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INTRODUCTION:
TOWARD A SOCIOLOGY OF BUSINESS

Craig Calhoun

The centrality of business activities, organizations and their products to modern, especially capitalist, societies can hardly be questioned. Business patterns endure beyond the actions of particular individuals, reflecting both cultural traditions and social structural foundations and constraints. Business practices constitute a distinct genre and field of social interaction. It is therefore surprising that sociologists almost never conceptualize business as a social institution, almost never give it a chapter in their textbooks, or study it as such. Family, education, health care and law are all much more frequently studied and analyzed as social institions than is business.

Perhaps business is beneath the dignity of sociologists with patrician pretensions and too much the enemy of those on the left. Perhaps it has seemed to some the turf of other disciplines, though that has hardly prevented a thriving political sociology or a growing sociology of the economy. We might think competition from business schools was a factor, at least in the United States, were it not for the prominence of sociologists on their faculties of organizational behavior. As the last instance makes clear, sociologists do study *businesses,* but usually under the rubric of formal organizations, or increasingly, as part of a sociology of the economy. And managers and workers are both studied (though far more the latter than the former). But this is the same as using schools as cases to study presumably universal patterns of organizational

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structure, teachers to study occupational mobility, and students to research work group dynamics, without ever asking anything about education.

The situation may be beginning to change, as papers in this volume make clear. Some of the most important work in this regard comes from studies of businesses in non-U.S. and especially non-Western settings, and/or in comparative research, where institutional arrangements cannot be taken for granted but must be addressed. The recent success of Japanese businesses in competition with the United States has sparked a good deal of attention to business as a social institution in the two countries, and the unification of the European market in stages leading to 1992 has begun to do the same in that context. Nonetheless, though the amount of work on business is increasing, most of it tends still to be caught within various sociological conceptualizations which are not linked into a more general institutional sociology of business.

FORMAL ORGANIZATIONS

Formal or complex organizations (or in the business school context, organizational behavior) is the most common of these. This is one of sociology's historically core areas; some of the best sociological work ever done has been on organizational structure and processes (Scott 1988, offers perhaps the best review). But there is a dominant feature to the conception of the sociology of organizations which is sometimes problematic. This is the assumption that 'organizations' comprise a unified object of study, that findings and explanations will apply to the phenomena 'organizations' as such. This assumption holds to any considerable extent only within a highly objectivistic, formalistic approach to social knowledge. It pushes organizational sociology in the direction of a search for universalistic generalizations rather than comparative historical and cultural specificity. The focus of a large part of the sociological work in the area thus has been on the pursuit of more or less universal findings about the effects of various formal organizational properties or constituent processes on each other—for example, the implications of size for hierarchy. This work has produced a number of such generalizations, but by pursuing only this approach it remains limited to grasping one dimension of the actual workings of the organizations studied. This limit has not been overcome in the recent proliferation of work on 'organizations and environments.' This has expanded the study of organizations to include various sorts of interorganizational and other relations with environments, but not reconsidered the focus on organizations as such (see Aldrich and Marsden 1988, for a good review). The various purposes or activities of organizations—for example, manufacturing, education, health care—have appeared in such studies mostly as control variables, occasionally as more central independent variables to account for variations. But the institutional conditions of the creation and existence of various sorts of organizations have been much less often addressed.[1]

What laws, cultural traditions, and political factors provide a basis and context for the business (or other) organizations of modern society? What sorts of practices and disciplines on the part of agents constitute such organizations and how are they maintained in the face of myriad other possibilities? These are questions seldom asked and even more seldom answered in the sociology of organizations. Its achievements are greatest with regard to those factors—especially organizational structure—that can be treated as basically formal and obtaining wherever organization is undertaken. The intellectual periphery into which organizational sociology casts some light, but which it cannot grasp, is largely the terrain of the specific institutional fields within which organizations are created and operate.

ECONOMIC SOCIOLOGY

One of the most promising developments in recent sociology is the growth of what is variously called economic sociology, sociology of the economy or (to avoid reification) sociology of economic activity.[2] The sociology of business might be seen to fit within this frame, but it should not be equated with it. Many usages of the notion of economy are somewhat at odds with the development of an institutional sociology of business.

In the first place, a large part of the work in "economic sociology" borrows heavily from economics, and borrows not only basic conceptualizations (e.g., "rational choice theory") but an implicit understanding of the economy as a reified *system*, amenable to an external, objectified analysis, like the weather, rather than as a fluid and malleable continuous creation of human activity. In this sense, most economics and the sociology produced on similar foundations are focused on discovering universal laws of social phenomena (or toned down equivalents) and dealing with particular cases by applying those laws. This is directly contrary to institutional analysis, which seeks to explain social phenomena as embedded in particular historical contexts—that is, both particular situations and particular processes of change (see Ragin and Zaret 1983).[3]

Though there seems no necessary reason for this, economic sociology as it has developed has been singularly inattentive to culture. This also may be largely a result of the influence of economics, which has no grasp of cultural phenomena, and of such broader analytic traditions as structural sociology and rational choice theory. The latter in particular has exerted a strong bias in favor of universalizing assumptions about human action rather than attention to cultural variation; coming to terms with culture may be one of the most important next steps if rational choice theory is to grow into a more complete sociological theory.[4]

Beyond problems of reification and inattention to culture—both potentially remediable—economic sociology seems so far pitched at a broader level than an institutional sociology of business. It would of necessity form part of the context for the latter, but there would remain need for attention not just to the more general workings of markets, for example, but to the concrete social relations and activities that instantiate them. A sociology of economic *activity,* thus, might include a sociology of business, though even this would place the emphasis somewhat differently.

The economy, even in a nonreified conception, is an abstract concept describing the confluence of activity from a variety of institutional fields; the activity in each field is shaped by concerns and constraints not altogether economic. In modern capitalist societies, and to varying degrees elsewhere, business is the most central of these institutional fields; others include family, law, education, government or politics, and so forth. Activity in the business field is largely oriented to the production of certain economic effects, like profit or growth of capital. But activity of other sorts also has large economic effects. Moreover, many "economic sociologists" and other analysts would treat as economic activity all actions that proceed on the basis of strategic calculation of costs and benefits, particularly under conditions of scarcity—or even actions that can be analyzed on such assumptions, regardless of their psychological processes (see, e.g., Becker 1976). In this sense, economic activity occurs with regard to a much wider range of topics than the economy as such.

So 'economy' and 'economic activity' are not quite symmetrical terms. And, contrary to recent thinking about overcoming the so-called "macro-micro divide" in sociology, macroeconomics is not a cumulation of microeconomics, but defines a different subject matter, on partially different foundations, with partially different assumptions. Institutions crucial to the economy may be constructed in large part out of activity not oriented to the economy. Business institutions are distinctive for their predominently (though not exclusively) economic focus. In comparative historical terms, the relative specialization of business institutions has been exceptional until fairly recently and is not universal today. In "actually-existing" socialist societies, for example, the absence of a clear distinction of state/political institutions from business/economic ones is crucial to how the economy and the polity work. Despite recent changes, in most such societies, there is not yet a field of business as such.

THE FIELD OF BUSINESS

One of the basic variables or analytic topics in comparative historical sociology is—or ought to be—the ways in which fields are constituted. Social life may be organized into more or fewer fields; they may be differently demarcated. Bourdieu (1980) thus has argued that smaller scale, more traditional societies (he has in mind particularly the Kabyle of Algeria) are distinctive in not being divided sharply into differing fields, in being more unitary—as for example the domains of family and kinship, law, government, economic production, and so forth, are closely overlapping if not identical. When we characterize some societies (not only modern ones) as complex, a large part of what we mean is precisely that they are internally differentiated into fields.[5] These different fields each involve a space of relative positions, relations of force among actors, 'objective' power relations that impose themselves on all who enter the field, and distinctive forms of power or capital that govern the activities of the field (Bourdieu 1985, p. 196, 1987). What Bourdieu offers in this notion of field is a way of describing social institutions that puts the issues of force, power and capital into consideration where functionalism had left them out.

Sociologists must address a wide range of questions to develop a more adequate understanding of business as an institutional field. To begin with, what are the various sorts of agents and groups that operate in the field of business? How are they related in the field of force? How is this field demarcated from others? At one level, the answers are simple. There are individual owners of capital, managers, other employees of various sorts, firms owned and run by individuals, family firms, corporations in which ownership and management are more or less distinct, government agencies, legal institutions insofar as they are mobilized for business purposes, traders on the floor of the commodities market, consultants and a variety of other kinds of actor or units of action. They are related above all by the flow of money and commodities through markets, by internal investments, work relationships and a variety of other sorts of relationships—old boy ties, agency, access to information, and so forth. The demarcation of the field is very vague, not least of all because business culture, relations and practices are so pervasive in modern societies. Where, for example, is the boundary that separates business from politics when one studies the office of the Secretary of Commerce or the lobbying and public relations activities of the Conference Board?

In other words, beginning to list the sorts of actors and groups that operate in the field of business, or the various sorts of structures into which they are organized, does not in itself yield much clarity. Even a good taxonomy requires more theory than is readily available. This is not to say that we are without classical theoretical forebears on which to draw—Weber, especially, but also Marx, Simmel, Schumpeter and others. Nor are we completely lacking in contemporary theorists working on directly germane problems—White (1981, 1983) on markets and agency, Coleman (1982) on the nature of the corporation, Lindblom (1977) and Williamson (1975) in their different ways on the contrast of administrative and market regulation of action. What is missing is a theoretical framework that gives a meaningful definition and topology of the field. Perhaps, needless to say, I cannot offer such a framework here, and though some of the essays in this volume make contributions to this effort, this is not their main task either.

THE CONTENTS OF THIS VOLUME

The papers in this volume fall into three basic groups. The first three address historical changes in American business institutions. The next four explore comparative particularities among business institutions, two on Asian business groups and two on business organizations and their employees in Japan and the United States. The final four focus on the political context of business activity and especially on the role of business elites in the formulation of social policy.

There are obviously a great many other lines of sociological research that should be integrated into a developing sociology of the business field. This volume does not substantially address, for example, such important topics as the study of managerial life (see Jackall 1988), the culture or structure of elite groups like boards of directors (see Mintz and Schwartz 1985; Herman 1981; Hirsch 1986; Useem 1980, 1984), the relationship of business to regulatory institutions (Shapiro 1984), the institutions of small business (Aldrich and Auster 1986; Light 1988), how businesses face risk and legal problems (Heimer 1985; Calhoun and Hiller 1988) and many others. It is not exhaustive, therefore, but suggestive.

One of the key issues for a sociology of business is the historical emergence of the contemporary institutional forms of business. No such form is more important than that of the large corporation.[6] In the first paper, William Roy takes up the problem of explaining the rise of the American industrial corporation—that is, one of the central historical instances of the development of one of the central contemporary forms of business organization. His main point is to suggest a more truly historical alternative to functionalist accounts (such as, in Roy's view, the magisterial one of Alfred Chandler) that explains this development in terms of the "needs" of agents or the capitalist system.[7]

For Roy, the corporation is not a sort of evolutionary adaptation to changing technology or systemic needs, but "a politically instituted organizational manifestation of a class structure based on the socialization of capital." Crucially, he challenges the common notion that the modern business corporation was either a necessary or an uncontested response to new technology. His account focuses on the material, including political, struggles of a class segment in creating this business form. Complementary to it, one might imagine a cultural account of the way in which the idea of a limited liability corporation as a unitary actor of full legal responsibility and powers came to be accepted, not only by courts by by ordinary people within and without such corporations.

In the second paper, Neil Fligstein takes up another aspect of business history—interestingly, one in which Alfred Chandler has also exerted a major influence. This is the question not of the origins of large firms but of the determinants of their growth. Fligstein begins with the point (perhaps not often enough recognized in economic theory) that growth, rather than profits as such, is the most general goal pursued by corporations or on their behalf by those who manage them.[8] He shows that over successive decades changes in strategies and structures greatly affected the growth prospects of the largest American business corporations. The effects of such innovations declined as each became more common; they were also affected by broader economic factors. Contrary to some speculations in recent literature on the population ecology of organizations, the largest of the firms continued to grow at high rates (though this comparison is only within a population of very large firms). Moreover, Fligstein finds some evidence that innovations continue to be used even where they do not produce growth. He explains this as "inertia," but in light of Roy's paper we might wonder about the reasons for this inertia. Does it serve the interests or power of some actors? Is it evidence for or against functionalist (or evolutionary) accounts: for, because such persistence might have to do with the interdependence of different dimensions of organizational operations; against, because this would seem to be a failure to 'adapt' or to meet organizational "needs"?

Lawton Burns' study of the transformation of hospitals from community institutions towards business organizations reveals yet another pattern of historical causation and institutional adaptation. He shows how the various interests (charitable, scientific, prestige and financial) motivating those in charge of hospitals, and the field of forces with which they had to contend, changed through several periods of U.S. history. The overall trend, he emphasizes, is the shifting definition of the hospital's purpose away from the provision of various services in the local community, especially as a charity institution, and towards serving those who pay for health care. Today, hospitals are increasingly owned by for-profit corporations (and thus like other businesses commodified and bought and sold themselves); even where they are still owned locally and/or operated as not-for-profit concerns, business criteria (along with certain technical and formalized quality guidelines) form the main bases on which hospitals seek to demonstrate their worth.

Following Selznick (1957), Burns' vocabulary is one of movement from *institution* as performer of socially and psychologically valued activities in a communal context towards *organization* as an intentionally created and expendable instrument for providing technical services in a rationalized, associative context. He criticizes the "forgetting" of the hospitals' original mandate. We can imagine, however, Roy's suggesting that the absence of an analysis of the actors (class segments?) and struggles that produce the specific changes undermines Burns' critical edge and makes his historical analysis at least partially functional. More generally, following Selznick, he reproduces in a specific context, the broad, canonical western story of a transition from community to association, a version of the shift from tradition to modernity.

The closely related papers by Biggart and by Hamilton and Kao touch on this theme in a very different context, that of the development of business institutions in capitalist Asia. Each sets out to describe certain basic features of business institutions in a country, focusing attention on the specificity of that country's institutions in a comparative context. The cases are similar, moreover, in both the patrimonialism that is crucial to institutional arrangements and the formation of suprafirm business groups.

Biggart's paper on Korea shows the distinctive adaptations made of Chinese, Japanese and American influences within the Korean context. Among the most interesting points for western readers who habitually equate Asian business practices with Japan's is the prominence of conflict within and especially between the Korean *chaebol.* Biggart's Korean case also suggests some useful comparative correction for tendencies to overstate the role of state activity and political dependency of business elites in the West. Western business elites in the early phases of capitalism indeed benefited from a good deal of direct and indirect state assistance (from plundered monastic wealth to favorable contracts), but they were not nearly so manifestly politically created as those in Korea after World War II. More pointedly, western business elites of the eighteenth and nineteenth centuries contributed to the growth of democracy (not always with that intention) by pushing for a firm separation of state and civil society (see discussion in Habermas 1962). Such a separation seems never to have had a significant place on the Korean business agenda. And though it has run into problems recently, Korean patrimonialism has been effective in producing economic growth, perhaps in large part, as Biggart suggests, because it "was a familiar, institutionalized means for political and economic organization." Though her argument is primarily historical in Roy's sense, she shows here an attentiveness to functional dimensions of accounting for the persistence and success of the institutional form, if not its origins.

Hamilton and Kao also show particularities within the partially similar patterns of business institutions in capitalist Asia. In Taiwan as in Korea, the western debate about politics versus markets is inapt insofar as the opposition figures minimally if at all in the local context. Empirically, they show that Taiwan's business groups are smaller than those in Japan or Korea, less central to the overall patterns of business organization, more like conglomerates and less hierarchically integrated, and knit together through "familial" relations rather than either the authoritarian dominance of single individuals (as in Korea) or links of reciprocal shareholding and mutual interests (as in Japan). Perhaps the most important conceptual point of the paper is its argument for a redefinition of the idea of family firm. Our understanding, Hamilton and Kao suggest, should go beyond explicit kin relationships in the western sense, to include all those relationships that are understood in terms of and bound by the norms of kinship. Taiwanese firms and business groups are able to operate with a minimal formal command structure precisely because of this 'familial' production of relationships of trust, loyalty and predictability. Family ties may thus be emotional and instrumental at the same time, much more flexible than western analysts usually assume, and part of a traditional vocabulary which is hardly opposed to all forms of modernization.

Concerns for trust, loyalty and other dimensions of the quality of relationships are raised not only in terms of interfirm links in business groups, but perhaps most often in regard to employer/employee relations within firms. Here the contrast with Asia, particularly Japan, has been especially prominent in the U.S. literature.[9] The papers by Kalleberg and by Leicht and Wallace both take up this comparison.

Kalleberg usefully reviews some methodological and conceptual issues involved in comparative studies of business organizations and their employees. Cross-national studies, he argues (following Gallie 1978), are particularly important precisely because they point up the inadequacy of explanations focusing on a single organizational process, and demonstrate the need for attention to interactions among a variety of factors—economic, political, social—in what I have called an institutional field.[10] He draws on a study of Japanese and American workers for illustration, and suggests a convergence in structural arrangements aimed at promoting more harmonious labor relations (internal to corporate hierarchies rather than market-based) and a sense of commitment to job and employer. He interprets the data from his study (described in more detail in Kalleberg and Lincoln 1988) as suggesting that a "welfare-corporatist" approach to business has been developed that yields a "universally applicable commitment-maximizing organizational form." Cultural influences are felt mainly through historical and continuing impacts on the development of organizational structures (e.g., the more widespread current adoption of the welfare-corporatist form in Japan).

Leicht and Wallace analyze the same data set and broadly follow Kalleberg's methodological suggestions, but reach partially differing conculsions. In particular, they argue that "the effects of organizational structures on work outcomes depend on the purposes that these structures serve within a larger cultural context." They find that support for "job entitlement" is higher in Japan independent of organizational-structural factors, and especially that it is higher among employees better placed in their firms and more shielded from labor market pressures. Structural factors seem to be more important in explaining variance in the U.S. case. Underlying the differences are contrasts in the purposes served in each country by otherwise similar organizational structures.

The last four papers turn from studying the business field as such to examining the relationship of business to the state. Janoski, like Kalleberg and

Leicht and Wallace, is interested in employment patterns and policies. His focus, however, is on the state dynamics that determine the adoption and nature of active labor market policies aimed at full employment. In trying to account for the differences between the United States and the Federal Republic of Germany with regard to provision of employment services and related policies, he implicitly broaches the question of the demarcation of the social field of business. Certain activities—for example, the provision of services to those seeking jobs—are seen as much more substantially the province of business in the United States and of the state in West Germany. Janowski's explanation of the differences is almost entirely internal to the legislative process and focused on interest groups that demand (or fail to demand) strong active labor market policies, for example, trade union associations and "left" political parties. In the present context, however, we are led to wonder about the role of business, in particular the strong opposition of most representatives of the U.S. business field to such policies. It would be interesting to know what in the organization of the U.S. business field, or of the economy more broadly, led U.S. businesspeople and lobbyists to take different positions from their counterparts in Germany. The manifest reasons given by U.S. business groups—doubts about the economic efficiency of active labor market policies—do not seem adequate to the explanatory task.[11] More generally, Janoski does not explore the role of "right-wing" parties very substantially, even though the work of Castles and others has provided evidence that overall social policy outcomes may be determined more by the effectiveness of the right at blocking innovations than by the left's efforts to implement them (see the discussion, and partial modification, of this argument in Orloff and Parker's paper.

One of the biggest debates in political economy at present is between "society-centered" arguments to explain social policy and those focused on state institutions as such.[12] For the most part, "society-centered" means a class-based argument. Increasingly, some proponents of "state-centered" arguments (like Orloff and Parker in this volume) have relabeled their position as "institutionalist," partly, as she notes, to indicate that they do not mean to exclude all extra-state factors from analysis. The two excellent papers by Steinmetz and Orloff and Parker exemplify this debate here, arguing respectively for the substantial correspondence between state social policies and bourgeois class interests, on the one hand, and for the central and significantly autonomous role of the state on the other. This debate raises the issue of how and to what extent the business field can be understood as demarcated from the state. All parties would grant at least some relative autonomy of political and economic institutions, but vary considerably as to its extent. One might suggest (following the specification of the difference between the business field and the more general realm of economic effects outlined above) that the economy (in the sense of the production and distribution of wealth) is never autonomous from politics (contest over relations of power), but that the field (or specific institutional arrangements) of business may have varying degrees of autonomy from the field of the state.

Steinmetz's analysis is set in the context of debate over whether Imperial Germany followed an exceptional path in which the political weakness of the bourgeoisie accounts (as influential arguments have it) for the weak or flawed development of democracy. Steinmetz lends support to those who suggest that the original conception of the German state as highly autonomous from and frequently at odds with business interests is faulty. He argues that the state had more limited autonomy and even though businesspeople did not play substantial direct roles in policy formulation, the state nonetheless was obliged to take their interests and opinions into account and did in fact produce policies that were largely congruent with business interests.[13] In particular, he attempts to show that "both poor relief and social insurance policies strongly coincided with the interests of leading fractions of the business elite."

By focusing on business-state relations as such, he advances the line of argument mounted by Eley and others against German exceptionalism; those earlier arguments had attended mainly to questions of democracy and bourgeois values, and somewhat neglected such specific analysis of disputes over institutional arrangements. In connection with this, Steinmetz also makes the very good point that we must distinguish the ideal-typical dimensions 'modern' and 'bourgeois'; we may describe social insurance as more "modern" (i.e., typical of international twentieth-century standards of operation) than public assistance without having said anything of its class character.[14] The debate on German exceptionalism (and more generally on capitalism and democracy) has often tended to elide the two dimensions. It has, moreover, often focused on the central operations of the state to the exclusion of local activities like much of the poor relief. Many forces thus combined to produce the specific character of German social policy, but even though the state retained some autonomy in responding to these various forces and making its own initiatives, its own goals—from stability to modernization to war-making—left it no choice but to make a substantial alliance with modern business.

While also taking a 'moderate' stance in the debate, Orloff and Parker favor the more state-centered or "institutionalist" position. They gain analytical purchase by a comparative design looking at reasons for the different social policies adopted by broadly similar Canada and United States between 1920 and 1940. They show the various ways in which state institutions and activities exert a structuring force on social policy independent of cultural, economic or class factors. Central to their argument is the claim that businesspeople in Canada were less opposed to social policy innovations because they had other reasons (notably Canada's less powerful situation in the international economy, it's earlier minimization of patronage through civil service reform and its more elitist, less fully democratic electoral structure) to favor a more interventionist state and to get used to the idea that state intervention would not necessarily be contrary to business interests. Some Canadian executives actually played a significant role in the development of Canadian unemployment policy, for example, even while their U.S. counterparts in similar companies worked actively to block comparable legislation. Orloff and Parker aim their argument mainly at class and cultural explanations of business attitudes, arguing persuasively for the importance of state institutional arrangements. They do not really consider the institutional organization of the business field as such in any depth. One wonders whether the internal character of the Canadian and U.S. business worlds might have made any significant difference. This is addressed only a little through efforts to show that levels of capital intensity and international organization of specific businesses did not determine their leaders' views on social policy.

On the other hand, a strength of Orloff and Parker's paper is the suggestion that historical trajectories or patterns of change might exert an important influence separate from their embodiment in contemporary social structures. Thus, the prior historical pattern of limited electoral democratization, elitism and elimination of most patronage through civil service reform may or may not have made Canada a more effective or probusiness state (something not specifically argued in the paper), but may nonetheless have affected business attitudes towards social policy innovations directly.

In the final paper, Stephens and Stephens take up the relationship of business to state through the question of how business elites participate in (or respond to) processes of democratization. Their setting is Jamaica, for which they combine construction of a narrative historical account with use of survey data on elite attitudes at several points in the course of democratization. This is perhaps the only historical study of this sort that has been able to benefit from such data. The gist of the Stephenses' findings is that business elites in Jamaica did indeed follow the widespread pattern reported in previous literature of turning from strong opposition to democratization to full acceptance and even defense of democracy when it proved not to be inimical to their class interests. The business elite for which they have data (which they call the bourgeoisie) seems to be a fairly narrow segment of large scale capitalists considered most influential by other elite Jamaicans. Thus, their data do not reveal the views of the business field as a whole; it would seem that many smaller scale business people would fit the description they offer of the middle, rather than upper, class (along with professionals, for example).

The Stephenses interpret their findings as demonstrating, contrary to a wide variety of other analysts, that the bourgeoisie was hardly the historical carrier of democracy. Their argument is persuasive in many regards, but needs three qualifications (beyond the obvious fact that it applies only to one case, and that a postcolonial country still subject to heavy "first world" influence). First, it seems likely to be influenced by the narrowness of their definition of bourgeoisie, as noted above; a broader grouping of businesspeople/townspeople may well have played more of the role ascribed to the bourgeoisie/middle class in most historical accounts. Second, their account is only of electoral democracy, not of the broader ranges of popular political participation. Third, relatedly, even if the bourgeoisie was not the carrier of democracy as such, it may well have helped lay a variety of crucial foundations for it. In the European case, at least, it would seem to have been vital to the development of the state/civil society distinction, to the ideology of free speech and the importance of public discourse about political affairs, and to pressure for extending the range of elites that rulers were obliged to consult about state policy (even though the bourgeoisie may in turn have wanted to close the door after its own inclusion).[15] In other words, as Habermas (1962) has argued, we may owe something of the emergence of the public sphere to the bourgeoisie even if it was in a contradictory position within that public sphere and with regards to its expansion.

CONCLUSION

There is a great deal to be gained, as I think the papers in this volume show, from developing an institutional sociology of business. I have suggested some advantages to conceptualization through Bourdieu's notion of "field," rather than in earlier, largely functionalist, terms that tended to minimize the importance of relations of power to constructing such a field. Business is too important a social field in modern societies to be neglected by sociologists or treated only indirectly through studies of formal organization, economic action, and so on. Moreover, work in the sociology of business might shed significant light on other prominent social problems, such as the recent debate over "state-centered" versus "society-centered" accounts of important social changes. Enough good work has begun to be done in this area, without always defining itself in terms of the sociology of business, that there seems good reason to hope for rapid progress in the near future.

NOTES

1. This is beginning to change with the rise of "the new institutionalism" (cf. Meyer and Rowan 1977; DiMaggio and Powell 1983; Scott 1987; Powell and DiMaggio forthcoming); this is a sociological sibling to the new institutional economics (e.g., Williamson 1985). The focus in such work is primarily on broadening the understanding of organizations to include not only their formal structural dimension and the technical aspects of performance, but also patterns of normative evaluation and bases of legitimation and transformation of organizational forms. In some ways, it echoes the early work of Selznick (1957). Several of the papers in the first half of the present volume reflect something of the same trend, though not all the authors conceive of their work as organizational analysis.

2. See, for example, Stinchcombe (1983), though that is an idiosyncratic original work, more than a review. The development of 'economic sociology' has followed hard on the heels of, and sometimes overlapped with, the revitalization during the 1970s and 1980s of political economy as an orientation within sociology. There are older precedents for a sociology of economic activity as well, from the early Parsons (culminating, perhaps, in Parsons and Smelser 1956), from followers of Veblin and certain Weberians (e.g., Bensman 1967), and from parts of the early reform tradition in American sociology. Even before that, the study of economic activity and institutions obviously loomed large in the work of the founding fathers of sociology, especially Marx, Weber and Simmel (Durkheim also had a fair amount to say on economic issues, but always from the point of view of a sociology more sharply demarcated from economics than the work of the others would suggest).

3. Work labeled 'political economy' generally has been more attentive to historical specificity than most 'economic sociology.' Political economists in sociology have not been a great deal more likely to focus on business institutions, however, except to some extent as an aspect of class analysis, for example, in the context of examining the failures of third world bourgeoisies (see, e.g., Frank 1967; Evans 1982). The phrase 'political economy' in recent sociology has often been a euphemism for Marxist orientations, or more broadly, a reference to work challenging the received wisdom of capitalist economics, and often in the process arguing for a return to a less reified and segmented view of economic life as essentially political. Several articles in this volume follow this tradition. One advantage to developing a sociology of business institutions may be to encourage a greater interrelationship between poltical economy and the nascent sociology of economic activity, both of which are impoverished by the substantial isolation and opposition that ideological commitments (explicit or implicit) have tended to impose on both sides.

4. See Burns (forthcoming), Coleman (forthcoming) and my comments on both (Calhoun forthcoming b). On the neglect of culture in rational action theory more generally, see Wacquandt and Calhoun (1989).

5. Bourdieu is somewhat ambiguous about the matter of historical and comparative analysis; he has not made specific or developed this suggestion about the central role of fields. More generally, various of his works offer different implications as to whether their categories and analyses are to be understood as transhistorical and cross-cultural in application, or whether they are specific to certain societies or sorts of societies. See Calhoun (forthcoming a).

6. We ought not to think the corporation is the only form needing explanation. Because of its relative historical novelty, power and political controversiality, the large corporation has attracted a good deal of attention. But individual entrepreneurship, family firms, partnerships and other business forms are not historically unproblematic. Still, given its centrality and the amount of research done on corporations, it is remarkable that sociological theory offers so little help in conceptualizing just what a corporation is. Coleman (1982) is almost the only important modern sociological work on this subject.

7. It seems to me, in passing, that Roy somewhat overstates his case. First, Chandler's argument turns not only on the technological and organizational functions served by the large corporation, but on the extent to which it served the interests of a powerful, emerging group—managers: "It continued to grow so that these hierarchies of increasingly professional managers might remain fully employed" (Chandler 1977, p. 11). Second, it oversimplifies to make the contrast between functional and historical logics simply one of antecedent versus subsequent causal factors. Such a view partially obscures, among other things, the possibility that the causes of a multitude of small innovations may be different from the factors explaining why one of them becomes a prevailing form. When the latter sort of explanation is functional, this does not mean that it attempts to explain origins by subsequent factors. It may mean that it attempts to explain persistence or increasing prominence by "fit" with coincident conditions. Objections may be leveled against such arguments, of course, but they are not quite those Roy specifies in his paper. More generally, a teleology of needs is common to many functionalist accounts, but not essential to

all of them. A sharper contrast may be between accounts in terms of self-regulating systems and those in terms of the power of agents. In the case of many of Chandler's empirical accounts—for example, the idea that railroad and telegraph operations needed more capital than single individuals could raise—one could rephrase "needed" as "were better able to profit from" and make the argument more one of Darwinian selection. This would avoid much of the logical problem, though it would leave the substantive question open as to whether such an account of 'natural' selection is better or worse than one in terms of human power relations.

8. In fact, Fligstein takes care to note that it is various sorts of managers who constitute the crucial actors in his analysis, though corporations are the units for which action is taken and about which its consequences are measured. Thus, successive changes in typical managerial backgrounds and orientations, not the more or less constant legal identity of corporations as such, are crucial to his account.

9. As Biggart notes briefly in her paper, the focus on Japan may be somewhat misleading with regard to the rest of Asia. Korea, with its patrimonial institutions, seems much more open to sharp conflicts between employers and employees.

10. One of his well-taken points is that it is not obvious what is the appropriate unit of analysis in comparative organizational research. he considers two alternatives, the firm and the establishment. Considering the Asian cases (e.g., following Biggart and Hamilton and Kao), however, we might suggest that the business group is also a potentially important unit of analysis.

11. In passing, it is also worth noting that Janoski seems to accept rather uncritically the notion that the German Social Democratic Party and the U.S. Democratic Party (minus Southerners) can reasonably be classified under the common label "left." This seems debatable throughout the period studied, though there were major shifts, especially the SDP's continuing modification of its earlier socialist program. Moreover, the very need to exclude Southerners from the ranks of the Democratic Party in classifying the "left" hints at a further issue: American political parties are much less tightly organized, disciplined and bound by common programs than are German, and the Democratic Party is particularly fragmented (though how much so has also varied over time). Relatedly, the "grand coalition" under Kiesinger may have played a more substantial role in getting key parts of German active labor market policy legislation passed than Janoski considers (compare, for example, Orloff and Parker's point on the advantages of wartime multiparty government in Canada for getting antipatronage policies implemented).

12. In a sense, Janoski's paper is a very state-centered account, though he is not a part of the "school" usually associated with that approach and he focuses more on conventional interest-group/legislative analysis and less on state institutions as such than do Skocpol, Orloff and other advocates of the more state-centered approach (see Evans, Rueschemeyer and Skocpol 1985).

13. In relation to Roy's argument, it is worth noting that Steinmetz follows Cohen's (1978) emphasis on the distinction between a functional explanation of a phenomenon (here, state policy) and a functional evaluation of the interests it served or its other effects.

14. This allows Steinmetz to note, quite correctly, that most aspects of the welfare state stem, at least in part, from motives and forces that are humanitarian, social democratic—the products of workers' struggles or state efforts at legitimation—while leaving open the question of how much they fit or conflict with bourgeois interests or desires.

15. Inclusion was widely argued in terms of a notion of responsibility, which in turn depended largely on property. The bourgeoisie argued that property of other sorts besides land should qualify people for participation in affairs of state (including elections), but at least initially retained the idea that only people of substantial property should be seen as sufficiently responsible (i.e., stable, independent and invested in the future) to be full citizens. The struggle to extend the franchise and other participatory rights further depended primarily on a move away from property qualifications, though on occasion workers advancing claims that their skills should be treated as property in the economic realm also put these forward in the political arena.

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