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PRACTICE RELATIVISM

STEPHEN P. TURNER
Department of Philosophy
University of South Florida
turner@shell.cas.usf.edu

SUMMARY: Practice relativism is the idea that practices are foundational for bodies of activity and thought, and differ from one another in ways that lead those who constitute the world in terms of them to incommensurable or conflicting conclusions. It is true that practices are not criticizable in any simple way because they are largely tacit and inaccessible. But to make them relativistic one needs an added claim: that practices are "normative", or conceptual in character. It is argued that this is not supportable by any explanatory necessity, and that the differences in outcomes, though real, are not instances of relativism.

KEY WORDS: non-conceptual knowledge, Kelsen, normativity, naturalistic explanation, tacit knowledge

RESUMEN: El relativismo de prácticas es la idea de que las prácticas sirven de fundamento para cuerpos de actividad y pensamiento, y que difieren unas de otras de maneras que llevan a aquellos que constituyen el mundo en esos términos a conclusiones inconmensurables o contradictorias. Es cierto que las prácticas no son criticables de ninguna manera simple porque en general son tácitas e inaccesibles. Pero para que sean relativistas hace falta una tesis adicional: que las prácticas son normativas o conceptuales. Aquí se arguye que esto no puede sustentarse en ninguna necesidad explicativa, y que las diferencias en resultados, si bien son reales, no son casos de relativismo.

PALABRAS CLAVE: conocimiento no conceptual, Kelsen, normatividad, explicación naturalista, conocimiento tácito

Relativism is an elusive notion. There are, one might say, different kinds of relativism, such as historical relativism, cultural relativism, the kind of relativism that exists between alternative fundamental theories of the world or paradigms in science, and the kind of relativism that exists between professional schemes of concepts that constitute the objects on which the professions operate in distinctive and incommensurable or not fully commensurable ways. There is also linguistic relativism, the relativism that holds between languages and results from the indeterminacy of translations between claims in different languages, and relativism of class, race, and gender, which operate with a base-superstructure or Überbau model. Cognitive, aesthetic, ethical, and other kinds of relativism have been seen as the consequence of hidden changes that produce new values or illuminate new values in successive historical periods. We are, in this picture, stuck with our historical starting point in terms of which we construct a world view and constitute objects in a moral and cultural universe that is distinctive and unlike, though it may be historically connected to, past moral and cultural universes constituted by past historical personages. It was of course Thomas Kuhn who, with the concept of paradigm, extended this picture to science.

In this paper I examine the notion of what I will call "practice relativism" as a distinct and analyzable form of relativism, but with a particular question in mind: what makes a practice conception relativistic? The question is meaningful because there are a variety of concepts of practice, and a variety that are relevant to science, that are not relativistic in any problematic way. So there is a question of what ingredient makes a practice concept relativistic in its implications.

1. *What Are Practices?*

The concept of practices is itself, in part at least, a kind of naturalistic or "sociological" notion. It usually figures in some sort of contrast for example between theory and practice or between knowledge and practice. The term refers to something that is supposed or treated as a fact with explanatory significance. A practice is something one engages in, in many cases at least quite consciously and intentionally. The primary use of the term is, however, descriptive. A practice is something that people engage in in the course of intentional conduct or action that is not itself intended, but is a condition of the conduct. The term describes or picks out the distinctive and usually repeated features of a way of doing things. Thus if we were to talk about the practice of mathematical physics in the nineteenth century in Cambridge, one might ask questions about the distinctive mathematical training that the wrangler competition produced, and show how this was a condition for the distinctive kinds of explicit, intended, theoretical results that persons with this particular mathematical training received, as Andrew Warwick does in his recent book, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (2003).

The term practice itself, however, is not well defined. One commonplace use of the term is found in the history of science, where the contrast is between theory, and what is in textbooks and manuals, and practice. One formulation of this usage, recently exemplified by John Pickstone (2001), equates practice to what anthropologists call material culture —practice referring literally to the tools that scientists use. This usage has some "relativistic" implications, but they are unproblematic ones. What scientists can discover is in part determined by the tools they have. So in some sense the facts accessible to them, which is to say their world of data, is determined by the tools: different tools would mean different facts, and perhaps theories which fit or seemed plausible in the world defined by one set of tools would not in a world defined by another set. The microscope, to take a simple case, disclosed a world which changed utterly the subject matter of biology, and therefore the range of possible biological ideas. But there is no conflict in "rationality" between pre- and post-microscope biology: with the tool in hand, one would think the old biologists would have come around to the viewpoint of the microscope-possessing biologists. The situation is no different from cases involving differences in the data that scientists possess at different times, and practice in this sense is literally part of the data, the given.

Pickstone is a traditionalist, indeed, historiographically, a kind of pre-Kuhnian. He does not use the language of constructionism, which would describe the tools themselves in relativistic terms, that is, as relative to the construction that has been placed on the tools, or in terms conceptually linked to theories which might themselves be incommensurable or otherwise "relativistic". Instead he describes the tools functionally —thus, for him, practice is more or less made up of objects with functions. He is concerned with a particular kind of problem about practice and knowledge, the question of the relations between the two, and he thinks that there are historically meaningful differences. He concedes that there is a kind of knowledge associated with the tools —craft skills and knowledge of material and substances. But he wants to distinguish this kind of knowledge, which is largely autonomous, from "science" (and in many of the crucial historical cases knowledge held by craftsmen rather than the scientists who benefit from the tools) from scientific knowledge proper. For Pickstone a practice is thus an activity, intentionally engaged in, with particular tools, of which the paradigm case is a craft with associated skills. There is a practical issue in history to which this relates —we have material artifacts of past science, or descriptions of them. We do not have any sort of historical access to the craft skills or minds of the craftsmen themselves, and of course the same holds for the Cambridge wranglers. We can use the archives to illuminate the coaching system, the cribs, the repeated features of explicit proofs, and so forth, but we cannot directly access the stuff in their heads.

This naive approach, which reflects the constraints on the historian of practice, is congruent with a more sophisticated version of practice theory applied to science developed by Andrew Pickering (1995), who defines practices as the assemblages of material things that are the conditions for doing science. Like Pickstone, for Pickering the objects themselves are, so to speak, inert. They do not have in themselves any sort of directionality or teleology. They are not symbols. Any meaning they have is attributed to them by their users, and the attributions and therefore the uses can change —indeed this, the novel uses of equipment, is a large part of experimental science. A scientist in possession of, or with access to, this assemblage can take it in any direction consistent with the assemblage itself. Here, as with Pickstone, theory and the substantive contents of science are not part of the concept of practice, but are a separate domain conditioned —causally— on the assemblage and the data it generates. There is a wide range of contingency in the process, and of course what happens at one stage of the process has consequences, though not determinate ones, for what happens next. The assemblage has functional relations, that is to say, there are things that one can do with a microscope and a staining solution that one cannot do with a theodolite and a staining solution. There is nothing holistic about these relations, however, except in the sense that the set of functional possibilities is limited at any given moment. One can add pieces, such as new tools or instruments, with new possible relations, and change the assemblage.

This is a relativistic conception of practice in one sense —it accepts the contingent character of the historical sequences that produced present "scientific knowledge" (cf. Turner 2002, chap. 3). But it is difficult to imagine a serious account of the historical development of science where there was not, for a finite historical period, some explanatory need for the recognition of contingency. The grounds we have for thinking that we could have come to other scientific beliefs are rooted in the recognition of actual historical contingencies, not in some sort of in-principle recognition that there is a grounding element of science that is in principle arbitrary and understood in a virtual sense as a matter of non-rational "choice". It is this second sense that I will take to be "relativistic" in what follows here. So my question will be "What does it take for a practice conception to be 'relativistic' in this strong sense?"

2. *What Do Practice Explanations Explain?*

It becomes evident in connection with these notions of practice that one way in which concepts of practice vary is in terms of their explanatory object, that is, with respect to the questions they are designed to answer. Pickering is interested in the following problem: how do modes of doing science evolve and change in the face of the resistances they encounter in the course of their continued application to new issues and problems? Pickstone is interested in a different question. How are the possibilities of knowledge in science and technology, including its ways of thinking, limited and created by the particularities of available technologies? How, for example, are the content and ways of thinking of a geologist in the nineteenth century related to the fact that geology was done with pickaxes and theodolites? These are both quite different questions than "how is it possible for there to be consensus in science?" or "how is it possible for scientists to resolve their disagreements by reference to experiment?" which have been attributed to Kuhn or expressed by him. And these are different yet from questions like "how is communication possible" or "how is semantic reference to the empirical world possible?" There is also an autonomous question about practice that falls in the domain of social theory that relates to the nature of practice as a natural object: what is the character of the sorts of things that, in the tradition of social theory, have been called "practices" or "traditions" (or mores, and so forth) and invoked as explanations of social phenomenon? Are they one kind of thing, or many, and if many, how do they relate and what differentiates them? One can identify many more such explanatory objects or problems.

What is the relation between these questions and between the concepts of practice that answer them, or, put differently, what kinds of answers does one get? There is an important provisional division to be made between two kinds of answers. One kind treats practice as a gap-filling notion, which has no substantive significance other than to refer to those conditions for the thing to be explained —whether it is communication, consensus, or the course of development in science— that are not part of the explicit content. In the case of consensus, the explicit content would be the publically stated reasons and claims that went into scientific discussion. The "practices" are everything else that is part of science, the mental life of the scientists, and so forth, that make discussion leading to consensus possible. This might include the kinds of habits of mind that are inculcated by training in science, the tacit knowledge about the way the physical or biological world works that enables scientists to make discoveries and judge claims in ways that are intelligible to other scientists, and so on. But these things are invoked, or for that matter exist, not as an accessible, natural realm of autonomous facts, but as facts which function only in relation to particular explanations, so that there is no point to theorizing them as autonomous objects. "Practice", in this usage, is relative to particular, transient, explanatory situations, and its content is relative to the particular thing to be explained. When Robert Brandom (1994), for example, argues that the process of making inferential practices explicit can go on indefinitely, he is invoking this sense of practice. Different explanatory objects would require different explanatory gaps to be filled, so what counts as practices is relative to explanatory aims. From this point of view, it would be no surprise if there was nothing in common between the appeal to practices in connection with the question of whether a given constitutional form could work in a country without a political tradition of a certain kind —such as a political tradition that separated religion and politics— and an appeal to practices in connection with the question of how language is possible, or how semantic reference to the world is possible.

But there is a problem with this view of practices. In the first place, "practices" is not merely a gap-filling notion in its established uses. In the second place, it is difficult, and perhaps impossible, to consistently carry through a pure "gap-filling" conception of practices.[1] Such "practice" family terms as "tradition" have a meaning apart from these particular episodic uses, and it seems, in these cases, that there is a reasonable, autonomous question in social theory (and perhaps philosophy) as to what such things are, and indeed there are available theories or accounts of tradition, practices, and the like both in the traditions of philosophy and social theory, associated with such figures as Gadamer, Alasdair MacIntyre, and Michael Oakeshott. In each of these cases, the theory of practice or tradition distinguishes between tradition and practices and philosophy, but at the same time insists on the dependence of philosophy on tradition. Practice is thus in these cases something beyond philosophy that cannot be fully comprehended within it or replaced by it, and this implies that it is, so to speak, something substantive, something in the world and therefore governed in part by considerations of causation. Oakeshott points to one of these considerations when he argues that one of the identifying features of practices is the fact that practices must be learned. This amounts to an acknowledgment that practices are part of the natural world, and that the causal processes of learning constrain our use of the term, even if practice cannot (as indeed it cannot, for Oakeshott) be reduced to these causal processes.

[1] Perhaps Joseph Rouse (2002), to the extent that it is an account of practices, could be construed as such an attempt.

Oakeshott's account contrasts to Pickering's —for Pickering the assemblage of objects that makes up practice has no intellectual content— but not to Pickstone's, since Pickstone's extended conception includes skills. But Pickering's conception is unusual enough to leave aside for the moment as an interesting anomaly. Learning does seem to be a denominator common to the other available theories of practice, or a condition of what they take to define practices. Brandom's notion of accountability, for example, depends on learning something —how to recognize what sorts of things one is made accountable for by one's actions and what makes others accountable, and although Brandom does not problematize the problem of acquisition (and indeed, as we will see, has a major problem with it), the account he offers depends on the content being acquired, on something being learned or mastered, and these processes occur, indubitably, in the world of cause.

Oakeshott is the practice theorist who deals most fully with the philosophical issues here, so it is instructive to see how he proceeds. He recognizes that there is a terminology problem —that we identify and characterize practices from within the practices, through reflection. The language of description relevant to acquisition is the language of the psychology of learning. There is a gap between these languages, such that there is no way to precisely characterize practice in the language of psychology, or learning in the language of practice. Thus in our conception (or identification, as he puts it, carefully avoiding the traps of the language of conceptualism) of traditions or practices and in our application of these concepts "there is always a mystery about when it has been acquired". I will return to this later, but at the moment let me simply note the similarity between claims like this and the slogan of Michael Polanyi, "we know more than we can say". Practice theory ordinarily has proceeded by invoking the notion that there is something "non" —non-conceptual, non-theoretical, non-explicit, or in the words of Gilbert Ryle a kind of inarticulate "knowing how" rather than an articulate "knowing that," that lies outside, or beyond, the explicit.

One place where theories of practice divide is over the way of thinking about this "non" stuff. One approach is to conceive of it on the model of the articulable, the conscious, the intentional, the normative, and so on. The other is to deny that it can be usefully conceived in this way, and to reject the analogizing implicit in the former approach, an analogizing that plays a large role in the history of the philosophy of science, in the form of such notions as framework, spectacles behind the eyes, and in contemporary philosophy in such forms as Brandom's language of commitment and score-keeping as applied to the pre-linguistic situation of the language learner. The difference between these approaches and its connection to relativism is the subject of what follows, though it is a connection that will not become clear until the end of the paper.

3. *Where Does Relativism Come In?*

The concept of framework is a good place to begin with the problem of understanding how the concept of practice becomes relativistic. The issue with conceptions of practice like Pickering's and Pickstone's is explanatory sufficiency. Do these conceptions enable us to explain what we want to explain? The usual complaint about these views of practice is that they do not. But there is a trick aspect to the question which is aggravated by a trick character in the notion of "explanation" as it applies to these cases. The problem can be put very simply. Pickering and Pickstone are concerned with more or less straightforward substantive biographical and historical facts. In Pickering's case, for example, the physicist Giacomo Morpugo's theoretical development or the expansion and standardization of operations research —that ordinary historiography cannot adequately handle, but which are in no sense questions that ordinary historians of science would not like to answer. The explanatory relations are typically also straightforward. The functional relations between parts of the ensemble do the explanatory work by specifying possible configurations and relations —asking what can be done with theodolites and pickaxes, for example. The explanatory objects of thinkers with richer practice conceptions are characteristically different and more abstract.

Rouse at one point in his recent book quotes Kuhn's comment that he intended not to explain scientific consensus but to explain how experiments enabled scientists to reach consensus (2002). This is a different kind of explanatory object —consensus and the conditions for the possibility of consensus are "facts" that are both abstract and collective. And there is a temptation to think that the explanations of these facts are similar in kind —collective and abstract. This is the same kind of thinking behind the idea of collective intentionality: because norms are collective, it seems that a collective fact of some sort needs to do the explaining. Much of this literature is devoted to the problem of getting such an explanation within the limits of an ontological individualism (e.g. Searle 1995, Sellars 1963, Gilbert 1990, 1996). These kinds of collective-collective explanations contrast with a class of competing explanations in which the aggregate or collective facts are accounted for by invisible hand processes, or in which supposedly collective facts are shown not to be genuinely "collective" but merely to be aggregate level descriptions of joint individual processes or facts.

Practice explanations can be either individual or collective. Michael Polanyi, who modeled his conception of knowledge as "personal knowledge" on the astronomer's "personal coefficient", had an individual conception. Science for him was an apostolic succession, in which masters trained successors; however, what the successors got was not the thing the master had, but the capacity to discover on their own, and in their own way. Discovery was individual. The process of ratification of discovery (on which he placed little stress) was public and in this sense collective, but it was only apparently so —what made science work was the interdependence of areas of science that checked one another rather than any moment in which "the scientific community" reached "consensus" which would have been a genuine collective fact.[2] For Polanyi, knowledge itself was "personal". Kuhn, to the extent that he can be considered a practice theorist, and paradigm as a practice concept, had a thoroughly collective picture of practices. Paradigms are "accepted examples of actual scientific practice which include law, theory, application and instrumentation together" (Kuhn 1970, p. 10) and their presence explained the collective capacity of scientists to come to agreement on the facts.

Sorting through the confusions over "paradigm" is not my purpose here, but suffice to say that Kuhn can be taken as giving a model collective-collective explanation, in which a collective fact about consensus is accounted for by a collective fact about paradigms. In this model the problem of ontological individualism is avoided by a device similar to one found in the collective intentionality literature, of treating the scientific community not as a mystical supra-individual entity which has such properties as a capacity for embodying a consensus but as a composite of individuals who "share" a paradigm and share scientific opinions. In *The Social Theory of Practices* (1994) I argued that this notion of sharing was, contrary to appearances, just as problematic as the notions of supra-individual entities it replaced, an argument I will not repeat here.[3]

[2] For an example of Polanyi's deflationary approach to consensus and ratification, see *Meaning* (Polanyi and Prosch 1975, p. 145).

[3] The argument, very briefly, was that the causal character of learning precluded the possibility of "sharing" in the sense required by practice theory (1994).

Polanyi was a fallibilist and thought that issues in science could go without being settled for a long time, but that in fact they were eventually settled, though there was no principle that assured that they would be. He used the same examples as Karl Popper, Marxism and Freudianism, of circular and closed, and therefore non-scientific, intellectual traditions (1962, p. 288), and one suspects that if he were to have considered Kuhnian paradigms, which are circular and closed, he would have simply observed that there are often holdouts in science —people unpersuaded by the dominant theories— but that elevating this fact to claims about incommensurability and the irrationality of paradigm choice travestied the history of science. Polanyi didn't need a collective-collective explanation, because he could identify processes that produced, through various invisible hand processes, the appearance of consensus that corresponds to the historical actualities of science. But Polanyi was not, by his own lights, a relativist, and insisted on a notion of scientific truth as the goal of the tradition of science, while rejecting the idea that it could be a goal external to the tradition of science against which science could be judged.

Kuhn can be treated as a relativist. He denied that the way in which issues in science were actually settled, in conflicts between paradigms, was rational, precisely because what counted as a fact or as a good reason was paradigm relative, and the choice between paradigms could thus not be made on rational grounds. Paradigms function like ungroundable premises for arguments, and to choose between them was to accept an ungroundable premise. They function this way because, for Kuhn, scientific practice is theory-laden or paradigm-laden through and through. The effect of this reasoning is to place practice firmly inside the "theory" side of the theory-data divide. Relativism follows naturally from this step, because being theory-like means proceeding from premises or assumptions that cannot be grounded, or, alternatively, tested, without circularity. The historical "fact" of alternatives implies that the alternatives cannot rationally be decided between, because the premises can't be grounded rationally, and thus must be a matter of choice or decision.

All this is familiar enough. What is more difficult is to identify the characteristic form of relativistic arguments in order to see how relativism arises as a problem. We can begin with the explanatory objects. The usual object is some sort of difference or diversity of viewpoints that cannot be accounted for in other ways. Needless to say, this "cannot be accounted for" is a problematic notion. Particular descriptions of differences may have the explanations of the differences built into them —thus a description of two groups of scientists having different paradigms, for example, *de facto* excludes any explanation of the differences that does not already appeal to the possession of paradigms. This dependence on description can be much more subtle, however. It is commonly thought that possessing a concept might be an object of explanation. But if this is treated as the object of explanation, and to the notion of possession of a concept we add some commonplace ideas about "concepts", the claim can be taken to imply something very elaborate. For example, if we say that concepts are normative, we find ourselves committed to the existence of a collective fact —normativity— which may seem to require an explanation with particular properties —for example that it can account for the collective, normative character of concepts— we will find ourselves forced to deal with the claim that normativity requires sharing and in particular sharing of the kind of collective intentionality that can produce normativity. Since a great many of the conventional descriptors of facts about science have these implications, at least in conjunction with standard claims in other areas of philosophy, taking these descriptions as having anything other than a provisional status narrows the range of possible alternative explanations. One response to this is to reject the descriptors and substitute others, as Pickering does, and as Quine did with respect to concepts by being "behaviorist", or to ignore them and style one's own problem in one's own terms, as Oakeshott and Polanyi did. The usual response to this is *tu quoque* arguments to the effect that one cannot avoid descriptions in terms of, for example, concepts and therefore normativity, an argument to which, again, I will return.

The problem of description is so closely bound up with the problem of explanation that "explanations" themselves may be little more than restatements of the descriptions. When Kuhn describes a particular historical situation in terms of the paradigms in conflict, for example, we appear to start out with normal historical facts —there is disagreement, generational conflict, and difference of opinion over key terms and over the significance of particular facts. But the evidence that establishes that there are paradigms, or that there *is* radical conceptual change, is the same as the evidence for the explanation. This raises the question of what it is to "account for" something in this model. The other forms of relativism with which I began —the *Überbau* model and racial relativism, for example, involve some sort of causal or at least constraint model of the relationship between base and superstructure. Here the relationship is something different, perhaps a kind of constitutive relationship.

The explanatory character of this relation is nevertheless quite puzzling. Elsewhere I have called these quasi-transcendental arguments, because they are generated from considerations of conditions of possibility. The conditionality, however, is largely definitional. Nevertheless, the argument is treated as though it has established a causal fact about the world. The peculiar character of this structure is obscured by the fact that the way in which paradigms, and for that matter practices, are usually understood, in these contexts, is generative, that is as an underlying source of many often disparate manifestations. Put differently, the explanation is one in which visible manifestations are understood to be the part or product of a constitutive whole which is not fully visible, but is nevertheless necessarily there. But the manifestations are themselves reconstructed into a unified fact, producing a kind of circularity. These should be grounds for questioning the claim that the thing —paradigm, practice, or culture— exists as a unified fact producing the manifestations at all.

Where this model turns relativistic can best be understood by seeing how it differs from the base-superstructure model. The base-superstructure model is relativistic because there is an external relativizing element in the explanation —class, race, and the like. The reason for the non-comparability of the manifestations is the non-comparability of the base. The typical resolution of the problem of relativism comes through a non-relativistic ranking of the base —the world view of the final class in history is thus the best view, or the view of the supreme race is the best view. How one can get a world view-free ranking of these bases is a question that needs to be answered by other means, perhaps, but in these cases the question is blocked by the idea that race and class are second-order facts, whose truth is determined in a different way than the facts that are manifested in the world view. Even though the bourgeoisie might currently be incapable of recognizing their impending historical demise, once it happens, even they will recognize it; a master-race, similarly, is not merely a matter of race theory, it requires objective mastery.

In the case of paradigms and practices there is nothing external to rank because there is no external explainer. But there is, typically, some sort of relativizing element. To understand how this works, it is perhaps simplest to start with an influential analogous case, Hans Kelsen's theory of law. Kelsen was an anti-naturalist who was concerned with the question of what makes law binding. There are, of course, different systems of law, binding on different people. So the question of the binding (or normative) character of law is a question of why *this* set of laws is binding on *these* people. Referring to the law itself —the law's own statements about its binding character— doesn't help with the question, because law cannot answer, without circularity or regress, the question of why these statements are themselves binding. Moreover, law in fact undergoes revolutionary change. Laws and systems of law cease to be binding, and new systems of law are proclaimed as binding, raising the question of what makes each of these divergent, successive, non-comparable legal systems binding.

Kelsen's general answer to this is to identify an element, which he calls the *Grundnorm*, which is simultaneously an answer to the question of what makes a legal system binding and an explanation of the distinctive binding character of the particular legal system. The notion thus has a normative role —it validates the system as law— and an explanatory one —it "accounts" for the binding character of the law, which is taken to be the explanatory object of legal theory. The *Grundnorm* is a theoretical object. Its existence —which more naturalistic legal theory denied— was claimed to result from a necessity —which naturalistic theories either failed respond to or rejected— to explain the binding character of the law. The explanation was theoretical, since *Grundnormen* are theoretical entities not to be found or identified with actual laws. But they were by definition capable of being both justificatory and explanatory. Moreover, they have the power to produce normativity.[4] They are *Grund* because they are the final answer to the question of what makes law genuine law. They are of interest here because they also contain what I am calling relativizing elements: they "account" for the fact of difference in legal systems, something that was done naturalistically in the writings of the dominant figure in legal philosophy prior to Kelsen, Rudolph von Jhering, whose account of law in *Zweck im Recht* (1877) was evolutionary, and which Kelsen supplanted.

What is striking about this "explanation" is its peculiar combination of natural and normative. Rather than accounting for revolutionary change in the law "sociologically", it defines it in terms of change in the *Grundnorm*. But the idea that this hypothesized thing, the *Grundnorm*, "explains" is questionable. In the first place, there is no evidence for bindingness other than the fact of acceptance, the fact that people treat the law as binding. So the naturalistic part of the "fact" that naturalism about the law allegedly can't explain is nothing

[4] One difficulty with this theoretical object, which Kelsen acknowledged at the end of his career, is that they cannot be both true and normative themselves, because this would beg the question and refer it back to a regress —the question of what grounds their normativity. Kelsen was thus led to regard the Grundnorm as a fiction in the strict sense of being false and impossible.

more than a fact made up and described in such a way that naturalism cannot "explain" it. Nothing new about ordinary historical facts is explained by the concept of a *Grundnorm*. It is strictly a concept necessitated in a quasi-transcendental way by the supposed normative fact of bindingness and the necessity to "account" for the multiplicity of binding legal systems. Even the claim that it "accounts" for bindingness is difficult to give much sense to: it accounts because it is asserted to account for it. There is no special evidence relative to this "accounting" and the relation is not causal (even though the *Grundnorm* would have to be a genuine historical fact with the consequence of producing bindingness) but constitutive. In this context, however, to say it is constitutive —constitutive of the bindingness of the law perhaps— is to say nothing, for the explanatory work here is being done not by some autonomously discoverable or reconstructable constitutive structure. The sole job of the *Grundnorm* is to bless the legal system as binding. So the grounds for thinking there is such a thing is the characterization of the problem of understanding the law as a problem of understanding "real" bindingness.

What one has, in short, is a concept with no empirical credentials, no explanatory value outside of this one role, which is accessible only through a kind of transcendental argument that cannot plausibly support a claim that it has causal significance, yet at the same time it has powerful consequences: it establishes or grounds claims to validity, and it does so on its own ground; i.e., it is not grounded in some further concept that tells one whether it is a valid conception of law, such as Natural Law theory attempted to provide, and it is arbitrary, thus producing relativism. It should also be noted that there is a mythical aspect to it. Of course, there is no actual historical phenomenon corresponding to creation of legality *ex nihilo*. There are revolutions, which establish accepted regimes, but acceptance is precisely the kind of "sociological" facticity that is claimed to be insufficient to "explain" legality as a normative phenomenon. Legality has to be retrospectively conferred on revolutionary regimes, in a process that has no empirical counterpart. It should be observed that there are parallels to this peculiar explanatory structure of norm-creating facts throughout the philosophical literature on normativity. For example, in the notion of collective intentionality in Sellars (1963) and Searle (1995), in H.L.A. Hart's oxymoronic surrogate for the *Grundnorm*, the notion of "authoritative reason" (cf. Postema 1987, p. 86) and I would argue, Brandom's use of (or as he would say, helping himself to) the pre-linguistic notion of commitments as an explanation of the normativity of rules (1994). Hart's account, described here by Postema, is this:

> The key to explaining the distinctive normativity of law, while preserving the conceptual separation of law and morality, Hart now insists, lies in recognizing a special kind of normative attitude characteristic of self-identified participants in legal practices, namely the standing recognition of, or willingness to regard, certain events or states of affairs as constituting "peremptory" and "content-independent" reasons for action. Where such an attitude is widely adopted, the occurrence of events will have not only natural but *normative* consequences —certain actions will be made right or obligatory, others wrong or offences, by those events. (Postema 1987, p. 86)

How does an attitude, which is a psychological fact, make an event have normative consequences as well as causal consequences? Adding adjectival qualifiers like "normative" or "legal" to one's description of the attitude doesn't help either. The only fact here is "sociological".

The constitutive version of the concept of paradigm has the same problems. The grounding nature of paradigms purportedly establishes forms of validation. What it explains is the "validity", internal to paradigms, as well as "reality" as it appears internally within paradigms. Whether Kuhn says this or not, it is a plausible extension of what Kuhn does say. The provision of an "account" of validity is what distinguishes Kuhn from a sociology of science, which would presumably limit itself to explaining acceptance, i.e. to explaining what scientists believe, or their attitudes. Sociologists would do so in terms of their beliefs about validity or attitudes rather than validity itself. Kuhn, however, tells us what validity in science is and can be, just as Kelsen tells us what legality is and can be. The fictional character of "grounding" in Kelsen —"fictional" because there is no act of grounding— is hidden by Kuhn in the fictive descriptions of radical change in conceptual schemes in science. Transcendental necessity results from narrative necessity: since there needs to be something to ground validity and the something must change, it must historically be the case that they do change in this way. One can give many other examples of this kind of fictionalizing in other cases of relativism, such as Margaret Mead's cultural relativist image of cultures selecting their values from the bin of possible human values (1928, p. 13). This fiction serves the same purpose —to make the moment of arbitrariness in decision seem both plausible and necessary to understanding. But there is no empirical reason to accept these descriptions as the sole adequate descriptions, and thus no necessity to the conclusion.

Kuhn is not as explicit as Kelsen in identifying the added element that makes a paradigm into a relativizing machine, but the material he works with is different. Kelsen had explicit law, which was insufficient on its own to account for its own lawfulness. Kuhn had textbook science, which was insufficient on its own to account for its own history. "Paradigm" was a way of talking about the added element which simultaneously "accounted" for the relevant features of science, namely its conceptual diversity in history, and confirmed the relativistic character of this diversity. But it was a concept that, unlike *Grundnormen*, did more than simply add a normative element. Instead, it hid the normativizing element among the many meanings of "paradigm" (if indeed it is there). Arguably, this was at the cost of coherence, as Margaret Masterman's classic dissection of the multiplicity of meanings of the term showed (1970; cf. Kuhn 1977, pp. 294–295).

4. *The Theory-Data Distinction*

Perhaps a small historical point is in order here. Kuhn cribbed most of the ideas of *Structure* from his mentor, James Bryant Conant, who was himself a promoter of the Harvard commonplace of the importance of "conceptual schemes", which outlived their usefulness and were discarded, and of the idea of radical conceptual change as a recurrent phenomenon in the history of science. But with respect to certain crucial issues, Kuhn and Conant diverged, and these issues happen to be relevant here. Conant thought of the theory-empirical relation as a continuum, and thought that scientific theories or the state of scientific knowledge in a given field could be theoretical to a greater or lesser degree. He identified progress with reducing the empirical elements —the more theoretical, the better the knowledge. Linnean botany, for example, is observational and empirical, but not very theoretical. Advance in this science would come, if it could come, in the form of making it more theoretical. He also thought that science was continuous with common sense and in this respect non-autonomous. This meant that he denied the "local holism" of Kuhn's concept of paradigm, which he disliked. But as it happens he denied it in a specific way that relates to practice.

Conant argued, consistent with this version of the theory-empirical continuum, that even in the course of radical conceptual change in science, there was considerable continuity in instrumentation and observations. In the sense of practice with which we began, there was thus no "radical change" with respect to large elements of practice and with respect to those parts of science toward the empirical end of the continuum. Kuhn's revision of this incorporated these elements of practice into his holistic account, a move that begins with his discussion of thermometry, in which he argues that the historical path to measurement, in this case and generally, is through theory. The point was clear: what Conant thought was largely empirical was completely theory-laden. A Bunsen burner was not an inert element of science that formed a condition of scientific activity, but a live part of a paradigmatic scheme of interpretation, subject to the same radical transformation as the rest of the paradigm.

Of course, the issue is not merely historical, since the intuition behind this, namely that the bench scientist is doing something that is more or less autonomous from theory and corrects theory, is the same intuition that Ian Hacking played on in *Representing and Intervening* (1983) that ended the romance with theory-ladeness of the 1960s and 1970s that culminated in Fred Suppe's collection (1974), in which the theory-observation distinction was buried unmourned.

What is striking about the extension of "theory-ladeness" to practice is the extent to which this model appears more and more as an imposition on material that cannot be understood in this way. There is no moment of decision in relation to practices analogous to the moment of theory-choice. Pickaxes are theory-laden only in the most wildly extended sense. The situation of decision that is central to the relativistic interpretation is largely virtual rather than real —the more common case is extended discussion that eventually leads to a resolution. Kuhn's real achievement, in a sense, was to make the conflict between new theories and old theories exceptionally vivid, so that it seemed as though there were numerous cases in which scientific truth was up for grabs between mutually antagonistic and mutually uncomprehending sides. But this was an achievement of historiographic dramatics —not for nothing was it made into a novel, complete with a suicide (Russell McCormmach, *Night Thoughts of a Classical Physicist*, 1983). Even the cases on which it was based, the cases in the didactic case studies of radical changes in conceptual schemes that Conant had created for the use of his undergraduate general education curriculum, were far less "irrational" and decisionistic. The historiographic appearance was the result of assimilating these cases to "theory-ladeness" and telling the stories in these terms rather than the cases themselves.

What the concept of practice returns us to is the mundane —the scientist who has mastered a particular technique to the point that they can get good results with it, and are in demand for this skill and their capacity to pass the skill onto others. The standard examples in the science studies literature, such as Harry Collins's study of replicating the TEA laser (1985), are concerned to show how difficult it is to do certain kinds of things in science out of the book; i.e. with explicit instructions alone. In this case, some people couldn't replicate the TEA laser even with a lot of help, and those who could required continuous interaction and contact over a long period of time. The point was to show that replication was not and could not be understood as a mechanical process of taking public documents in science as sufficient for understanding a crucial process in science. The process, confirmation, is often thought to be free of the problems of accounting for discovery. Collins shows that it too depends on tacit knowledge. This kind of case is very far from the situations Kuhn describes. One can perhaps imagine that scientists trying to make a laser work are in some sense in a similar cognitive state to the scientists in a Kuhnian revolution, incapable of getting their bearing and of determining to their own satisfaction what is going on. But the solution —contact and interaction— is not the Kuhnian one of commitment and decision. Indeed there is no role for decision, virtual or otherwise, in these cases of tacit knowledge. It is more plausible to think that this kind of "practice" is fundamentally different in character from, even though curiously entwined with, the explicit, theoretical, conceptual parts of science.

If we accept, as a hypothesis, that practice in this "skills" sense might be unassimilable to the "theory" side of the divide, we are faced with a question. If the relativism that results from the Kuhnian interpretation of practice is the product of notions of non-rational decision and commitment built into the narrative structure of radical conceptual change, and practice in this sense cannot be assimilated to the theoretical, or more broadly the conceptual, is this sense of practice afflicted with the problem of relativism?

To assure that we are not merely being caught up in verbal distinctions here, it is perhaps useful to clarify what is at stake. In using the notion of theory-ladeness I was self-consciously invoking a dead vocabulary of the 1950s. But along the way I have suggested that the same problem appears under a variety of live vocabularies dealing with the classic dualisms: nature-normativity, reason and experience, with the dualism implied by the Sellarsian notion of the space of reasons, and more generally by conceptualism and the Kantian question of whether the mind infuses concepts into objects. The problem then is whether relativism arises from putting practice on one side or another of the relevant dualism.

The reasoning that places practices on the concept side goes like this: concepts are normative in the sense of normativity that includes validity and so forth, practices are theory-laden, meaning conceptual, therefore practices are normative in this validity involving sense. If the normativizing element is, as in the examples I have discussed earlier, relativizing, practices is a relativizing notion, and practice relativism follows. Practices become the relativizing constitutive base, to use the Marxist locution, which is manifested in the superstructural elements of scientific fact, theory, and validity. Practices are relativizing with respect to validity because they enable validity to be established with different results in the different systems of practices based on them: what is valid in one legal system or paradigm may be invalid in another, and this is because the normative, validating element varies between them.

The general form of the problem of relativism here thus goes beyond practice. The idea is simply that the sources of normativity, namely normative attitudes, collective intentions, commitments, and so forth, vary. The Kantian story is non-relativist. Considerations of validity apply to every intelligent being. But Kant could claim this by claiming that there is only one source of normativity. As soon as we acknowledge historical diversity with respect to fundamental constitutive presuppositions —the acknowledgment that is the gift of neo-Kantianism— we have relativism. And one can read the history of philosophy after neo-Kantianism, and indeed within neo-Kantianism, as a long series of attempted escapes from this problematic; escapes which nevertheless accepted the core picture of the production of facts through fundamental, constitutive concepts.

What happens if one dispenses with the notion that practices are conceptual, that they belong on the theory side of the dualism in question? What are the costs of doing so, or rather what about our theories of practice, validity, and so forth needs to be revised? And what are the benefits with respect to relativism? Dispensing with the relativizing elements —commitment, collective intention, normative attitudes— makes a large difference. As I suggested at the outset, differences that can be explained by reference to the data side of the theory-data distinction don't have the relativizing implications that follow from differences at the level of fundamental theoretical premises. This holds, of course, for the remaining dualisms. But the cost of dispensing with the notion that practices are, in some sense, conceptual seem high. Diversity still needs to be explained. Worse, the explanation of diversity in terms of data seems to be doomed by the problem of sufficiency. There is something more than data, or more than pickaxes, that is needed to account for the specific conditions of particular theoretical stances or subjectivities. The differences pointed to by the notion of paradigm, even if they are not to be accounted for by the notion of paradigm, still need to be explained. And it is difficult to see how there could be an account of subjective diversity that does not have its source in the subjective —in notions of commitment and the like.

The problem, in a word, is content. A concept of practice sufficient for a reasonably wide range of explanatory uses must be one which allows for sufficient content to perform these tasks. The dilemma is that the best and perhaps only source of content is from the conceptual side of the dualism. The most decisive argument against a non-conceptual notion of practice would be a denial of the possibility of non-conceptual content. If by content we mean something that can be a reason for, and "reasons for" are already in the normative "space of reasons", it seems that the denial follows from the dualism: even if there is something mental that is not conceptual, it is not "content" and not admissible in an account of practices as part of the activity of providing "reasons for" that science consists in.

There are, however, many "ifs" in this reasoning. There is also a long tradition in which different descriptions are employed, many of which evade the dualisms in question. The conceptualist description of practices, as I have suggested, is questionable. Harry Collins's examples of the problem of laser building and Michael Polanyi's example of bicycle riding as a model for the tacit knowledge needed for scientific discovery foreground cases that conceptualism can handle only by defining them as conceptual, or if conceptualism is made into a necessary truth about content that excludes the possibility of non-conceptual content. If we accept the possibility that skills, discernment in, and "senses of" such things as balance in riding a bicycle are non-conceptual, but also contentful, in that the capacities are learned or the product of learning joined with innate capacities, we have quite a different situation, in which the problem of sufficiency is potentially soluble without relativism.

It is striking that the literature on non-conceptual content, which is not motivated by examples in science, employs the same kinds of standard cases. Adrian Cuzzins gives the example of riding a motorcycle in London, being stopped by a policeman, and being asked "do you know how fast that you were traveling?" His comment is that this struck him as a deep philosophical puzzle. He did not know in the sense of knowing what the speedometer said, but he *had* to "know" in the sense of being able to gauge the traffic as a condition "of wiggling through and around heavy traffic and past the road dividers and traffic bollards of a London street" (1990, p. 156). This knowledge is different in kind.

> In the case of a novice who has to *infer* the significance-for-motorcycling of their speedometer-given speed, the characteristic functionality of conceptual knowledge interferes with the characteristic functionality of experiential knowledge. The interference can also go in the other direction. The great advantage of experiential content is that its links to action are direct, and do not need to be mediated by time-consuming —and activity-distancing— inferential work; work which may at any point be subject to skeptical challenge. Experiential knowledge of the kind possessed by the skilled motorcyclist may be subject to *resistance*, but not to skeptical challenge. (1990, p. 151)

He goes on to say that this virtue is at the same time a cognitive vice. This content is situation-specific and private, that is, it is *my* sense, so that it "cannot by itself provide what we have come to regard as the constitutive requirements on *thought* content: generality, objectivity, standardization, transportability of knowledge from one embodied and environmentally specific situation to another" (1990, p. 151). Cuzzins suggests that there is a non-conceptual special kind of "mundane normativity" at stake in the content, unlike that of truth and validity, which he calls "activity guidance" (1990, p. 159) and associates with skill and mastery.

From one point of view, this is a dead end —this kind of practical normativity, if indeed it is normativity at all, does not help with truth and validity. But several points need to be kept in mind before we give up. If my complaints about the pseudo-explanatory use of commitment, collective intentionality, and the like by conceptualism are correct, there is no viable alternative to reconsidering this apparent dead end. And there are several pressing considerations that support doing so. There is the issue of dispensability: every serious account of legal practice acknowledges that the rules of legal construction together with written law are insufficient to produce the determinate legal outcomes that the law requires, and traditionally there has been an appeal to the notion of "judicial sense" to fill this gap; science too is through and through a skilled activity that involves knowing more than we can say, as Polanyi puts it; and in the core of semantics is the problem of rule-following, which has resisted reduction, Kripkean and otherwise, but which even in the hands of Wittgenstein needed to be supplemented by such things as "primitive reactions" (cf. Rubinstein 2004), and, it is plausible to argue, something akin to the "judicial sense" in order to account for the capacity of the rule follower to go on in a way that made sense to others. Nor is this absent from science —the "in the gut" test plays a large and, if Polanyi is correct, a necessary role in such tasks as assigning plausibilities to hypotheses and observations, a task which is itself necessary to science as it is actually practiced.

The indispensability of skills raises the question of the relation between the overt, the behavioral and explicit, and the non-conceptual. If there is such a thing as non-conceptual content, and "concepts" are theoretical constructions with explanatory purposes in the natural world or in the explanation of mind rather than Platonic entities apprehended through some sort of mystical process of participation, there is the hard question of whether non-conceptual content might do all the explanatory work that concepts were thought to do, or that concepts might be better understood in terms of the non-conceptual content that conditions performances that we interpret as "conceptual". As Robert Stalnaker puts it,

> John McDowell argued [. . .] that both kinds of information states [the contents of speech acts and contents akin to experience] are of the same kind and that content is conceptual all the way down. I am inclined to agree with McDowell that the different kinds of states have the same kind of content, but I am suggesting it is non-conceptual all the way up. (Stalnaker 2003, p. 106)

This point can be put in terms of practice theory in the following way. There is a huge mass of habitual inference that precedes speech, which is not articulated in speech, but that enables people to speak about the same things. Carnap himself wrote that the evaluation of observations "is usually carried out as a matter of habit than a deliberate, rational procedure" and said that the task of rational reconstruction was "to lay down explicit rules for the evaluation"

(Carnap 1992, p. 73). Reading back "concepts" into this mass of habit has been a strategy of Kantians since Kant himself. But is there an explanatory necessity for doing so? It may be that outside of the artificial explanatory context of a certain kind of philosophical semantics, there is no such necessity. Then the explanatory burden shifts: the problem becomes one of establishing that the problems that conceptualism solves are not artificial. I suspect that they are.

For the study of science itself, matters are even more easily resolved. The explanatory objects we choose need not involve these theories. We can explain something like the rough kind of consensus on the facts of science that leads to textbook science, for example, as distinguished from the idealized picture of a perfect consensus based on shared cognitive frameworks presented by Kuhn, that is, the fact that explicit agreement is reached by people who have attained recognized skills in laboratory work and in reasoning about science. We can say that without a convergence of skillful activity there is no science or law. If this convergence, together with other unproblematic, non-relativizing explanans, is sufficient to establish such validity as we have in science —if it accounts for a suitably deflated notion of scientific truth, for example sufficiently accounts for such things as successful replication, it is perhaps sufficiency enough. And if it is sufficient to account for the actual contingencies of scientific activity without involving appealing to relativizers, such as the notion of paradigm, we have the best of both worlds: a historically adequate model without relativism.

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