The Epistemic and Moral Authority of Science:

A Discussion of Theodore L. Brown's *Imperfect Oracle*

ABSTRACT Key Words: authority and consensus, epistemic authority of science, moral authority of science, expert scientific opinion, scientific autonomy, science as a network, metaphors for science as an institution. *This book discussion focuses on Theodore L. Brown's* **Imperfect Oracle**. *Richard Moodey, a sociologist, and Jay Labinger, a scientist, raise questions about some of Brown's views on the epistemic and moral authority of science and Brown responds.*

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Individual or Institutional Authority in Science?

Jay A. Labinger

In the Preface to this exploration of the relations between science and society, Ted Brown suggests that science exerts *less* influence than one might expect, given its central role in modern culture, and proposes to try to understand that state of affairs by examining the nature and origin of scientific authority (ix-x). The main text is divided into two sections, respectively titled "Foundations" and "Science in Society." In the first, Brown outlines a categorization of various types and sources of authority, and traces the historical evolution of scientific authority from ancient Greece through modern times. The second section offers more detailed examinations of science's interactions with four segments of contemporary (mostly American) society: the law, religion, government, and public affairs. A final chapter, "The Prospects for Scientific Authority," draws these threads together, and offers some modest suggestions for improvement.

In both his historical survey and the individual topical studies, Brown provides concise but compelling accounts of where we are today, and how we got there. The material and its presentation are commendably informative, as well as entertaining. However, those looking for an overarching explanatory schema will soon realize — as Brown himself clearly does — how elusive that goal remains. Historically, the locus of scientific authority is presented as quite diachronic: while to a large degree there has been a gradual shift from individual scientists to a more institutionalized "science," some individuals continue to hold on to a disproportionate share even today. More importantly, it is far from clear what determines, in any given situation, which scientists will be perceived as particularly authoritative. Brown cites the "fateful" example of Einstein's letter to Roosevelt, calling for development of a nuclear weapon: although Einstein had no particular expertise in the field, he was able to get the president's ear — which the true experts probably could not have done — because of his "enormous charismatic authority" (85). Whence comes such charismatic authority? Brown implies it was a combination of factors, unique to Einstein's particular case. How then can we explain, much less predict, where authority will reside in any general circumstance?

One difficulty lies, at least in part, in the emphasis Brown places on the distinction between epistemic and moral authority — expressed concisely as "the capacity to convince others of how the world is" vs. "the capacity to convince others how the world should be" (23). In the first place, the difference is not so clearcut as Brown would have us believe. His discussion of Pasteur, which is featured prominently in the historical account, provides a good illustration (60-66). Having recognized the role of microorganisms in many diseases, Pasteur recommended that physicians follow sterile procedures; according to Brown, "By instructing the members of the august Academy as to what they *should* do in the course of surgical work, Pasteur was exercising *moral* authority" (63). But that seems debatable. I expect Pasteur would have believed that he was exercising *expert* authority, by trying to teach them what needed to be done to prevent disease. If his advice was not well received (as it was not), that would represent resistance to his claim of expertise, not his moral authority; the latter would be invoked only if the question of whether disease should be prevented were at issue. Is this hairsplitting? Maybe a little. But surely the difficulty of determining whether and how moral authority follows from expert authority is exacerbated if the line between them cannot be clearly drawn.

A further complication is that the important distinction between *trying* to exercise moral authority and actually succeeding is not always kept clear. Brown speaks of moral authority as "the license to argue convincingly about how the world should be" (270). Who issues, or needs, such a license? Anybody is free to argue about anything; whether the argument is convincing depends at least as much — probably much more, in most cases — upon those at whom it is aimed. Brown mentions Linus Pauling among those whose scientific authority (recognized by the 1954 Chemistry Nobel Prize, among other honors) could be extended to the realm of public policy (22, 25, 91). However, his arguments against nuclear testing were (initially, at least) much more convincing to the Nobel Peace Prize Committee than to the US government and public — according to Wikipedia, a headline in *Life* magazine referred to his 1962 Nobel Peace Prize as "A Weird Insult from Norway" — while his efforts on behalf of Vitamin C, a topic much closer to his field of scientific expertise, ultimately proved fruitless.

It is also telling that we see much more of how scientific authority is challenged than of how it is accepted in all four of the chapters on social institutions, even though the institutions themselves operate very differently. Only in the case of science and religion are the two sides portrayed predominantly as natural adversaries, locked in a contest for authority; no such conflict seems to be inherent in the relationships between science and the law, government, or public interest. Indeed, much of the chapter on science and the law presents the legal and scientific establishments as natural partners, engaged in the largely *cooperative* efforts of defining the role of scientific expertise in the courts and dealing with the significant differences between the two domains. (Most important among the latter is, perhaps, the fact that in their respective quests for knowledge, the law relies *much* more heavily on procedural rules than does science.) But because legal proceedings are in and of themselves highly adversarial, in any particular case at issue claims of scientific authority will often meet resistance no less vigorous than in any science vs. religion dispute.

Much the same is true of the chapter on science and government: opposing parties (in the general sense of the word) will try to enlist or discredit scientific authority to further their position, with economic and political considerations playing a much larger role than any factors that could be considered internal to science. Brown's discussion of the "ozone hole" controversy illustrates this beautifully (218-227). Molina and Rowland's initial paper, claiming possible damage to the ozone layer by CFCs, appeared in 1974; its scientific merit was promptly challenged by (among others) DuPont, a major manufacturer. The first regulatory action was issued in 1977, by which time only a limited amount of additional scientific evidence had been accumulated. Nonetheless DuPont had already changed their position and backed regulation, in large part because they had taken the lead in

developing and producing substitute refrigerants, and saw themselves as well placed to profit thereby.

One issue, which is considered at some length in the chapter on government, does not seem so inextricably linked to political or economic battles: the question of the autonomy of science. But here again there is a problem related to definition of terms: are we talking about autonomy as something that belongs to science as an institution, or to individual scientists? And to what degree do the demands of those two attributions come into conflict? Brown quotes Polanyi ("The Republic of Science" in *KB*, 59) as seemingly unconcerned by any potential conflict: "The authority of scientific standards is thus exercised for the very purpose of providing those guided by it with independent grounds for opposing it....Scientists exercise their authority over each other. Admittedly the body of scientists, as a whole, does uphold the authority of science over the lay public." Brown suggests that, although the "elitist overtones" may sound out-of-date, the vision is still valid (104-106); but I'm not sure I would agree.

For example, in his lengthy consideration of the issue of climate change, Brown comments that "the exercise of expert authority depends on the perception of a scientific consensus. When there is an impression that scientists are in significant disagreement on a scientific issue, expert authority wanes" (232). The key question is: what counts as "significant" disagreement, and what does it take to create an impression thereof? Notably, in this controversy, it doesn't seem to take very much! Those "climate change deniers" who contest the existence of a reliable scientific consensus are able to cite heterodox opinions to good effect; the relative numbers and reputations of the scientists on the opposing sides don't appear to matter very much at all.

I suggest that this is a consequence of the existence of an *inherent* conflict between individual and institutional autonomy, which Polanyi and other have largely swept under the rug. Brown again cites Polanyi (104; a paraphrase this time): "Discoveries of the greatest ingenuity are often those that break with accepted communal beliefs. It is this balance, between the guiding role of professional standards and challenges to them, that imparts an authority to science." But the public mostly sees only those individual scientists who make the ingenious discoveries. The communal beliefs, the professional standards and the balance are virtually invisible, in either the news media or popular representations of science in movies, TV, etc. It is perhaps not surprising, then, that heterodoxy is disproportionately accepted in contests of authority, or that the "balance" can, far from imparting authority, often tend to undermine it.

What, then, is to be done? Brown explicitly states that his aim is to be more descriptive than prescriptive, but he obviously feels (as noted above) that science *should* play a stronger role in society. His main suggestion is that we need better public understanding, not so much of the *content* of scientific discovery, but of how the scientific enterprise works: "a turn toward more personal presentations of science, including narratives that relate stories of scientists at all stages of their scientific development, will make science more approachable....A more personal approach that relies on narrative as well as logical argument establishes closer connections with nonscientists, and in doing so enhances science's moral authority" (291-292). I strongly agree with that proposal. I would only delete the word "moral," not only because of the problematic definition discussed above. Improving public awareness of the *human* nature of scientific work, and the importance of an interdependent community as opposed to a small cadre of superstars, could be beneficial for all the ways, in all the arenas, that science and society come into contact.

Institutional Science as Person or Network?

Richard Moodey

Imperfect Oracle is an attempt by a scientist to exercise epistemic and moral authority. Ted Brown defines epistemic authority as "the capacity to convince others of how the world is," and moral authority as "the capacity to convince others of how the world *should be*" (23). He attempts to convince readers of how one aspect of the world is by describing and explaining the historical origins and the current state of scientific authority. He attempts to convince readers of how the world should be by arguing that science ought to have greater authority. In the last three sentences of the book, he sums up much of what he thinks the American world is and how he thinks it should be:

> Scientists and engineers have been instrumental in creating contemporary American society. They have done so by informing us about how the world is. It is vital to the future of humanity that they more fully apply their understandings and skills to helping us decide how the world should be; to forming a just, liberal, and livable society (294).

By writing *Imperfect Oracle*, Brown has practiced what he preaches, applying his scientific "understandings and skills to helping us decide how the world should be."

For me, Brown's moral authority is greater than his epistemic authority. He has been more successful in convincing me of how the world *should* be than he has been in convincing me of how the world *is.* His moral arguments support convictions I had before reading *Imperfect Oracle.* I agree with him that scientists and engineers should pay more attention to making their knowledge accessible to leaders in other institutional domains and to the general public, and I agree with him that leaders and the public should pay more attention to scientific knowledge. He has been less successful in convincing me of how the world is because some of his theoretical propositions are inconsistent with two of my long-standing convictions. First, I believe that it is a mistake, in social science discourse, to write about collectivities as if they were big persons. Second, I am convinced that authority is not something that inheres in a person or collectivity, but is a *relationship* between a social actor and one or more other actors.

Before digging more deeply into my disagreements with Brown, I want to note that I find his stories to be quite convincing. In Part One, "Foundations," he describes the history of scientific authority in Europe and the United States. His chapter on "Scientific Authority in Contemporary Society" also contains chronological narratives of concrete events. He tells stories about Greek science, medieval and early modern science, science and warfare, and science and business. In Part Two, "Science and Society," he has chapters on scientific authority in relation to three specific institutional domains – the courts, religion, and government. His penultimate chapter is on "Science and the Public," and his final chapter is on "The Prospects for Scientific Authority." It is in this chapter that his advocacy for increased scientific authority becomes most explicit. In Part Two, he tells stories about legal controversies over expert witnesses in court, fingerprinting, DNA identification, recombinant DNA, toxic substances, cloning, stem cells, evolution, contraception, abortion, ozone depletion, science education,

and the media. In his telling of these stories, Brown avoids letting his advocacy for increased scientific authority become a bias. He recognizes the legitimacy of non-scientific authority as well as recognizing internal problems within science.

I can illustrate the contrast between my appreciation of his stories and my dislike of some of his theoretical propositions by the story he tells in his introductory chapter, and by the theoretical assertion he makes about the story. In 2001, Eddie Joe Lloyd was released from prison after serving seventeen years of a life sentence for murder. Participants in the Innocence Project at Yeshiva University had worked on his case for several years, gathering evidence, some of which was subjected to DNA testing. Representatives for the Innocence Project presented their findings to the Detroit Police Department and the Wayne Country Prosecuting Attorney's Office. Representatives or agents of these organizations decided that they would add the authority of their organizations to that of the Innocence Project by joining the Project in filing a motion to vacate Lloyd's conviction. One or more officers of the court, including one or more judges, read the motion and ruled favorably upon it, resulting in Lloyd's release (1-2). Brown here tells a story about real people acting and interacting, often in their roles as agents or representatives of organizations. Reading it convinced me that it is an accurate account of what actually happened. I did not say to myself, "I wonder if this is how it happened; I'm going to Google it, to see what other sources say. "

After this convincing narrative, Brown raises a good question about the story, but answers it by asserting something with which I disagree. He asks why it was that DNA testing, a laboratory procedure that few people really understand, had – and continues to have — such powerful effects. He answers: "Society simply takes the word of scientists for all that – it accepts science's authority" (3). Brown boils down all the actions and interactions of the real people into an interaction between science, imagined as an entity capable of speaking a word, and society, imagined as an entity capable of hearing and accepting the word spoken by science. I contend that this language misrepresents how the world is. There are no interacting "big persons" named "science" and "society," capable of speaking and listening, arguing and being convinced. Brown has not convinced me to abandon my long-held conviction that in social scientific analysis, it is a serious mistake to think and write about collectivities as if they were big persons. It was not society that submitted to the authority of the scientists who developed the basic science behind DNA testing, or to the authority of the technicians who conducted the tests on the biological materials, or to the authority of representatives of the Innocence Project, who vouched for the authenticity and relevance of the materials tested in the laboratory. Rather, real people who acted as agents of the police department, the prosecutor's office, and the court read the motion prepared by agents of the Innocence Project, and agreed that Lloyd was mistakenly convicted. These individuals had the power to act as they did because of the positions they held in their respective organizations. Neither science nor society performed any actions.

If Brown had made the same assertion in a casual conversation, or even in popular journalistic writing, I would not object to his saying that society accepted science's authority. In common sense discourse, we regularly use the big person metaphor for collectivities, and only a fool would attempt to reform common sense discourse. But in scientific discourse, it is important to get the words right. Members of scientific networks monitor and criticize what others in the network say and how they say it. Brown repeatedly praises Michael Polanyi's analysis of this process in "The Republic of Science"[1] (26; 94; 104-107; 266; 274-5). In common sense discourse, we regularly speak of the sun rising in the east and setting in the west, but that way of expressing the relations between the earth and the sun is not acceptable in scientific discourse. Writing about collectivities as if they were big persons should not be acceptable in social scientific discourse.

Brown repeatedly uses the big person metaphor to write about the relations between science and society. In his preface, for example, he says that he uses "the concept of 'authority' as a kind of lens through which to view science's interactions with the larger society" (x). Brown expects his readers to assume, as he does, that "science" and "society" refer to the kinds of things that can interact with one another. I say that he expects his readers to assume this, because I have not found any passage in *Imperfect Oracle* in which he presents arguments or evidence that "science" and "society" refer to entities that have the ability to interact. I contend that these words refer to highly abstract ideas that cannot interact. Each of us experiences interactions daily, and we can observe the interactions of others. Treating science and society as if they were big persons implies that they can interact, even though we can't see them do it. By saying that "authority" acts as a kind of lens, Brown suggests that authority is something like a microscope or a telescope that enables us to "view" interactions we are unable to see with our naked eyes.

When Brown focuses explicitly on what he means by "science," he does not say that it is a "big person:

> I will not attempt here what may in any case be impossible: to produce a kind of litmus test for what constitutes "science" or "scientist." Nevertheless, I think that we can draw some boundaries, albeit rather flexible or even in some cases indistinct, around a body of knowledge and current practices that represents a characteristic outlook toward the physical world, acceptable approaches to the study of nature, an avowed ethic of practice, commitment to critical evaluation of findings, and a commonality of social practices of communication, such as peer-reviewed journal publication (11-12).

This seems reasonable, but it does not seem reasonable to assert that "a body of knowledge and current practices" interacts with society. Of course, Brown does not say this. When he personifies science, he slips into a common sense way of talking about it and ignores the inconsistency between the big person metaphor and what he says explicitly about the meaning of "science." The big person metaphor is also inconsistent with Polanyi's image of science as a network, within which scientists in overlapping neighborhoods monitor one another's work (104-107).

My criticism of Brown's personification of science and society does not come from a belief that metaphors have no place in scientific inquiry. I agree wholeheartedly with his emphasis on the importance of metaphors in understanding and expressing abstract ideas. In *Imperfect Oracle*, Brown summarizes the argument he makes at great length in *Making Truth: Metaphor in Science*: [2]

> For the most part, abstract ideas are understood in terms of core metaphors and concepts that form a largely unconscious set of understandings that we apply to interpretations of experience.[3] Embodied realism essentially dismisses much of the traditional arguments regarding reality and definitions of truth as largely irrelevant to an understanding of the nature and limitations of human knowledge. It affirms the idea that there is a mind independent physical world, but contends that our understanding of it in terms of abstract concepts is expressed metaphorically, drawing on the core physical and social experiences that make up our lives. Thus, there is no general, abstract, truth-bearing language that states how the world is. Rather, scientific observations and concepts are understood largely in metaphorical language (96-7).

I criticize Brown's use of the big person metaphor, not because I object to using metaphors to express our understanding of either the physical world or the social world, but because I think the big person metaphor is the *wrong* metaphor to use in our attempts to understand and write about science and society. I think that the network metaphor is heuristically much more fruitful.

The situation is a bit like one Brown discusses in *Making Truth.* He describes J.J. Thomson's "plum pudding" model of the atom. The positive charge of the atom was analogous to the pudding, and the negatively charged electrons were analogous to the plums scattered throughout the amorphous mass of pudding. "By 1910," Brown reports, "Thomson's plum pudding model of the atom had taken center stage." But in 1911, Ernest Rutherford published a paper that introduced a different metaphor or model, in which the positive charges were all concentrated in a tiny nucleus, and the electrons were outside of the nucleus, surrounding it and occupying most of the volume of the atom. The plum pudding model was gradually abandoned in favor of a model that was more consistent with empirical observations and held out greater potential for further development.[4]

Polanyi's network metaphor offers greater potential for further development than does the big person metaphor. This is the case for both "science" and "society." The heuristic potential of a metaphor depends upon what Mark Johnson and George Lakoff call "cross-domain cognitive mapping."[5] The cross-domain mapping suggested by the big person metaphor leads to looking in the collectivity for analogues to parts of a person – head, brain, heart, hands, etc. None of these analogies are very fruitful. The mapping suggested by the network metaphor leads to more fruitful analogues. Individual persons correspond to the knots or nodes in the network, and social relationships and/or social interactions correspond to the strings connecting the knots. This opens up network analysis to systematic treatment of the implications of different kinds of social relationships serving as connecting strings, including relationships in which the person at one end of the "string" has the capacity to give orders to, or to convince, the person at the other end.

In the definitions of epistemic and moral authority I quoted in the opening paragraph, Brown calls authority a *capacity* to convince others. He seems to locate that capacity within either a person or a collectivity. I say this because of the way he words the questions he says are "at the heart" of his inquiry: "Does scientific authority inhere in individual scientists, in science as an institution, or in both? On what grounds can science claim to exert authority? What are its limits? Can science itself claim to exert moral authority?" (13) The words "inhere in" suggest to me that Brown thinks of auth ority as something that can be contained within an individual or an institution. I prefer to think of individual authority as a *relationship* between persons; it is a function of both the person who attempts to exercise authority and of the reactions of the person or persons whom the first person attempts to convince or command. No matter how great the would-be superior's powers of persuasion, her capacity for convincing another person depends upon the other's openness to being convinced, and her capacity for successfully commanding another person depends upon the other's willingness to be commanded. Brown's capacity to convince me about how the world should be is greater than his capacity to convince me about how the world is because of differences in the convictions I brought to my reading. His moral and epistemic authority will vary from one reader to another, not because he says different things to different readers, but because different readers bring different predispositions to their readings. I believe that this relational variability holds true, not just for different readers of books, but for all interactions in which one actor attempts to exercise authority over one or more of the other participants.

Brown does hint at both the variable and relational dimensions of authority. In his introduction, he calls authority "a measure of the capacity to instill belief; to engender not only understanding, but also assent; to move those affected toward changed attitudes; and to encourage actions" (5). Saying that authority is a *measure* of the capacity implies variability, if not specifically relational variability. He hints at the relational dimension of authority in his chapter on "Science and the Public," when he says, "the idea that the relationships between science and society are, in a sense, bilateral is 'intrinsic' to the discussions of science and society" (249). But I believe that an adequate theoretical definition of authority must be explicit about this relational variability. The relational and variable aspects of authority do, however, come out in his stories. For example, in discussing conflicts about teaching evolution in the public schools, he makes it very clear that state legislators, school board members, and citizens in local school districts vary in their willingness to accept what the vast majority of biologists and anthropologists assert to be true (184-5; 286). The capacity of any one of these scientists to convince another person of how the world is depends as much upon the predispositions of the other as it does upon the knowledge and eloquence of the scientist. The epistemic authority of that scientist will vary from relationship to relationship.

Brown acknowledges that it is harder to conceptualize the authority of science as an institution than it is to conceptualize the authority of individual scientists. After reviewing sociological models that explain how the authority of scientists depends upon the social organization of science, he says:

> The challenge for such models is to move from the authority of the individual scientist, which will be highly variable and limited in scope, to a more general scientific authority. Writing in the post-World War II period, Michael Polanyi attempted just this leap. His intriguing essay "The Republic of Science" anticipates much that has been written since (104).

My quarrel is not with Brown's exposition of Polanyi's position, but with his failure to develop either Polanyi's network metaphor for science or his own explicit definition of science. The network metaphor has been used by a few sociologists since the 1930s, but was not widely used until the very end of the twentieth century. Brown's question about the authority of individual scientists can be framed in terms of "egocentric networks," in which an individual is the central node, connected by social relationships and interactions with many other individuals. And his question about the authority of science as an institution can be framed in terms of "whole-network" analysis, in which a set of individuals is treated as a collectivity with some sort of boundary, however fuzzy.[6] This conceptualization of a whole-network is compatible with Brown's characterization of science as "a body of knowledge and current practices that represents a characteristic outlook toward the physical world" (11). The social network is a collectivity of people who sustain that body of scientific knowledge, who engage in those current scientific practices, and who support that characteristic scientific outlook toward the physical world. Moreover, there is a similar, fuzzily bounded, network of people who constitute social science, a network that overlaps, but is not identical to, the network of natural scientists.

I believe that there is a connection between Brown's personifications of science and society and his tendency to use the word "authority" to refer to something that can be contained within an individual social actor. When individual authority is conceived as something contained within one person, rather than as a relationship between two persons, it becomes very tempting to think of collective authority as analogous, something that is contained within one collectivity, imagined as a big person. But the big person metaphor is a sociological dead end, very much like the plum pudding metaphor of the atom. It will continue to be used, because it is so deeply embedded in our common sense discourse, but my hope is that it will gradually disappear from scientific analyses

of the social world.

In spite of my disagreements with some of Brown's theoretical propositions, I recommend his book to those who are interested in the authority of science. The stories are interesting, informative, and convincing, and Brown does an excellent job of summarizing positions taken by participants in the past as well as the more recent "science wars."

Endnotes

[1] Michael Polanyi, "The Republic of Science: Its Political and Economic Theory," *Minerva* 1(1962): 54-74.

[2] (Urbana and Chicago: University of Illinois Press, 2003).

[3] "For example, the abstract idea of time is expressed in terms such as movement ('In the coming weeks we will see a change'), distance ('That is a long time into the future'), and money ('You're wasting my time'; 'I'm running out of time'; 'If if I could just buy a little time')" [Brown's note].

[4] *Making Truth*, pp. 79-84.

[5] *Philosophy in theFlesh : The Embodied Mind and Its Challenge to Western Thought* (New York: Basic Books, 1999).

[6] Cf. Peter Marsden, "Recent Developments in Social Network Analysis" in Peter J. Carrington, John Scott, and Stanley Wasserman (eds.), *Models and Methods in Social Network Analysis* (New York: Cambridge University Press, 2005), pp. 8-30.

Author's Response to Jay Labinger and Richard Moodey

Ted Brown

I very much appreciate the thoughtful readings of my book by both Dick Moodey and Jay Labinger. They have offered insightful commentaries and focused attention on specific topics that in their views require clarification or that might have been better addressed. I'm happy to try to respond to some of their points.

I begin with some of Jay Labinger's comments. He begins by questioning my attempt to distinguish between epistemic and moral authority: "the difference is not so clearcut as Brown would have us believe." His reservations notwithstanding, it is important to attempt this very important distinction. The *expert* authority of the scientist rests on knowledge held of some aspect of the physical world; knowledge gained through study, experimentation, analysis, critical evaluation by other scientists – the array of activities that we think of as the scientific method. To the extent that the scientist has expert authority, she can speak of how some aspect of the world is. Moral authority is something else; to quote from Wiktionary: "The quality or characteristic of being respected for having good character or knowledge, especially as a source of guidance." Given the topic of the book, this definition is consistent with one I gave (117): "the capacity to instill beliefs as to how the world *should* be – to convincingly argue for particular actions that will presumptively change the way the world is." Though scientists may be recognized as having expert authority on some topic, when they give advice as to what actions

should be taken in light of that expert knowledge, there is no assurance that those toward whom the advice is directed will accept it. They might turn down the advice for any number of reasons which do not involve rejection of the underlying expert authority. Labinger questions my assertion that Pasteur's attempts to convince the French medical establishment to adopt hygienic practices illustrate a failure of his moral authority even though his expertise was not directly challenged. But a reading of Debre's account of Pasteur's struggles to bring about more hygienic practices clearly points to factors other than simply a rejection of Pasteur's expert authority in the physicians' negative responses to his urgings (Debré, Patrice, *Louis Pasteur*, Johns Hopkins University Press, 1994, Chapter 10). I readily acknowledge, however, as I did in the book, that the two aspects of authority are inextricably mixed. Where there is dubiety about the expertise of one who would give advice, there is little ground for moral authority to stand on. Conversely, in the face of reservations regarding moral standing expert advice may fall upon deaf ears.

In a related vein, Labinger points out that the exercise of scientific authority is essentially a bilateral transaction. I am entirely in agreement. There are many reasons why scientists or science generally may not succeed in exercising either epistemic or moral authority, or both. One point on which I might comment, though, is Labinger's impression that the book deals more with challenges to scientific authority than with its acceptance. It seemed to me that the nature and limitations of scientific authority in society are best brought home by looking at instances in which science's authority is challenged. I hoped that by analyzing particular episodes I might be able to illuminate facets of science's roles in society in relation to those of other sectors. Attempts from any particular social sector to exercise authority in society are frequently challenged by other social actors. In the resolution of the resulting conflicts we can discern the grounds on which authority is claimed and challenged. For example, we learned much about how science operates in the public sphere by examining the "ozone hole" controversy to which Labinger alludes. I'm not sure what Labinger is driving at by arguing that "economic and political considerations play[ed] a much larger role than any factors that could be considered internal to science." in the resolution of the controversy. He seems to be implying that the science had only a limited role to play in the "ozone hole" story, but I don't believe that to be the case. Of course the epistemic authority of the scientists was challenged. Of course there were political machinations and economic argument of huge moment as the story played out. But wasn't it the authoritative voices of a couple of scientists that forced the powers that be to take the matter seriously in the first place? As the epistemic issues were sorted out through continuing research (Labinger's factors "internal to science") they carried the day. It was by no means inevitable that the Montreal Protocols would evolve to put an end to chlorofluorocarbon production: it happened for a variety of reasons, but science's epistemic *and* moral authority played vital roles.

Finally, I want to comment on Labinger's take on the concept of autonomy. He seems to suggest that I have failed to properly distinguish between the autonomy of the individual scientist and that of scientific institutions. I thought I had covered this rather completely in Chapter 1 (35-37), and at many other places in the book. Beyond that point, however, Labinger argues that there is an "*inherent* conflict between individual and institutional autonomy, which Polanyi and others have largely swept under the rug." He alludes to Polanyi's view, spelled out in his "Republic of Science" paper, that science advances in part through breaks with conventional thinking, through challenges to the accepted state of affairs. It is this continual testing of the accepted wisdom that imparts vitality to science and helps to buttress its epistemic authority. Labinger worries that relatively few scientists come to public attention, that instead a relatively few luminaries dominate the limelight. That may be so, but it is not clear that this is relevant to the issue of autonomy. The magnitude of a scientist's public presence need not bear on his or her autonomy as a working scientist.

There is the separate question of how individual scientists, as opposed to institutional science, claim authority in discussions of controversial topics such as climate change or stem cell research norms. In "The Republic of Science"[1] Polanyi envisioned networks of overlapping expertise that combine to create a consensual view, a "scientific opinion" that would have authority in society by virtue of the processes by which it was formed. Although much has changed since Polanyi's day, there is no doubting the existence of a communitarian culture in contemporary science. It is simply a fact that many scientists tend to be disputatious, jealous and skeptical, and where the science involved is salient in the public sphere, those qualities become more apparent. The non-scientific public does not fully comprehend how science really works in the matter of sorting out what counts as significant science, in particular that the standing of research being actively pursued differs in significant ways from more established work that has been subjected to numerous tests. Highly visible controversies have the potential, therefore, to weaken the authority of science, because the perception of consensus has been lost.

Dick Moodey finds much to like about the book, but he expresses two major reservations that call for responses on my part. The first, which mirrors one of Jay Labinger 's concerns, is that I am not always as careful as I might have been in describing the social dynamics of the exercise of authority. Moodey quotes a phrase from the introduction: "Does scientific authority inhere in individual scientists, in science as an institution, or in both?" This choice of words does suggest that authority is something inherent, and – taken alone – would seem to ignore the bilateral transactional nature of non-coercive authority. I might better have written at this point of "the potential for exercise of scientific authority" or something to that effect. A reading of the book as a whole, however, reveals that I am fully aware that the successful exercise of epistemic or moral authority is predicated on its acceptance by those to whom it is directed. Indeed, much of the book is about the limitations of science's authority. The transactional nature of the exercise of authority is emphasized throughout: "To comprehend the authority of science we will need to understand the processes by which it gains legitimacy, against what other sectors of society it contests for authority, and the circumstances in which authority is exercised." (25) Chapters 9 and 10 emphasize the roles of cultural forces in determining acceptance of scientific authority, and the importance of effective communication. There is really no disagreement between me and either reviewer on this point.

The major part of Moodey's review is taken up with his fundamental disagreement with the way in which I have portrayed science. Moodey believes it is a "serious mistake to think and write about collectivities as if they were big persons." And later, "Writing about collectivities as if they were big persons should not be acceptable in social scientific discourse." I am at something of a loss as to how to address this objection, because I don't fully comprehend his "big person" metaphor. I can't imagine how one could talk about many aspects of "science" without thinking of it as an entity. Conceptualizing science *solely* as a horde of individual agents with individual characteristics and capacities, does not leave us with something that operates in society in a significant way. Surely this is not what Moodey has in mind. It is not John Smith the chemist, or Susan Brown the biologist who can individually constitute what we think of as science. Rather, science is a collective, an organized center of knowledge and social activity (11-13). I don't think that Moodey would disagree with this; his objection seems to be that it is improper to think of science as speaking, testifying or arguing as though it were a person, metaphorically speaking.

Moodey illustrates his position by referring to the Eddie Joe Lloyd story I told in the introductory chapter. In that episode Lloyd's previous conviction for murder was overturned on the basis of DNA evidence. I asked how it was that a murder conviction based on what was at the time deemed to be strong evidence could

be overturned by a presentation of scientific data relating to DNA analysis, when people in general, presumptively including those involved in overturning the conviction, really don't know the theoretical constructs that connect DNA to individual identities, the methods of carrying out the tests, and so on. Yet they are willing to accept the scientific evidence as truthful accounts of something in the real world, evidence of sufficient persuasiveness to warrant overturn of a murder conviction. How does this happen? I said "Society simply takes the word of scientists for all that – it accepts science's authority." (3) Moodey writes: "Brown boils down all the actions and interactions of the real people into an interaction between science, imagined as an entity capable of speaking a word, and society, imagined as an entity capable of hearing and accepting the word spoken by science." That is not just what I intended to say, but let's count it as close enough. Yes, real people spoke and listened that day; they were agents that brought about the courtroom result. But let's imagine that someone named Fred Sylvester was the one who presented the DNA analysis that day, and answered whatever questions were raised. No, wait, Fred was out sick that day and Karen Alexander went to the courtroom in his stead. The people in the courtroom didn't say, "We can't proceed with this. We need Fred." Assuming that Karen Alexander presented qualifications for describing the DNA work and responding to queries about it, she served as well as Fred. Neither Fred nor Karen as particular individuals were essential to the exercise of scientific authority. Rather, it was acceptance on the part of those representing "society" in the proceedings of an extensive body of knowledge and practice, consisting of the contributions of countless people over time. It is true that Karen, in presenting to the court that day, gave science a human face. I think I made it abundantly clear in the book that communications by individual scientists and representatives of science play powerful roles in the exercise of scientific authority. But the source of scientific authority and its legitimacy rest to a substantial degree on the acceptance of science as a social institution. Moodey objects to my suggestion that "science" and "society" were actors in this drama. I did indeed write, "The story with which I have begun is a simple illustration of science exercising authority. In this case, it is acting as our hypothetical unimpeachable witness." Moodey seems comfortable with the uses of metaphor, and in fact is quite discursive on this subject in the review. But I have trouble in accounting for his countenance of metaphorical usage in one case and objection to it in another. He does at one point say that he doesn't object to metaphorical usage in general, but that the "big person metaphor is the *wrong* metaphor to use in our attempts to understand and write about science and society." In making his argument he falls into critiquing the big person metaphor (his term, not mine) because he can't locate the "analogues to parts of a person – head, brain, heart, hands, etc." But it is a commonplace in metaphor theory that every metaphor has limited scope for application. Moodey has clearly driven his "big person" metaphor beyond what he has a right to expect from it. Had he not chosen the unfortunate term "big person" he might not be looking for hands or feet. For that matter, I have trouble thinking of individual persons as knots or nodes in the network model that he espouses.

I should emphasize that this is a book about the authority of science in society. It is not meant to be a general treatise on the nature of science as a social entity, though I believe I have said enough on that topic to justify my view that science is in many respects best thought of as a discrete social entity. Indeed, many distinguished sociologists of science, from Ludwik Fleck, Robert Merton, Michael Polanyi, and Thomas Kuhn to the many who have written about the social structure of science more recently (Chapter 4) take as their point of departure the idea that science is a discernably discrete social entity, and that it has effect in society as an entity in its own right. It is true that society's perceptions of and reactions to science are in substantial measure the products of its experiences with individual scientists, who may exercise authority as individuals . However, the authority exercised by individuals is grounded in their credentials as members of that larger social entity.

Because this is a journal of Polanyi scholarship I want to say a bit in defense of my interpretation of Polanyi's model of science. He begins "The Republic of Science" as follows:

> My title is intended to suggest that the community of scientists is organized in a way which resembles certain features of a body politic… The first thing to make clear is that scientists, freely making their own choice of problems and pursuing them in the light of their own personal judgment, are in fact co-operating as members of a closely knit organization. …Both the criteria of plausibility and of scientific value tend to enforce conformity, while the value attached to originality encourages dissent. This internal tension is essential in guiding and motivating scientific work. The professional standards of science must impose a framework of discipline and at the same time encourage rebellion against it. They must demand that, in order to be taken seriously, an investigation should largely conform to the currently predominant beliefs about the nature of things, while allowing that in order to be original it may to some extent go against these. Thus, the authority of scientific opinion enforces the teachings of science in general, for the very purpose of fostering their subversion in particular points.

What these lines show is that Polanyi conceived of science as a closely knit community, one that acts, for example to enforce the teachings of science, impose frameworks of discipline and so on. He goes on to describe overlapping fields and networks of competences and expertise: "And, of course, each scientist who is a member of a group of overlapping competences will also be a member of other groups of the same kind, so that the whole of science will be covered by chains and networks of overlapping neighbourhoods. Each link in these chains and networks will establish agreement between the valuations made by scientists overlooking the same overlapping fields, and so, from one overlapping neighbourhood to the other agreement will be reached on the valuation of scientific merit throughout all the domains of science." Thus it is clear that the structure involves networks and neighborhoods *of knowledge and practice*. Scientists create and maintain them through their individual activities and cooperations. Tellingly, he says,"This network is the seat of scientific opinion." This scientific opinion is active in the world outside science: "Representatives of scientific opinion will pounce upon newspaper articles or other popular literature which would venture to spread views contrary to scientific opinion. The teaching of science in schools is controlled likewise. And, indeed, the whole outlook of man on the universe is conditioned by an implicit recognition of the authority of scientific opinion." He goes on to say, "Moreover, only a strong and united scientific opinion imposing the intrinsic value of scientific progress on society at large can elicit the support of scientific inquiry by the general public. Only by securing popular respect for its own authority can scientific opinion safeguard the complete independence of mature scientists and the unhindered publicity of their results, which jointly assure the spontaneous co-ordination of scientific efforts throughout the world."

The world has changed a great deal since those words were written. However, despite enormous changes in the scope and complexity of contemporary science the organizational structure, ordering and coordinating of views *within* science still proceed much along the lines Polanyi outlined. Ironically, they come to public attention most apparently in cases where they might be thought to have failed. As a recent example, the intense public attention on lapses from good practice and evidences of questionable behavior on the part of a few prominent scientists working in the area of climate change has exposed how tightly networked a given area of research can be, and the processes by which an authoritative voice is formed. This example illustrates that science's capacity to exercise authority in the public domain as an institution rises and falls with the reputations of individual scientists. Thus, as I pointed out in the book, there is an inextricable coupling between the scientific authority exercised by science as an institution, and that exercised by individual scientists whose epistemic authority ultimately derives from their places in the world of science.

I am grateful to Jay Labinger and Dick Moodey for their consideration of my book . Their thoughtful comments have helped me to see how I might have done better in some places in writing the book, but they have also encouraged me to believe that my efforts to shed light on an important topic were not entirely off the mark.

Endnotes

[1]Michael Polanyi, "The Republic of Science: Its Political and Economic Theory," *Minerva* 1(1962): 54-74.

WWW Polanyi Resources

The Polanyi Society has a World Wide Web site at http://www.missouriwestern.edu/orgs/polanyi . In addition to information about Polanyi Society membership and meetings, the site contains the following: (1) digital archives containing all issues of *Tradition and Discovery* since 1991; (2) a comprehensive listing of *Tradition and Discovery* authors, reviews and reviewers; (3) the history of Polanyi Society publications, and information on locating early publications not in the archive; (4) information on *Appraisal* and *Polanyiana*, two sister journals with special interest in Polanyi's thought; (5) the "Guide to the Papers of Michael Polanyi," which provides an orientation to archival material housed in the Department of Special Collections of the University of Chicago Library; (6) photographs of Polanyi; (7) links to a number of essays by Polanyi as well as audio files for the McEnerney Lectures (1962) and Polanyi's conversation with Carl Rogers (1966).