

WHO KNOWS? WHAT CAN THEY KNOW? AND WHEN?

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At a recent symposium on feminism and science, several participants discussed feminist criticism of androcentric bias in developmental psychology.¹ Granting that many of the criticisms were warranted, a psychologist balked at the relationships others found between them and feminism. In reference to one of the issues under discussion, he argued, "*Anyone can see* that you can't build a theory about psychological development from studies limited to males. There is no need to assume there's a relationship between feminism and the ability to see that." I waited for someone on the podium to ask the obvious question: if the problem with an empirical base limited to males—a common limitation in developmental psychology—was obvious, then *why*, prior to the advent of feminist science criticism, hadn't developmental psychologists seen it?²

Maintaining that the problem is and *was* obvious may save some of the assumptions many scientists (and some epistemologists) still hold dear: for example, that evidence wears its identity as evidence (and for what) on its sleeve; that science is a transparent, unproblematic (if difficult) endeavor; and that good science has nothing to do with either gender or politics—views implicit in physicist Sheldon Glashow's self-described article of faith that "any intelligent alien anywhere would have come upon the same logical system as we have to explain the structure of protons."³

But the maneuver has a substantial cost. If the choice of an empirical base in the above case was obviously wrongheaded, then we must conclude either that what is obvious is anything but obvious (except perhaps in retrospect) or that many developmental psychologists *set out* to construct

androcentric theories. Alternatively, we might conclude that feminists are more attuned to what is obvious, are better observers, or are otherwise just better scientists. These are possible but improbable explanations. Indeed, the interesting instances of androcentrism are precisely those in which no scientist missed the obvious, or consciously manipulated or misconstrued the data, or was just less bright than his or her feminist colleagues or critics. Given that there are such cases, and surely we agree that there are, it strains all credibility to assume there is no relationship between feminist politics and the ability to recognize androcentric assumptions, methods, and models—or between androcentric models and theories, and the social and political contexts within which science has been practiced. If our favored views about knowers and evidence deny such relationships, then it is incumbent on us to ask afresh: “Who knows?” “What can they know?” and “When?”

These questions are of current interest at what Linda Alcoff and Elizabeth Potter describe as “the intersections of feminist theory and epistemology ‘proper’,” as well as in feminist theory more broadly.⁴ As such, the questions serve as an appropriate topic of discussion for this symposium. As much to the point, this symposium itself—both in terms of a perceived need to reconsider feminist philosophy and in terms of what we as participants say in it—will have implications for answers to the questions.

In addressing the questions, I start with what seems most obvious to me, namely that the answers to them will not be obvious. They will be theory based, emerging concomitantly with answers to other questions we come to ask in the course of organizing, explaining, and predicting our experience. Alternatively put, these questions emerge—or, better, persistently reemerge—in the middle of things; they are questions asked from here. Today, you and I might agree that answers to them will draw at least on neuroscience, evolutionary biology, sociology and history, and sociological and historical studies of science. And some of us would agree that the answers will draw on the various arenas, including feminist scholarship and politics, in which experiences and knowledge traditionally omitted or devalued are beginning to be considered and theorized about, and in which the implications of their omission or devaluation *and* their current interest are being considered. We don’t need to agree on the details of the above list to agree that answers to the questions before us are not and will not be obvious.

It is also obvious that in addressing these or any other questions, we should not be limited to or by the dictates of what *has* seemed obvious. Subjecting received views to scrutiny, including those we favor, is at least a necessary requirement of objectivity—a point that holds no less for so-called common sense and philosophy, including epistemology, than it does for high-energy physics and economic theory. The general view I will endorse is that we should demand that all accounts of experience offered by feminist

theorists or philosophers (or anyone else) either be compatible with our present understandings of our experiences or offer coherent, if different understandings of them. Support for this view, and a view concerning what might warrant such reconstructions and to what they might appeal, will emerge in my discussion of the questions at hand.

“Who knows?” is a question about epistemology’s agents, a query about the identity of knowers. It need not be a question only about which individuals within a pre-agreed-upon domain of proposed knowers meet our criteria. It can also be a query about the domain itself: whether, for example, it is empty (as some postmodernists, including feminist postmodernists, argue) and, if not, whether it contains individuals—and then whether such individuals are virtually interchangeable (as epistemologists long assumed), or whether they are historically and culturally specific and, at present, gendered (as some feminists argue); or alternatively, as I have argued in another place, whether the elements in the domain are not individuals at all, but groups or communities—and then whether, in specific historical and cultural contexts, such communities are characterized by social relations of epistemological consequence, including, for example, gender, race, and class.⁵

“What can they know?” and “When?” also invite talk of agents, but they are as fundamentally queries about evidence. As this comment reminds us, these three questions are deeply connected; our approaches or answers to one will bear on and be borne upon by those we offer to the others. Consider, for example, the view of evidence implicit in Sheldon Glashow’s article of faith: the view that evidence is wholly independent of us and, at some level, definitive and self-announcing. If evidence were so, we would need demand no more of knowers (at least good knowers) than that they be collectors (and perhaps even just absorbers) of it. Relatedly, if we understand evidence to be something that only individuals can gather and hold, we may find it appropriate to construe the agents of epistemology as individuals, perhaps defending this understanding by reference to the fact that sensory receptors, which serve as our only access to the world, are features of individuals and not, *per se*, of groups.⁶

To be sure, we would need to find a way of accommodating cases in which both the standards and the evidence the standards allow are *obviously* esoteric and *obviously* historically and community specific—consider, for example, the evidence provided for new subatomic particles by electromagnetic tracks in \$65 million collision-detectors, or that provided for evolution by “imperfections,” or, indeed, that which underwrites current claims about proton structure. Were we of a mind to save Glashow’s view of evidence, we might relativize the standards and evidence in such cases to communities of specialists, and work to explicate the notion of their “obviousness” by means of a long (and one would expect complicated but at each step obvious) account of how even such standards and evidence can be

traced (at least in principle) to more immediate events and experiences, accessible (at least in principle) to anyone with the appropriate sensory organs and neurobiology. But I remind us that questions about evidence are always asked *from here*. We have learned, or perhaps relearned, much about evidence in the last four decades that indicates that the view of evidence just outlined is doomed—so we have learned much that is relevant to any current effort we undertake to answer the questions at hand.

We now recognize, for example, that what we say and believe about the social and natural worlds within which we function and of which we are part, far exceeds all the evidence we have or ever will have.⁷ There is “slack,” to use Quine’s term, between *all* of our theories and the evidence we have for them or ever will have.⁸ Alternatively said, it is commensurate with our collective experience that we will eventually abandon our current theories (though not, of course, all at once) for theories that are commensurate with much of our experience to date but incompatible with our present theories.

We have also learned that indefinitely many theories might equally well organize and explain what we experience: that we are not warranted in assuming there is a unique, true (or even most probable) theory of nature awaiting discovery. Put another way, it is commensurate with our collective experience that an alternative theory of nature that did not include Boyle’s Ideal Gas Law (or, for that matter, any “law”), a theory that organized things differently, might equally well explain and predict what we experience.⁹ This is not to say that alternatives are currently viable; Boyle’s Law, for example, is deeply embedded in our current best theories. It is to say that science might have evolved differently.

Another lesson of the last forty years is that there is nothing in our collective experience to warrant the assumption that our sensory organs are sufficiently refined to discriminate a “best” theory or a “most probable” theory (if, indeed, there is such a thing) from alternative candidates. It is commensurate with that experience and with our knowledge that our sensory organs are refined to a degree that (so far) they enable us to survive by organizing and predicting relevant future experience. But there is nothing to warrant the inference that they are adequate to the task of encompassing *all* that goes on—all the rhythms and order, or perhaps an even more basic disorder, of nature.

A fourth lesson about evidence is that the experience and knowledge we bring to bear on the theorizing we undertake in philosophy, science, and other arenas, including, of course, common sense, will *include* experience and knowledge shaped by the social relations of gender, race, and class that currently characterize our society. And, as many of us have argued, we cannot take the lesson of, say, feminist science criticism to be that stricter methodological controls are needed to “filter out” these factors and relationships in science, for the factors and relationships are surely

present in feminist science critiques.¹⁰

One can view these lessons as the bad news. The first three underscore the Hobbesian view that the inn of evidence, like that of truth, has no signpost. The various theories we construct to organize, to explain, and to predict experience are, as Quine makes the point, "bridges of our own making," underdetermined by all the evidence we have or ever will have. The fourth lesson broadens the factors relevant to our construction of knowledge, including that undertaken in science communities, to encompass social relations, politics, values, and other factors long regarded as a threat to objectivity, if not its death knell.

But denying these lessons, holding on to the view that there is one most probable theory or that there are real boundaries between "serious" knowledge and the social and political relations that characterize our society, would be—from here—at best an article of faith, no more warranted, no more defensible, than any other article of faith.¹¹ It is far more reasonable to reconsider those aspects of our views, including those concerning agents and evidence, that were predicated on assumptions that we are now in a position to recognize as untenable.

I also note that although we no longer have the option of believing that knowledge will someday be complete or of denying that gender and politics have anything to do with serious science and philosophy, what we make of either or both of these lessons remains an *open* issue—deeply contested in feminist theory, including work at the intersections of feminism and philosophy, as well as in so-called "mainstream epistemology." To maintain that knowledge is socially constructed, and that gender and other social relations are somehow related to that construction, is not to answer the question of what, if any, empirical constraints govern the building of knowledge; nor is it to specify the nature of any such constraints. To hold, for example, that gender is related to science leaves open the question of whether the relationship lately discovered is appropriately construed as one between, say, scientific practice and an attribute of individual scientists (as some, but by no means all, feminists argue); or, as I have argued elsewhere, between scientific practice and a complex web of historically specific social relations; or, as some feminists and postmoderns argue, between scientific practice and a category so deeply a matter of social construction as to be of little theoretical use.¹² In short, maintaining that knowledge is socially constructed and that social relations are of epistemological significance does not itself constitute a theory of evidence or a substantive refutation of the notion that we need such a theory. So we return to our more immediate topic, the reemergence of the questions, "Who knows?" "What can they know?" and "When?" But we now approach these questions, I hope, with the understanding that our theories, including epistemology, evolve in response to our experiences, and that it is time, based on the experiences of the last four decades, to rethink traditional epistemological stances.

In the space remaining I will sketch, in broad outline, the approach I would take to the specific question with which my discussion began, of whether it was or is obvious that basing a general theory of psychological development on studies limited to men is wrongheaded, and I will relate my approach to some others discernible in feminist discussions. It should go without saying that many feminists will disagree with the frame I have attempted to put on the three questions before us, as well as with my interpretation of their own work.¹³

My answer to the above question is, of course, "No, it was not obvious that limiting the empirical base to males would produce at best partial, at worst distorted results." The answer is underwritten by a view of evidence which builds on the developments I sketched earlier and on Quine's arguments for holism.¹⁴ There is, on this view, no reason to posit a discrete piece of evidence missed by developmental psychologists and lately discovered by feminists in that field.

Underwriting the research in question was a larger body of psychological theory, with its own methodology, standards, and history. This containing body of theory and accepted practice constituted part of the evidence for the general notion of psychological maturity, for specific models thereof, and for the assumption of discrete developmental stages. Within this containing theory and the psychological tradition, feminist scholarship has revealed, there was a tacit and consequential assumption that men can serve as the norm or model for the species. This assumption underwrote the methodology now criticized and made it reasonable in testing a theory of psychological development to either ignore or discount what seems to us obvious counterevidence—the women who didn't fit the model—or to conclude that women's development is truncated or deviant.

If psychology had been the only discipline making this assumption, it might have been plausible, if not obvious, that something was badly amiss. But, to a large extent, developmental psychology derived (as it will always derive) its empirical significance, explanatory power, and plausibility by being doubly embedded, in a broader psychological theory and an even broader system of going theories and standards. And, it turns out, a general assumption that males can serve as the norm for a species was tacitly made by many other sciences and disciplines: it underwrote organizing principles and research questions; it was interwoven in various theories; and it has been, of course, implicit in much of so-called common sense, supported by and reinforcing social and political relations and practices.¹⁵

It would be a mistake to conclude that the only support for the developmental theories in question derived from their coherence with a larger system of theories and practices within which they were embedded. These developmental theories did make room for and indeed claimed to be based on experience: they both organized and were compatible with experiences, and they had explanatory power—they allowed for explanations

and predictions about some of what happens. The problem, of course, was that the experiences on which they were based and against which they were tested, represented, from the outset, an unrepresentative subset of human experiences—not only in terms of gender, but also in terms of class, race, and culture. Moreover, when we evaluate the experiential base today, many of us bring to bear *the fruits* of changing social and political relations of the last three decades, including feminist theory broadly and feminist science criticism in particular. In short, the experiences and judgments that are now *possible* and *relevant* to claims about psychological development were not always even possible.

The thesis I have been trying to illustrate can now be simply stated: our evolving theories, standards, and practices, narrow and broad, shape and mediate the range and nature of the evidence available to us. Which experiences are relevant to our current investigations, what assumptions and claims are obvious, what objections are relevant, are thus also so mediated and shaped. Accept this thesis and it becomes obvious that what is *very obvious today* can easily have been *anything but obvious* ten or twenty years ago. We need not convict earlier researchers of either conscious biases or the practice of bad science, as the latter was traditionally understood, to account for our being able to see clearly what they were blind to.¹⁶ From this it follows that reevaluation is almost always in order. (It also follows, of course, that it is no more in order for feminist scholarship than for any other area.)

My approach has much in common with those of other feminists, including feminist empiricists, standpoint epistemologists, and pragmatists. In terms of the first two views I will note, it also has much in common with other current approaches in epistemology and philosophy of science, particularly Quine's approach. The most general shared view is that the limitations and the inclusiveness revealed in the last four decades are immanent—products of our own efforts to explain things—so that, as I would put the point, evidence remains a substantial concept, to be explicated, at least in part, in terms of the relationships between experience and knowledge. A second common view, supported by the development and evolution in feminist scholarship but also by the history of science, is that general and specific standards of evidence, and hence what we will countenance as evidence, emerge concomitantly with our efforts to explain and predict experience. They are neither self-evident nor transcendently derived.

From these two views, it follows that the picture of knowers as solitary absorbers of evidence is untenable, its collapse fundamentally related to the collapse of the view that evidence is self-announcing and wholly independent of our efforts to explain our experience. Many feminists grant knowers an active role in the generation of knowledge; of equal importance, many insist that knowers are situated—historically, culturally, and in relation to community-specific standards, practices, relations, and knowl-

edge, including (but not exhausted by) political views, relations, and practices—and we insist that experience is made possible and shaped by such standards, relations, and knowledge.

One view of the question, “Who knows?” then, discernible in recent feminist discussions, is that acceptable answers include “Everyone,” “*Some of us*,” “All of *us*,” but only very problematically, “Only me.” What you or I know depends inextricably on the knowledge, standards, and practices of the various epistemic communities of which we are members, and these and the experiences they permit will form the basis upon which both you and I and our respective communities will judge our claims, as well as those of others. And it is on the basis of our current standards and knowledge, and the experiences they make possible, that we will—as philosophers or feminists or both, as feminists or developmental psychologists or both—reconstruct our prior understandings of our own or others’ experiences to make the most overall sense—revising, if need be, our views about who knows, what they know, and how.

The disagreements I have mentioned and the reconstructions I have advocated need not conjure up the demon of incommensurability. Those who disagree with parts or all of my analysis, for example, can understand what I have said, and at some level we can know what it is we disagree about and why; for in addition to those views and assumptions about which we may disagree, there are many more we share (e.g., physical-object theory, a heliocentric view, and so on). Both these, and our future experiences, will shape our future interest in and answers to the questions before us and others. Alternatively said, although feminists and nonfeminists within various academic fields and sciences disagree about many things, indeed although feminists disagree about many things, these groups do not disagree about everything. Feminist communities and academic and scientific communities are subcommunities of larger, more inclusive epistemic communities; moreover, there are overlaps (i.e., feminist philosophers).

Relatedly, such disagreements and reconstructions need not conjure up the demon of relativism, here understood as the view that all claims are equally warranted. There are two general constraints on knowledge and claims: experience, and larger systems of knowledge and standards.¹⁷ As the case we have considered indicates, not all theories or methodologies are equally commensurate with what we know and experience.

By now, it may be clear that these several points carry implications for this symposium, and it is to some of these implications that I devote my concluding remarks. There are two ways to understand an invitation to “reconsider” feminist philosophy. Given that there is no monolithic enterprise denoted by that phrase, let alone a completed body of knowledge, perhaps the advocates of the project believe that the reevaluations and reconstructions feminists have undertaken of aspects of philosophy, science, literary theory, common sense, and so on, and those they might in

the future undertake, are *in principle* out of bounds. I assume you and I agree that if this is the motivation for this symposium, it is fundamentally inconsistent with what philosophy professes to be.

An alternative construal of the task envisioned (although belied by the title the planners chose) is that we were being invited to undertake an evaluation of one or several aspects of feminist work in philosophy—say, feminist theories about the philosophy of science or some particular science. Were this the intended project, a more narrowly focused discussion would have been in order. In that discussion, we might have been able to discern some common assumptions underlying the work in question. And in such a context, evaluating the assumptions, questions, and answers discernible in some reasonably defined range of feminist theories about science is *in principle* a reasonable undertaking.

The “in principle” is crucial here. I have argued that a major lesson of the last four decades is that all questions are asked and answered *in medias res*: that the criteria by which to judge their reasonableness, as well as answers proposed to them, will always be relative to a going body of knowledge, standards, and practices, and the experience these shape and allow. In terms of the questions I have discussed, for example, we have found that answers to them are not starting points but radically interdependent with other things we know and other projects we undertake.

Hence, it is incumbent upon those who would have us reconsider some aspect of feminist theory, to make clear—and the planners of this symposium have not—where the proposed reconsideration is *to issue from*. Are we, for example, to reconsider feminist critiques of the philosophy of science from the vantage point of traditional empiricism or positivism? The rationale for and the worthiness of the reconsideration, the relevant criteria, and the obstacles to understanding would be quite different from a reconsideration that started out from a view of empiricism along the lines that Quine advocates, or van Fraassen, or Kuhn, or from a postmodernist perspective, or from the vantage point of critical theory. Those who would engage us in a reconsideration of some aspect of feminist scholarship, or who would interest us in their reconsiderations, need to tell us from where—with what questions, against which standards and knowledge, and with what understandings of evidence—our deliberations are to begin, so that we may judge whether the project is worthwhile.

My own sense, given that the answers to the questions on which I have focused are both central to our efforts and neither obvious nor self-evident, is that the only *wholesale* appraisal of the work being undertaken at the intersections of feminism and philosophy worth paying attention to will be provided by the long-term success or failure of feminist approaches to these questions: their coherence with what we come to know and experience, and their explanatory power.

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Postscript: The Hard Work of Epistemology

There are several substantive issues which divide Professor Haack and myself that are well worth discussing. These include whether it is individuals or groups who are the primary acquirers and bearers of knowledge, whether the underdetermination of theories is a substantial doctrine which has as a consequence that there is no one true theory, whether epistemology shares a radical interdependence with our other best going theories, whether the notion of a value-free science is either coherent or desirable, and the consequences of a strict fact/value distinction for value theory.

Unfortunately, attention has been paid to issues which are not worth debating and on which Professor Haack and I are in agreement, her belief to the contrary notwithstanding. These include the centrality of evidence to scientific investigation and the centrality of experience to evidence,¹⁸ the undesirability of letting politics be the arbiter when available evidence is inconclusive,¹⁹ the lack of evidence for sex-differentiated cognitive abilities,²⁰ and the absence of any clear viable alternative to empiricism.²¹

Were we to discuss the issues worth discussing, we could perhaps come to agree on them—or at least come to an understanding of what actually divides us—and we could perhaps come to agree—or at least come to understand why we cannot agree—about whether feminist empiricism is, as I contend, a significant form of empiricism, or, as Haack suggests, pseudo-empiricism.

For the present, it must suffice to insist that reasonable judgments as to the viability or lack thereof of the several and diverse research projects at the intersections of feminism and epistemology require just as much hard work as do serious judgments in other areas of epistemology.²²

March 24, 1993

1. "Colloquium on Science, Technology, and Culture," sponsored by The Center for the Critical Analysis of Contemporary Culture, Oct. 22, 1991, Douglass College.

2. I have chosen a rather straightforward example; the androcentrism implicit in research methodologies and theories revealed by feminist criticism is often far more subtle. Even in this case, the problem was deeper than my comments here indicate. When girls and women were studied using the models generated by the studies under discussion and did not "fit" the models, developmental psychologists often concluded that their development was truncated or deviant. See Carol Gilligan, *In a Different Voice: Psychological Theory and Women's Development* (Cambridge, MA: Harvard University Press, 1982); Sandra Harding, *The Science Question in Feminism* (Ithaca, NY: Cornell University Press, 1986); and Lynn

Hankinson Nelson, *Who Knows: From Quine to a Feminist Empiricism* (Philadelphia, PA: Temple University Press, 1990).

3. Quoted in "Does Ideology Stop at the Laboratory Door? A Debate on Science and the Real World," *The New York Times*, Oct. 22, 1989, Section 4, p. 24. Ironically, Glashow's version of "science's credo" is inconsistent with the more sophisticated views outlined in recent publications by professional science associations. See, for example, Committee on the Conduct of Science of the National Academy of Sciences, *On Being a Scientist* (Washington, DC: National Academy Press, 1989).

4. Linda Alcoff and Elizabeth Potter, eds., *Feminist Epistemologies* (New York and London: Routledge, 1993), Introduction.

5. Nelson, *Who Knows*. See also Helen E. Longino, *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry* (Princeton, NJ: Princeton University Press, 1990); and the essays in Alcoff and Potter, *Feminist Epistemologies*.

6. One might also argue that abandoning individualism either severs the connection between claims about that world and the evidence it provides, or puts off its explication unnecessarily (e.g., Philip Kitcher, "Socializing knowledge," delivered at the American Philosophical Association meeting, New York, 1991). I have argued against both views in Lynn Hankinson Nelson, "Epistemological Communities," in Alcoff and Potter, *Feminist Epistemologies*, pp. 121-59; and in my *Who Knows*.

7. The next several paragraphs parallel arguments I offer in Nelson, "Epistemological Communities."

8. W. V. Quine, "Posits and Reality," *The Ways of Paradox and Other Essays* (New York: Random House, 1966), p. 241.

9. The claim is, of course, a consequence of underdetermination, not of Quine's indeterminacy thesis. W. V. Quine, *Word and Object* (Cambridge, MA: MIT Press, 1960); Elizabeth Potter, "Gender and Epistemic Negotiation," in *Feminist Epistemologies*, pp. 161-86; and Nelson, "Epistemological Communities."

10. The literature revealing these relationships and exploring their epistemological implications is extensive and heterogeneous. See the works listed in nn. 2 and 5.

11. Nelson, "Epistemological Communities."

12. Several recent issues of *Hypatia* are devoted to the nature and viability of the analytic category gender. Unfortunately, some critics of feminist philosophy fail to distinguish the different projects, assumptions, and claims that work in this area encompasses.

13. It should go without saying and I regret that it does not.

14. See also Nelson, *Who Knows*, and "Epistemological Communities," and Lynn Hankinson Nelson, "A Question of Evidence," *Hypatia*, vol. 8, no. 1 (April 1993).

15. The works listed in nn. 2 and 5 provide overviews of the literature documenting the pervasiveness of the assumption and its consequences.

16. The phrase "bad science as traditionally understood" is meant to capture the demands long taken to be not only necessary to, but sufficient for, good scientific practice. I argue in *Who Knows* that one implication of feminist science criticism is that these demands need to be supplemented with self-conscious and rigorous attention to the ways in which so-called common-sense experience, values, and politics shape and are shaped by scientific practice.

17. I explicate and support Quine's arguments for a "coherence account of evidence" in Nelson, *Who Knows*, pp. 20-29, 108-18, 244-54; "Epistemological Communities," pp. 129-42; and "A Question of Evidence." I do not argue, as Professor Haack claims, that Quine advocates a "coherentist" account. In *Who Knows*, I argue:

Coherence emerges as an overarching criterion of evidence in Quine's positions and, as explicated and implied in these, it is a dual constraint. Theories and beliefs need to be consistent with our experiences of the world and with other going theories. The first constraint distinguishes Quine's criterion of "coherence" from idealist or skeptical "coherence" accounts and, specifically, from "coherence theories of truth." Simply put, the world matters. The second constraint incorporates Quine's view that episte-

mology is one theory in a larger network of going theories. (pp. 25-26)

This account of evidence is, of course, part and parcel of Quine's work from "Two Dogmas" to *The Pursuit of Truth*.

18. Nelson, *Who Knows*, pp. 1-42, 82-124, 244-49, 271-75; "Epistemological Communities," pp. 129-42; and "A Question of Evidence."

19. Nelson, *Who Knows*, pp. 145-66, 238-49, 270-75, 305-9; "Epistemological Communities," pp. 142-51; and "A Question of Evidence."

20. Nelson, *Who Knows*, pp. 193-205; and "A Question of Evidence."

21. Nelson, *Who Knows*, pp. 1-42, 82-136, 222-54, 270-75, 309-17; and "A Question of Evidence."

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