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POWER, POLITICS, AND SCIENCE IN THE STUDY OF COMPLEX ORGANIZATIONS

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In 1877 a New York cigar manufacturer grumbled that his cigar makers could never be counted on to do a straight shift's work. They would "come down to the shop in the morning, roll a few cigars," he complained to *The New York Herald*, "and then go to a beer saloon and play pinochle or some other game." The workers would return when they pleased, roll a few more cigars, and then revisit the saloon, all told "working probably two or three hours a day." Cigar makers in Milwaukee went on strike in 1882 simply to preserve their right to leave the shop at any time without their foreman's permission.

In this the cigar workers were typical. American manufacturing laborers came and left for the day at different times. "Monday," one manufacturer complained, was always "given up to debauchery," and on Saturdays, brewery wagons came right to the factory, encouraging workers to celebrate payday. Daily breaks for "dramming" were common, with workers coming and going from the work place as they pleased. Their workdays were often, by 20th-century standards, riddled with breaks for meals, snacks, wine, brandy and reading the newspaper aloud to fellow workers.

—Tom Lutz (2006)

Reading the third edition of Charles Perrow's *Complex Organizations* has felt like a climb down from the tree branch one has been sawing away at to take a renewed look at the forest and the wider landscape. Shifting accustomed sightlines in this way can bring new visions. One aspect of the book's argument that came into sharp focus for me in this reading, which I do not recall from my reading years ago of an earlier edition, is the author's articulation of the relationship between the substance of organizational theorizing and the methodologies engaged in framing that theorizing. Power processes play a significant role here, too, not just in organizations themselves.

To put the point somewhat differently, the prosecution of science is itself organized—there are various epistemic communities, and there are the institutions in which they work—and both its organizations and its organizing are marked by politics and power, including the power of ideas but also of structures that foster and hinder their articulation. In reflecting on where the organizational studies field has been and gone in the 20 years since the publication of that third edition, I find that interests and power are still largely disappeared from US-based theorizing, except in small groves on the outskirts of the disciplinary landscape. I think it important, then, not only to assess the content of the field with respect to whether it has become a more politically inflected theorizing, but also to explore whatever processes might have led it in the other direction, to be lacking in political orientations, or perhaps even to be decidedly apolitical.

It seems to me that the ways of power in organizational *science* may well affect the extent to which power and politics have been, and will be, perceived by theorists as having a presence in organizational *practices*. This is what I engage in this essay. As readings are not disembodied and universal, I begin by locating my own positionality as a reader, to use the term that is increasingly invoked in interpretive research methods, informed by ethnographic and feminist standpoint and other theories arguing for this sort of reflexivity. I then turn to a short review of some of the historical aspects raised by the book in order to set the stage for my central concern with power and politics in the doing of scientific studies of complex organizations. With the help of Perrow's observations and insights, I will show how the historical and scientific narratives are mutually constituting.

THEORETICAL GENEALOGIES

I read *Complex Organizations* as a political scientist—even more specifically, as a political and organizational ethnographer—not as a sociologist; and so questions of power and politics are “self-evident” to me. By this I mean that when I walk in the door and reconnoiter an organization new to me, I naturally attend to its structure, politics, and culture; I must consciously turn my attention toward its human relations and systems features. As I reflect on this book from the perspective of its author's focus on sociology, it suddenly strikes me as odd that there should be no subfield called “political science (or politics) of organizations” parallel to the subfield “sociology of organizations.” Were such a subfield to exist, perhaps there would be a stronger influence of the political in organizational studies. Within the structuration of the US political science discipline, organizational studies have, instead, largely been relegated to the field of public administration, in which the curricular and scholarship apparatuses—the programs and departments, conferences, and journals—reside that could potentially bring a political orientation to organizational studies.

And yet this assignment has its own internal academic-structural politics. The discipline in which the study of political processes in organizations should be a natural has disappeared “organization” within its subfield taxonomy—public administration is largely seen as a sub-subfield of what is called “American” [government]—thereby ghetto-izing its study in a distant, these days not particularly well-regarded suburb

not visited by many. For example, two then-doctoral students at one of the top five political science departments in the US spoke with me not long ago about their respective dissertation projects. Both were exploring aspects of labor-management relationships, each in a completely different setting. It was clear to me that each of them was engaging issues with organizational studies implications: organizational structure, bureaucracy, hierarchy, authority, *power*, and other organizational theoretical concepts played significant roles in their respective analyses. And yet not one of their professors had suggested that they might take a course in organizational studies or read any works in that field—although one had received such advice from a friend *studying in a different department!*

Those political scientists who do study organizational elements today typically present their work as institutional analysis—and that work is, on the whole, marked by the same structural-functionalist or more broadly positivist problematics identified by Perrow in his critique of the sociology of institutions. Moreover, as Peregrine Schwartz-Shea points out, the approach of many institutional political scientists is often so economic in its orientation and theorizing that it drives out an engagement with political concerns (personal communication, September 2006). This was not always the case within political science. In the 1960s in particular, and well into the 1970s, the boundaries among political science, urban politics, public administration, and public policy were not as clearly drawn as they are today. The ranks of political scientists who have seriously considered power and politics in organizations, as Jong Jun reminds me (personal communication, October 2006), include such scholars as Anthony Downs (1967), Norton Long (1962; 2000), Robert Dahl and Charles Lindblom (1963), Aaron Wildavsky (e.g., Pressman and Wildavsky 1973), and James Q. Wilson (1989). The field of policy implementation that flourished following the publication of Pressman and Wildavsky's book (called, simply, *Implementation*, until you got to the subtitle) perforce engaged the dynamics of organizational life in the context of policymaking and its political processes: organizations are, after all, the vehicles through which public policies are implemented.

Mainstream public administration in the US, however, has increasingly become largely instrumental in its concerns. Its heartland is budgeting and finance, privatization, entrepreneurship, achieving excellence with declining resources, personnel (or human resource management), performance measurement, and the like. A perusal of the public administration panels at recent Western, Midwest, or American Political Science Association (APSA) meetings demonstrates that their main concerns are not organizational theoretical. Indeed, public administration programs today offer fewer and fewer courses in organizational theory and behavior. This leaves a chasm separating the mainstream from those public administration scholars theorizing about organizations and administrative practices and their politics and power dimensions. It has been filled primarily by political sociologists, often working from a critical rather than a structural-functional perspective, but their theorizing is still informed more by sociological concerns than those of a more political scientific character. Have the latter disappeared entirely?

I became a student of organizational studies in what would have been, according to Perrow's brief genealogy, its second generation. As a specific focus of disciplinary

concern and organization, the field of study started coming together in the 1960s, as he notes. This isn't to say that there weren't earlier works. Perrow grounds its history on Max Weber (1864–1920) and his writings on bureaucracy; moving forward from there through Chester Barnard and Frederick Taylor to Joan Woodward and contingency or “technological” theory; by way of the Hawthorne studies of the 1930s at the bedrock of the human relations school; Herbert Simon and James March and neo-Weberian decision making; Phillip Selznick and other institutional sociologists; and environmental and economic theorists. Along the way we encounter the case studies of Peter Blau, Herbert Kaufman, and Michel Crozier, among others.

It is worth rehearsing this ancestry, laid out in far greater depth and detail in the book, because significant bits of it seem today largely forgotten or ignored. It is an important genealogy for several reasons. The first concerns the way it frames the academic institutional history of organizational studies as a field. The narrative traces the origins and development of organizational studies back to a time when organizations were treated as a large class of work-related “structures” bearing family resemblances, regardless of their source of funding or kind of ownership. The topics they engaged appealed to those studying organizations in political science and sociology departments and schools of education, planning, government, public policy, law, and social work, as well as business and management.

This was before the massive growth of business schools divided the organizational studies community, segmenting research, associations, journals, and textbooks in ways that increasingly isolated organizational theory from public administration and political topics. The US Academy of Management has become, in so many ways, an Academy of Business. Its conference papers, sections, journals, and presidential addresses have increasingly become preoccupied with the concerns of corporate life, to the near-exclusion of the public, bureaucratic organizational forms found in governmental organizations, nonprofit organizations, nongovernmental organizations, and so on. There are Divisions for the study of public organizations—the Academy of Management has both “Public and Non-Profit” [PNP] and “Health Care Management” Divisions (recognizing that some hospitals and other types of health care institutions are privately owned and funded, rather than public or nonprofit)—but both exhibit the extent of the reach of business and management schools and departments into nonprofit and health care domains and manifest the “businessification” of organizational theory, as they are on the whole devoid of critical, power-related perspectives, being much more concerned with the techniques and technologies of management practices.

That non-corporate organizations are marginalized within the Academy of Management can be seen from the fact that the PNP Division serves as a catch-all for all those “other,” marked organizations, listed in its mission statement as “government agencies, the military, social services, cultural and educational institutions, membership and professional associations, and advocacy, religious, and charitable organizations.” Their marking—a linguistic device that “others” its targets by designating them, through the use of an adjective, as somehow different from the unmarked, “normal” term—is clear from the taxonomy of Division names: no other Division—Management History, Organizational Behavior, Organizational

Development, etc.—stipulates its domain with respect to a particular type of organization. Even health care topics are located in *Health Care Management*, not *Health Care Organizations*.

In a field dominated by the Academy of Management and its journals, the apolitical character of organizational theorizing can be explained in part by the predominance of scholars whose interests lie in studying business and management from executives' and managers' perspectives, in which "politics" and "power" are anathema (rather than, say, from the perspective of labor where they would be "natural") and where conflict needs to be smoothed over. Indeed, this history of expanding managerialism is apparent in the narrative arc of Perrow's account itself. The third edition added a chapter on economic theories of the firm—agency theory, transaction cost economics—the very language of which, quite aside from its theoretical substance, bespeaks its orientation. The managerialist perspective was exceedingly evident in a significant segment of the field of organizational culture studies, which blossomed as the third edition was being prepared. The "corporate culture" portion of this field took up, from the perspective of control, where Taylor left off. This theorizing instructed executives that they need only add more symbols and more rituals in order to bring their modern-day "cigar makers" into line. It denied agency to those on the receiving end of such actions, a point that Kunda's (1992) field research debunked. Such tensions between manager-focused, apolitical theorizing and a more agency-focused, "politicized" theorizing continues to play out in the study of organizational learning and of organizational identity. Recounting the "ancient history" of works by Blau, Dalton, Kaufman, Selznick, Crozier, and others that were core to the field in earlier times serves to argue against a narrowed rewriting of the meaning of "organization," and it de-naturalizes the history inscribed on the field by this growing managerialism.

Rehearsing the heritage of publications is also important for a second reason: it highlights the changing place of case studies, classified by Perrow as part of the institutional school, over the field's history, and this has important methodological implications. One part of Perrow's critique of institutional school theorizing in the 1970s targets the "trivial" organizations they chronicled. Although he found reprieve from this in 1980s-era studies, my sense is that the organizational theorizing practiced in management departments has returned to matters of lesser consequence as judged from a sociopolitical or sociocultural perspective: I find them, overall, devoid of consideration of the ways in which organizational values and their societal settings are mutually implicating. This is, perhaps, one of the by-products of the a-historicism that pervades the textbook culture of the contemporary business management curriculum and attendant theorizing. With few exceptions (e.g., Hatch 1997, arguably not a textbook in the American sense), these accounts have little or no sense of history, as if there were no organizational life before the coming of Ford or General Motors, Jack Welsh or Bill Gates (and even when they include historical material, it is characteristically unreflective, treating knowledge as having accumulated in a linear fashion marching toward progress; for more reflective histories, see, e.g., Jacques 1996, Shenhav 1999).

The earlier, organizationally related or focused case studies included in Perrow's account were grounded in the mundane details of organizational life and work

experiences gathered through “old-style” Chicago School participant-observer or ethnographic qualitative methods. Here, too, a generational amnesia is at play: the current historical narrative often suggested by organizational scholars using such methods, including those seeking to re-ground theorizing in work practices, is that these methods have been missing from organizational studies until the last few years. Whatever problems were introduced by the structural-functional presuppositions that often appeared in earlier organizational case studies—and I hold with Perrow on the liabilities of such assumptions, especially when they are part of a tacit knowledge assumed to be widely shared within the epistemic community studying organizations—they serve as important antecedents marking the long-standing presence and centrality of such methods in this field. Apparently, we need to be reminded, and to remind our students and colleagues, of this history.

The third reason the litany of this work is important is because it points out the utility, and even advantages, of monographs. While their authors did write journal articles and the importance of such publications is not in question, these monographs remind us that organizational studies at an earlier time recognized the importance of a book-length manuscript. It was a necessity for the working out of ideas and argumentation grounded in large amounts of observational data of everyday lived experiences at work in organizations. “Organization” and “work” were both part of the study of organizations then; and so there was room in the discipline for studies of doctors (Becker et al. 1961), of managers (Dalton 1959), of forest rangers (Kaufman 1960), of office workers (Blau 1953;1963; Crozier 1964), and so on.

Today, the reward structure for professional advancement in organizational studies increasingly devalues books. The institutionalized measurements of the value of research, of researchers, and of institutions are the tiered lists of more and less acceptable journal outlets for publishing this research, a practice that began in the United States in one form and has now taken on a life of its own in the Research Assessment Exercise (RAE) in the UK and its equivalents in The Netherlands, Canada, and elsewhere. Many of these evaluation metrics rank monographs lower than journal articles or they do not include books at all. This disincentive pressures researchers not to “waste” their time writing books that literally will not count in the evaluation process, and it is leading to the replacement of book-length doctoral dissertations on organizations with a set of published articles, a practice found in mathematics. The litany of organizational studies’ history serves as a reminder that books had—and might still have—some value from the perspective of knowledge generation, something that is sacrificed by RAE and similar scores used for accounting and disciplining purposes.

I want to suggest by these observations that the apolitical state of affairs in organizational studies today may be explained, at least in part, through a “science studies” perspective that turns a reflexive eye on scientific fields or disciplines themselves, treating them as research sites with their own epistemic communities and work practices (see, e.g., Kuhn 1970; Latour 1987; Latour and Woolgar 1988; Lynch and Woolgar 1990; Traweek 1988; Burawoy 2005; Yanow 2005; for a broad overview from one social scientific disciplinary perspective, see Wæver 2006). In my reading of mainstream organizational studies scholarship in whichever disciplinary

home it resides (i.e., Academy of Management-style research, public administration, and especially public management) published since the third edition of his book appeared, Perrow's critique is still, sadly, accurate. Much of the theorizing conducted across these several fields is both a-historical and apolitical. Power and its processes are ignored by mainstream theorists even though they are central—from political science and other perspectives—to what organizations are about. The fact that there are three—but only three—books written (as of this writing) by established organizational studies scholars in the last 25 years exploring power and politics in organizations (judged by their titles: S. B. Bacharach and Lawler 1980; Pfeffer 1981; Hardy 1995) makes the point. Influenced by Pfeffer's work in particular, and perhaps by Kanter's (1977), textbooks published since the 1980s have added chapters on power and politics in organizations; but the field as narrated in their pages has not advanced much beyond these works.

COUNTER-NARRATIVES

Outside of the mainstream, however, there are at least three groups within the American context working against this grain. Each of them was established at the time Perrow's third edition was published or subsequently, and each draws influences from European theorists and has an international membership. This extra-US influence is not insignificant or immaterial to the reasons that power and political processes figure in the theorizing produced by members of each group.

The Public Administration Theory Network, founded by Guy Adams, created a space for 20 years for those who wanted to theorize about organizations (mostly governmental or nonprofits/NGOs) within their sociopolitical, cultural settings. Its scholarship primarily engaged the implications of the 20th century Continental approaches of phenomenology, hermeneutics, and critical theory for administrative practices within organizations. Other research explored pragmatism, postmodernism, and psychoanalytic theories. Some of the foundational work produced by members of the Network and their associates demonstrated how power and political processes can be central to organizational theorizing (see, e.g., Adams and Balfour 2004; Fox and Miller 1995; Hummel 1987; Ingersoll and Adams 1992; Jun 2006; Stivers 2001; Yanow 1992a, 1992b, 1996; see also Denhardt 1981; Goodsell 1985, 1988; Ramos 1981).

The second group is the APSA's Conference-related Group on Theory, Policy, & Society (TPS) created by Frank Fischer and myself in the mid-1990s, which now also sponsors programs under the aegis of the European Consortium for Political Research, as well as a European conference. This forum provides a home for work, also drawing on Continental and other philosophies, which investigates public policymaking processes, including policy implementation, a field in which organizational studies figures centrally. It has given rise to a major body of work exploring interpretive policy analysis, including language and its rhetorical and argumentative capacities, as an alternative to traditional approaches. Other research engages questions of how local knowledge or organizational and other political impediments enable or impede realizing deliberative, democratic, decision-making

processes that include silent, and silenced, voices (see, e.g., Fischer 2003; Fischer and Forester 1993; Hajer and Wagenaar 2003; Yanow 1996; see also Fischer and Sirianni 1984; Forester 1983). Both TPS and the Theory Network critique the technical-instrumental rationality of the “traditional” (since the 1970s), mainstream public administration, public management, and public policy literatures. They ground their organizational theorizing, instead, in the social realities of power and politics and situate analyses of organizations in their broader, societal settings.

The third group is the Critical Management Studies (CMS) Interest Group within the Academy of Management. Created in 1998 as the CMS Workshop and institutionalized in 2002 as an Interest Group (IG), its domain statement reads:

CMS serves as a forum within the Academy for the expression of views critical of established management practices and the established social order. Our premise is that structural features of contemporary society, such as the profit imperative, patriarchy, racial inequality, and ecological irresponsibility often turn organizations into instruments of domination and exploitation. Driven by a shared desire to change this situation, we aim in our research, teaching, and practice to develop critical interpretations of management and society and to generate radical alternatives. Our critique seeks to connect the practical shortcomings in management and individual managers to the demands of a socially divisive and ecologically destructive system within which managers work (Critical Management Studies 2006a).

CMS-IG has a partially overlapping membership with the Critical Management Workshop whose first conference met in 1999. Based in England, it adds a focus on “labour process studies” to the concerns of its US counterpart (see, e.g., Alvesson 1987; Calás and Smircich 1996; Clegg 1990; Hassard 1993). Members in these two groups often publish in the journal *Organization*, now subtitled “The Critical Journal of Organization, Theory, and Society.” This field of research has been growing rapidly (Critical Management Studies 2006b).

What these three groups have in common is a philosophical or theoretical orientation towards subjectivity and intersubjectivity, not a detached objectivity. An approach such as this emphasize the situatedness of the subject of study—its groundedness in a particular time and place—and such an historical, social, and cultural perspective is much more likely to be attuned to the political as well, including the political dimensions of research. Methodologically, that is, such a stance requires inquiry to be grounded in persons’ lived experiences, along with an attention to the researcher’s own positionality and its potential effect on research processes and findings. Such an approach explicitly acknowledges that the theorist’s perceptions, formed by prior experiences, education, and other elements of personal background, shape what is and is not seen. These play a role in constructing what is taken as organizational and social reality. Such approaches make all three groups a home for political, policy, and organizational ethnographies and other interpretive methods (Yanow and Schwartz-Shea 2006) engaging power processes. Theorists

from all of these groups could mount a cogent, grounded, power-oriented analysis of the cigar makers, their work, and their lives described in the epigraph to this essay.

What I discovered in this reading of his book that I had not seen before is that, as its narrative progresses from early management theorizing to human relations theorizing and beyond, Charles Perrow appears increasingly to share this methodological predilection. Parts of the critique that he sounds point to issues in the philosophy and politics of science in organizational studies. In my reading of current academic events, these issues bear directly on the reasons that power has largely been extruded from US-based organizational theorizing.

POWER, SCIENCE, AND ORGANIZATIONAL STUDIES

From a science studies perspective on organizational studies, the long, overarching sweep of Perrow's account makes clear how much more particular about methods we have become in recent years. The work practices of researchers have changed significantly, as seen, for instance, in such methodological concerns as careful attention to procedural and evidentiary details in the design of research, methods of execution, and "writing up." Perrow writes about leadership studies of the 1940s to mid-1960s that "enthusiasm has often outrun careful research," and, quoting a review article, "Those studies which do appear to support such a thesis [concerning job-enlargement] frequently contain a number of deviations from normally acceptable research practices" (1986, 90). I suspect that, were they to be submitted for review today, many of these articles would not pass muster because of changed research practices.

It is not just review processes and methods, however, which have changed, but ontological and epistemological sensibilities as well. A tension articulated in philosophies of the early 20th century and lurking beneath the surface in empirical social science throughout its remainder has increasingly come to the fore. It resides between the impetus to derive general-universal laws and principles—which eventuates in a science that is context-free, blurring the wide variety of organizations into a single Organization—and a more situation-specific theorizing that is grounded in the details and differentia of context. (This tension may, in fact, explain in part why review processes are now so much more stringent.)

Read today from the vantage point of such concerns, the long lens of Perrow's account brings to light the ways in which the field of organizational studies is, and has been, marked by this tension. The modern, Western roots of organizational studies arguably lie in the work of Max Weber, and so the field developed at the same time that social and evolutionary positivisms were refining their arguments for the logic of a social philosophy (a social "science" in later terminology) to parallel natural philosophy (natural "science"). The central features of this philosophy lay in employing the power of human reason in systematic observations of the social (or natural or physical) world in order to derive generalizable, universal laws or principles concerning events or behaviors in that world. This was part of a revolutionary displacement of authority that shifted locating Truth and behavioral principles from divine-oriented faith or decrees of human priests or monarchs to

the unmediated, reasoned judgment of any individual. Progress would come from the layering of newly discovered, reason-derived, universal principles on the backs, or shoulders, of earlier ones. So, Weber observed the specific case of the Prussian military and derived a set of general principles, still in use, to characterize bureaucratic forms of organizing wherever they exist. The logic of this “science turn” can also be seen in Taylor’s principles of “scientific” management, Fannie Farmer’s “scientific school of cooking,” Mary Baker Eddy’s “Church of Christ, Scientist,” and other such applications of social positivism to the social and political worlds of the 19th century as it turned into the 20th, and beyond.

Beginning toward the end of the 19th century, this science turn was critiqued by phenomenologists and hermeneutic philosophers, who rejected the possibility of a context-free science and generalizable principles. Whether for reasons of language (the original arguments were written in German and only began to be published in English translation toward the middle of the 20th century) or some other impediment, the first hesitations concerning universal organizational and management principles did not appear until Joan Woodward published her initial fieldwork in 1958. This analysis—what today we would call multi-method, given its use of survey, case study, and interviews—of 100 industrial firms introduced the idea of contingent principles as an alternative to universal ones. This contingency was particularly reflected in Woodward’s (1965) research design, in which she discussed findings with the participants in the research project prior to publishing the report, a practice she may well have pioneered, which is increasingly used today in interpretive-qualitative methods to enhance the trustworthiness (a.k.a. “credibility” or “validity”) of research (Erlandson et al. 1993, 133; Schwartz-Shea 2006).

The idea of contingency displaced universality not only within the “technological” approach, but also in studies of leadership within the human relations school, such as those undertaken by Fred Fiedler (also in the mid-1960s). Perrow notes that the Vroom-Yetton model found “that leaders do not have one dominant style,” instead using “different ones, depending on the situation” (1986, 92, n. 37). Reflecting on the introduction of the idea of contingency into leadership studies, Perrow writes: “Our strong inclination to *believe* in a dominant style rather than contextual variations in style may be an attribution we make to *convince ourselves of order* in a world of supervision that is at least very flexible, if not actually disorderly” (93, n. 37 continued; emphases added).

The observation holds equally well for any area of organizational studies searching for universal laws: believing may lead to seeing just as much as “seeing is believing.” This understanding harks to one of the central features of the development of social philosophy/science in the 19th century: the idea that generalizable principles will enable us to predict future events, which itself will enable us to control human behavior with respect to those events. Perrow calls us to attend here to the possibility that our desire for predictability and control—for a world of reason and order—may lead us to see these features when they are not there and to theorize them even in the face of evidence to the contrary. With respect to people’s personality and their power positions, he wrote: “We expect certain kinds of behavior . . . so we classify the behavior to fit the expectations . . .” (115). This itself encapsulates a central

phenomenological principle. We bring to a new setting or event our *a priori* knowledge and what Kuhn (1970) called “conceptual boxes.” These, alternately labeled “frames,” “lenses,” “paradigms” or *weltanschauungen*, are developed out of lived experiences, education, family background, and so forth. They filter sense data, enabling us to make meaning of the jumble of light waves and sound waves vying for our attention at any given moment. This filtering process shaped by prior knowledge describes research sense-making as much as it describes the human condition in general. The tension I referred to above is expressed methodologically in the difference between force-fitting observations of nature into prior conceptual boxes, as Kuhn put it, or trying to remain open so that those theoretical concepts emerge from what researchers encounter in the field. The former is predicated upon objectivity; the latter denies that objectivity—the researcher standing outside of that which is researched—is ever possible.

Perrow’s critique goes beyond making the point that much of organizational studies is analyzed and written from a management orientation, a characteristic strongly felt within the Human Relations (HR) model, which presumes individual development (maturity, self-actualization) on organizational terms rather than in terms of some other priorities that individuals might set for themselves (e.g., 1986, 98–99). The HR model is also after universal principles: “. . . it is distinctive of his theory,” Perrow writes about Rensis Likert, “(and most human relations theories) that all organizations are considered to be alike” (102). In Perrow’s view, leadership research and the HR School more broadly are marked, by and large, by a search for the “one best way” of managing, leading, organizing, and so forth—a search embedded in efforts we see today to identify “best practices,” a set of general laws to be implemented universally.

Is there an alternative? Perrow thinks so, and his position echoes the concerns of a hermeneutic phenomenology. “It is quite possible,” he writes, “that our social theories in general, and organizational theory in particular, have been altogether too rational. . . . But what if much of our world exhibits low coherence, accidental interaction and consequences, highly situational (rather than enduring or basic) determinants of behavior . . . ? . . . It is possible that *the stabilities we assert are fictitious* and that the disorderly universe . . . are [sic] far closer to the mark” (117–18, added emphasis). In his critique, Perrow begins to confront the assumptions of instrumental rationality built in to positivist-inflected organizational science with the absence of purpose, or the randomness or chaotic quality, that postmodernist thinkers claim characterizes social realities. Yet one need not proceed all the way to postmodernism to find an alternative approach to research. It is sufficient to observe that the mainstream of organizational theorizing is marked by a positivist methodological paradigm that requires a context-free science; that if power is, indeed, a relational attribute (as Perrow defines it; page 259 ff.), it is more likely to be perceived through analytic methods that are sensitive to relational nuances; that these nuances are highly context-specific, therefore requiring methods that are best suited for accessing contextual data (such as participant-observation)—and then to conclude that the hegemonic paradigm emphasizing statistical analyses of empirical applications of behaviorist theories is unlikely to produce theorizing that is sensitive to power and politics.

If we invoke, instead, the set of methods and their underlying methodologies that put human meaning-making at their center—those that share presuppositions with or derive them from phenomenology, hermeneutics, and so on or that just emphasize different aspects of power relations (P. Bachratz and Baratz 1970)—we begin to develop a research orientation more attuned to perceiving the situated relationalism of power and politics. Perrow finds this set of presuppositions in March and Olsen's "garbage can" theory (1986, 135–40), although he calls it a "primitive digging tool" (138). But more sophisticated tools, resting on these same presuppositions, do exist, such that politically astute analysis need not be restricted to garbage-can-type digging. These are the sorts of contextualized methods gathered under the umbrella term "interpretive"—participant observer, ethnographic, ethnomethodological, deconstructionist, Foucauldian, social constructionist (phenomenological), etc. (Yanow and Schwartz-Shea 2006). They are eminently suitable for perceiving—observing and noting; attending to—political processes.

Since 1986, many scholars in those same three, small, marginal (and perhaps intentionally marginalized) groups mentioned in the first part of this essay, drawing on interpretive methods such as these, have developed a politically informed theorizing about organizations. Their work is joined by increasing acclaim for context-sensitive methodologies and methods such as these in other parts of the academy. These calls, along with research using these methods, recognize the need for reflexivity about the ways in which research and theorizing are not neutral, including the researcher's own positionality. This is one of the central hallmarks of interpretive methods today. It is quite in keeping with Perrow's earlier observation that "... we are becoming more aware that theorizing is not a 'neutral' activity, but one guided by strong interests and values that need to be explicated" (1986, 146).

These methods are particularly suited to research questions that explore discrepancies between word and deed. This orientation dovetails all too nicely with what Perrow calls the "exposé" school of institutional analysis. Certainly, a hermeneutic argument posits a set of meanings "underlying" their representational artifacts; a phenomenological one also assumes meanings that are not necessarily on the surface of lived experiences; and critical theory is at times even more "extreme" than these in insisting that all is not as it might seem. (Some critical theory, in fact, can be as a-contextual as the research Perrow is critiquing, given its insistence that "class" is operative always and that a failure to see this is "false consciousness.") These orientations can themselves lead to research that takes on the character of exposé. Conducted within a realist methodology (e.g., as in "realist" ethnographic research and writing; see Van Maanen 1986), such approaches pose the danger that researchers will assume that what is "under" the surface is what is "really" going on.

It is crucial, then, for researchers to remind themselves that the artifacts (language, objects, acts) they—we—label symbols, myths, rituals, and the like are just as "real" as their underlying meanings, especially for those for whom they have meaning. (The ones that "lose" their reality—such as "dead" metaphors or the weekly meeting which is openly referred to as "the weekly ritual"—have become "empty" symbols, divorced from the meanings that spawned them.) Situated, context-informed studies need not take a realist, exposé orientation; interpretive methodologies and methods

shift the focus to what is meaningful to actors in the situation under study (including the researcher); and in this approach, what is of interest is the multiplicity of (potential) meanings and social realities, not the effort to “discover” which of these is the “real” one. This, *pace* Perrow (1986, 173), is what makes academic inquiry different from other forms: an attitude of doubt, coupled with a systematicity of inquiry processes, rather than an effort to prove something true (Yanow and Schwartz-Shea 2006).

My point is this: it may well be the paradigm of positivist-objectivist presuppositions dominating mainstream, US organizational studies, together with its methodological characteristics and accompanying scientific work practices that require context-free “objectivity,” which keeps theorists from attending to political processes in meaningful ways—indeed, which keeps them from being able to see evidence to the contrary. The view of, and perhaps even desire for, progress, with its requirements for stability, order, and regularity, built in to positivist philosophy, may well carry over into the social scientific theorizing, methodology, and methods under its sway, rendering power either invisible or an aberration that needs to be controlled against. Politically inflected organizational theorizing takes place in those studies that abandon such methodological limitations in favor of interpretive, meaning-focused, situated methodologies.

CONCLUDING THOUGHTS

How we narrate the history of a field of study itself is an exercise in power and politics. Using the concept of genealogy in reference to Perrow’s history invokes its usage by Michel Foucault (e.g., 1979) in reference to the contingent, rather than rationally “inevitable,” aspects of historical development and the presence of power in shaping in human life. It seems to me that the issue of power is and has been, on the whole, more explicit in and central to European theorizing historically, unlike in the United States where it has been largely avoided or declared dysfunctional. And so it is not accidental that the three groups of theorists whose theorizing does attend to power are trans-Atlantic and trans-Pacific in membership and theoretical influence, while much US-based theorizing continues to be internally focused (one might even say ethnocentric) and excruciatingly apolitical. Yet the work of these three groups remains marginal to the American theoretical tradition, despite some profound and well-regarded thinkers within those groups. American-based scholarship advances a concept-driven, top-down analysis; the more European-influenced one, a more grounded, experience- or practice-driven one. Perrow captures the distinction when he writes, “. . . if we see individuals as basically rational, materialistic beings, that will foreclose much inquiry” (1986, 192–3). He makes the point in the context of discussing network theories, but it holds for every aspect of organizational (and other social science) theorizing: their theories shape researchers’ perceptions of what constitutes data (as Perrow notes with respect to managers, page 235; see also Brown 1976).

There is still one other area of theorizing that has challenged this American tradition and, yet, which remains on the margins both of it and of the three non-mainstream groups, overall: feminist theorizing. Kathy Ferguson’s (1984) feminist

theoretical analysis of Weberian bureaucracy theory, for example, remains of central significance for its critical appreciation of the role of power and positionality in the seemingly neutral treatment of that organizational form; yet it is not included in most genealogies of the field. Both mainstream organizational studies and the three smaller groups appear to be “gender-deaf” (Kees Boersma, after F. Wilson 1996; personal communication, August 2006). A political perspective on organizational theorizing and its history might well take a “difference” approach (see Minow 1990) and explore how various of the seemingly apolitical aspects of organizational structures and their environmental and economic entailments and implications rest on assumptions of an unmarked norm, thereby disappearing those who fall into whatever that society deems a “marked” category (e.g., race-ethnicity, physical ability, sexuality, unaccented language facility, citizenship status, and so forth). Even within CMS, the dominant voice considers class, and perhaps race-ethnicity and masculinities, but by and large not these other concerns.

Many of us who came to the study of organizations in the 1970s were motivated by a desire to improve things: management “effectiveness,” surely, but also to make governmental organizations work better for citizens and those who work in them. The first impetus led students to organizational studies programs in psychology departments and schools of education and social work, with their focus on organizational “behavior”; the latter, to departments and schools of government, public administration, planning, and public policy. We cannot accomplish such changes by restricting organizational theorizing to corporations and corporate life; nor can we achieve them if we ignore political processes. Perhaps a greater appreciation for the political dimensions of the work practices of organizational science and scientists—and an expansion of the realm of acceptable methodologies and methods to include those well-suited for the study of power and political processes—is needed before we can move organizational theorizing to where Charles Perrow, and others, believe it should be.

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