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BARRIERS TO THE ADVANCE OF ORGANIZATIONAL SCIENCE: PARADIGM DEVELOPMENT AS A DEPENDENT VARIABLE

JEFFREY PFEFFER
Stanford University

The level of paradigm development—technical certainty and consensus— characterizing a field of study has numerous consequences for the social organization and operation of that field. These consequences, ranging from the ability to obtain resources to the ease of working collaboratively on research, have an impact on the subsequent development of the field (i.e., through a positive feedback loop). Although the degree of technical certainty or consensus is clearly affected by the fundamental nature of the subject of study, consensus is also produced by social practices that differentiate fields that are more or less paradigmatically developed. The study of organizations is arguably paradigmatically not well developed, in part because of values that emphasize representativeness, inclusiveness, and theoretical and methodological diversity. Although these values are attractive ideals, there are consequences for the field's ability to make scientific progress, which almost requires some level of consensus, as well as for its likely ability to compete successfully with adjacent social sciences such as economics in the contest for resources. Recognizing the trade-offs and processes involved in scientific progress seems to be a necessary first step for thinking about the dilemmas that are implicit in the sociology of science literature.

In the sociology of science literature, few concepts have enjoyed as wide acceptance or provided as much conceptual leverage as that of the level of paradigm development. "Thomas Kuhn (1970) differentiates among the sciences by the extent to which they have a developed paradigm or shared theoretical structures and methodological approaches about which there is a high level of consensus" (Cole, 1983: 112). To this point, most, if not all, of the existing research has been devoted to operationalizing the concept of paradigm development, seeing if there really are differences in the sciences in terms of the amount of consensus, and examining the effects of paradigm development on a range of outcomes.

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In this article, the first part of my argument entails reviewing the evidence that (a) there are differences in the level of paradigm development across scientific fields and that (b) these differences have significant consequences for a number of important outcomes.

Given the importance and predictive power of the concept of the level of paradigm development, it is unfortunate that little attention has been given to asking why it is that some fields have more consensus than others. This is an important issue because the second part of my argument is that consensus is a necessary, although clearly not sufficient, condition for the systematic advancement of knowledge. Thus, because researchers are concerned with the development and growth of organizational science, they can benefit from understanding something about the factors associated with more or less paradigmatically developed fields. This article is far from the first to make this point. Zammuto and Connolly (1984: 30), for instance, argued that "the organizational sciences are severely fragmented and . . . this fragmentation presents a serious obstacle to scientific growth of the field."

After I have shown that paradigm development is theoretically important and that consensus is a critical precondition to scientific advancement, in the third part of my argument I address the factors that seem to affect the development of scientific paradigms in general and organizational science more specifically. In particular, I explore the dual effects of the value placed on theoretical and methodological diversity and participation. As in other contexts, there are trade-offs involved; this is not to say that the trade-offs should be made in one way rather than another, but that researchers should be conscious of them and their long-run implications for the field.

THE MEASUREMENT AND EFFECTS OF THE LEVEL OF PARADIGM DEVELOPMENT

As originally operationalized by Lodahl and Gordon (1972), paradigm development refers to the technological uncertainty associated with the production of knowledge in a given scientific field or subspecialty. Technological certainty means that there is a wide agreement on the connections between actions and their consequences (Thompson & Tuden, 1959), or in this case, agreement that certain methods, certain sequences and programs of study, and certain research questions will advance training and knowledge in the given field. Whitley (1982: 335) noted that "the meaning, relevance, and significance of research results for theoretical goals vary in clarity and straightforwardness in different fields. Even where techniques are standardized, the overall significance and importance of results may remain vague and subject to disputes."

Measures and Indicators of the Construct

Lodahl and Gordon (1972) surveyed faculty and department chairs in 20 departments in the fields of physics, sociology, chemistry, and political

science, and they asked respondents to rank seven fields (the four surveyed plus biology, economics, and psychology) on the "amount of consensus over paradigms (law, theory, and methodology)" (Lodahl & Gordon, 1973a: 192). These authors found good agreement on the rankings of the fields in terms of their paradigm development. They further found that the social scientists reported less agreement over course content, graduate degree requirements, and the content of survey courses than did those in the physical sciences (Lodahl & Gordon, 1973a: 193).

Surveying to obtain measures of consensus is time consuming; this technique also potentially measures perceived rather than actual level of consensus. Therefore, researchers have developed a number of archival or unobtrusive measures of the level of paradigm development of a field. Price (1970) suggested two measures. One is the proportion of Ph.D. graduates employed in college or university teaching. He argued that this number reflected the place of each branch of learning in society:

> In some fields, such as history and philosophy, most of the embryonic researchers get their Ph.D.'s and then proceed toward some sort of career as a teacher. In that case society is paying for students to become teachers to beget students; research becomes an epiphenomenon. In the most "scientific" departments at our universities only about 20 percent of the Ph.D. output is fed back into education, and society gets for its investment . . . also the training of Ph.D.'s who become employed in the nonuniversity world. (Price, 1970: 5)

The second measure is the percent of references in published works that were themselves published in the preceding five years, an index that corresponds well with intuitive ideas of hard science, soft science, and nonscience.

Salancik, Staw, and Pondy (1980) reasoned that fields with highly developed paradigms, in which there was more consensus, should be characterized by more efficient communication—less time needed to be spent defining terms or explaining concepts. Lodahl and Gordon (1972: 61) had noted that "the high consensus found in high paradigm fields . . . provides an accepted and shared vocabulary for discussing the content of the field." This idea led to the use of the length of dissertation abstracts (in words), the length of dissertations (in pages), and the proportion of publications in a field that are in the form of articles rather than books (Konrad & Pfeffer, 1990) as indicators of the level of paradigm development (see also Pfeffer & Moore, 1980a).

A high degree of consensus also makes interdependent activity more possible. Thus, another indicator used by Salancik and his colleagues was the length of the longest chain of courses in a department, where a *chain* is defined as a course being a prerequisite to another course, and that course being a prerequisite to another course, and so on. The length of a course chain was highly correlated with communication efficiency,

and for the seven fields measured by Lodahl and Gordon, a scale developed from these indicators correlated above .8 with the survey results (Pfeffer & Moore, 1980a: 397). The possibility of coordinating interdependent activity also means that it is easier to organize and manage the work of others on research. Lodahl and Gordon (1972) found that scientists in fields with highly developed paradigms wanted and used more graduate assistants than those in fields with lower levels of paradigm development. Thus, the preference for and use of graduate students and assistants in the research process is another indicator of the level of paradigm development.

The Effects of the Level of Paradigm Development

The level of a scientific field's paradigm development has a number of substantively important effects. Table 1 presents a listing of many (although certainly not all) of the consequences of the level of paradigm development. There is evidence that more highly developed fields fare better in the contest for resource allocations, both as distributed by external funding agencies and by the administration within a given college or university. For instance, Lodahl and Gordon (1973a, 1973b) found that the physical sciences were much better funded than the social sciences regarding either university funding or funding from outside sources; this finding held true when department size and quality were taken into account. Such a finding is not surprising because "policy makers and the public can be more certain of results from the more developed sciences" (Lodahl & Gordon, 1973a: 196). Pfeffer and Moore (1980b) found that the level of paradigm development affected both the amount of grants received by departments and the budget allocations to academic departments on two campuses of a large state university. Of course, because research has shown that grants and contracts are an important source of subunit power in universities (Pfeffer & Moore, 1980b; Salancik & Pfeffer, 1974), the fact

TABLE 1
Outcomes Affected by the Level of Paradigm Development

Resource allocations including funding levels of departments
Dispersion in funding across departments; dispersion in talent
Connection between productivity and pay
Connection between wage dispersion and job satisfaction
Connection between social ties and the National Science Foundation's grant allocations
Connection between social ties and journal publications
Connection between social ties and editorial board appointments
Governance of academic departments
Department head turnover or average tenure
Journal rejection rates
Time to publication for research
Power of fields and departments and salary paid to faculty
Working collaboratively rather than alone on research
Cross-citation practices among fields

that external funding advantages translate into internal funding advantages is to be expected. But Pfeffer and Moore's results indicate that paradigm development has an effect on resource allocations, even when departmental power is taken into account. Moreover, because the analysis examined changes over time, it accounted for the possibility of differences in initial funding levels due to inherent differences in the fields.

The level of paradigm development affects not only differences in the level of resource allocations but also the dispersion of such allocations. Lodahl and Gordon (1973a: 197) reported the average level of funding per faculty member in physics, chemistry, sociology, and political science for departments rated as distinguished, strong, good, or adequate plus. They found that "funding is more highly concentrated by quality levels in the physical than in the social sciences. The more distinguished physical science departments enjoy three times the overall funding of lower-quality physical science departments, while the more distinguished social science departments have only one and one-half times the overall funding of their less-distinguished counterparts" (1973a: 196). A study of individual reputation in these four fields revealed that "ability, like funding, is more dispersed in the social sciences" (Lodahl & Gordon, 1973a: 198). Thus, there is less concentration of both talent and resources in less paradigmatically developed fields.

Because paradigm development affects the ease and certainty of evaluating scientific research, Konrad and Pfeffer (1990) observed that in fields with more highly developed scientific paradigms, there was a greater effect of academic research productivity on pay. Pfeffer, Leong, and Strehl (1976) earlier had observed that publication was a more important predictor of both departmental prestige and prestige mobility in more paradigmatically developed fields. Beyer and Snipper (1974) reported that the quality of faculty degrees and mean research funds per faculty member were more strongly related to the quality ratings of physical science departments as contrasted with social science departments. Thus, it seems that objective measures of performance translate into status or financial rewards with more certainty in more highly developed fields. This consensus over the evaluation of scientific contributions also affects individual reactions to wage inequality. Pfeffer and Langton (In press) used the 1969 Carnegie survey of university faculty to study the effect that wage dispersion within departments had on members' job satisfaction. They found that a given level of wage inequality had less effect on members' dissatisfaction in departments in high-paradigm fields, in which there was more consensus on standards for evaluation.

If consensually shared beliefs about the nature of knowledge and methods in a field are present, such beliefs will guide decisions on grant allocations and publication. If such technological certainty is absent, decisions are more likely to be made on other, more particularistic bases. One such particularistic basis of allocating resources is sharing an affiliation with the recipient of the allocation. Pfeffer, Salancik, and Leblebici

(1976) found that the National Science Foundation's grant allocations were more strongly related to institutional presence on the advisory board in fields that were less paradigmatically developed, controlling for departmental size and quality. Pfeffer, Leong, and Strehl (1977) found that institutional membership on editorial boards had a greater effect on institutional representation in journal publications the less paradigmatically developed the field, even after measures of institutional quality and size were statistically controlled. Beyer (1978) surveyed journal editors in four fields and found some evidence that particularistic criteria (e.g., personal knowledge of the author, institutional affiliation of the author, and position within a professional association) were somewhat more likely to be used in less paradigmatically developed fields.

Yoels (1974), in a study of seven scientific fields, examined the effect of paradigm development on the tendency of editors-in-chief to appoint people from the same institution to their editorial boards. He found that "the selection of editors for social science journals is more subject to the influence of 'particularistic' criteria than for physical and natural science journals" (1974: 271). Yoels's results for editorial board appointments are consistent with the study of grant allocations and journal publications: In each instance, there was evidence that similarity in institutional affiliation affected outcomes more strongly in less paradigmatically developed fields. Another study (Lindsey, 1976) examined the scholarly productivity of members of editorial boards in various journals in psychology, sociology, and social work. Editorial boards were more consistently staffed with more productive scientists in personality and social psychology than in counseling psychology, and psychology, overall, had higher quality (in terms of article publication and citations to their work) editorial board members than did sociology, which, in turn, ranked well ahead of social work. Appointments to prestigious gatekeeping positions were more highly related to scholarly contributions in subspecialties that were more paradigmatically developed. In more developed fields, more universalistic, quality-based measures were employed in allocation decisions.

The level of paradigm development is also related to governance of academic departments. For instance, Lodahl and Gordon (1973a) reported that departments in high-paradigm fields enjoyed more autonomy from the central university administration, in part because of the greater visibility and predictability of consequences of their actions (see also Beyer & Lodahl, 1976). In a study of English universities, Beyer and Lodahl (1976: 120) reported that the authority of the department chair was higher in the more highly developed physical sciences.

The turnover (or average tenure) of academic department heads is related to the department's level of paradigm development. Not surprisingly, there is more turnover, controlling for other factors, in departments with lower levels of paradigm development (Pfeffer & Moore, 1980a; Salancik et al., 1980). Paradigm development is, after all, an indicator of consensus. The greater the consensus and the greater the certainty on the

connections between actions and their consequences, the less the conflict, and the less the conflict, the less either voluntary or involuntary turnover in leadership positions there will be.

Paradigm development is related to journal rejection rates. Hargens (1988) analyzed journal rejection rates for 30 scholarly journals over time, to control for the effects of space shortages as contrasted with paradigm development. He gathered data on both submissions and the number of papers published. He argued that if journal rejection rates were a function of space shortages, changes in submission rates should account for changes in journal rejection rates over time. The fact that journal submission rates had a trivially small effect on rejection rates, even though submission rates varied substantially over the period, whereas the independent effect of earlier rejection rates was strong, Hargens interpreted as impugning the claim that variations in rejection rates were caused by differences in space shortages (Hargens, 1988: 140). He concluded that "space shortages affect journals' backlogs rather than their rejection rates" (Hargens, 1988: 141). Journal acceptance rates in the physical and biological sciences were typically in the .6 and higher range, whereas in anthropology, sociology, psychology, and political science journal acceptance rates were typically .2 or lower (Hargens, 1988: 150).

Hargens also found that review times were substantially shorter in the more paradigmatically developed fields. This finding is consistent with Beyer's (1978) findings that time to publication is shorter in journals in the more paradigmatically developed scientific fields. Garvey, Lin, and Nelson (1970) studied lags in the information flow process and the transfer of information from the informal to the formal (journal publication) domain. They found that the elapsed time from the earliest report of research to publication in a journal was much shorter in the physical sciences compared to the social sciences. However, "these longer lags should not be attributed to lethargy or inefficiency on the part of individual social scientists; rather, they are lags which stem from the characteristics of the dissemination system currently functioning in the social sciences" (Garvey et al., 1970: 68). The biggest factor associated with the lag from research results to dissemination was the higher rejection rate in the social science journals. Even for articles that were eventually published in a so-called core journal, in the social sciences some 25 percent had been previously rejected by one or more journals.

Because of the greater consensus in more paradigmatically developed fields and the greater certainty of technology, collaborative research is easier to organize and accomplish in these areas. Just as communication is more efficient and course sequences can be longer in high-paradigm fields, so too is it easier to organize the activities of larger groups of people in a collaborative research venture. In exploring what affects patterns of research collaboration, Pfeffer and Langton (In press) found that the level of paradigm development was the single most important factor affecting whether or not people worked alone on research,

with one or two others, or in a larger group. Work can be better and more efficiently organized in the presence of greater task certainty. Whitley (1982: 337) noted that "the more predictable are task outcomes, the more work can be systematically planned outside the work process, work roles allocated on a full time basis, tasks highly differentiated and results coordinated and controlled through a formal hierarchy, with an elaborate communication system. . . . Scientific fields where task uncertainty is higher are less likely to formulate and carry out research programmes in a systematic way which directs work across employment organizations."

The level of paradigm development affects researchers' ability to take coordinated action. Beyer and Lodahl (1976: 114) argued that "faculty members who have more consensus can form stronger and more effective coalitions than those in fields rife with internal conflicts." This unity and consensus gives those departments and fields that are more paradigmatically developed more power (Pfeffer, 1992). This power, in turn, can produce higher levels of resource allocations in the form of budgets and higher faculty salaries. Although there is some evidence that within business schools, salaries are higher in fields in which there is more consensus such as finance, accounting, and production and operations management compared to fields such as management and marketing (AACSB, 1992), Moore and Pfeffer (1980) found that the level of a department's scientific paradigm development had no significant effect on faculty acceleration or deceleration in pay advancement at the University of California.

Finally, because paradigm development is associated with power, it affects patterns of citations. In a social network, one would expect to observe more communication from people in positions of lower power directed to people in positions of more power, and people who have more powerful positions should be more central in the structure. In exactly the same way, there is more tendency, when cross-citations are observed, for citations in low-paradigm fields to come from fields that are more paradigmatically developed. For instance, there are many more citations to economics in both the sociology and organizations literature than there are citations in economics to either organizations or sociology. In a 1992 computer-based search of three bibliographic files covering economics, sociology, and psychology, regarding articles addressing topics in any of these fields or organizational behavior, I found the following: in economics there are 105 articles on organizational behavior, 580 on sociology, and 315 on psychology. By contrast, both sociology and psychology files produced more than 1,000 articles referencing economics. If one examines recent issues of any of the leading organizations journals, one would find substantial citations to economic concepts such as transaction costs, efficiency wages, and agency theory, but one would be hard pressed to find a single citation to organizational articles treating these or related topics in any of the major economics journals. Baron and Hannan (In press) reported a 650 percent increase in citations to economists in the *American Sociological Review* and an 1,100 percent increase in the *American Jour-*

nal of Sociology between 1970 and 1980, but no further increase since that time. Although their major point is that there is very little impact of either economics on sociology or the reverse, they noted that "data on cross-journal citation patterns . . . show essentially no influence of the sociology journals . . . either in the late 1970s or at present" (Baron & Hannan, In press: 3).

It is evident that the level of a scientific field or academic departments' paradigm development has a number of effects that follow logically from the impact that consensus and technological certainty have on behavior. It is also clear that a number of these effects are substantively important. But perhaps the most important effect of paradigm development and the consensus implied by that construct is on the subsequent development of knowledge in a field.

Where Does Organization Studies Stand?

The study of organizations has numerous subspecialties, and these certainly vary in terms of the level of paradigm development. Nevertheless, it appears that, in general, the field of organizational studies is characterized by a fairly low level of paradigm development, particularly as compared to some adjacent social sciences such as psychology, economics, and even political science. In addition to the factors already noted (a high rate of citing other social sciences; low salaries compared to other business school disciplines; high rates of manuscript rejection in the major journals), many previous commentators on the field have noted its pre-paradigmatic state. Zammuto and Connolly (1984: 30) noted the low level of interconnection of materials in textbooks, an indicator of a low level of conceptual connection and interdependence. Webster and Starbuck (1988), who examined only industrial and organizational psychology rather than the field as a whole, argued that the development of knowledge was progressing slowly. They called attention to the fact that the strength of relationships reported in research on a set of topics was getting weaker over time. They also cited a study by Campbell, Daft, and Hulin (1982) that asked respondents to suggest the major research needs during the next 10 to 15 years. The 105 respondents produced some 146 suggestions, of which 106 were unique; they were contributed by only one person. Webster and Starbuck believed that this study indicated there was little consensus in the field about what were the most significant research issues.

Miner's (1984) examination of the relationship between usefulness, scientific validity, and frequency of mention by scholars for 24 theories—he found little connection among these three indicators—was prompted by his concern for the absence of a systematically developing scientific paradigm in the field. Burrell and Morgan's (1979) review of only sociological paradigms documented the theoretical diversity in the field. One might have thought, now more than 10 years after the publication of this influential work, that progress would have been made in evaluating the

relative usefulness of these different theoretical foci and winnowing down the avenues to be explored. However, if anything, the field is more fragmented and diverse than it has been. Donaldson (1985) asked whether or not there can be a science of organizations. The debate over theory and method that raged in the early 1980s (e.g., Burrell & Morgan, 1979; Clegg, 1977; Donaldson, 1985) is no closer to resolution today (see, e.g. Marsden, 1993). Indeed, whether or not one wants to achieve a high level of paradigm development, to the extent that implies consensus, is itself open to dispute in the field:

> Their [Burrell and Morgan's] prescription is, in fact, a strategy for achieving plurality and diversity in organizational analysis, a guard against "dominant orthodoxies swamping promising heterodoxies and stunting the growth of innovative theoretical development (Reed, 1985: 184)". (Marsden, 1993: 99)

Proponents of functionalism, postmodernism, critical theory, realism, and many other theoretical approaches today contend vigorously in the study of organizations. Whatever else one might think of this state of affairs, it is, by definition, a state that signifies a field that is fragmented and that does not share the consensus characterizing more paradigmatically developed disciplines.

PARADIGM DEVELOPMENT AND THE ADVANCE OF KNOWLEDGE: POSITIVE FEEDBACK LOOPS

A given level of paradigm development is itself associated with processes that maintain the level of development that exists. In other words, developed fields will tend to advance more consistently and more rapidly, and less developed fields are quite likely to remain comparatively un- or underdeveloped. Fields are unlikely to change their relative positions, but how they do so is an important topic taken up later in this article. At one level, this stability is almost patently obvious. Consider how outcomes affected by paradigm development, listed in Table 1, are themselves likely to affect the subsequent development of knowledge in a field.

Fields that are more paradigmatically developed fare better in the contest for resources. Although at one point the National Science Board argued for compensating funding to ensure the development of disciplines in a pre-paradigmatic stage, there is little evidence that this advice has been heeded and lots of evidence that in this domain, as in others, the rich get richer. As noted previously, university funding patterns magnify the external inequalities in funding in favor of more paradigmatically developed fields. These resources are not likely to be wasted. Because more developed fields receive a disproportionate share of both external and internal funding, such fields are able to mount more extensive research efforts. These more extensive and better funded re-

search efforts are themselves, other things equal, likely to lead to a greater rate of knowledge accumulation in those fields that are already more paradigmatically developed.

Moreover, the fact that research resources and academic talent are more dispersed in less paradigmatically developed fields also has implications for the rate of development of the field. Lodahl and Gordon (1973b: 82) reported that "quality was not associated as strongly with levels of funding in the social as in the physical sciences. The result was less reinforcement of existing quality patterns." They go on to trace the implications of this resource dispersion for the development of knowledge in these already less paradigmatically developed fields:

> The present diffusion of research support may not be advantageous to the development of the social sciences in universities. It is possible that the best talents are scattered and the funds are following them, but it is also possible that social science funding is being diluted because of . . . the low visibility of consequences in the less developed sciences. (Lodahl & Gordon, 1973b: 82)

Funding may be diffused because particularistic factors operate with more effect and because there is in fact less consensus on quality evaluations in the less paradigmatically developed fields. But whatever the factors producing the results, the diffusion of both talent and research support makes the development of knowledge more difficult. Research support is diffused over a larger number of people of varying skills, so that funds are not allocated to what would necessarily be their highest and best use. And the diffusion of talent makes the benefits of interaction and collaboration more difficult to achieve.

In less paradigmatically developed fields in which there is less collaboration and in which taking coordinated action is more difficult, it is less likely that dense networks of researchers crossing university boundaries can or will emerge. There will be fewer, smaller, and less well-organized "invisible colleges." But the very absence of these more tightly integrated, cross-organizational networks makes it more difficult to resolve technical uncertainty and to develop consensus that extends across organizational boundaries. As a consequence, the very absence of consensus and the social organization it promotes makes developing more consensus and technical certainty difficult and highly problematic.

The fact that productivity is less closely tied to pay in less paradigmatically developed fields means that there is less reinforcement for producing research in these fields. Although pay is not the only, or even perhaps the most important, incentive for academics, the diminished connection between pay and productivity cannot provide less paradigmatically developed fields with an advantage in terms of incentive or motivation. Furthermore, the lower rates of manuscript acceptance and greater delays in publication also reduce the positive reinforcement of research for those in fields with less developed paradigms.

There are other effects of the higher journal rejection rates. On the one hand, an 80 or a 90 percent rejection rate means that those who are able to publish should (and do) feel comparatively advantaged, part of an elite and very select group. On the other hand, these high rejection rates mean that by far the vast majority of research effort in the field is wasted. Even if some of the rejected articles are subsequently published elsewhere, there is often more effort put forth as authors revise and rewrite the papers. For papers that are ultimately accepted by the first journal to which they are submitted, it is almost certain that the authors will experience the revise-and-resubmit process, a consequence of the lack of agreement among referees. "At most major journals in the social sciences, the overall recommendations given by reviewers of the same paper correlate only about .25" (Marwell, 1992: iii). Revising and resubmitting papers is a process that is unknown in many of the physical sciences, and it requires additional expenditure of time and effort not on advancing knowledge but on getting one's scientific results placed in the public domain. The lack of certainty in the technology that links activity to consequences means, inevitably, that much more activity will be wasted in less paradigmatically developed fields. Although there may be less wasted activity on the part of the more talented or experienced members of any academic discipline, it is nevertheless the case that a high rate of rejection speaks to a substantial waste of effort and resources—a waste much less likely to occur in fields in which there is more certainty about what to do and how to do it.

The fact that social ties and other particularistic criteria loom larger in decisions about funding, journal publication, and editorial board appointments in less paradigmatically developed fields means that there is less reinforcement provided for quality work and, instead, there is more reinforcement for engaging in political strategies of career advancement. This differential reinforcement must inevitably lead to wasted effort (from the point of view of the development of knowledge and advancement of the field) in influence activities, a point made with respect to organizational resource allocation more generally by Milgrom and Roberts (1988). Thus, the very reward system in less paradigmatically developed fields tends to divert efforts to social influence and political strategies and does not send a consistent message that productive scholarly effort is the surest way to achieve status and recognition.

The fact that coordination is more difficult in less paradigmatically developed fields and collaborative research is less likely as a consequence means that such fields will tend to lose out on the advantages of social facilitation effects, peer influence, and support in the research and teaching process. Although academic teaching and research are not identical to production work, it is likely to be the case that many of the advantages of teamwork now being discovered in other work settings also operate, at least to some degree, in academic environments. If research-

ers work in isolation, it is harder for them to achieve the benefits of social support and intellectual cross-stimulation.

The fact that fields with less developed paradigms are more likely to import ideas from fields with highly developed paradigms (witness the importation of economic concepts and theories into sociology, political science, and organizational behavior) means that the boundaries and domain of the less paradigmatically developed fields are more often in contest and being negotiated. A lot of boundary maintenance and definition activity occurs that would otherwise not be required. Moreover, this process, if carried to the extreme, means that the less developed field simply disappears, as it is taken over by its more developed rival. Although this is one way to develop a field of knowledge (to have its questions subsumed by another, more paradigmatically developed specialty), it is a course of development that leaves the research questions of the absorbed field not necessarily answered in the way they would have been had the field retained its boundaries and its academic integrity.

THE IMPORTANCE OF CONSENSUS FOR KNOWLEDGE DEVELOPMENT

The preceding ideas make the point that the very effects of paradigm development work in a self-reinforcing way to maintain fields at their given comparative level of development. But a more fundamental point should be made; namely, that consensus itself, however achieved, is a vital component for the advancement of knowledge in a field. Without some minimal level of consensus about research questions and methods, fields can scarcely expect to produce knowledge in a cumulative, developmental process.

This argument is neither new nor novel. "Kuhn (1970), Polanyi (1958), Lakatos (1970), and Ziman (1968) have argued convincingly that some degree of consensus is a necessary though not a sufficient condition for the accumulation of knowledge in science or in any other type of intellectual activity" (Cole, 1983: 134). As Stephen Cole (1983: 134–135) argued:

> Accumulation of knowledge can occur only during periods of normal science which are characterized by the adherence of the scientific community to a paradigm. It is only when scientists are committed to a paradigm and take it as the starting point for additional research that progress can be made. Without agreement on fundamentals, scientists will not be able to build on the work of others and will spend all their time debating assumptions and first principles. . . . Most new and contradictory ideas prove to be of little value. If scientists were too willing to accept every unorthodox theory, method, or technique, the established consensus would be destroyed, and the intellectual structure of science would become chaotic. Scientists would be faced with a multitude of conflicting and unorganized theories and would lack research guidelines and standards.

Webster and Starbuck (1988: 95) noted that an absence of consensus about theories fostered "divergent findings and incomparable studies that claim to be comparable." They argued that theories should play a stabilizing role in the social sciences, as they do in the physical sciences, organizing the collection of data and interpretations of the world, and they should not be discarded too readily or replaced for reasons of fad or fashion. They noted:

> As much as correctness, theories need the backing of consensus and consistency. When scientists agree among themselves to explain phenomena in terms of base-line theories, they project their findings into shared perceptual frameworks that reinforce the collective nature of research by facilitating communication and comparison and by defining what is important or irrelevant. (Webster and Starbuck, 1988: 127)

Whitley (1982: 338) noted that in fields with greater task uncertainty, "local considerations and exigencies will have more impact on the nature of the work carried out and how it is done." Although local adaptation may be useful in some circumstances, it is likely to lead to proliferation of concepts and methods that make the development of knowledge difficult. As Zammuto and Connolly (1984: 32) noted, doctoral students "are confronted with a morass of bubbling and sometimes noxious literature. Theories presented are incompatible, research findings inconsistent." This lack of agreement leads to difficulties in doctoral training, including high rates of attrition, a long period of time needed to complete the degree, and problems in training doctoral students in distinguishing good from bad theory and methods.

In the study of organizations, it is almost as if consensus is systematically avoided. Journal editors and reviewers seem to seek novelty, and there are great rewards for coining a new term. The various divisions of the Academy of Management often give awards for formulating "new concepts" but not for studying or rejecting concepts that are already invented.

WHERE DOES CONSENSUS COME FROM?

Why is it that some fields are more paradigmatically developed and have more consensus than others? One answer to this question, which undoubtedly has some empirical validity, is that there are simply inevitable, irreducible differences across scientific areas of inquiry that are inherent in the very nature of the phenomena being studied and the knowledge of these different subjects. For instance, it may be that people, the subject of organizational science, sociology, and psychology, are simply more unpredictable and difficult to explain than the behavior of either light waves or physical particles or the course of chemical reactions. This answer, however, does not explain the difference in paradigm development between, for instance, economics and either sociology or organiza-

tional studies. Economic activity is activity undertaken by individuals, and there is little evidence that such activity is either more predictable or easier to comprehend than the subject matter of organizational science. Moreover, there are differences even across subspecialties of organizational studies. For example, it is my impression that population ecology is characterized by much more consensus either than the field as a whole or than most other topic domains within it. There is enormous consistency in terms of the methods used (event-history analysis, most often using the computer program RATE), the dependent variables studied, the literature that is cited, and most important, on what are judged to be the next important problems to work on (e.g., Carroll, 1988). This is the one branch of organization studies in which one can frequently hear (as I have) that it is important to get research done and published quickly, because, otherwise, in a year or so, it will be made obsolete by what other researchers are doing. This time urgency, because of the predictable advance of the domain of inquiry, is one sign of a highly developed paradigm.

As one who has been a participant-observer of business schools specifically and universities more generally for some time, it seems clear to me that consensus is, at least to some degree, created and imposed in those fields or subspecialties where it exists. It is imposed in several ways.

Cole (1983: 137–138), describing how science works generally, noted:

> One of the primary mechanisms through which consensus is maintained is the practice of vesting authority in elites. . . . Generally, the stars of a discipline occupy the main gatekeeping roles. . . . For the gatekeepers to establish consensus, they must have legitimated authority. . . . Legitimacy is granted by virtue of one's being a star. If the gatekeeper positions are filled by "average" scientists, it will be difficult for the authority exercised to be granted legitimacy.

The Academy of Management has, for good reasons, intentionally constituted itself as a representative body, and representativeness is a treasured value of the organization. Elitism is shunned. Compare the editorial boards of any of the Academy journals to the editorial board of *Econometrica*, for instance, in terms of the number of different institutions and the institutional prestige represented. Similar comparisons in the officers of professional associations will reveal the same, simple fact: that the Academy and other organizations and journals involved in the discipline of organization studies are substantially less elitist and more egalitarian, and they have spread the distribution of power much more widely than one will observe in more paradigmatically developed physical science fields or even economics. The explicit incorporation of representativeness when slates of officers or editorial board members are selected—where representativeness is defined in terms of geography, public/private college or university, field of specialization, gender, career

stage and academic rank, theoretical perspective, and academic achievement—as is often done in this field, may have a number of desirable effects, but building consensus on a paradigm is not likely to be one of them.

Consensus is enforced when members of a field develop a set of methodological standards and ensure that these are consistently maintained. In the study of organizations at present, a good idea can obviate obvious empirical shortcomings. For instance, although some researchers of the effects of personality on organizational behavior adhere to methodological rigor, many authors do not (see, e.g., Davis-Blake & Pfeffer, 1989). In fields with highly developed paradigms, researchers also prefer some issues and points of view rather than others. The difference in this case is their commitment to enforce a set of research standards that are more central to the definition of science in those fields, more agreed upon, and zealously maintained.

Members of high-paradigm fields enforce both theoretical and methodological conformity. They do this by reserving the most desirable places only for those who conform to the disciplinary orthodoxy and criticizing, regardless of their power or the validity of their ideas, those who depart from the established paths. For instance, shortly after the election of Bill Clinton to the presidency in 1992, Robert Reich was appointed to the team advising the president-elect on economic policies. This appointment enraged conventional economists, and an article in the *New York Times* provides a good illustration of how a discipline maintains consensus and enforces its standards:

> Although the general approach of Mr. Reich is increasingly shared by others, his specific work has nevertheless been criticized by trained economists, mostly on the ground that he is a lawyer, not a Ph.D. in economics, and his insights lack the rigor and precision that economists provide through their training in mathematics and economic theory.
>
> That criticism has been one reason that Mr. Reich . . . has failed to get a tenured professorship at the Kennedy School, where he has been a lecturer . . . for more than a decade, under a contract renewed every few years . . . the criticism of Mr. Reich's credentials . . . has intensified with his appointment last week as chief of the economics transition team. (Uchitell, 1992: 17)

Michael Piore (Doeringer & Piore, 1971; Piore & Sabel, 1984) holds a somewhat similar position in the discipline because of his different use of methodology and different theoretical approach. Although he is a tenured member of the economics department at MIT, many economists in private conversations maintain that he isn't really an economist—regardless of their opinion of his work—because he doesn't think like one.

Contrast these examples with organization studies. The contents of

the July 1992 special issue of the *Academy of Management Review*, although perhaps an extreme example of the proliferation of theoretical perspectives ranging from feminism to conversation analysis and radical humanism, make the point that the field not only has, to use the current political parlance, a very large "tent," but a tent in which fundamentally any theoretical perspective or methodological approach is as valid as any other. Those who study organizations energetically seek out ideas, perspectives, and techniques from numerous allied social sciences, the humanities, economics, anthropology, political science, psychology, and with the current play given to deconstruction and conversation analysis, from linguistics and English.

My argument is, at its heart, a very simple one: A substantial amount of the variation in the level of paradigm development is a consequence of the social structure, culture, and power relations that characterize the discipline (i.e., how it is organized and the factors that create and perpetuate that organization). Here, again, there are forces at work that tend toward stability of whatever system is in place. A field in which control is concentrated in the hands of a comparatively small elite is one in which power is much more institutionalized and control by the dominant paradigm is quite likely to be perpetuated. By contrast, an area of inquiry characterized by diffuse perspectives, none of which has the power to institutionalize its dominance, is one in which consensus is likely to remain elusive and the dispersion in resources, rewards, and activity will be great.

CONCLUSION

There is evidence that disciplines can change in their level of paradigm development, in spite of the many self-reinforcing feedback loops described in this paper. When Lodahl and Gordon (1972) conducted their survey, political science was the least paradigmatically developed field. But over time, actually beginning in the 1960s, political science evolved. Adopting many of the methods and theoretical assumptions of economics, political science became noninstitutional, as researchers emphasized theories that are (a) reductionist (behavior is seen as the aggregate consequence of individual action); (b) utilitarian (behavior is presumed to result from calculated self-interest); (c) functionalist (history and the passage of time tend to produce appropriate and efficient outcomes); and (d) instrumentalist (the allocation of resources and decision making are seen as the central foci of interest) (March & Olsen, 1989: 3). Those in political science have developed a much more coherent, consensually shared paradigm, and it is now probably one of the more paradigmatically developed social sciences.

This evolution may have come at a cost, at least according to some theorists. As the evolution was unfolding and the rational choice paradigm was gaining increasing preeminence, Ball (1976: 172) argued for "being tenacious in defending and tolerant in criticizing research pro-

grams." He noted that in response to Kuhn, critics "emphasized the narrowing of focus and the 'dogmatism' of 'normal science.' If that is what a normal or mature science looks like, then political scientists should want no part of it. Paradigms . . . tyrannize; and so political scientists committed to free inquiry should resist all blandishments to make theirs a 'normal' science" (Ball, 1976: 153). Ball's pleas fell largely on deaf ears as the discipline evolved in the directions already mentioned.

The question for organizational science is whether the field can strike an appropriate balance between theoretical tyranny and an anything-goes attitude, which seems to be more characteristic of the present state. Those who bemoan the present condition of presumed positivist hegemony (e.g., Marsden, 1993) need only to consider economics or political science, adjacent social sciences whose members are interested in many of the same things, to see how truly open and unstructured organizational theory really is.

It is crucial to distinguish among disagreement over (a) the substantive research questions that are considered to be important, or the goals of knowledge development in the field; (b) the ways in which relevant variables should be measured and modeled; (c) the methods used to collect and analyze relevant data; (d) the theoretical models of behavior used to guide the measurement process, to analyze the data, and to comprehend the phenomena of interest; and (e) the rules for determining which approach to each of these four domains is more or less fruitful. A field characterized by disagreements over all five areas will almost certainly be unable to make progress of any consequence. Theoretical and methodological diversity may be adaptive as long as there is some agreement over fundamental goals and on a set of rules to winnow the measures, methods, and theories on the basis of accumulated evidence. In the study of organizations, there appears to have been more agreement on these issues in the past than there is at present, when almost every aspect of the research process is contested.

A diversity in ideas and in methodology can be useful to the field as long as the diversity can be resolved at some point. The question is whether the social structure and organization of the field encourage resolution of diverse ideas or the continued particularistic advancement of separate agendas, often with explicitly political undertones. At present, I believe that the field encourages the development and advancement of differences and separate agendas rather than attempts at integration or resolution. More than 10 years ago, I (Pfeffer, 1982: 1) argued that "the domain of organization theory is coming to resemble more of a weed patch than a well-tended garden. Theories . . . proliferate along with measures, terms, concepts, and research paradigms. It is often difficult to discern in what direction knowledge of organizations is progressing." The situation has not changed, and, if anything, there are now more diversity of ideas and measures and more contention over the rules for organizational science than there were a decade ago.

Richard Marsden (1993: 101) noted:

> Paradigmatic change is not a purely cerebral affair, but depends on the outcome of political conflicts between custodians and opponents of a paradigm. Resistance to change is the norm; breakthroughs typically occur when the hegemony of the "invisible college" is broken.

But Marsden's unit of analysis needs to be extended outside the boundaries of the discipline to adjacent fields of inquiry. In this context, the contest is not just within the various branches of organizational science; it is between organizational science and related disciplines. The hegemony of the invisible college that may be broken is the hegemony of those who have fostered theoretical dissensus.

It already seems clear that the 1990s are not going to be a great decade for higher education in general or for business schools in particular. In state after state, budgets for colleges and universities are being severely constrained, and tuition, in both public and private schools, is rising rapidly. Business school applications are down some 20 percent this academic year and have fallen in the past several years, although not as dramatically. After all the articles in the popular press criticizing business schools and business education, and after the decades of truly phenomenal growth, it is scarcely surprising that the halcyon days are over.

I think we know two things about political processes: (a) power is more likely to be exercised when resources are scarce (e.g., Salancik & Pfeffer, 1974) and (b) unity of perspective and the ability to take collective action with ease provide an important source of power (Pfeffer, 1992). It seems fair to forecast that contests for resources are likely to increase in universities and in schools of administration in the coming years. It also seems reasonable to suggest that the theoretical and methodological diversity and disagreements that characterize the study of organizations are disadvantages rather than advantages in this coming struggle.

Do researchers in the organizational sciences have to become like their competitors to survive and prosper? Must they, to use a phrase of one commentator on this argument, follow the lead of economics and mutilate the phenomena they are studying in order to compete and survive?

Disagreement in theoretical approaches and even in methodology will not prove detrimental as long as there is some agreement about what the fundamental questions or issues are and as long as there are some agreed upon ways of resolving theoretical and methodological disputes. At the moment neither condition holds. There is no commitment to a unifying set of research goals or questions being pursued by varied means. There is no agreement as to whether the field should serve the powerful, presumably business and government interests, or the powerless. There is little apparent agreement about how to resolve the controversies among competing paradigms—not only disagreement about which one is correct or useful, but disagreement about how to even go about figuring

this out. Because of these fundamental disagreements, debates about basic epistemological issues, even though useful at one level, never seem to produce much resolution. Rather, they are repeated periodically, often covering the same ground.

It would be interesting and useful to study the history of related fields such as political science and economics to understand exactly how paradigm consensus was achieved. My sense is that such consensus was developed by a group of individuals forming a dense network of connections and unified view, who then intentionally and systematically took over positions of power and imposed their views, at times gradually and at times surreptitiously, on the field. There seems to be nothing in the natural order of things that suggests that mathematical rigor should be valued over empirical richness or realism. Rather, the criteria, the status hierarchy, and the enforcement of rules were and are very much political processes.

Many researchers entered the field of organizations because of its theoretical and methodological openness and pluralism. But anything carried to an extreme can be harmful, and given the current climate, downright dangerous. Without a recommitment to a set of fundamental questions—perhaps pursued in a multitude of ways—and without working through a set of processes or rules to resolve theoretical disputes and debates, the field of organizational studies will remain ripe for either a hostile takeover from within or from outside. In either case, much of what is distinctive, and much of the pluralism that is so valued, will be irretrievably lost.

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Jeffrey Pfeffer received his Ph.D. from Stanford University. He is currently the Thomas D. Dee II Professor of Organizational Behavior at the Graduate School of Business, Stanford University. His current research interests include the causes and consequences of wage distributions in organizations and human resource management.