© Academy of Management Review
1994, Vol. 19, No. 2, 331–341.

PFEFFER'S BARRIERS TO THE ADVANCE OF ORGANIZATIONAL SCIENCE: A REJOINDER

ALBERT A. CANNELLA, JR.
RAMONA L. PAETZOLD
Texas A&M University

Pfeffer's (1993) discussion of the causes and consequences of paradigm development led him to the conclusion that organizational scholars should place control over publication into the hands of a comparatively small elite group who would force a consensus by excluding views that diverge from a dominant paradigm. In his view, this action would lead to a number of positive benefits for organizational scholars and organizational studies in general. We argue from a different set of assumptions than those of Pfeffer. In our view, knowledge is socially constructed, and, thus, scholars are unable to make unambiguous claims on some absolute truth. Given this assumption, the enforced consensus and dominant paradigm called for by Pfeffer would lead to a stagnation in knowledge evolution. Further, we argue that the concept of consensus and its role in the evolution of knowledge has been overstated. In contrast to Pfeffer, we conclude that a high degree of consensus, however achieved, would suggest that the evolution of knowledge has been slowed, not facilitated.

> Science, since people must do it, is a socially embedded activity. It progresses by hunch, vision, and intuition. . . . Facts are not pure and unsullied bits of information; culture also influences what we see and how we see it. Theories, moreover, are not inexorable inductions from facts. The most creative theories are often imaginative visions imposed upon facts; the source of imagination is also strongly cultural. (Gould, 1981: 21–22)

Pfeffer (1993) has written an interesting and provocative article, in which he identifies barriers to the advancement of organizational science. The publication of this article provided an opportunity for the open discussion of some issues that many organizational scientists consider important, and Pfeffer may have articulated a view that many organizational scholars find appealing. However, we believe the central premise of the article (that more tolerance for diverse approaches, theories, and methods is harmful to the organizational sciences) to be fundamentally flawed.

The authors gratefully acknowledge the helpful comments of Richard Woodman, Blaine McCormick, and Jay Barney on earlier drafts of this article.

Our rejoinder is organized into three sections. The first section is a brief statement of our own philosophical perspective. In the second section, we discuss epistemology. We outline some underlying assumptions of Pfeffer's logic and critically evaluate the veracity of those assumptions for research in the behavioral sciences. In our view, Pfeffer argues from a paradigm that we cannot accept, a paradigm implying linear cause-and-effect relationships, the linear accumulation of knowledge, technological certainty, and, consequently, a high degree of consensus. Further, we discuss the self-conscious manner in which Pfeffer compares research in the social sciences to that in the natural or physical sciences. We suggest, instead, that the choice is not simply one between "arts/humanities" and "natural/physical sciences," but rather that there is room for an organizational science that is a distinct yet interdisciplinary field. Like Mahoney (1993: 179), we believe that a greater appreciation for epistemology "provides a basis for methodological understandings and tolerance of diversity and multiplicity in research designs." In our final section, we discuss the concept of consensus both from our own perspective and from Pfeffer's. Further, we discuss the role of consensus in the evolution of knowledge. In sharp contrast to Pfeffer, we conclude that a high degree of consensus, however achieved, suggests that the evolution of knowledge has been slowed, not facilitated. Based on our own paradigmatic beliefs in the social construction of science and knowledge, we argue that the evolution of knowledge requires fuzzy boundaries and a tolerance for (if not acceptance of) a plurality of paradigms.

Our Philosophical Perspective

We believe that all knowledge is socially constructed. This conclusion is perhaps the most important point of Kuhn's (1970) work. As noted by a book reviewer from the journal *Science* (repeated on the back cover of Kuhn's book):

> Its author, Thomas S. Kuhn, wastes little time on demolishing the logical empiricist view of science as an objective progression toward the truth. Instead, he erects from ground up a structure in which science is seen to be heavily influenced by nonrational procedures, and in which new theories are viewed as being more complex than those they usurp but not as standing any closer to the truth.

Because we find ourselves unable to determine how close our theories are to some absolute truth, we are unable to evaluate paradigms in a way that would enable us to know that any particular one is a priori deserving of a dominant position in organizational science. Science is not a magnificent march toward absolute truth, but a social struggle among the scholars of the profession to construct truth (Astley, 1985). We believe that when confronted with this most basic of assumptions, much of Pfeffer's logic unravels.

For example, Pfeffer believes that by narrowly defining work that is acceptable or publishable in organizational science, at least four outcomes will occur: (a) there will be a higher degree of technological certainty, (b) there will be a higher degree of consensus, (c) there will be more opportunities for collaborative research, and (d) a larger share of funding and public approbation will be there for the organizational sciences. If those theorists holding the resulting single paradigm are able to make no claim on absolute truth (above that of other paradigms), the results that Pfeffer claims will be realized may be no more than a grand deception. Of course, even though *we* believe knowledge to be socially constructed, those who control the financial resources upon which many researchers depend may not hold that paradigmatic view. One could argue that organizational scholars should act as if they believe in a single paradigm so as not to be denied their share of those resources. We find this argument unappealing and inconsistent with our values. Paradigms are an important part of the sociology of science, but competing paradigms should not be ignored.

By learning a paradigm, researchers acquire both theory and methods; additionally, researchers acquire the standards by which to judge analyses and results within the field. As new paradigms emerge, those paradigms necessarily conflict with old ones, for they force a reexamination of the very science that spawned them. Science requires conflict between competing schools of scientific thought, according to Kuhn (1970). Kuhn provided historical description of this process and argued that it is akin to conflict between competing political institutions. Also, Kuhn noted that each paradigm's group uses its own paradigm to argue in that paradigm's defense. Because there is no "metaparadigm" with which to make the choice between or among paradigms, each scholar must argue from his or her own paradigmatic frame. This circularity of argument is not always apparent to the defender. Further, Kuhn noted that the circularity need not render the argument ineffectual as long as it is persuasive. Neither logic nor experiment alone can settle the conflict among paradigms.

Epistemology and the Evolution of Knowledge

> Epistemology: Is knowledge knowable? If not, how do we know this? (Allen, 1972: 49)

A person's position regarding a paradigm becomes most clear when he or she uses terms, such as *knowledge*, as though all readers will implicitly hold shared meanings for them. Pfeffer (1993) discussed knowledge development, not only assuming linearity in the accumulation of knowledge, but also assuming that all organizational scientists can agree on what knowledge is. What counts as knowledge? Pfeffer cited Cole as providing that "[a]ccumulation of knowledge can occur only during periods of normal science which are characterized by the adherence of the scientific community to a paradigm" (1983: 134). We argue that this view of knowledge and normal science reflects a misreading of Kuhn (1970).

Kuhn provided an extraordinary *historical* evaluation of how science (physical/natural science) has evolved over time. This history reflects Kuhn's construction, and although it must be informed by Kuhn's philosophy of science, there is no justification for reading the history normatively. Kuhn's construction of history reveals what he called *normal science,* a period in which research is cumulative (even perhaps of the *footnote on footnote* variety). This is to say that normal science is highly directive and paradigm bound, in the sense that the researcher knows what he or she wants to demonstrate and designs instruments accordingly (Kuhn, 1970). Others (Cole, 1983; Lodahl & Gordon, 1972) have added new meanings to the social construction of normal science through their interpretations of paradigm and paradigm development. However, some philosophers of science have questioned the existence of normal science as depicted by Kuhn (e.g., Feyerabend, 1983). In other words, Kuhn's "normal science" could have been imposed on periods of scientific activity that did not meet Kuhn's notion of *scientific revolution;* he viewed scientific discovery from such a distant vantage point that considerable historical smoothing was possible. The extent to which normal science–type of periods (i.e., periods without constant and serious challenge from competing views) can exist is debatable. For example, Feyerabend observed that "great scientific advances are due to outside interference which is made to prevail in the face of the most basic and most 'rational' methodological rules" (1983: 162). Further, the pace of evolution may be increasing. Given the revolutionary changes occurring in the environment of organizations, coupled with the sheer number of organizational scholars and the high level of interaction among scholars, a reevaluation of the traditional paradigmatic frame of reference would seem to be in order (Daft & Lewin, 1993).

Relying on notions of *normal science,* Pfeffer (1993) assumed that *knowledge* within organizational science can result only if there is a dominant paradigm that is coupled with a high degree of consensus and technological certainty. He relied on comparisons with the physical sciences to demonstrate ways in which organizational science can be viewed as deficient. This paradigm-laden argument positions organizational science in a destructive, dichotomous epistemology. In order to avoid the problems of *art* (regarding funding and other outcomes), organizational scientists should be careful not to incorporate methods drawn from the arts or humanities (e.g., hermeneutics, deconstruction), they should, instead, make organizational science look like *physical science* (e.g., highly quantified). We argue that the art/science dichotomy is a false and tired one. The role of personal, "subjective" evidence has been relegated more to art, whereas scientific, "objective" evidence has been the purview of science. How these assignments have been made (i.e., constructed, not divinely handed down) are what leads many researchers to reject the art/science dichotomy. Even Popper recognized the social construction of science: "[W]hat is to be called a 'science' and who is to be called a 'scientist' must always remain a matter of convention or decision" (1934: 139). Scientific rationality may have permeated our modes of thinking, but the privilege accorded it in our society is not essential and may not be desirable to the extent that it precludes other ways of knowing. Additionally, the perception of technological certainty in the physical sciences has been considered a misperception by many philosophers of science (e.g., Feyerabend, 1980, 1983; Harding, 1986, 1991; Longino, 1990).

Both art and science are socially constructed. Science is but one of many ideologies that exist within society; no "scientific methodology" can separate science from art or from any other ideology (Feyerabend, 1983; Mahoney, 1993). An alternative understanding of how beliefs can be accorded the label of knowledge should be applauded, not deprived of voice. After all, one's understanding can be justified (or even glorified) only by comparison to other understandings and perspectives. To the extent that the physical sciences may lack this pluralism, they are diminished.

There is no doubt that science—particularly physical science—has great authority in our society. The quantification of science has helped to provide legitimacy for this authority (Mahoney, 1993; Zald, 1993). Many authors have noted that it is beginning with the movement toward quantification, and the development of statistical methodologies to accompany and influence scientific methodologies, that the hegemony of science (as traditionally represented through scientific method and objectivity) in society began to occur (Hacking, 1986; Harding, 1986, 1991). In the early stages, scientists may have assisted decision makers concerned with social policy issues; today, scientists have become directly responsible for economic, political, and social control (Feyerabend, 1983; Harding, 1986). Pfeffer (1993) seemed to accept this degree of control associated with the physical sciences as an important measure of their advancement, and he argued that organizational scientists need to mimic physical scientists in order to achieve the same outcomes. This line of reasoning suggests that a forced, dominant paradigm is required for organizational science to receive its share of social encomia and public funding. An earlier movement toward quantification and statistical "sophistication" in the social and organizational sciences may, in fact, have been motivated by just this reasoning (see, for example, Schumpeter, 1954; Stigler, 1986; and Zald, 1993). However, even within Pfeffer's paradigm, this type of causal reasoning is unwarranted. Any association between the physical sciences and social outcomes may not be causal. Even if it is, the causal relationship may be unique to some aspect of the physical sciences (their subject matter? their timely development? the "visibility of their results"? [Lodahl & Gordon, 1973: 81]), thereby making it fruitless to argue that organizational science should be more like its older sibling.

Organizational science is relatively young as a distinct discipline. Its scholars continue to draw from a variety of disciplines, and it has fuzzy boundaries, as noted by Pfeffer (1993). Organizational science could be viewed as pre-paradigmatic, in the sense that no one, new paradigm has emerged as a dominant force, distinct from the older, competing paradigms that have been incorporated from the social sciences. This view need not be problematic, however. A unified view of organizational science requires interdisciplinary understanding (by definition, it must incorporate and countenance all views of knowledge within the variety of disciplines over which unification is to take place). In the absence of unification, pluralism in views remains important:

> A scientist who is interested in maximal empirical content, and who wants to understand as many aspects of [his or her] theory as possible, will accordingly adopt a pluralistic methodology, [he or she] will compare theories with other theories . . . and [he or she] will try to improve rather than discard the views that appear to be lost in the competition. (Feyerabend, 1980: 47)

These alternative theories must be viewed broadly; according to Feyerabend, "they may be taken from wherever one is able to find them—from ancient myths and modern prejudices; from the lucubrations of experts and from the fantasies of crooks" (1980: 47). Whether Pfeffer considers them the "lucubrations of experts" or the "fantasies of crooks," critical theory, feminism, and postmodernism fall within Feyerabend's call for openness in theoretical and methodological perspective.

We support this spirit of openness to other views. That is not to say that "anything goes," as Pfeffer (1993) fears. Continued dialogue in the marketplace of ideas (i.e., academic journals, academic meetings) is required for the quality of theories, methods, and paradigms to be evaluated. Research that can be understood coherently to follow from stated assumptions and that clearly communicates its objectives, findings, and limitations should be given an audience. Such work is inherently legitimate; its legitimacy does not derive from the approval of academic gatekeepers (the "elites"), or from a degree of consensus regarding its perspectives.

The Notion of Consensus

In *Webster's New World Dictionary of the English Language* (1968), consensus is defined as "1. agreement, especially in opinion; hence, 2. general opinion." Pfeffer's (1993) discussion of consensus bears little resemblance to this definition. Implicitly, Pfeffer's discussion might define consensus as the absence (to external observers) of visible dissention. For example, Pfeffer suggested that those who depart from established paths should be ostracized "regardless of their power or the validity of their ideas" (1993: 614). We find this discussion to be antithetical to both our notion of consensus and our code of ethics. If close correlations could be observed between actions and outcomes (correlations that would seem to reflect causal relationships), then there would be a high degree of consensus among organizational scholars. In contrast, because it is difficult to agree upon cause-effect relationships in organizational science, ignoring scholars who have divergent ideas is done at our own peril.

Pfeffer suggested that if we do not begin to ignore scholars who have divergent ideas, our field will "remain ripe for a hostile takeover from within or from outside" (1993: 618). This conclusion is unwarranted. In order to "cure" our lack of consensus, a prospective raider has only to produce theories that provide greater understanding and high predictive validity—something that a half century of research in organizational science has thus far failed to do. A consensus such as that called for by Pfeffer requires blind faith and unquestioning adherence to a dogma decreed to be "true" by the elites of our field. Even when surrounded by evidence that the theory is incomplete, Pfeffer would have us ignore the evidence until it overwhelms us. We believe that *this* is the context in which a hostile takeover would occur. Should members of another field wish to mount a hostile takeover of organizational science, we say "let them come," fully recognizing that such a takeover requires the convincing of members of our field (as well as many outside our field) that some particular perspective is the best one offered to date.

Does science require consensus? We think not, and Kuhn and other authors mentioned previously provide ample evidence to support our view.[1] The concept of progress in knowledge is best achieved when there are critics among us who constantly push us to reassess our assumptions and refine our theories. Both Bourgeois (1984) and Dewey (1929) argued that the desire for the reduction of uncertainty is driven by the need for psychological security. We believe that the same can be said about the desire for consensus. Pfeffer's world of high consensus provides little comfort for the scholar who believes that knowledge is socially constructed and, thus, stagnates when confronted by consensus.

Interestingly, Mahoney (1993) made a similar point when responding to Camerer's (1985) call for methodological purity. Mahoney cited Jevons (1871/1965: 275–276):

---

[1] Pfeffer (1993: 611) draws heavily from Cole (1983) to support his argument that consensus, "however achieved", is a prerequisite for the advancement of knowledge as well as his argument that much authority in the organizational sciences should be vested in elites. Interestingly, Cole and Cole (1979), in an article published in *Nature*, evaluated both consensus across raters and reviewer's perceptions of the quality of science in National Science Foundation proposals. These authors concluded that the eliteness of the proposal's author had no association with reviewers' perceptions of the quality of the proposal. Further, the degree of consensus among reviewers did not vary across the social and physical sciences. In a second article published in *Science*, Cole, Cole, and Simon (1981: 885) concluded: "Contrary to a widely held belief that science is characterized by wide agreement about what is good work, and what are promising lines of inquiry, our research both in this and other studies in the sociology of science indicates that concerning work currently in progress there is substantial disagreement in all scientific fields."

> In matters of philosophy and science authority has ever been the great opponent of truth. A despotic calm is usually the triumph of error. In the republic of the sciences sedition and even anarchy are beneficial in the long run to the greatest happiness of the greatest number.

Conformity to a central paradigm would require that we train ourselves and our students to ignore any work that strayed from the established "calf path" (Foss, 1895). Further, although Pfeffer's solution (restricting the entry of ideas decreed to be "different") doubtless would increase the comfort level of those who are already established, it will also increase the costs of entry for new scholars and restrict innovative results on the output side.

Pfeffer believes that much of what is published today is of little value, in the sense that he believes these articles do not contribute to the development of knowledge in organizational science. Words like *development* are largely illusory when used with the word knowledge, because whether knowledge *develops* depends on one's paradigm. Perhaps because of this, Pfeffer proposed a system of elites (somewhat akin to high priests, in a religious sense), who proclaim what is right and wrong, appropriate or inappropriate. Because these elites are unable to "prove" their beliefs, they require an elaborate ritual structure to increase others' faith in their abilities (Astley, 1985). This is nothing more than an elaborate ruse, a scientific enactment of Plato's tale of Socrates and Glaucon (see Appendix). We find the idea of a hierarchy in organizational science, coupled with the notion of a system of elites from the "best" schools, to be offensive. Pfeffer's vision would lead to a tyranny of the elites, who would protect their positions by denying the existence of evidence that challenges their views and by undermining the credibility of those whom they cannot control.[2]

The test of publishability should be coherent persuasiveness—an internal logic and cohesion capable of winning support. This is far from Pfeffer's fear of "anything goes." Norms of fairness and rules of operation based on honesty, integrity, and respect could and should be enforced. In the social sciences, scholars depend upon their reputations, and those reputations are best served by honest and ethical behavior. Researchers must acknowledge conflicting views. Norms of particular content, particular logic, particular quantitative "rigor" (limitations on idea specification) should be eschewed. Pfeffer's view, which might be compared to central planning, is anathema to an open academic marketplace of ideas. Researchers who challenge existing views contribute to an ongoing dialogue, a dialogue that we believe is essential to the evolution of knowledge in organizational science.

---

[2] For an example of how some "elites" have responded to challenges to their theories, see McClosky's (1989) discussion of George Stigler or Hawking's (1988) discussion of Isaac Newton.

REFERENCES

Allen, W. 1972. ***Getting even.*** New York: Warner Books.

Astley, G. 1985. Administrative science as socially constructed truth. ***Administrative Science Quarterly,*** 30: 497–513.

Bourgeois, L. J. 1984. Strategic management and determinism. ***Academy of Management Review,*** 9: 586–596.

Camerer, C. F. 1985. Redirecting research in business policy and strategy. ***Strategic Management Journal,*** 6: 1–15.

Cole, S. 1983. The hierarchy of the sciences? ***American Journal of Sociology,*** 89: 111–139.

Cole, J. R., & Cole, S. 1979. Which researcher will get the grant? ***Nature,*** June 14: 575–576.

Cole, S., Cole, J. R., & Simon, G. A. 1981. Chance and consensus in peer review. ***Science,*** November 20: 881–886.

Daft, R. L., & Lewin, A. Y. 1993. Where are the theories for the "new" organizational forms? An editorial essay. ***Organizational Science,*** 4: i–vi.

Dewey, J. 1929. ***The quest for certainty.*** New York: Minton, Balch.

Feyerabend, P. 1980. ***Against method.*** London: Verso.

Feyerabend, P. 1983. How to defend society against science. In I. Hacking (Ed.), ***Scientific revolutions:*** 156–167. Oxford, England: Oxford University Press.

Foss, S. W. 1895. ***Whiffs from wild meadows.*** Boston: Lothrup, Lee, & Shepard.

Gould, S. J. 1981. ***The mismeasure of man.*** New York: Norton.

Hacking, I. 1986. Making up people. In T. C. Heller, M. Sosna, & D. E. Wellberg (Eds.), ***Reconstructing individualism: Autonomy, individuality, and the self in Western thought:*** 222–236. Stanford: Stanford University Press.

Harding, S. 1986. ***The science question in feminism.*** Ithaca, NY: Cornell University Press.

Harding, S. 1991. ***Whose science? Whose knowledge?*** Ithaca, NY: Cornell University Press.

Hawking, S. W. 1988. ***A brief history of time: From the big bang to black holes.*** New York: Bantam.

Jevons, W. S. 1871/1965. ***The theory of political economy.*** New York: Kelley.

Kuhn, T. S. 1970. ***The structure of scientific revolutions*** (2nd ed.). Chicago: University of Chicago Press.

Lodahl, J. B., & Gordon, G. 1972. The structure of scientific fields and the functioning of university graduate departments. ***American Sociological Review,*** 37: 57–72.

Lodahl, J. B., & Gordon, G. 1973. Funding the sciences in university departments. ***Educational Record,*** 54: 74–82.

Longino, H. E. 1990. ***Science as social knowledge.*** Princeton, NJ: Princeton University Press.

Mahoney, J. T. 1993. Strategic management and determinism: Sustaining the conversation. ***Journal of Management Studies,*** 30: 173–191.

McCloskey, D. N. 1989. Why I am no longer a positivist. ***Review of Social Economy,*** 47: 225–238.

Pfeffer, J. 1993. Barriers to the advance of organizational science: Paradigm development as a dependent variable. ***Academy of Management Review,*** 18: 599–620.

Popper, K. R. 1934. Scientific method. In D. Miller (Ed.), ***Popper selections:*** 133–142. Princeton, NJ: Princeton University Press. 1985.

Schumpeter, J. A. 1954. **History of economic analysis.** Oxford, England: Oxford University Press.

Stigler, S. M. 1986. **The history of statistics: The measurement of uncertainty before 1900.** Cambridge, MA: The Belknap Press of Harvard University Press.

Zald, M. N. 1993. Organization studies as a scientific and humanistic enterprise: Toward a reconceptualization of the foundations of the field. **Organization Science,** 4:5513–528.

APPENDIX

Plato's Tale[3]

Citizens of the republic, Socrates advised, should be educated and assigned by merit to three classes: rulers, auxiliaries, and craftsmen. A stable society demands that these ranks be honored and the citizens accept the status conferred upon them. But how can this acquiescence be secured? Socrates, unable to devise a logical argument, fabricated a myth. With some embarrassment, he told Glaucon:

> I will speak, although I really know not how to look you in the face, or in what words to utter the audacious fiction. They [the citizens] are to be told that their youth was a dream, and the education and training which they received from us, an appearance only; in reality during all that time they were being formed and fed in the womb of the earth.

Glaucon, overwhelmed, exclaimed: "You had good reason to be ashamed of the lie which you were going to tell." "True," replied Socrates, "but there is more coming; I have only told you half."

> Citizens, we shall say to them in our tale, you are our brothers, yet God has framed you differently. Some of you have the power of command, and in the composition of these he has mingled gold, wherefore also they have the greatest honor; others he has made of silver, to be auxiliaries; others again who are to be husbandmen and craftsmen he has composed of brass and iron; and the species will generally be preserved in the children. . . . An oracle says that when a man of brass or iron guards the State, it will be destroyed. Such is the tale; is there any possibility of making our citizens believe in it?

Glaucon replied: "Not in the present generation; there is no way of accomplishing this; but their sons may be made to believe in the tale, and their son's sons, and posterity after them."

[3] From *The Mismeasure of Man* by S. J. Gould, 1981, New York: Norton. Copyright 1991 by Norton. Reprinted by permission.

Albert A. Cannella, Jr. is an assistant professor of strategic management at Texas A&M University. He received his Ph.D. in strategic management from Columbia University in 1991. His research interests are centered on executives, and include succession, compensation, governance, and leadership.

Ramona L. Paetzold is an assistant professor of management at Texas A&M University. She received her D.B.A. from Indiana University and her J.D. from the University of Nebraska. Her current research interests include sexual harassment, employment law, and feminist research methods.