’ holding between a concept and the material world, as wholly dependent on the world. Once meanings have been ‘fixed’ by a definition, says Searle, there is nothing relative about it. Searle's theory is to be found in Searle (1995). For a criticism of Searle and a range of other anti-relativist positions see Bloor (2011, 444–446).

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