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Administrative Science Quarterly, Vol. 47, No. 3. (Sep., 2002), pp. 411-421.

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ASQ Forum

Disconnects and Consequences in Organization Theory? ●

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If we look at the historical roots of the study of organizations, a central concern, a defining question, or theme was "What are the consequences of the existence of organizations?" This concern was deeply embedded in the work of a founding father of our discipline, Max Weber (1964). It can also be found, historically, in the work of Michels (1962) and Burnham (1941). In the '50s and '60s, in particular, there were a number of writers who took up this issue in a variety of ways, including Selznick (1949), Boulding (1953), Presthus (1962), Gouldner (1954), Whyte (1956), Etzioni (1961), and Blau and Scott (1962). The question of the consequences of the existence of organizations was addressed at two levels: first, how organizations affect the pattern of privilege and disadvantage in society; second, how privilege and disadvantage are distributed within organizations. We believe that the former question all but disappeared from discussion in the *ASQ* in the '80s and '90s, while the latter has received only fitful treatments.

Of course, in the '40s, '50s, and '60s, there really was no organization theory as such. There was the sociology of organizations, with some influences from public administration/political science, such as the work of Herbert Simon (e.g., 1960). The towering figure was Max Weber, whose work in the late nineteenth and early twentieth century set the agenda for our understanding of organizations. In the '40s and '50s, it was Philip Selznick who became a critical figure, with his classics on TVA and leadership. In this period, sociology and the study of organizations were synonymous.

Weber's agenda was clearly sociological, in the sense that his concern was with the nature of organizations in society and the justifications for the evolution and existence of particular forms. Thus, he outlined and analyzed the development of charismatic, traditional, and rational-legal authority, with their associated organizational forms (Weber, 1964). His genius lay in relating these patterns of authority to religious beliefs, power, and status. In establishing the rise of rational-legal authority and its associated organizational form, the bureaucracy, Weber made two particularly important points. The first was that not only was this form associated with higher levels of efficiency in the production of goods and services, but because of its particular authority base, a consequence was change in the nature of class and status in society. This was a theme picked up by subsequent writers, such as Burnham (1941), who emphasized the emergence of a managerial class and the societal consequences of the divorce of ownership from control. The second of Weber's points concerns the basis of organizational functioning, in legitimacy and authority. Weber's (1964) phrase was "imperative coordination," pointing out that certain groups within organizations have the right to coordinate, control, and direct, precisely because they have achieved legitimacy in society at large. Because of the changed bases of legitimacy of authority, patterns of privilege within organizations are different; indeed, for Weber, in the modern organization privilege is rationally distributed.

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0001-8392/02/4703-0411/\$3.00.

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The authors acknowledge Christine Oliver and Don Palmer for their encouragement and constructive suggestions.

Selznick can be labeled a neo-Weberian because his work took us into the same kinds of questions, but there is a sense in which he was concerned with the dark side of organizations. In *TVA and the Grass Roots*, Selznick (1949) demonstrated how the formal, rational authority of an organization could be subverted by informal and illegitimate patterns of authority and decision making; he showed that external groups would fight for control of an organization. His use of the concepts of formal and informal cooptation entailed the ideas of legitimacy and illegitimacy in decision making. In *Leadership in Administration*, Selznick (1957) outlined the value base of organizations, demonstrating how leadership is ineluctably entwined with issues of why and what for, as well as how. As a forerunner of modern-day institutional theory, he pointed out that organizations are ineluctably suffused with values. Etzioni's (1961) work also had a neo-Weberian emphasis, classifying organizations on the basis of patterns of compliance, namely, utilitarian, normative, or coercive. The emphasis, as with Weber, was on organizational members' reasons for obeying the orders given to them. He argued that coercion was as much a basis of some organizations as authority. Similarly, Gouldner (1954) examined the unanticipated consequences of bureaucratic functioning, introducing, among other things, the idea of a punishment-centered bureaucracy. Gouldner described the conditions under which those with power attempt to impose bureaucratic procedures on others. Blau and Scott (1962), in one of the first systematic texts on organizations, explicitly classified organizations in terms of who benefits, seeing this as a critical way of thinking about organizations.

Throughout this period we see the following two questions and foci: (1) What is the role and effect of organizations in society? This question focuses on the societal consequences of the existence of organizations; and (2) Who controls organizations? This question focuses on privilege and the exercise of power within organizations. We do not want to make, and could not make, the general argument that these kinds of issues have disappeared from intellectual discourse about organizations. Some writers sustain this tradition, but they are in a minority. Two writers, in particular, who have done so are Charles Perrow (1986) and Stewart Clegg (1989, 1990), the former of whom remains resolutely in both the discipline and the institution of sociology, and the latter, while having made the move from sociology to a business school, would doubtless describe himself as a sociologist of organizations rather than as an organization theorist. And, of course, there is the whole tradition of critical theory that is particularly well represented in Europe but less so in North America. Others, notably Stern and Barley (1996), have deplored the lack of attention to societal consequences and have urged they be reincorporated within organizational theory and research, a position with which we agree.

There is a sense in which the migration to business schools and the emergence of a separate discipline of organization theory is a reflection of how important organizations are in our society. Max Weber was right in giving so much attention to organizations as the central phenomena of modern soci-

ety. Indeed, there was probably a brief time in the '60s when organizational scholars thought that if we studied all societal institutions from an organizational perspective (e.g., education, religion, health, politics), then we would quite quickly have most of the answers to understanding the functioning of society! Books appeared with titles like *The Organizational Society*. An associated thought was that if the study of organizations is so important, then it could no longer be left to small, possibly marginal groups, such as sociologists located in faculties of social science. It required a cadre of scholars specialized in, and devoted to, the study of organizations.

The rapid growth of organizational researchers located in schools of business raised the volume of empirical inquiry. But, as Stern and Barley (1996) noted, it also changed the framing of the central theoretical questions. Proclaiming a discipline of organization theory and locating it within schools of business led to a focus rather different from that which would have occurred within faculties of social science, primarily as part of sociology. The sociological focus was replaced by a more managerial orientation. We would characterize a sociological approach to studying organizations as being concerned with *who controls and the consequences of that control*. The central question emanating from a business school, in contrast, leans more toward understanding *how to understand and thus design efficient and effective organizations*. The perspective is that of the senior manager. These different overall questions lead to quite different sub-questions and research topics. In particular, the question of consequences, i.e., efficient and effective *for whom?*, is usually left unasked. Effectively, organization theory became disconnected from one of its primary disciplines.

The move to business schools means a move away from a location in the university that is inherently critical and often policy oriented (the social science faculty) to one that is professionally oriented, with the purpose of developing and enhancing a particular sector, business specifically and organizational functioning in general (the professional faculty). And, of course, the business school, historically, is dominated by economists and functional disciplines, so there are very strong pressures to demonstrate relevance to the understanding and practice of management. Yet as scholars in the university qua university, we need to think of our wider intellectual role, and the *Administrative Science Quarterly*, as the leading journal for the study of organizations, should be in the vanguard because it reflects and thus promotes the agenda of organizational scholarship.

The issue of consequences is now typically handled in three ways within organization theory. One way is to completely ignore the question, focusing on understanding organizational dynamics as though they can be decontextualized from issues of power and privilege. A second approach is to work with an implicit assumption that shareholder interests should prevail, i.e., that organizations operate for the interests of shareholders, and benefits "trickle down" both to employees and to the rest of society. This could be seen as a revisiting of a sociologically based, open-system, societal model that examines both external and internal aspects of power and

privilege. Of course, this model has been vigorously debated both historically and contemporaneously. Historically, it was linked to notions of classes and power elites (Berle and Means, 1937; Burnham, 1941; Mills, 1956; Florence, 1961). It was posited that a new managerial class was emerging, replacing old power elites in society. These are large questions now rarely pursued by scholars (an exception is Mizruchi, 1992). Instead, the problem of power and control is viewed through the lens of agency theory, seeking incentives that "discipline" managers to serve shareholders. Thus the theme of corporate governance has primarily become an issue not of "Who benefits?" (with the latent ancillary question of "Why should they?"), but of the efficiency and effectiveness of alternative incentive systems. It is the journalists who have latched onto issues of the laxity of corporate governance and who have taken up the old issues of the way in which interlocking directorships and shareholdings are inimical to society because of concentrations of power and self-interest.

A third approach to consequences is through stakeholder models. These models do recognize that organizations serve a variety of constituencies, but the emphasis is usually on how organizations can be more effective and profitable by systematically analyzing those stakeholders and serving their needs. The concept of stakeholder itself keeps us concentrated on the inputs and outputs of an organization and on the centrality of the organization itself, rather than on its effects.

Clegg and Hardy (1996), in their introduction to the *Handbook of Organization Studies*, derived three questions from the prevailing literature: What are the different kinds of organizational forms (analytical description)? How do these various forms arise (causal models)? and How do these organizational forms work (dynamics and processes)? These questions are different from the themes of earlier decades and show the shift of organizational analysis to a concern with the issues and interests of managers and business firms (cf. Hinings, 1988). Topics such as organizational configurations, decision making, change, conflict, globalization, and even aspects of gender and diversity, are looked at within frames such as contingency theory, strategic choice, institutional theory, and population ecology. For the past 30 years, the lens has been narrowed theoretically, but the focus deepened in terms of topics about efficient and effective organizational operation.

A small number of studies that have appeared recently in the *ASQ* explicitly acknowledge the deep economic and political consequences of organizational actions. The influence of organizations on the distribution of power and privilege within society is hardly dealt with. Some approach it (e.g., Zuckerman, 2000; Ingram and Simons, 2000), but somewhat tentatively and incompletely. Palmer and Barber (2001) revealed, tellingly, how the social class of corporate elites relates to acquisition behavior. Davis and Mizruchi (1999: 220) provided a fascinating analysis of how the role of commercial banks in the U.S. has changed, with implications for "political and governance cohesion among the corporate elite." But even

these exemplary studies stop short of examining consequences. *Is* wealth redistributed? *Are* political institutions affected? *Are* life chances influenced? We are informed of structural shifts but not of consequences.

Rather more papers reveal an interest in the intraorganizational dynamics of power, but most contributions only hint at its consequences. Seidel, Polzer, and Stewart (2000), for example, looked at the relationship between ethnic origins and salary negotiations. Westphal and Milton (2000) looked at the influence of minorities on boards of directors. But rarely examined is the ideological structure of organizations (e.g., the assumption of shareholder control as appropriate) or how alternative structures of control affect or might affect different interests within the organization. For example, Hayward and Boeker (1998) provided a fascinating account of how the distribution of power within investment banks affects the behavior of analysts, showing when and why they provide inflated estimates of stock valuations. But these authors did not complete the story by examining how this organizational behavior might affect the functioning of financial markets and thus have implications and consequences for investors, employees, etc. Insofar as researchers do consider consequences, innovation is the consequence of interest. Thus, how organizational configurations vary in their innovative capability is a recurring theme. How structures of advantage are created by organizational configurations is ignored.

Of course, a case could be made that the questions posed historically in our subject are either no longer relevant today or that they are best left to sociologists and political scientists outside business schools. Indeed, in some ways the issues are addressed by sociologists and appear in their own disciplinary-based journals. Organizational researchers inside business schools have their own, full research agendas. Nevertheless, we believe that the questions are very relevant and should be more salient. Aldrich (1999: 7) reminded us that "The concentration of power in organizations contributes not only to the attainment of large scale goals, but also to some of the most troublesome actions affecting us." He went on to suggest, "We might view the growth of organizational society as a record of people enslaved and dominated by organizations. . . ."

Recently, Galbraith (as reported by Steele, 2002) echoed the same warning, cautioning that democratic institutions are vulnerable to the ability of organizations, especially large organizations, to influence public decision making. They may do so wittingly or deliberately. They gain preferential access to politicians and have the capacity to mobilize consequences of significance. They affect the functioning of core societal processes whose consequences reverberate widely. A recent example is the Enron affair, in which the actions and interactions of several types of organizations—publicly traded corporations, public accounting firms, the accounting profession, and regulatory arms of the state—affected individuals and organizations whose "economic futures are tied to the underlying health and resiliency of our capital markets" (Levitt, 2000: 1). We ought, therefore, to think about the effects and consequences of organizations, individually and collectively,

on individuals and collectives. We ought to return to an examination of, and theorize about, effects at the level of a society and, for that matter, the role of organization theory in policy, a very underdiscussed area. And these questions, we believe, should be part of the mainstream dialogue.

Our reason for making this case goes back to the historic concerns. Organizations are neither uniformly benign in their effects, nor are they separable from the sociocultural context that they shape and to which they respond. The answers to questions of consequences, and for whom, that follow from designing more effective and efficient organizations are not self-evident. Much of the early work on organizations developed critiques of bureaucracy that had to do with the ways in which goals and purposes became deflected (cf. Blau, 1951; March and Simon, 1958). And those processes of deflection relate not just to issues of organizational design but to the effects of organizational action on the shaping of societies. And we can see these wider questions being raised in newer topics of organizations and the environment, gender and diversity, and globalization.

Selznick (1949), Gouldner (1954), and Perrow (1986) all pointed to these issues. Particularly, they emphasized that the actual behavior of organizational members is determined more by the conflict between opposing factional interests within an organization, or between those factional groups and such groups outside an organization, than by any overarching goals or unified, legitimate structures. Indeed, such goals and structures are the work of powerful organizational members who have the ability to design structures and systems and manipulate incentive systems. Organizations are arenas of conflict, with senior managers able to achieve dominance.

There have always been critics of the direction that organization theory has taken over the past 30 years. During this time, there were critiques of the predominant, functionalist, positivist approaches increasingly dominating organization theory, especially in North America (e.g., Silverman, 1970; Benson, 1977; Burrell and Morgan, 1979; Zey-Ferrell and Aiken, 1981). In some ways, divergent perspectives have become more acceptable or publicized (e.g., Oakes, Townley, and Cooper, 1998). But these divergent perspectives are seen much more as part of the so-called "paradigm wars" than as raising questions with which mainstream organization theory has to be concerned. Some have argued that U.S. scholarship is somewhat selective. Clegg and Hardy (1996) suggested that the North American intellectual establishment may be potentially unwilling to deal with unwelcome ideas. They believe that this is especially so when they are written and proposed by non-North-American scholars. As they further put it, ". . . protectionism is not atypical of the broader intellectual establishment in the US, where the rationalist, quantitative, normative approaches associated with functionalism and normal science have gained their strongest foothold" (Clegg and Hardy, 1996: 7). Mizruchi and Fein (1999) raised a similar concern, tracing differences in institutional research conducted in the U.S. and Europe. This selectivity of focus, we are suggesting, is partly an unwitting consequence of the shift of organization theory into business

schools, but there is another contributing factor: the decline of attention to history.

Answering questions that focus on the role and effect of organizations in society requires long-term perspectives, a grasp of history, a focus on understanding, so that the complexities of political and social movements are not reduced to dummy variables in a regression equation, and an interest in speculation. Yet there is a sense in which organization theory has become without history, without context, and without time. The focus is thoroughly on being contemporary, being generalizable, and building causal models. This is, of course, a reflection of our times. Even Ph.D. students tend to think that something published before 1985 must be irrelevant. Similarly, focusing on issues of privilege and disadvantage, the role of factional interests, and the exercise of power requires a sense of time and context. The notion of interests itself, however, is not one frequently explored in organizational analysis, and the emphasis on efficiency in organizational design and change presents consequences that may be seen as domination from one perspective but are more likely described as building teams or integrated cultures. In a time of considerable change, we need to conceptualize change as potentially harmful to many organizational members, both because of direct effects such as layoffs and because of the elimination and subjugation of some interests and the creation of others in the patterns of organizational privilege. And such redistributions may occur over considerable periods of time. To understand interests and privilege, it is necessary to know about trajectories and histories, the contexts in which they are born, dissipate, and decay.

At least in part, this amounts to a critique of the model of science used in our discipline and seen in the journals, including the *ASQ*. What the discipline means by developing scientific theories is theories that are generalizable across time and context. Yet it is possible to argue from an examination of the *ASQ* and other major journals (those that count for North American tenure) that our generalized theories are highly contextualized in time and place. That is, they are based on 1980s and 1990s data from North America and, primarily, the U.S.A. Yet we do not see the specification, in the theory, of the impact of the particular time and place as a limiting condition for the theory.

We have already suggested that there are organization theorists who do ask these questions and who serve as potential models for research. For most of his career, Pettigrew (1985, 1990) has been making both theoretical and methodological calls for context and time-sensitive studies. Mizruchi (1992) has repeatedly struck upon the theme of corporate elites and political action. We have also drawn attention to Clegg and Perrow. A recent study that picks up some of these issues (and also lives up to North American standards of rigor) is that of Rueff and Scott (1998) on Bay Area healthcare. Reinforcing our point, however, although Scott is widely recognized as an outstanding organization theorist, the study was actually conducted from within a sociology department.

So What Does This Mean for Research Agendas?

One reviewer of an earlier version of this essay suggested, insightfully, that it read like an exercise in "packing your bags for a trip that you are never going to take." So, in pointing to future directions, we make three suggestions. First, there are certain ongoing areas of research that lend themselves naturally to probing the questions raised above, namely, executive compensation, new organizational forms, globalization, gender and diversity, and organizations and the environment. The issue is to take each of these research areas and develop programs that examine both societal effects and impacts on the internal distribution of power and privilege. Each of these areas is relevant in contemporary terms, and, in principle, an organization theorist could make a critical, policy-oriented contribution. For example, the study of executive compensation is an area in which there has, in fact, been a considerable amount of research published in the *ASQ* (cf. Beatty, 1994; Westphal and Zajac, 1994). And such research is related to research into ownership (cf. Palmer and Barber, 2001), with its historic roots in the major questions raised by Weber. We would suggest that within these studies is the possibility of "extending" analysis to examine consequences. For example, Palmer and Barber (2001) showed that firms that are controlled by owners and run by chief executive officers with core functional backgrounds were less likely to adopt the multidivisional form. Was this because they stood to lose power and privilege from switching to this form? Or do we couch our explanations in efficiency rather than power terms? The concept of interests should be a central one in such studies. Similarly, what does the changing pattern of executive compensation do for the societal role of senior executives? Do we see another new class of highly remunerated and powerful people? What is the power of a Jack Welch, outside the organization, as a spokesperson for business, and how is that tied to his organizational position and management pronouncements? One of the "problems" for us as researchers is that some of this requires a degree of speculation that goes beyond our data, but isn't this what the "Conclusions" and "Discussion" sections of a paper should be for? And shouldn't the *ASQ*, with its leading role, encourage more speculation within the boundaries of a normal paper?

Another research area that has become of more importance is that of the emergence of new organizational forms. The writings of Weber, Berle and Means, Burnham, and Mills came very directly out of the rise of new organizational forms, in particular, the bureaucracy, with its attendant divorce of ownership from control and the rise of the managerial class. Do the new organizational forms that have arisen over the past two decades and that continue to emerge have any similar implications for the distribution of power and privilege internally and externally? Does a globally distributed, differentiated network organization change internal privilege? What effect does a flexible manufacturing organization have on the relationships between occupational and status groupings within society? What difference do joint ventures, strategic alliances, and partnerships make, in these terms? The point is that our research agendas should not be

contained by the boundaries of the organization nor centered on whether changes are efficient for the distribution of goods and services.

These same questions could be rehearsed for studies of organizational globalization, gender and diversity, and organizations and the environment because of their immediate relevance to questions of the internal and external consequences of the existence of organizations and, more particularly, of changes in those organizations. As we follow the changes that occur in organizations, we need to raise questions about the impact of such changes by asking not just whether they are efficient and effective, but efficient and effective *for whom?* And this brings us back to the question of legitimacy. Power and privilege inside and outside organizations have less to do with force or coercion than with the legitimated right that certain groups have to coordinate, control, and direct. Such legitimacy is derived from society at large. What is the linkage between organizational changes and societal legitimation?

Second, and somewhat tentatively, we propose a refocusing of attention. Much organizational analysis focuses on either the individual organization as the unit of analysis or a population of organizations. Recently, there has been a shift in focus to the level of organization fields. One way of opening the analysis of organizations in society would be to reframe studies of fields as studies of institutional processes. These would include, for example, financial systems, the legal system, the political system, the health system, and so on. Of interest would be the roles within these systems played by organizations of different kinds and persuasion and the consequences of them doing so. Central institutional processes within society, such as these, are sequences of interlocked organizations. Perhaps by moving the focus of attention from organizations per se to sequences of organizations within institutional systems, the questions of power and consequences would become more salient.

Third, much of our argument suggests that organization theory has a role as "policy science" as well as management science. Often, journals ask authors, at the end of articles, to comment on the relevance of findings and arguments for practitioners. Probably what is more necessary is that the journal should ask us what the wider policy implications of the research findings are for societal groups and for organizational groups. We need a stronger recognition that organizations have consequences of a varied nature. And editors should force us to speculate beyond the immediate boundaries of our research in policy directions.

Organization theory has more purchase and importance than merely as a contemporary tool for senior managers. It has much to say about the nature of contemporary society, how it is developing (or might develop), and how the way in which we design, operate, and change organizations fundamentally and collectively advantages some members and disadvantages others. Scholars in one stream of organization theory, following Stinchcombe (1965), argue that as organizations evolve, they continue to reflect the imprint of their founding

(Kimberly, 1987; Aldrich, 1999). Our concern is that this may be less and less true of organization theory itself. Both its disciplinary roots in sociology and its grand themes are in danger of disappearing.

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ASQ Forum/Hinings and Greenwood

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