

Relativism, Pragmatism and the Practice of Science.

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Relativism, Pragmatism, and the Practice of Science

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But science in the making, science as an end to be pursued, is as subjective and psychologically conditioned as any other branch of human endeavor—so much so that the question, What is the purpose and meaning of science? receives quite different answers at different times and from different sorts of people. (Einstein 1934: 112)

Relativism is worrisome. Many regard it as a bad thing; a virus set to infect your whole way of thinking if your ideas seem to leave even a small opening for it. Others profess to believe that relativism is quite a good thing. Since no one knows exactly what relativism is, both camps could be right. Here I examine some non-idiotic forms of relativism whose ‘dangers’, I argue, amount to no more than an anti-foundationalism familiar from the pragmatic tradition. Seen pragmatically, relativism turns out to be robust with respect to standard anti-relativist arguments. Seen pragmatically, it might also help soothe the anxiety over nihilism or irrationality (or general chaos) that it triggers in some. For, as I suggest, pragmatic forms of relativism offer an appropriate setting for understanding good scientific practice.

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1. PARADOX AND ARGUMENT

‘Everything is relative to everything.’ All sides agree that this idiot form of relativism is silly and incoherent—and also that no one has ever held it. Opponents of relativism (including some whose ideas come uncomfortably close) charge that more sophisticated forms of relativism are also inconsistent, or otherwise conceptually defective. Their first line of attack usually develops some version of Plato’s arguments in the *Theaetetus*, where Plato argues against Protagoras’ saying, ‘Man is the measure of all things, of those that are, that they are, and of those that are not, that they are not.’ (Compare William James, ‘The trail of the human serpent is thus over everything.’) Suppose we call relativism about certain alleged universal concepts (or *universal relativism*) the doctrine according to which truths about those universals are relative.¹ (Ignore for a moment the question of relative to what and in what way.) Depending on the universals, this might yield relativism about reason or standards or values, or even about truth itself. Notice that the very idea that universals are relative sounds contradictory, and so (it is claimed) the argument will show. A typical version can go like this. Clearly universal relativism (truths about certain universals are relative), if it were true, would be about those universals and so would apply to itself. Thus it would follow that universal relativism, if true, is relative. But if it is relative, then it is not true in the intended sense. Hence, universal relativism is caught in a dilemma. It is not true, if it is true. Therefore, since it is also not true if it isn’t, universal relativism is not true. Variants on this involve the idea that not many believe in universal relativism (alternatively, that some do not). So if the relativism in that doctrine makes truths about universals relative to what many believe (alternatively, to what some believe), then, by its own lights, it is not true. (This is a little closer to Plato in the *Theaetetus*.) Again, universal relativism if true is not true, and therefore universal relativism is not true.

¹ Here I formulate relativism metalinguistically: truths about universals are relative. That formulation suits the argument of this section. Later I descend to the object language where it is the universals themselves that are relative. The redundancy feature of truth allows this latitude, free of harm—at least I hope so.

The idea behind this Platonic line of argument is that relativism, when stated so broadly, is self-refuting. Charges of this sort appeal to philosophers, who have been trained to prosecute them (Putnam 1981: 119 and Boghossian 1996 are good examples of the genre). But, as Dewey reminds us, '[A]rguments and objections are but stimuli to induce somebody to try a certain experiment—to have recourse, that is, to a non-logical, non-intellectual affair' (*MW* x. 325 n. 1). So philosophers understand that proto-refutations, such as these, can be looked at instead as tools for adjusting the assumptions or presuppositions that engage the refuting arguments. They can help us, that is, realign our thoughts with the actions that prompt and test the thoughts. In the case of these Platonic arguments we can look at the assumption that if the truth of relativism is itself only relative, then relativism is not true as intended. This is a shaky assumption, since it is the relativist's actions that count here, and relativists are likely to show that they regard being relatively true as all the truth there is, and so true enough (see e.g. Meiland 1980). Faced with this relativist response, the critic of relativism may shift ground from logic to rhetoric (or marketing), moving from the claim that relativism is self-refuting to the claim that it is self-defeating. For suppose it suffices for universal relativism that it is true enough provided only that some believe it (whether one or many), then how could it be a doctrine that the relativist can advocate to non-believers (for whom it is already false!) in order to persuade them to change their minds? Indeed, what would be the point of having people change their minds? And if relativism is a doctrine that cannot be sold on its merits, then what merit does it really have?

The relativist could pursue a number of responses, but here I would emphasize only one. It is that universal relativism has its own appeal, and so perhaps it does not need much marketing in order to be sold. More fully, the charge that relativism is self-defeating is based on the idea that to persuade someone to adopt relativism requires theoretical advocacy, something like an argument in which I demonstrate to you the merits of relativism. But how can I persuade you that truths about certain universals are relative unless we share lots of ground in common, including lots of what we believe to be true about these very universals—as well as lots about logical inference and other universals. (This is the kernel of Davidson's (1984) attack on conceptual relativism, an attack that questions the very notion of a conceptual scheme.) So it looks as if we would need much that is not relative in order to be an advocate for relativism, and that may begin to look like needing too

much. The relativist may well respond that perhaps we need not be an advocate in this sense, so perhaps we need nothing non-relativistic at all. For assuming that I want you to join in my relativism, why should I try to persuade you of it by an argument conceived of in this purely intellectual way? Conceived of pragmatically, however, as an inducement to try the 'experiment' of adopting universal relativism, perhaps all I need do is display it and invite you to examine it for yourself (see Goodman 1978: 22). This is a minimal but well-known pragmatic strategy ("Try it, you'll like it") and, given the apparent lure of relativism, it may be all the advocacy or argument the relativist needs.

In common with certain other sweeping philosophical doctrines (skepticism and solipsism come to mind), relativism has about it an air of self-referential paradox. From Plato's time to ours, the suspicion lingers that relativism is somehow inconsistent. It is self-refuting, or if not that, then self-defeating, or if not that, then in some other way it is conceptually flawed. The history of relativism, however, suggests the contrary conclusion. As above, relativism seems a robust doctrine, well able to survive despite all the arguments brought against it. If survival is a mark of fitness for ideas as well as for species, then relativism looks fit. If, further, fitness implies proliferation, then we see fitness confirmed by several contemporary movements that have taken a relativist turn. We need to look at this recent revival of relativism, but here I want to turn away from argument and paradox to look instead at some considerations that make relativism, however resilient, seem undesirable and even dangerous—a 'dictatorship' (Benedict XVI 2005) with 'pernicious consequences' (Boghossian 1996).

2. STANDARDS AND TRUTH

What are the relata of relativism? That is, to what are the universals relative? Candidates here include social relata such as individuals or groups. There are conceptual relata such as points of view, conceptual schemes, language games, or linguistic frameworks. Finally, there are very general relata that interweave these first two, such as practices or cultures or forms of life. All of the above might be indexed for time, and no doubt there can be other candidates as well. Suppose the universals are standards, and the relata are social practices. Then we get a characteristic relativism according to which standards are relative to

social practices. Why should this be worrisome? In Carnapian terms the worry would not be over internal questions. To the extent to which the social practices are determinate, the standards will be fixed by the practice (even if they are not transparent to the practitioners). But if we are asked an external question, one about improving our practice or adopting better standards, then apparently our relativism leaves us with no place to go. In the end, all we can say is that this is how we do things and that is not. The spade turns here.

Thomas Kuhn's (1970) account of scientific development involves just such a relativism. For Kuhn, in the case of normal science, standards for the practice of science are internal to the paradigm that governs that practice. Thus when the buildup of anomalies leads to crisis, and revolution is in the offing, judgments about whether to change our practice, and how, have no home ground. It looks as though we have no resources to deal with choices involving revolutionary change. In fact, over the years, Kuhn had a great deal of practical advice to offer about how such choices are to be made. He says they are based on a number of criteria (or 'values'), including considerations of accuracy, consistency, scope, simplicity, and fruitfulness (1977: 322). But he emphasizes that, in the absence of a governing paradigm, the application of these criteria is not clear-cut. We have in effect to extend the notions of accuracy, consistency, and so forth, anew. In the eyes of his early critics (e.g. Shapere 1967 or Scheffler 1967), Kuhn's relativism leads to irrationality. It leads, that is, to there being no rational basis for revolutionary change. Kuhn and his supporters always resisted this charge. They were right to do so, since the posited criteria of choice certainly provide a rational basis (reasonable means toward reasonable ends) for judging newly proposed practices: a basis, that is, that (arguably) leads to reliable science. Indeed, these criteria, as Kuhn presents them, are even invariant, or absolute, over different sciences and epochs. It is just that the application of these criteria is not fixed by past practice. We have to extend the practice at the very same time that we determine what is scientifically simplest, most fruitful, and so on. The general point here should be familiar from Dewey's 'experimentalism': that we learn in inquiry itself how better to conduct it.

As I see it, then, the worry over a relativism that makes standards relative to practice is not properly speaking a worry about irrationality. Although the concern might be expressed in those terms, we can see from these reflections on Kuhn that there need be no issue of irrationality raised by this form of relativism. Rather, the worry is that if standards are made relative to practice, we have no substantive resource that

determines how to respond when established practices are challenged. Just as with Kuhn's five criteria of choice, in a significant sense we help the criteria for change become substantive as we go. I believe that other sorts of relativism provoke the same worry.

Consider relativism over truth. Suppose we hold an acceptance theory of truth, so that something is true just in case the right people accept it under the right circumstances. This makes truth relative to acceptance behavior. One version of this might be a picture of truth as idealized rational acceptability. This is the picture that Hilary Putnam (1981) once promoted as central to his 'internal realism'.² There the right people are perfectly rational agents, and the right circumstances are those ideal for the acquisition of knowledge. Uncomfortable with all this idealization, and with the idea of this as a substantive 'theory' of truth, Richard Rorty suggests a more human reading where the right people are just us, and the right circumstances are when we are at our best (1993: 452). Elsewhere I have tried to show that, in general, this form of relativism is subject to a Platonic-style refutation; in effect, that on this relativistic conception, truth judgments become unintelligible (1989 and 1996: ch. 8). The argument, roughly, is that the grammar of truth leads to an infinite regress of conditions of acceptability. For if something is true, then it is true that it is true, and this can be repeated indefinitely. Thus, given the redundancy feature of truth (the equivalence of 'P' and 'it is true that P'), any seemingly simple judgment about the truth of an assertion, on this view, turns into an unintelligibly long jumble of judgments about acceptability conditions. This is a terrific argument. Indeed, it is close to one that Putnam himself formulates against relativism in general (1981: 119–24). But it can hardly be expected to put a stop to the game. For we really need to ask what it is that bothers us here, apart from grammar.

The answer, surely, is that acceptance, no matter how well dressed, is not truth. Unless we rig it so that 'the right people' accepting under 'the right circumstances' just amounts to accepting something when and only when it is true, no honest acceptance formula can be expected to capture truths and only truths. This is clear in Rorty's reading, where we want to ask whether, even at our best, couldn't we be wrong? What Rorty calls the 'cautionary' use of truth is just the use where we recognize that, yes indeed, of course we can be wrong—even at our best.

² See Mueller and Fine 2004 for the ins and outs of Putnam's treatment of realism and truth, and its relation to a consistent pragmatic core.

and so on and so on. We are threatened by an indefinite regress. The implication is that relativism allows no foundations for science—or for knowledge, or for morality. There are no well-defined principles or norms (or whatever) that can always be counted on to guide us in moving from present practice to a future practice where the very norms or principles currently in play may no longer apply, or may not apply in the same way. To guide us we have only schema or rules of thumb that are incomplete and, apparently, incompleteable. The worry, in short, is that we have no (firm) leg to stand on. That worry is triggered by a correct perception about the consequences of relativism. Relativism is incompatible with foundationalism. But is this pernicious?

4. DO WE NEED A LEG TO STAND ON?

In 1905, his ‘miracle year’, Albert Einstein published several short papers that were instrumental in changing the practice of physics. It is only a slight exaggeration to say that physics has never been the same since. His paper on the photoelectric effect introduced the then revolutionary idea of the quantization of light, and later won him the Nobel prize. It was one of a series of papers in which Einstein developed many of the tools and ideas that culminated in the modern quantum theory. Two papers that he wrote on Brownian motion helped establish the reality of molecules. But the paper we remember most was the paper on the special theory of relativity. It was a rather off-beat piece of work, combining a little elementary philosophical analysis (‘What is time?’) with a heuristic derivation of known equations (the Lorentz transformations) for which hardly any experimental applications were discussed, and with no references at all to the literature. Much has been written about the background to that paper on relativity, and about Einstein’s way of doing physics more generally. Of course Einstein was no nihilist or irrationalist, and in his own reflections about science Einstein was not a philosophical relativist either. (The name ‘relativity’ was given to Einstein’s theory by Max Planck, and it stuck. Emphasizing what the Lorentz transformations leave fixed, like the spacetime interval, Einstein would have preferred to call his work ‘the theory of invariants’.) Yet scholarship about Einstein’s scientific work finds that it was also not foundational, in the sense of positing a firm ground for extending practice. Although he certainly valued unity and logical simplicity in his scientific work, these (and other values) were like Kuhn’s criteria,

needing to be re-situated with changing practice. As a leading scholar notes, one must view his work ‘in context, taking into account both the inner logic . . . and the contemporary problematics’ to which it was responding (Darrigol 2004: 618). No reputable scholar holds that Einstein worked from a set of ready-made principles that determined the outcome of his science.

Like Dewey and the pragmatic tradition, Einstein held that science was continuous with everyday thinking, not an esoteric field with arcane methods and practices all its own. He held, in particular, that the formation of everyday concepts, as well as the formation of concepts in science, was not determined by any logical means, no matter how ‘inevitable’ the concepts might seem to be or how close to ‘pure’ sense perception they appeared. Rather, he regarded scientific concepts as a free creation of the human mind.³

By ‘free’ he meant both that concepts are not innate and also that they are neither given in nor logically derived from experience. The only test for scientific concepts is whether they can be organized in a logically simple system that finds fruitful empirical applications. In framing new ideas in science and in changing how we practice science, nothing in past practice forces any particular movement into the future. That is not to say that how we go on is independent of what we have been doing. Certainly the course of science produces what Bruno Latour (1987) calls ‘black boxes’: that is, modules of theory, instrumentation, and experimental technique that support one another in ways that scientists rely on to do their daily work. But black boxes can be opened and tinkered with. In the history of science they are often discredited. Nothing in our current practice determines what attitude to take to our black boxes: which to accept and which to try to subvert. Further, nothing in our current practice determines whether we will succeed, either way, if we try. This again is Dewey’s instrumentalism, or experimentalism. Doing science involves feedback from ongoing practice. We need to reflect on that practice and choose whether to proceed as usual or to try something new (if only we can come up with something new to try). We cannot operationalize those decisions. There are no fixed rules of practice that tell us to turn left if today’s results are such-and-such, and to go straight ahead otherwise. Each such call is a judgment call, and,

³ The expression ‘free creation of the human mind’ occurs in Dedekind 1888, who applies it to numerical concepts. Einstein read that work during his Bern years, and the expression seems to have made its mark (e.g. 1954: 291)

as Einstein suggests, these scientific judgments—like most everyday judgments—are not forced.

They are still judgments, however, and not arbitrary whims. They depend on reasons, ideas, experience, and skills; on interests and values; on goals and directions; on material, conceptual, and economic resources and rewards. They depend on the cooperation of the natural world. They also depend on our allies and on our adversaries. Everything that pertains to complex human action enters into judgments about how to practice science. Almost all the items that underwrite our judgments are also underspecified in some or other respects. Very often even our goals are not so clear and firm as we might like to imagine. Much less so are the ideas, interests, resources, and so on that enter into what we decide to do. As anyone who has practiced science knows, that includes our data and other experimental results, which are always subject to reinterpretation and re-evaluation. Usually we can rationalize whatever course of action emerges as a suitable modification of past practice. That is, we are able to show how it relates to judgments about goals and values and how it reflects appropriate means for attaining those goals while respecting those values. It is important to see that to behave rationally, in this sense, does not imply that the behavior is fixed by clear-cut rules or principles antecedent to the action itself. To behave rationally means only that we can rationalize our behavior in terms of suitable means, goals, and values that may themselves emerge only in the course of action.

This is the Einsteinian conception of the openness of science. It is a conception of science as a rational enterprise without foundations. I think it a realistic conception of how science works and, if you are a relativist about standards or the like, you will have to adopt something like it. If it is more or less on target as a description of ongoing science, then two things follow. One is that in doing science we do not, in fact, have a leg to stand on. The other is that we do not, in principle, need a leg to stand on. Unforced judgments that rely on underspecified constraints seem to be all the resources that science has, and all that science needs for its continued success and progress.

5. DEFUSING RELATIVISM?

Relativism, and the nonfoundational view of science that it implies, certainly allows for reasons, principles, and facts as integral components in the judgments that enter into scientific practice. What relativism

goes against is the sort of foundationalism according to which these components are clear-cut and firm, and the judgments in which they issue inevitable. It is difficult to find support for such a foundationalism in the practice of science. There may appear to be abundant support, however, for the different idea that there is a foundation for the validation of scientific decisions after they have been made. Neopositivism distinguished between the context of discovery and the context of justification, with the former defying strict logic and the latter exemplifying it. One tempting way of defusing the worry over relativism, then, would be to attach the concern about lack of foundations to the context of discovery, where it may seem unlikely to do any harm. The idea is that there may indeed be no strict scientific method that underwrites a logic of discovery, but once we have set off on a course of action, then, surely, there are strict principles for evaluating our new knowledge claims.

But this easy-sounding resolution is not correct. For the cognitive relativism we have been examining applies as well to the context of justification as to the context of discovery. Both are relative. Justification, no less than discovery, makes use of culturally bound and variable instruments of reason. The confidence intervals, correlation coefficients, and other statistical instruments of today's science are a product of nineteenth-century thought. Nothing of the sort was available to help Galileo interpret his data in Pisa. When Pasteur tells us that chance favours only the prepared mind, it was his own newly developed experimental methods that he was recommending, not Mill's methods, and certainly not Descartes's. Even the medical paradigm of placebo-controlled, double-blind trials has a history (why do you think it is 'double' blind?) and a shifting set of emendations (e.g. randomization) and learned limitations. These instruments of reason influence what principles are accepted, what facts are regarded as salient, and what reasons are given what weights. If this is so, then the best that could be said for justification is that it is determinate relative to such cultural instruments; i.e. relative to accepted practice. Part of the quest for certainty, as Dewey notes, tests ideas only against traditional, antecedent conditions. So, immersed as we are in current practice, it may well seem to us that validation is firmly rooted. That appearance, however, is a mirage. In fact, with regard to justification we are in no better position than before over the question about how to go on, about how to extend our practice into the future. To justify our choices, we need to make use of instruments of reason as they are reconfigured in the light of what we

learn as a result of the very choices we are wanting to justify. Thus we cannot make the justification of scientific choices any more operational than we can make the discovery of those choices.

Otto Neurath was a pragmatist who resisted the distinction, drawn by some of his neo-positivist colleagues, between the context of discovery and the context of justification. His legacy contains a different image: that of having to rebuild our boat of science, plank by plank, while at sea. This is not a bad image for the nonfoundational approach to science that relativism implies.

6. SOCIAL CONSTRUCTIVISM

In more recent times, social constructivism has emerged as a significant nonfoundational approach to science. No doubt, part of why the 'great fear of relativism' (Hacking 1999: 4) has become a prominent cultural topic today is that social constructivists (along with some of their postmodern allies) feature relativism as one of their chief delights. They flaunt their relativism, which they regard as a virtue, and they actually promote it as an advance in our understanding of science. Indeed, different proponents of social constructivism may vie with one another over who is more relativistic than whom. It is with a certain enthusiasm, for example, that Andrew Pickering describes his account of science as a 'hyperrelativism' on 'the wild sides' of his constructivist colleagues (1995: 207–8). The issue for Pickering and those tamer relativisms is precisely the issue we have been treating: how to project established practice into the future.

A properly 'social' constructivist account would feature social factors as the primary determinants of what happens. This is the tack of the so-called strong program in the sociology of scientific knowledge. There one looks at established routines that can be socially inculcated, to the norm-building role of authority in a community, to social interests, and to shared goals as possible ways of explaining what causes beliefs to be held and what leads to the patterns of behavior that reflect these beliefs. The relativism of the strong program is marked by the assumption 'that all beliefs are on a par with one another with respect to the causes of their credibility' (Barnes and Bloor 1982: 23). This 'symmetry postulate' makes credibility relative to social factors, such as those cited above, and the constructivist part of this social constructivism makes validity (or justification) relative to credibility. Notice that this relativism has a

causal-scientific flavour. According to the program, social factors cause certain beliefs to be held (or judged appropriate), and the task for the sociology of scientific knowledge is to find the causes. Despite this emphasis on social causes, the position of the strong program is not truly deterministic. Although at any given moment a particular constellation of social factors may determine what scientists do, that constellation itself is relative. It is a product of specific local circumstances that might well have been different. Thus, in spite of its emphasis on social factors, for the strong program there is no fixed foundation for changing practices. Its relativism functions to bring this out.

If one is skeptical about how much understanding of scientific belief can be had by looking only at social factors, then one might consider other strands in the constructivist movement, for not all constructivisms have an exclusively social flavor. Pickering (1995) gives a more balanced account, emphasizing both the role of the social and of the material world in the 'accommodations to resistance' that, for him, constitute scientific practice. He recognizes that independently of scientists' goals, plans, and interests, the material world also influences what scientists do. Pickering is shy about using the term 'constraint' here and saying simply that the material world constrains what we do. Pickering resists the language of constraints because of his pragmatic orientation. He wants to emphasize that the way the material world influences us is context-bound and emerges only as we act. That is, Pickering wants it to be clear that neither social nor material factors are foundational in ongoing scientific practice. What he has against 'constraints' is that they sound too much like timeless foundations.

Bruno Latour's actor-network theory (1987) is yet another kind of 'social constructivism' that de-emphasizes the social (also the constructivism). Latour takes over from Michel Serres (1982) the notion of quasi-objects, hybrid entities that are neither social nor natural but share features of both. Latour calls them 'actants'. Those in the quantum business might describe an actant as an entity whose state of being is a superposition of the social and the natural. Tools and other artifacts would be primary examples of actants, but then so would corporations and virtually every other kind of thing that relates both to nature and to us. The point is that the concept of the social world and that of the natural world are each constructed in relation to one another—constructed, that is, from actants. Thus neither concept should be fundamental in understanding science. Like Pickering's, Latour's orientation is also pragmatic, with an emphasis not only on science in action, but also

on relations as fundamental to science—just as in Dewey and James. Scientific practice is action made possible by temporarily stable networks of social-natural relations, and, at the same time, scientific practice is constituted by the work of extending and stabilizing these networks. Once again, the topic is how to extend present practice into the future, and the response of actor-network theory is that no foundational guides determine the extension.

If this brief survey is representative of the relativism to which social constructivists aspire, then it turns out to be something of a misnomer. For their relativism is primarily a commitment to a conception of scientific practice as open and nonfoundational. By advertising this commitment under the banner of relativism, they expose themselves to the hyper-charged armory of anti-relativist rhetoric, including the familiar charges of self-refutation and postmodern nihilism. Other vices are sometimes associated with constructivism—for instance, idealism or irrationalism—and it is questionable whether they too are actually to be found there. On the other side, one could question whether the primarily sociological tools that the constructivist movement brings to an analysis of scientific practice are up to the job. But, whatever may be the outcome of those other discussions, I must conclude here that what constructivists call relativism, if we bracket the misleading terminology, is just as they say it is; it is a virtue. In moving away from ‘foundations’ and ‘scientific method’, it constitutes an advance in our understanding of scientific practice.

7. CONCLUDING THOUGHTS

Relativism (idiot relativism excluded) is not paradoxical. Despite the clever work of a number of philosopher-prosecutors, we can dismiss their charges that relativism is self-refuting or self-defeating. Nor is relativism (even over standards or truth) dangerous. It does not lead to chaos. It does not imply nihilism or irrationality. To the contrary, insofar as it is concerned with standards, truth, and reason, it presupposes standards, truth, and reason. The false accusations are compounded from a correct perception and a mistake. The correct perception is that relativism is not compatible with firm and determinate foundations. The mistaken idea is that, without such foundations, what we do and value is arbitrary—anything goes. Again to the contrary, not anything goes without foundations. What does go, and goes well, however, is

the particular institution of science itself. Thus the nonfoundational consequences of relativism seem to be a virtue, not a vice. While these considerations may rescue relativism from the false charges brought against it, they are not intended to vindicate relativism as a general doctrine. The extent to which a specific form of universal relativism is viable depends critically on the universals specific to that form, to what they are said to be relative, and how so. Relativism varies in kind and degree, as well as in plausibility. No one verdict is ready-made to fit all.

If relativism about truth takes the form of an acceptance theory of truth, for instance, then it is surely wrong. For truth is not acceptance, and any attempt to pin it down that way is bound to fail. If relativism about standards takes Kuhn’s form, then its viability depends on whether Kuhn’s account of scientific practice—in terms of paradigms, normal science, and his dynamics for revolutionary change—is viable overall. Some doubt that it is. If relativism adopts a version of the strong program and holds that justification in science is relative only to the interests and beliefs of particular communities of scientists, then this version would seem to leave out the role of the material world in scientific practice, and if that were actually left out, it would be going too far. Similarly off track would be a postmodern relativism claiming that validity (or justification) is relative only to such things as ideological correctness and political power, without regard to other desiderata, including truth, evidence, and reason.

If these last two instances of relativism seem not just wrong-headed but extreme, then we might recall that mainstream physics also contains some rather extreme relativisms. The debate that goes back to Newton and Leibniz over a substantival conception of space and time versus a purely relational conception is precisely a debate over whether the universals of space and time are relative or absolute with respect to material bodies. That debate in physics continues today, after Einstein, over the role of absolute structures in spacetime itself. Niels Bohr’s Copenhagen interpretation of quantum mechanics, some version of which most contemporary physicists accept as the received view, involves a basic relativism about reality. This relativist understanding of the micro-world is also known as ‘contextualism’—a handy pseudonym for relativism when you would rather keep a low profile. According to Bohr’s doctrine of complementarity, basic physical properties of an atom—like its spatial location or its momentum—are relative to conditions of observation. Because position and momentum are not simultaneously observable, according to Heisenberg’s famous Uncertainty Principle,

Bohr argues that one cannot ‘define’ these properties, and so ascribe them to an atomic object, except under specified conditions of observation. Thus a fundamental relativism (or contextualism) of properties in the micro-world becomes necessary, or so Bohr claims.

So, not only standards, truth, and reason, but also space, time, and even reality itself may be judged relative. But if we were to adopt relativism about the first trio of universals, then would it ever be possible to decide about the relativism (or not) of any of them?

The answer is that yes, if we work at it, most likely we will be able to grade relativisms into better and worse—because generally we do. But that sort of question, which seems inevitable in this context, brings us back again to the issue of foundations. It seems that to be human—in Descartes’s terms, to be a thing that thinks—is to ask about ways to transcend the human condition. Not only the great religions, but also many of the myths and ideologies that run through different cultures, our own included, respond to that urge for transcendence. Like pragmatism, relativism responds as well. Like pragmatism, it responds in the negative. It rejects transcendence. The nonfoundationalism built into relativism says, pragmatically, that all that we have to count on is us. For some, that prospect seems too terrifying (or boring?) to live with. For others, it is just an invitation to roll up our sleeves and get to work. As Michel Serre says, for some it is

as though it were always a matter of constructing (or tearing down) a very solid edifice, whose peaks or foundation would organize all stability. It’s possible to compose outside of solidity—in fuzziness and fluctuation. Nature itself does nothing else, or almost. (1995: 112)

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