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Faith, Hope, and Regulative Ideals:
Epistemological Reflexivity in the Sociology of Religion^{1, 2}

James V. Spickard

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ABSTRACT

This article explores the epistemological consequences for the sociology of religion of the reflexive turn that has swept social science in the last two decades. Building on similar reflexivities in the writings of Charles Sanders Peirce and H. Richard Niebuhr, it argues that social science, like religion, is grounded in practical faith. A communal enterprise, social science can never justify its knowledge foundationally, nor can it find secure knowledge in the present. Instead, it lives in eschatological hope of increasingly ‘getting the story right’—either in the sense of painting a tentatively accurate picture of the social world or by demonstrating the objective limits of such picture-making. That hope serves as a regulative ideal, which adjusts scholarly practice in service of these ends. Like all action toward ideals, scholarly practice is not self-justifying, but rather depends on a faith in the eschatological positive culmination of its communal activity.

This approach throws new and useful light on recent controversies about foundationalism and relativism, objectivity and subjectivity, and on the role that social standpoints play in the sociological research process. It also has consequences for the sociology of religion in particular, as it points to a more fruitful way of defining our subject matter.

¹ A previous version of this article was presented at the 30th Biennial Conference of the International Society for the Sociology of Religion, Santiago de Compostela, Spain, 27-31 July, 2009

² I owe a debt of gratitude to the late Charles McCoy, Professor Emeritus at the Pacific School of Religion, for four years of conversations on these issues. His views can be found in McCoy (1980). We disagree on several key points, some of them central, but I would not have been able to write this article, had we not taken each other’s work seriously. Thanks also to the late James McClendon, to Mary Jo Neitz, and to Ole Riis for further dialogue.

The sociology of religion has lagged behind other fields in the ways we think about how we generate knowledge. Where other disciplines argue about the relative merits of positivism, social constructionism, critical realism, and standpoint theory, we, with few exceptions, rehash the thirty-year-old debate about objectivity versus subjectivity—and do so in relatively naïve terms. I had hoped that the collection on reflexive ethnography that some colleagues and I pulled together a few years ago would change this (Spickard *et al* 2002). We wrote of the reflexive turn that anthropological ethnographers have taken in the last couple of decades, and showed how this turn has strengthened, while changing, our knowledge claims. Alas, I have not seen much effect. Too much work still treats subjectivity and objectivity as the only two possible poles.

Maybe I should not be surprised. After all, the sociology of religion has something of an inferiority complex among the social sciences, studying as it does a phenomenon that many sociologists imagined to be disappearing (Bruce 2002). Even folks who study what Peter Berger (1999) now calls “re-surgent religion”³ typically see it as a reaction to modernity—i.e., as something grounded in the past, something opposed to scientific views of the world, and so on. The irony, of course, is that Christian theology has been reflexive for the last hundred and fifty years. By this I mean that many, though not all, Christian theologians recognize that only God has certain knowledge; human knowledge is at best a guess, shaped by culture, history, and socially situated points of view (McCoy 1980). Truly objective theology is thus impossible. This does not leave theologians mired in subjectivity, however; it means that they must start their thinking from their actual situation, not from some suprahuman standpoint (Niebuhr 1989). Fortunately, locational beginnings do not necessarily imply subjective endings. One would think that the sociology of religion, with its interest in religious matters, would have absorbed this lesson. One would think that we would be leading the epistemological discussion instead of failing to notice it. This, unfortunately, has not happened.

It is time to try again. In this essay, I shall describe the current four-fold division of research epistemologies that I mentioned above: positivism, social constructionism, critical realism, and standpoint theory. Then I shall

³ In this article, I maintain a typographical distinction between words in double-quotes and those in single-quotes. I use double-quotes to indicate actual quotes from identifiable authors; single-quotes are shudder-quotes, which I use to draw attention to concepts whose ontological referent is open to question.

outline a fifth position, compatible with but in fact underpinning the last two, which I think are the strongest of the four. I shall explore the logic of this fifth position, which I call eschatological realism, in an attempt to push beyond my previous work on the subject (Spickard 2002). I shall then comment on the implications of this form of realism for social-scientific inquiry.

II

Joey Sprague (2005: 31-52) provides a particularly clear formulation of the differences between four research epistemologies in the social sciences, which she calls "positivism", "constructionism", "critical realism", and "standpoint theory". Without arguing that every self-proclaimed positivist, constructionist, etc. identifies with the exact positions that she outlines, she identifies four intellectually distinct and coherent approaches, which seem to me to cover the current field. She focuses on the different knowledge claims made by each of these approaches. She also focuses on the presumed relationship for each between the knower and the known.

Positivism, in Sprague's account, claims that there is a real world 'out there' and that human beings can know that world through the facts they discover about it. Science explores relationships between these facts by creating theories which it then attempts to disprove. In its currently dominant form, positivist science proceeds by falsification: examining the facts to see whether they express the relationships that we hypothesize (Popper 1932). It furthermore claims that discovery is made possible when we minimize our individual subjective viewpoints in favor of objective measurement—falsification's prime instrument.

All three parts are important—the existence of a real world, our ability to test its properties factually, and the stress on minimizing subjectivity. All three are also questionable. First, it is not trivially obvious that there is an external world beyond our perceptions of it—despite Samuel Johnson's famous non-verbal reply to Bishop George Berkeley (he kicked a rock, both hurting his toe and missing Berkeley's point).⁴ The 18th-century European philosophers (Hume, Berkeley, Leibniz, and Kant) wrestled mightily with this issue with some success; that success did not make the issue go away. Among their conclusions was that knowing the world is not a matter of simply letting the facts speak for themselves—positivism's second claim. Later philosophers continued this line of thought. For example, W.V.O. Quine (1951) noted that mere 'facts' don't get us very far, because hypotheses are

notoriously underdetermined by the facts that supposedly test them. To quote Sprague (2005: 35),

When testing any one hypothesis, a scientist is also testing that set of hypotheses embedded within it—all the background assumptions contributing to the worldview that supports the hypothesis in the first place.

A given empirical failure may be caused by the failure of any of these assumptions. Theoretically speaking, we have no way of knowing which particular assumption might cause the failure, short of more testing; yet this testing is as subject to the same background multiplicity as was the original effort.

Third, minimizing subjectivity does not solve these problems, because subjectivity does not cause them (though it does seem reasonable that rampant subjectivity would make it difficult to spot background hypotheses that need testing). In fact, the claim that human subjectivity is the chief obstacle to secure knowledge is itself a hypothesis, needing to be tested—which means also testing all the other hypotheses with which it is packaged. Positivism is thus in the unusual position of presuming something which, by its own canons, must be proved. This does not seem terribly secure ground on which to base research.

Social constructionism takes a different approach. Whether or not there is a world 'out there'—and constructionists take various positions on this—our knowledge of that world is socially generated. That is to say, the world does not present itself to us immediately; instead, our knowledge is constituted by human activity. This is claimed at various levels.

At its most gentle, constructionism notes that our knowledge of the world is created in social organizations—today by universities, scholarly groups, etc., which put on conferences and publish journals (such as this one) to advance human understanding. Even positivists accept this, and recognize that universities, scholarly groups, conferences, journals, and so on can shape the knowledge that gets produced by them. Templeton, Lilly, and Pew grants clearly shape the sociology of religion (especially in the U.S.), though probably not in insidious ways. But shape it they do, and what a given society accepts as 'current knowledge' is always subject to such forces.

A further constructivist assertion is more serious: that "Any order or perceived regularity in phenomenon is not 'out there' in the empirical world, [because] we give order to our perceptions through the application of a cultural framework" (Sprague 2005: 36) This goes beyond Kant's (1781) claim that our perceptions are structured by the categories of pure understanding. Kant argued that we can only perceive the world in certain modes, and we thus cannot say anything about that world itself, separate from our perceiving it. He thought, however, that the categories by which we grasp the world

⁴ The story is found in Boswell (1794: v1, p472).

(time, space, causality, and some others) are universal—or at least universal to humans. Thus scientific knowledge is possible, though only of our perceptions, not of things in themselves.

Constructionism, following Durkheim (1915), argues that Kant's categories are themselves social products. This operates in three ways. First, there is Durkheim's own claim that the basic categories of knowledge (space, time, causality, etc.) are formed in the process of social life. They are thus grasped differently in different societies. 'Primitive' societies, said Durkheim, misattribute causes, for example by mistaking collective effervescence for the presence of the totem god. He thought that modern societies were less likely to do so—a position for which he has been roundly criticized (see Douglas 1975: ix-xxi).

Second, different societies have different ways of understanding the world, and they tend to generate knowledge that matches their conceptual systems. To use E.E. Evans-Pritchard's (1937) famous example, I may think that the collapse of a granary on my cousin, killing him, is pure chance, because my society teaches me that events can be meaninglessly random. An Azande, on the other hand, will see the collapse as the result of witchcraft, because chance plays no role in Azande conceptual universe. As Peter Winch (1964) noted of this case, both I and the Azande are being rational within our discursive worlds. We both recognize that termites chewed through the granary's support poles, so that it was bound to fall, but the Azande ask a further question, which I do not: Why did the granary fall when my cousin was resting under it, avoiding the hot sun? I cannot justify my rejection of this question, except by touting the superiority of my own socially affirmed way of seeing things. This is precisely the Azande claim about their point of view. In both cases, this amounts to circular reasoning. 'Chance' and 'witchcraft' are parallel concepts, each gaining its explanatory strength from the socially-supported worldview in which it resides.

Third, say the constructionists, different societies have different standards of what constitutes 'truth' and 'falsehood'—terms which amount to tools that separate preferred beliefs from those that the society in question finds less cogent. As Barry Barnes and David Bloor (1982) note, something is 'true' because it matches the standard for 'being true' that a society has erected at a given time. These standards shift, both in science and in everyday life. Constructionists generally argue that all such standards are social, which means that no one standard has a better claim for 'truth' than do others. The point is not that there is no 'truth'; the point is that what is known as 'truth' is always 'true' relative to the standards that a particular society uses, and those standards are not, themselves, independently verifiable. The constructionist is perfectly comfortable accepting that his or her "preferences and evaluations are as context-bound as those of the tribes" (Barnes and

Bloor 1982: 27) because there is no other choice. There is no extra-social method for weighing the adequacy of the standards themselves.

A few constructionists take a further step, arguing that 'reality itself' is a social construct. Frankly, I am not sure how to decode this statement, if by 'reality' is meant anything more than what a given society accepts as real at a given time. How would one get access to that 'real' reality, to see whether reality itself — as opposed to people's idea of it — varies?⁵

One does not need this final step to argue that 'truth'—in shudder quotes—is a social construct. We cannot know things absolutely. For constructionists, our knowledge is secure only relative to a particular social context and to a particular socio-intellectual history. When laid up against positivism, with its promise of secure, replicable, reality-based knowledge, this amounts to an epistemological revolution.

Not everyone is happy with this situation. The idea that 'truth' varies from place to place, no matter how subtly propounded, disquiets a society whose intellectual life is still based on the Enlightenment faith that knowledge and truth will set us free. And, Sprague (2005: 39) notes, it seems more than a bit suspicious that the shift from a firm to a variable truth comes at precisely the time when women, people of color, non-Westerners and other formerly excluded persons have at last been admitted to positions of power in our knowledge-generating organizations. Critical realism and standpoint theory respond to this supposed relativism, each in its own way. Like positivism, they claim that there is a world 'out there' and that this world can be known. Like social constructionism, however, they claim that the world cannot be known unproblematically, precisely because of the ways in which knowledge is embedded in social life.

Critical realism⁶ "holds that the world exists independently of our thinking about it, there are patterns to the way it works, and our perceptions of it are varied" (Sprague 2005: 39). This external reality is complex, however, with multiply overlapping causations, some of which operate probabilistically and not all of which are visible at any one time. This means that "science ... [is] a process in motion attempting to capture ever deeper and more basic strata of a reality at any moment of time unknown to us and perhaps not even empirically manifest" (Bhaskar and Norris 1999, quoted by Sprague

⁵ For two examples of this kind of radical constructionism, see Gergen (1999) and Barnes and Bloor (1982); for some responses, see the collection edited by Hollis and Lukes (1982), which contains Barnes and Bloor's article.

⁶ For a detailed look at critical realism, see Bhaskar (1975), Archer (1995), Bhaskar *et al* (1998).

2005: 40). Among these deeper strata are the realizations that, in Sprague's (2005: 40) words,

the knower is socially constructed, shaped by the discourses of culture and science. The known is complex and changing, including in response to social action. Thus the relationship between the knower and the known is mediated by discourse but amenable to adjustment and increasing refinement, though the known will never be more than an increasingly accurate approximation of reality.⁷

Standpoint theory adds an important qualification: that different knowers have different access to knowledge, depending on their social location. "Standpoint epistemology argues that all knowledge is constructed in a specific matrix of physical location, history, culture, and interests, and that these matrices change in configuration from one location to another" (Sprague 2005: 41, citing Harding 1998). This changes both one's core concepts and the knowledge they produce. Nancy Hartsock (1985), for example, points out that if one begins from the standpoint of the capitalist, one conceives of 'power' as a commodity: something that one can possess, exchange, or steal. If, on the other hand, one begins from the standpoint of the worker, one conceives of it as a relationship, in which one party can dominate or control the other. Hartsock points to a third alternative: "Beginning from the position of [women] who do the work of nurturing makes it possible to develop a notion of power as capacity or potential, as in the word empower" (Sprague 2005: 42).

In the sociology of religion, Mary Jo Neitz (2004, 2008) has demonstrated the centrality of women's experiences of religion, and how theorizing from the standpoint of only men causes us to misunderstand all parts of the religious scene. Linda Woodhead (2008a) showed how ignoring gender impoverishes secularization theory (see also Woodhead 2008b). Penny Marler (2008) pursued this matter quantitatively, explaining much of what has happened to mainline religion in the United States by "watching the women". None of these authors argues that such insights are closed to men. They point out, however, that intellectuals, like other people, are more likely to notice things that impinge on their experience. Our various standpoints give us starting points; they do not close off investigation. Speaking for myself, I cannot ignore the insights of religious minorities, especially those for whom doctrine and hierarchy are of little import, any more than Neitz, Woodhead,

⁷ Critical realism further affirms that the objects sought by research are the patterns that structure sense perception, not merely—as some radical positivists claim—those sense perceptions themselves. This point does not enter into my argument.

and Marler can ignore the insights of women. Sprague argues that it is by bridging such differences that our fields progress.

Our social structures, however, make this difficult—particularly those structures that have traditionally excluded women and minorities from positions of influence in our knowledge-producing organizations, and which still exclude members of the working and lower classes. There is a reason, for example, why White male sociologists have spilt so much ink on the (supposed) fight between secularization theory and rational-choice theory—both of which treat human actors as abstract, genderless beings. In our social world, only men have the privilege of imagining that they lack gender. Only Whites have the privilege of imagining that they lack race. Only heterosexuals have the privilege of ignoring peoples' varied and complex sexualities. (I won't go on.) Both secularization theory and rational-choice theory, despite their strengths, miss something essential about the patterns of religious life.

In sum, standpoint theory, like critical realism, posits a real world to investigate, and both recognize that the knowledge by which we grasp that world is shaped socially. Standpoint theory does a better job of noting that this shaping happens differently for different people, and it puts forward a research program that lets us explore just how.

Unfortunately, neither approach answers the root epistemological question: What makes scientific knowledge possible? Both focus on that knowledge's tentativeness, while resisting the constructionists' more radical claims. Yet, neither critical realists nor standpoint theorists stop at tentativeness in their empirical work. Marler (2008), for example, argues that "watching the women" produces better knowledge than ignoring them, and I agree with her. Yet, this claim requires not just empirical, but also epistemological justification. For all its faults, positivism gripped this nettle. I shall spend the rest of this talk exploring what we must assume to be the case, if either critical realism or standpoint theory is to produce secure knowledge on a similar level.

III

What makes for secure knowledge? Most attempts to generate such knowledge do so by trying to base it on a firm foundation.⁸ A house built on rock can withstand rains, floods, and wind, or so Jesus said (Matthew 7: 24-27). Well-founded knowledge is similarly supposed to protect us from any storm.

The problem is: there is no rock. Neither pure reason nor sense experience gives us clear certainty, nor can they do so in combination. "Just the

⁸ For an accessible philosophical treatment, see Fumerton (2005).

facts, ma'am" and "It stands to reason" both presume the very stability they are trying to prove. The first imagines that 'facts' are unproblematic; the second that 'reason' is universal. We can similarly undercut any proposed foundation by inquiring about its own grounding. To cite a parallel from the religious sphere, the statement "It's in the Bible" begs the questions of who decided what would be in our version of the Bible and who authorized our way of reading it. Nature's texts are no different, because our questions about what counts as nature and about how we should 'read' it can be differently answered

Moreover, we cannot stabilize a foundation by trying to ground that foundation itself in more basic stuff; for that stuff needs justification, too. The result is like the apocryphal Hindu story of the world resting on the back of an elephant, which stands on a turtle. What holds up the turtle? Another turtle. "After that, Sahib, it is turtles all the way down" (Geertz 1973: 28-29).⁹

Lest you think me flip, I should point out that Wayne Proudfoot's famous critique of William James' The Varieties of Religious Experience (1903) amounts to a turtles-upon-turtles argument. Proudfoot (1985: ch5) claims that people's religious experiences are grounded not in emotions, as for James, but in the sense experiences interpreted by ideas that underlie emotions. This opens the door to reductionism: what James thought was a 'religious experience' is (for Proudfoot) really just over-excited neurons, interpreted religiously. Correlation masquerades as cause, because Proudfoot, like James, sees religious experience as sense-experience plus over-beliefs—and Proudfoot further presumes that sense-experience is a more basic level on which to describe what is 'really' (in shudder quotes) going on.

The point of the anti-positivist revolution was that such reductions-to-foundations are impossible. There is no epistemologically secure foundation to which we can retreat. There is no rock on which to stand. What, then, makes scientific knowledge any more certain than analyzing the entrails of pigeons? If the building metaphor will not do, what will? I suggest that we look at the actual practice of science, including social science, for clues.

Science is famously not a set of propositions about the world, nor is it a firm body of results. It is, instead, a social activity. Communities of scientists attempt to understand the world around them, not usually in its totality but with as much certainty in their particular expertise as they can muster. They—we—present what we have learned through our researches, argue

about our and others' data, question or affirm each others' research methods, show off new methods, and, on occasion, try to pull together the big picture of what it all means for our fields of study.

I have written elsewhere that there are six such big pictures popular with contemporary sociologists of religion (Spickard 2006a, 2006b). Each one is underdetermined by the facts. Indeed, each attempts to tell us which facts are and are not relevant, often according to whether or not those facts inconvenience the picture in question. This clearly does not produce certain knowledge, but we can draw some conclusions about knowledge from the ways in which we argue about our data, our methods, and our big pictures. These conclusions amount to a sort of foundationalism-in reverse.

To grasp this, let us highlighting four aspects of science (again, including social science), seen as a social activity. The first is the fact that science is grounded in communities of scholars. Like all communities, scientists engage in a core activity—learning about the world—which is just one of many possible things that communities can do. Others include raising children, earning a living, capturing 'bad guys', and playing pinochle, each of which is just as important as science, in the larger scheme of things.

In the U.S., at least, one enters the scientific community by earning a PhD, a rite of passage if there ever was one. In doing so, one learns the scientific community's rules—how to structure an intellectual argument, what counts as evidence, and so on. Most of these rules are irrelevant to other parts of life: good intellectual arguments, for example, don't help much with raising children, nor are they particularly useful if one earns ones living selling socks. Indeed, they may hinder other activities, as scholarship's tacit rules—"Do your own work" and "Get the right answers"—if fully absorbed, often produce professors who are unable to work with one other and who are addicted to being right. We all know the type. Were there no universities to put up with such character flaws, many of us would likely starve.

My point is that all communities are communities-of-endeavor, and they are also communities-of-rules. Besides scientists, there are communities of religious people, communities of sports fans, of hedonists, rock musicians, Internet addicts, and so on—including communities in those mythic places that we most often think of when we use the word "communities", that is, small towns. Each of these communities is engaged in some core activity—worshipping, team-rooting, living together—and each has rules that its members must obey. Those rules are sacred. Think, for example, how we react to news of scientists who fudge their evidence. They have violated the basic rule of our clan, from which they are banished when discovered. It is far more acceptable for a scientist to make a mistake than it is to lie, or to ignore or distort evidence for the sake of making a point. Not that this is not done, but doing so violates our most cherished ideals.

⁹ I have found this story attributed to Stephen Hawking, Bertrand Russell, and William James, among others, but the oldest firm source I've found is John Locke (1690: v1, pp 391—392).

This is my second point about the scientific community: that seeking 'truth' and truth-telling are central to our professional *raison d'être*. Not that we possess truth in any absolute sense; few of us are foolish enough to think that. But we do pursue 'truth' in our professional lives, as opposed to merely pursuing pleasure, career security, or power. Put otherwise, we try, to 'get the picture right' in our various researches. We try to 'understand the world around us as it actually is'. We insist that our statements about the world must have 'warranted assertibility', to use John Dewey's (1941) famous term.¹⁰ That is, we justify our statements by the best evidence available to us. In asserting them, we accept them as provisionally correct, pending whatever future evidence our investigations uncover.

My third point is related. We make scientific progress, collectively, by proving ourselves wrong. For example: a few years ago David Voas, Daniel Olson, and Alasdair Crockett (2002) found a previously overlooked mathematical relationship between the then-dominant index of religious pluralism and measures of religious participation. They discovered that the apparent correlations between the two factors are mathematical artifacts, not real. This threw out the results of some 190 studies and forced the abandonment of the pluralism index—a definite step forward, scientifically speaking. Karl Popper (1932, 1962) famously outlined a formal theory of such scientific fallibilism; we enshrine it practically in our famous "null hypotheses". We recognize that we cannot prove something's truth, but we can prove its falsehood—at least relative to our current measurement and analytic techniques. Assertions become "warranted", to return to Dewey's phrase, when they have been tested by science and have not yet been found wanting. Scientific assertions are not 'truth'; they are just the best we can do at the time.

So far, this is all relatively well-known and, I hope, unproblematic. Scientific fallibilism has two consequences, however, which have received less attention. First, science advances, but it does not do so by looking forward to a known goal. It can't, because we, as humans, do not know what will ultimately prove to be the case. Physicists, for example, have only recently discovered the existence of "dark energy", and they still do not agree about what it is. They agree, though, that it has revolutionized cosmology. Such revolutions prevent us from measuring how far we have yet to go in our

¹⁰ Dewey did not regard what he called "warranted assertibility" as a simple substitute for the term 'truth'; instead, he used it as a less ambiguous replacement for the terms 'belief' and 'knowledge' (1938:9). His emphasis on warranted assertion, however, parallels my concern to treat 'truth' as an eschatological ideal, thus emphasizing the human agency involved in its pursuit without denying that this pursuit is connected with the real world.

truth-seeking. They do not, however, keep us from gathering evidence and disproving—or attempting to disprove—whatever theories seem to be the most plausible at the current moment. We measure our progress retrospectively by seeing how far we have come from those theories that we have, collectively, rejected.

To take an example from the sociology of religion: Peter Berger famously used to argue that the plurality of worldviews typical of modern life makes religion less plausible (Berger 1967). In a recent book (Berger *et al* 2008), he now argues that he was mistaken: modernity's pluralism can produce many different religious responses. Without taking sides in this Berger *vs.* Berger debate, we can note, first, that he has rejected his old theory on evidence that he (and others) find compelling and, second, that this constitutes progress—at least in his eyes. If new evidence were to emerge that convinced him that his original view had been right all along, he would reject his revision and see this as progress too. Scientific progress looks backwards, not forwards. It measures how far we have come, not how far we have to go.

Yet science also has a forward-leading thrust. As an intellectual activity, proving 'falsehood' depends, conceptually, on the notion that it is possible for us to understand things better than we do now. Deciding that we have been wrong requires us to acknowledge that we can be right—or at least right-er than we have been heretofore. If the scientific community, collectively, had no sense that we could improve our present knowledge, then we would not attempt to do so. Science is thus possible because the community of scientists affirms that the process of collecting, examining, and refining evidence and arguments will improve our knowledge. We measure this improvement retrospectively, but we produce it prospectively. We look forward in the faith that our future view of how things are will be better informed than is our present view. We believe that we will be able to measure our progress against where we are now. This belief in the possibility of intellectual improvement, of there being a 'truth' about the world and that we might eventually know it, draws us forward.

I should point out that this belief cannot be proved. It could be accurate, or it could be a confidence game. In fact, we do not know whether our collective researches will come to understand the world better than at present.¹¹ Put somewhat differently, the concept of 'truth' that draws science forward is not a fact but an ideal. The scientific community holds truth-telling as a

¹¹ We do not even know—and cannot know—whether the world of the future will follow the same rules that we are now uncovering. This, too, is an unprovable belief, but not one that I shall pursue here.

moral value controlling scientific practice, but the belief that truth exists also regulates that practice in another way. That belief is a regulative ideal (Kant 1781: 551ff)—a belief that makes scientific progress possible.

Kant argued that certain concepts are, themselves, epistemologically insecure but still worth holding because they make a particular practical activity possible. In his view, the concept of God regulates and makes possible human moral and intellectual life. It is, he said, impossible to demonstrate God's existence, but the idea of there being an ultimate power in the universe allows morality to flourish. Whether he was right or wrong about this does not matter here; the mere fact that ideals regulate human activity is important. As scientists, we cannot demonstrate the truth of our statements. In fact, we can be pretty sure that many of them are false, at least in some degree. But we don't (yet) know which ones are false, nor in which parts, nor do we know exactly how these parts will be corrected. And we cannot, in principle, know these things—not until our empirical investigations bring them to light. Yet our belief in the existence of 'truth'—our regulative ideal—makes it possible for us to keep testing what we know. It encourages that testing, and thus makes it possible for the scientific community to correct its mistakes.

I am, in this discussion, following Charles Sanders Peirce (1877; 1902). To use his somewhat convoluted phrasing, "Truth is that concordance of abstract statement with the ideal limit towards which endless investigation would tend to bring scientific belief" (quoted in Feibelman 1969: 212). We can never reach that limit, but its existence as a goal guides our efforts to achieve it. The very act of trying to do so, of directing our work as if truth were within reach, makes science possible.

This approach to the matter turns foundationalism on its head. Achieving scientific truth is not a matter of placing knowledge on secure foundations. Instead, it is a matter of scientists regulating their activities by a forward-leading ideal—one that tells them that they can have a more accurate picture of their part of the universe than they have now. The ideal of 'truth' does not tell them how to make the picture accurate, for that cannot be specified. Nor does it tell them what that 'truth' will look like when they reach it. It does, however, tell them that an accurate picture is possible, that truth can be attained, albeit eschatologically. That is enough to let the game continue.

This is, by the way, a very different way image of science than that proclaimed by Thomas Kuhn (1970), whose description of the progression of scientific paradigms lacks Peirce's notion of self-correction. Read carefully, Kuhn sees science as a free-for-all in which career-building and boredom with the status quo often shape scientific developments, and for which the inability to measure progress against a stable target leads to the abandonment of any notion of progress at all. His is, at root, a constructivist model of sci-

ence, one with decided free-market tendencies. Among the things that Kuhn leaves unexplained is the certainty in the minds of most scientists that their disciplines do progress, and their belief that those disciplines' current understandings are better than those of the past. Peirce's notion of the ideals toward which scientists direct their efforts puts these aspects of science at the center, while still emphasizing the conceptual change found at the core of every scientific enterprise.

IV

Where are we, in this argument? In the first part of this paper, I presented four research epistemologies currently active in the social sciences, none of which grounds knowledge acceptably. Then I examined four aspects of science-as-practiced, concluding that scientific knowledge is justified not because it is built on a firm foundation but because the process of 'doing science' is governed by a regulative ideal that leads scientists to correct their collective work. Justification is thus retrospective rather than prospective, but it depends on a forward-looking belief that the world exists and is knowable.

Positivism presumes a real, knowable world. Critical realism and standpoint theory agree with caveats, particularly about the kinds of extra-scientific factors that can shape scientific inquiry. Eschatological realism—for that is the term I propose for this epistemological approach—accepts these caveats and adds that we cannot know for sure whether the world is real and knowable until we have completed our researches. Furthermore, we will only find out its status then if we act now on the assumption that she world is real and knowable; that's what makes our investigations possible. Assuming does not prove, but not assuming makes proof impossible.

I hope that I have already said enough to make the term 'eschatological realism' clear. This approach is definitely realist, in the sense that it claims that there is a real world, though it does not claim this as a theoretical *a priori*. Instead, it claims it as a practical a priori, which it believes that science will validate, if pursued to the end of time. That's the eschatological part—to remind us that we're not there yet. It also reminds us that being not-there-yet is a core part of the scientific enterprise.

(Yes, I think that the sociology of religion is a science, though I shall leave discussing that topic to another venue.)

This leaves two questions to ask. The first is whether eschatological realism provides an epistemological justification for critical realism and for standpoint theory. These two approaches, remember, recognize that knowledge is constructed socially and that it is shaped by that construction. Against radical constructionism, however, they argue that this shaping does not prevent us from knowing the world in itself. Science is still possible, if

undertaken with care. Neither approach, however, identifies the epistemological assumptions that must be made, if this possibility is to be realized.¹² Nor does either provide a mechanism by which these assumptions are themselves tested.

This is where eschatological realism fills a void. By specifying that science must posit a knowable real world, whose exact nature it can affirm only retrospectively, this approach avoids the problem of making *a priori* claims about that nature. Simultaneously, science continuously tests its discoveries, rejecting those that do not measure up according to the scientific standards of the time. Knowledge is thus not justified in advance, nor even in the present, but only *a posteriori*—i.e., from a standpoint that has absorbed whatever empirical evidence that testing provides. These standpoints are, ideally, progressive and increasingly inclusive.

This works even on the level of epistemology. Constructionism, for example, pointed out some radical flaws in the positivist view, such that naïve positivism is no longer intellectually tenable. Critical realism responded to radical constructionism’s internal incoherence with enough effect that there remain few radical constructionists in the world, at least few who have thought through the issues at any depth. Standpoint theory built on critical realism by noting that the knower’s social location matters, but it rejected radical constructionism’s related claim that location determines knowledge absolutely.¹³

In this sense, the development of epistemologies mirrors the development of science. Indeed, it is governed by the same mechanism—the ideal of ‘getting the picture right’. It is precisely this governing that Thomas Kuhn’s (1970) “progression of paradigms” missed. He presented scientific development as undirected, ruled only by what the community of scientists accepts at the time. Peircian epistemology argues that the community’s consensus is regulated by an ideal — ‘truth’, ‘getting the picture right’, or however one wishes to frame it. Were there no such ideal, the process would be in principle undirected. With such an ideal, it is potentially directed — though the

¹² Critical realism, at least in Bhaskar’s (1975) version, posits the existence of generative mechanisms that produce the data that sciences collect. It sees science as a process of progressively discovering those mechanisms — as opposed to limiting itself to finding only correlations between events, as is the case with most empiricisms (including positivism). Standpoint theory adds to this the notion that some social locations make it easier to see certain mechanisms than others. The first step — the positing — is left relatively unexamined, as are its consequences.

¹³ Arguing otherwise leads to various philosophical problems, including W.V.O. Quine’s thesis about the indeterminacy of translation. See Quine (1969; 1975).

actuality of its direction can only be known when it has reached its culmination.

Eschatological realism shows us how this process works, how this ideal regulates scientific practice such that secure knowledge is possible. Of course, it is only secure eschatologically—at the end of time. But our regulative ideal gives us a potential means of getting there.

There is, however, an important second question to pose: What consequence does our settling for eschatological as opposed to foundational certainty have for us in the present? For that is all that this form of realism promises: that certain knowledge can be attained an unspecified future, for which I am using the shorthand ‘the end of time’. It does not give us certainty here and now, though I would argue that nothing does. To put the question somewhat differently: How must we orient ourselves today, as scientists, in order to make future certainty possible? Two main elements stand out.

First, faith in science is precisely that: a faith. Frankly, we have no idea whether our investigations will work out, even in the long run. We think we can trace ‘progress’ through the ages, from ideas that ancients thought described the world rather well to those that we now find much better. The natural sciences abound with examples, but social science is not bereft of them. For example, I think it is safe to say that religion will not have disappeared by 2044, two centuries after Marx (1844) predicted its demise. (Marx might counter that class oppression still flourishes, and thus religion, but I doubt that he expected it to last so long.) Nor is religion just “the sigh of the oppressed creature, the heart of a heartless world, and the soul of soulless conditions,” much less “the opium of the people”. As scholars, we have made intellectual progress, though those committed to radical atheism rather than scholarship might disagree.

This point is important. The perspective I am advancing does not argue that progress toward ‘truth’ applies to all ways of being. The idea of ‘progress’ seems rather irrelevant to other life-worlds, including those dedicated to raising children, earning a living, capturing ‘bad guys’, and playing pinochle—each of which is, as I previously noted, just as important as science. The scholarly community’s progress toward ‘truth’ depends on its upholding ‘truth’ as a regulative ideal. Other communities have other ideals that regulate their conduct. I see no reason why progress should apply across the board.

Yet, even the progress that we think we see in our own work—and see retrospectively, I remind you—may well be an illusion. We do not know for certain that science progresses and we cannot know this—not until the end of time. But in fact we, as scientists, live in faith that it does, and we live in the faith that our efforts to ‘get the picture right’ will, in time, produce better

knowledge than we have now. Science thus lives a life of faith as clearly as does any religion.

The second element in our orientation is that we need to treat science as a process, not a product. Science is a way of engaging with the world, one focused on self-correcting inquiry. It is not a set of established findings, nor is it a worldview in any concrete sense of that word. Scientific findings change, perhaps not moment to moment but at least decade to decade. They change in unpredictable ways. We therefore cannot use current scientific facts to justify our views, because those facts may be found false tomorrow. Rather than faith in a set of results, scientific faith is faith in what Mary Douglas (1975: xvii) called "the evolving cognitive scheme". The community of scientists does have reigning paradigms and those paradigms shift over time. The process of doing science, however—of community-based inquiry regulated by the ideal of attaining 'truth'—does not. Our scientific faith is not that one or another result will stand the test of time, but that this regulated process will, in the end, produce secure knowledge.

What does all this have to do with the sociology of religion? Some readers may not like this last turn of my argument, but you will, I think, recognize the logic by which it is driven. I have not argued, here, that science is a religion—whatever that might mean—nor even that science is like a religion, just because both require faith. I find that old argument a conceptual dead-end, so I'm going to run the analogy the other way.

For it strikes me that religion, like science, is a process, not a product. Religion is not most importantly a set of propositions about the world. Nor is it most importantly a set of organizational structures, assigned to that differentiated institutional task of managing relationships with unseen beings (cf. Beyer 2006). Much less are religions at root a collection of groups with weird ideas, whom we study because of their weirdness or because they are good examples of social processes or because they are doing something that we think they have a 'right' to do and thus deserve our intellectual support.

If science is a process of engaging with the world, seeking its 'truth' as a member of an inquiring community, and living in the eschatological faith that these inquiries will ultimately result in secure knowledge, then religion is also fruitfully seen as a process of personal and collective engagement, though for different ends. It, too, is a practical activity grounded in time. It, too, is a collective activity, like childrearing or pinochle-playing—one with its own rules, its goals, and its regulative ideals. What exactly those are does not concern me here; nor does it matter, for my argument, whether the various different religions' rules, goals, and ideals overlap very much, if at all. What is important is to recognize that 'doing religion' involves orienting one's activity toward certain ideals, in the faith that those ideals will lead to one's goal.

This is how H. Richard Niebuhr (1963, 1960, 1989) understood the life of the religious person. In his work, *Christians and Jews* (I would add at least Moslems and perhaps others) find themselves in relationship with a God who accompanies them on their journey. They have mutual responsibilities, one to another, that orient them to life. 'Being religious' is a whole-life activity, not just an affirmation of creeds or of group belonging. It involves a root orientation that structures people's everyday lives—one which sociology frequently fails to capture. Too often, we attend to doctrine and to organizational structures, while losing sight of the processes by which religious people live their lives from the inside (cf. McGuire 2008). Frankly, I think we can do better. On the analogy with science, I think we would better understand religions, were we to inquire about the root orientations that they demand of their adherents. Or, to use a term that is perhaps more apt, we need to understand the orientations of those who have heard their call.

I see a connection, here, with Max Weber's two famous essays on the vocations of science and politics (1918a, 1918b). "Science as a vocation" involves the commitment to a community-based process of inquiry, whose success depends on a regulative ideal of 'truth'. Weber emphasized scientists' commitment to 'truth', though he did not add that scientists must take that 'truth' on faith. Treating religion similarly as a vocation—one which calls people to live in faith towards particular, specifiable ideals—seems to be to be a powerful starting point from which sociologists of can rethink our subject matter.

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