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Sociology, Other Disciplines, and the Project of a General Understanding of Social Life

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No discipline has contributed more to interdisciplinary social science than sociology, and no discipline has suffered more from the poor showing of that enterprise. Sociology, according to August Comte, was to be "queen of the sciences"; more recent sociologists have generally aspired only to be the most synthetically encompassing of the social sciences. Nonetheless, as Rigney and Barnes put it, "for better or worse, contemporary sociology has inherited a dual identity. In principle it has been a *synthetic* discipline; more often in practice it has served as an *interstitial* discipline, filling in gaps among the other social sciences and working along their borders" (1980:116).

Even sociologists who would claim a central and not an interstitial place for their discipline often argue for a discrete division of labor among the social sciences. Durkheim began the tradition with his determination to distinguish sociology from psychology. Though psychology remains the field most cited by sociologists (see Tables 1 and 2), this has not prevented many leading members of the discipline from carrying on Durkheim's argument. Peter Blau, for example, who is perhaps *the* paradigmatic representative of "standard American sociology" over the last forty years (see Mullins 1973; Collins 1979; Calhoun and Scott 1989), has been consistent in arguing against what he sees as the reductionism inherent in any sociological recourse to psychological factors for explanation. He was as explicit in his attempt to define sociology by opposing it to psychology in his incarnation as an exchange theorist (Blau 1964) as in his later structuralist

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phase, though by the 1970s he argued against the need for macrosociological arguments to seek even partial foundations in microsociology (Blau 1977; 1986). Indeed, Blau has extended his definition of sociology by exclusion of other subject matters to political science and economics; on this view, sociology studies that part of social life that is not politics, economics, or psychology. Blau thus not only distinguishes social from economic exchange (1964), for example, but accepts the conventional definitions of disciplinary boundaries as referring to inherent and substantively significant differences of subject matter (1969). Blau is not atypical in this; he represents a normative position in the discipline. Nearly every introductory sociology textbook on the market carries a paragraph (or several) claiming to distinguish sociology rather sharply from the other social sciences.[1]

Yet sociology retains some claim to be the study of society as a whole or in general. For some, the holistic or generalist claim is simply held in contradiction to the disciplinary division of labor argument. Others disregard the putative division of labor when they go about their own empirical studies, and work, for example, as social psychologists or economic sociologists. Others make reference to the Parsonsian distinction of three systems of action—society, culture, and personality—to justify a claim to deal with all of society while making no more than passing reference to at least some of the other social sciences. This argument is limited, of course, by the presumption that economics, political science, and much of history (not to mention social as distinct from cultural anthropology) must also study the social system, or at least some of its aspects. Most sociologists do not worry about this issue very much (except when teaching introductory sociology, apparently) because they work on such highly specific aspects of social life that any claim to study society in general is something of a religious belief about the eventual cumulative contribution of tens of thousands of separate inquiries, not a description of their own work. Since the demise of functionalist hegemony in social theory, the majority of these empirical specialists have seemed inclined to give up even any frequent ritual homage at the shrine of general sociology. They are content to ignore (or even dismiss as philosophy, not science) those social theories that try to work at that level (and too often general theorists repay the favor by ignoring research).

This paper will argue that the disciplinary division of labor in the

1. The text of which I am coauthor is no exception. The publisher, supported by reviewers at "non-research oriented" schools, in fact insisted in the most recent edition on making stronger distinctions than could be held to correspond to normal disciplinary practice.

social sciences is essentially arbitrary and largely the result of academic politics, but that this has not stopped social scientists from trying to elevate mere convention to the status of rational principle.[2] The principles used to describe such disciplinary division of labor are several: that culture is a matter for anthropologists; that psychologists study individuals while sociologists study some emergent phenomena; that historians study (choose one) dead people or people of particular times and places, while sociologists study (choose the corresponding one) living people or timeless social laws. None of these principles, however, is intellectually coherent and justifiable. For the most part, they are merely the ideological false consciousness of the disciplinarily self-interested and myopic. And, I shall suggest, they are pernicious. They are pernicious because they impede fruitful, multifaceted attention to important *sociological* problems. They are pernicious because they encourage undergraduate students (including future legislators and members of boards of trustees) to adopt a schematic and impoverished view of social inquiry (and noncoincidentally discourage the brightest of these from becoming sociologists). They are pernicious because they discourage adequate attention to the cultural and historical specificity of sociological research and theory. They are pernicious because they reinforce trends toward overspecialization and undermine efforts to give accounts of the fullness of social life. And perhaps most of all, these principles are pernicious because they weaken the usefulness of social science in providing for practically relevant public discourse.[3]

2. I will not attempt to analyze in any depth the factors that produce the pattern of disciplinary specialization or its accentuation in postwar America (see Buxton and Turner, this volume, chap. 11). No doubt, the scale of both universities and disciplines scientism, a turn from "scholarship" (mastery of traditional learning) towards research (production of "new knowledge"), the growth and bureaucratization of funding sources, and nonacademic employers all contributed to the compulsion to establish and enforce boundaries. Disciplines have always been institutionalizations of power (as Foucault has argued); their capacity to organize intellectual discourse has simply been enhanced (relative, for example, to that of the university or the political or literary public). My concern here, however, is not with the general causes of this process, but with its specific effects in and on sociology.

3. An aspect of this can be seen in the introductory courses of all the disciplines, but especially of sociology, where teaching about the discipline sometimes competes with teaching about its alleged subject matter. This is a major reason why such courses seem so focused on jargon, abstract conceptual schemes, and discussion of contrasting theory groups, and why they are currently failing so signally to recruit many of the most talented undergraduates into further study of sociology. Similarly, the discourse of social theory itself is both impoverished and made less publicly appealing by its constriction within disciplinary boundaries and its frequent failure to engage issues of general public

Lest this polemical theme turn some readers aside, I shall offer also, and first, an account of the relations of sociologists to other social sciences during the last forty years. I shall look primarily (perhaps unfortunately) from inside sociology out—that is, at the sorts of relationships we have built with members of other disciplines and the use we have made of their work, rather than the use they have made of ours. A key point of departure for this argument will be an analysis of the pattern of citations in the major sociological journals to articles from journals in other disciplines or interdisciplinary fields. Proceeding one by one through the other major social sciences, I shall try to show why the notion of clear and principled disciplinary divisions of labor does not hold, though there are certainly differences of characteristic or statistically preponderant style and substance among fields. I shall focus in more detail on anthropology and history, two disciplines that share with sociology arguable claims to offer a general understanding of social life. Because of these shared claims, sociologists have tried much harder to adduce principles to rationalize the differentiation of their field from anthropology and history. The de-

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concern, making the connection between those issues as immediately formulated and as embodied in more enduring theoretical problems. Why do Americans look so often to Europeans for great theory? Because Europeans are both less constrained by disciplinary boundaries and more willing to make the link between "scientific" concerns of social theory and public discourse about current social issues. One of the reasons why Parsons is almost alone among postwar American theorists in acquiring international standing and importance in general theoretical discussion is because he too refused to be limited by disciplinary boundaries. Bourdieu noted something of this in an early article on sociology and philosophy in France since 1945. He observed a distinctively high level (by comparison with America) of "intercommunication among French intellectuals in different fields and of different persuasions" (1967:167). This manifested itself, for example, in Simone de Beauvoir's request to see the proofs of Lévi-Strauss's *Elementary Structures of Kinship* while she was writing *The Second Sex;* she wrote an article on Lévi-Strauss's work simultaneous with its publication for the semi-popular but intellectually serious journal *Temps Moderne.* Bourdieu, by the way, is ambivalent about this feature of French intellectual life. While he offers no praise for narrow disciplinary boundaries or the refusal of public engagement, he does see something problematic in the "logic peculiar to the French intellectual field that requires every intellectual to pronounce himself totally on each and every problem" (1967:174). His concern is akin to that of Foucault about theoretical totalism or foundationalism, and is linked to Bourdieu's distaste for abstract, theoretical system-building of the Germanic sort. Bourdieu himself ranges widely across fields, of course, and has not remained disengaged from public discourse (his engagement, moreover, does not involve a sharp distinction between political essays and academic or scientific work). But Bourdieu's rejection of the theoretical and intellectual "totalism" he saw in Sartre, for example, has been deep enough that he has systematically avoided giving a general statement of his theoretical position separable from his various specific investigations.

marcation from psychology has centered on one idea, level of analysis, while economics and political science are generally seen as highly similar enterprises to sociology but focused on specific subject matters that receive their general context from sociology, and that indeed are included within (but not made the focus of) sociology.[4] These accounts will of necessity be brief, undersupported, and schematic; I hope they will be suggestive.

One issue to which I will not do justice is the internal functioning of interdisciplinary fields. Throughout the last forty years there has been a trend towards growth in interdisciplinary social science publications (see Crane and Small, this volume, chap. 5). This is perhaps goods news for interdisciplinary cooperation, though (a) some of them represent quasidisciplines such as urban studies or area studies (see Winsborough, this volume, chap. 7), and (b) the rise of citations to such journals has been offset by declines in citations to the journals of other disciplines.[5] I am particularly sorry not to have been able to take up the issue of the relation of sociology to area studies programs, as these are among the most enduring ventures in interdisciplinary collaboration. The first point at which a significant number of citations to area studies journals turns up in our sample is 1965 (nearly all of that year's large number stemming from a single article by Shmuel Eisenstadt). Such journals are cited with some consistency but not great frequency thereafter. The tendency seems to be for area studies, where they prosper, to be consolidated into a quasidisciplinary field separate from the original disciplines of the practitioners and, correspondingly, to have less influence on sociology or any of the other core disciplines than might be hoped. It is hard to say how much this is due to the ethnocentric insularity of mainstream sociology, and how much to the centrifugal pull of the various area studies programs and fields.[6]

4. Given our present-day concern with boundaries, it is worth recalling that early meetings of the American Sociological Association were often held jointly with those of the American Political Science Association and the American Economics Association. Moreover, in the era of Giddings, Small, and Cooley, public figures from Theodore Roosevelt (while President) to Jane Addams addressed the ASA meetings and were engaged in dialogue with its members (Sica, 1989).

5. The same is basically true of interdisciplinary behavioral science journals, distinguished largely by a greater involvement with psychology, but including such fields as marriage and family studies, social work, and aging.

6. This varies with cultural and geopolitical area, and with social science discipline. Soviet and East European studies, and more recently Asian studies, have had relatively greater prominence, for example, than African studies, especially in political science but also in sociology, largely because of their strategic geopolitical significance. Middle East-

There is important work to be done here, exploring, for example, the question of when and why some interdisciplinary fields develop a substantial and enduring identity, and often a quasidisciplinary status, while others never gel in that way. But I have not done this work, nor even fully conceptualized the issues. Certainly interdisciplinary programs fail, or at least fail to achieve their intellectual promise and the ambitions of their creators, for a variety of organizational and political reasons. Universities enter periods of retrenchment. New programs lose political struggles with traditional departments—for example, over assignment of FTEs (full time equivalents = a measure of enrollments and positions), or over who will make tenure and promotion decisions. Ph.D.'s from interdisciplinary programs often have a hard time finding jobs and an especially hard time finding acceptance within traditional disciplinary departments.[7] My contention, however, is that there are deeper reasons lying under these purely political ones, and they have to do with the attempt to base an essentially arbitrary disciplinary division of labor on intellectual principles. Though spurious, these principles are linked in important ways to disciplinary self understanding and undermine more intellectually sound attempts at disciplinary reconstruction. I will return to these issues in the conclusion, asking about the relation of sociology to the project of providing a general understanding of social life. I take this to be central to our intellectual future, to our ability to attract the best students to sociology, and to how well our work serves to nurture public discourse.

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ern studies have been dominated by political attention to the Arab-Israeli conflicts, with relatively less sociological attention to the societies and cultures of the region. Latin American studies benefit from proximity and the relative accessibility of the relevant linguistic skills (both in the sense that it is easier to learn Spanish or Portuguese—that is, they are less difficult languages and more widely taught—and in the same sense that two languages provide access to the entire continent, unlike polyglot Africa). The presence of relatively well-developed sociological traditions in some areas—notably Eastern Europe and Latin America—serves to boost the involvement of U.S. sociologists in those fields, and also to make it easier for data to be assembled to enable the kinds of studies which can go beyond description to analytic work likely to have a significant impact on mainstream American sociology. African studies has been one of the weakest area studies traditions, though it has gained some support from the interest of African-American students. This has been the area most likely to be seen as deserving anthropological rather than sociological attention, on account of its presumed primitiveness. A variety of other prejudices are also at work to reduce sociological attention to Africa: the low level of economic development, the lesser involvement of American foundations and government funders, and the low level of news media coverage.

7. This has been a problem even for graduates of some of the most distinguished interdisciplinary programs, such as the University of Chicago's Committee on Social Thought and Committee on Human Development, and the University of California at Santa Cruz's Program in the History of Consciousness.

Sociologists and the Other Social Sciences

Sociology is neither the most open nor the most insular of social sciences. In a study of citations in one leading journal per discipline between 1936 and 1975, Rigney and Barnes (1980:117) found sociologists citing articles in sociology journals 58.1 percent of the time, compared to in-citation rates of 41.2 percent for political scientists, 50.7 percent for anthropologists, 73.4 percent for psychologists, and 78.8 percent for economists.[8] The frequency with which sociologists cite articles from outside the discipline declined from the late 1940s to the middle 1950s, remained low throughout the 1960s, then rose gradually through the 1970s and 1980s, returning to a just slightly higher level than at the beginning of the period (see Tables 1 and 2).[9]

8. For reasons that are unclear to me, Rigney and Barnes found a substantially lower rate of *American Sociological Review* citations to psychology journals, and a somewhat higher rate of citation to political science journals, than I did. They regard sociology as having "by far the greatest aggregate tendency to cite other social sciences" (1980:116), though it is not clear that their data show this. They find sociologists citing articles in the other four disciplines studied (psychology, anthropology, economics, and political science) 4.7 percent of the time, while anthropologists cite the others 3.2 percent, political scientists 3.1 percent, economists .9 percent, and psychologists .7 percent of the time. Their analysis, however, completely disregards citations to other social sciences (e.g., geography) and to interdisciplinary behavioral and social science journals (e.g., *Administrative Science Quarterly, Social Science Quarterly, Journal of Family Issues, Demography,* etc.). As a result, their figures for mean percentage of citations to other disciplines seem somewhat misleading. As Tables 1 and 2 show, citations to interdisciplinary journals constitute a large part of the citations outside of sociology proper, especially from the 1960s on. It appears to me that taking these "others" into account would challenge the claim that sociology has by far the greatest tendency to cite the other social sciences. Such "others" account for 23.2 percent of sociologists' citations in the Rigney and Barnes sample, compared to 46.5 percent of political scientists' and 36.6 percent of anthropologists'.

9. The citation count reported here is based on a sample of all articles in one randomly chosen issue per volume of the *American Journal of Sociology* and *American Sociological Review* between 1948 and 1988. These two journals are overwhelmingly the most important in the field, if frequency of citation is an indicator; the next closest, *Social Problems* and *Social Forces* rarely receive more than a fifth as many citations as the lesser of the *ASR* or *AJS*. In order to control for disparities in actual numbers of citations (especially because of variation in the page length of the journals), the analysis is focused on relative proportions of citations. Averaging the results into five-year clusters smoothes out the impact of individual articles (e.g., a single article in 1967 that included 67 citations to biomedical journals—27 percent of the total citations for the issue, and more than 60 more biomedical citations than in any issue sampled five years before or after it). The pattern of decline in citations outside the discipline from the late 1940s through to the middle 1950s is more pronounced in the *ASR;* an unusually high level of citations to psychology and psychiatry journals in one sampled issue of the *AJS* raises the out-citation rate for the 1953–57 period.

Table 1. American Sociological Review Reference Pattern, 1948–87 (in percentages)

	'48–52 (286)	'53–57 (279)	'58–62 (306)	'63–67 (658)	'68–72 (497)	'73–77 (678)	'78–82 (859)	'83–87 (983)	Average
Sociology	46	62	60	55	58	52	45	44	49
Psychology[a]	23	13	12	10	5	10	8	6	10
Interdisc. social science	4	3	3	6	5	5	5	5	5
Behavioral science[b]	2	1	3	5	4	3	5	4	4
Biomed. & phys. science	3	3	2	6	1	5	3	1	4
Org., admin., mgmt., labor	1	0	1	2	3	1	3	6	3
Economics	1	2	2	2	1	2	5	6	3
Population	0	2	1	2	1	0	4	6	3
Political science	0	2	0	0	4	1	3	2	2
Crime, deviance	1	1	1	1	0	2	4	2	2
Public opinion, policy	1	3	2	1	2	2	2	1	2
Education	2	1	1	3	1	3	2	1	2
Stat. and measurement[c]	1	2	2	1	4	1	3	3	2
Pol. econ., development	1	1	0	1	1	1	2	6	2
Anthropology	6	1	5	0	1	3	1	0	1
Law	0	1	2	1	1	1	1	1	1
History	0	0	0	1	1	2	1	2	1
General science	1	0	0	0	2	2	2	1	1
Gender, women's studies	0	0	0	0	0	0	0	1	0
Philosophy	1	0	0	0	1	0	0	0	0
Other	5	2	5	1	3	4	2	4	3

Note: Parenthetical figures in column heads are actual numbers (N)

[a] Includes psychiatry.

[b] Includes epidemiology, marriage and family, youth, adolescence, age.

[c] Unless codable under a discipline.

At the same time, there were substantial changes in the fields from which sociologists drew (and by implication, to which they related). The prominence of psychology, though still great, underwent a secular decline. Anthropology, similarly, became a much less frequent source of citations. On the other hand, economics and the interdisciplinary fields of organizational, administrative, management, and labor studies enjoyed substantial increases in prominence, particularly toward the end of the time period. Similarly, interdisciplinary social and behavioral science publications became more frequent sources and the field of political economy rose from a minor relation (consisting pri-

Table 2. American Journal of Sociology Reference Pattern, 1948–87 (in percentages)

	'48–52 (154)	'53–57 (161)	'58–62 (240)	'63–67 (195)	'68–72 (291)	'73–77 (753)	'78–82 (333)	'83–87 (372)	Average
Sociology	42	38	46	51	57	49	45	49	48
Psychology[a]	11	28	12	9	8	5	18	6	10
Interdisc. social science	9	5	3	3	5	10	6	6	6
Behavioral science[b]	1	1	0	3	5	4	3	8	4
Economics	3	2	0	3	2	2	9	5	4
Org., admin., mgmt., labor	1	3	2	5	1	4	5	0	3
Stat. and measurement[c]	2	1	6	3	6	2	1	2	3
Political science	1	0	1	1	3	2	3	1	2
Public opinion, policy	2	3	4	4	1	1	0	1	2
Education	1	2	4	4	2	1	0	3	2
Anthropology	2	3	4	5	0	2	1	0	2
History	0	0	3	1	1	3	1	4	2
General science	1	1	1	3	3	2	1	0	1
Population	2	1	0	1	2	2	0	1	1
Biomed. & phys. science	1	1	1	1	0	1	2	0	1
Pol. econ, development	0	0	0	0	0	1	1	3	1
Philosophy	0	0	0	0	1	2	0	0	1
Crime, deviance	1	1	0	0	0	0	1	2	1
Gender, women's studies	0	0	0	0	0	0	0	1	0
Law	1	0	0	0	0	0	2	0	0
Other	21	11	12	2	4	7	2	7	7

Note: Parenthetical figures in column heads are actual numbers (N).

[a] Includes psychiatry.

[b] Includes epidemiology, marriage and family, youth, adolescence, age.

[c] Unless codable under a discipline.

marily of articles in the *Journal of Political Economy*) into a significant source of citations (particularly with the rise of interdisciplinary Marxist journals and development studies). Population also grew substantially in the frequency of extradisciplinary citations. Education was a fairly stable source of citations, significant but not major. Political science was a very infrequent source of citations early on but became prominent in the later 1960s and has remained significant, though not always high. The importance of nondisciplinary statistics and measurement journals seems to have grown gradually through the period. Public opinion and public affairs journals were fairly frequently cited in the middle years of the period, the heyday of academic

public opinion research, but much less so at either end. Crime, deviance, delinquency, and corrections drew considerably fewer citations early on than I expected. Some fields were never prominent sources of citations—at least during this period. Philosophy drew only a handful of citations, for example. Gender studies predictably received no citations in the early years, but also very few after the founding of *Sex Roles* and *Signs*. Finally, though it drew nearly four times as many citations overall as gender or philosophy, law still received only half those of the next closest discipline.[10]

The most interesting overall trend in the citation patterns is the dramatic secular rise in number and rate of citations (see Figures 1 and 2). Apparently disciplinary norms changed in the direction of requiring substantially more citations in each article. It is remarkable (from the point of view of the 1980s) how many articles in the *ASR* and *AJS* up to the early 1960s contained no references whatsoever. It seems unlikely that all the increase in citations is due to increase in relevant literature. Some must be due to attempts to "armor-plate" arguments with authorities, perhaps stimulated by rising levels of competition. Some, perhaps, is due to a continuing turn towards more papers attempting to put forward, modify, or test general sociological propositions and fewer describing research results without at least the pretense to such general "theory-building" cumulation. Whatever its causes, the shift in style is remarkable.[11]

10. Obviously, a variety of extraneous factors influence citation count statistics, making citation analysis a fairly blunt instrument for investigating disciplinary patterns. Some fields (e.g., history) are more apt to be cited from books, and thus to be undercounted in this analysis. Other fields (e.g., psychology and biomedical sciences) promote citation to more articles in each published piece, perhaps because of a shorter mean length of articles. Some journals are hard to classify—e.g., *Sociometry* and its successor *Social Psychology Quarterly,* which I have treated as sociology rather than psychology publications because of their affiliation with the American Sociological Association. I have classified journals under "statistics and measurement" only if they were not readily classifiable by discipline (as were, for example, *Psychometrika* or *Econometrica*).

11. This shift is more marked in the case of the *ASR*. There are a number of other, mostly minor, differences between the two journals. These can be noted in Tables 1 and 2, where fields are listed in descending rank order of total citations. A curious difference is the much greater number of citations to periodicals classified as other—a wide range of popular magazines, general intellectual periodicals, and journals from the humanities—in the *AJS* during the late 1940s and 1950s. It may be worth remarking that biomedical science journals, population studies and crime, deviance, and delinquency journals are all cited less often in the *AJS*. Anthropology, history and economics are all cited slightly more often in the *AJS*. The differences are fairly minor, much more striking is the basic consistency between the two patterns. Despite the differences of style and taste represented in each journal, they showed the same preponderance of in-discipline citation and a broadly similar distribution of citations to other disciplines and interdisciplinary fields.

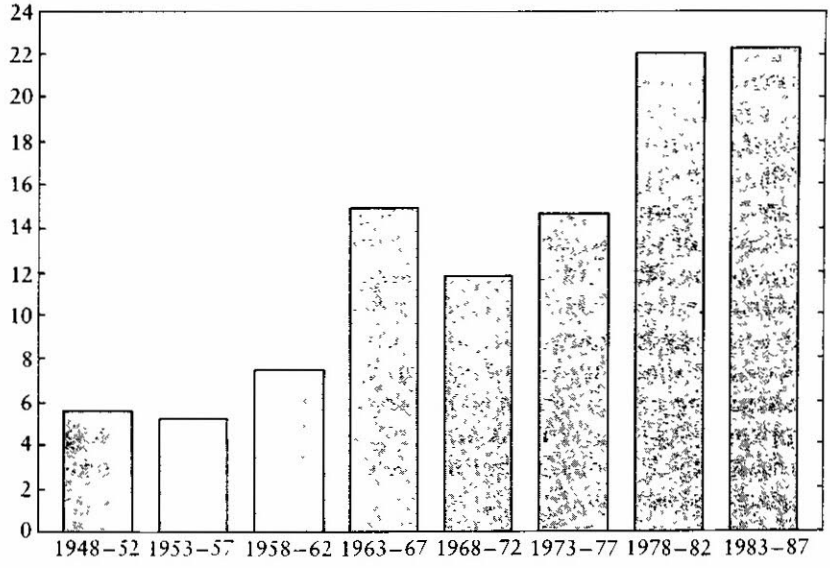

Fig. 1. *American Sociological Review,* Mean Citations per Article, 1948 to 1987.

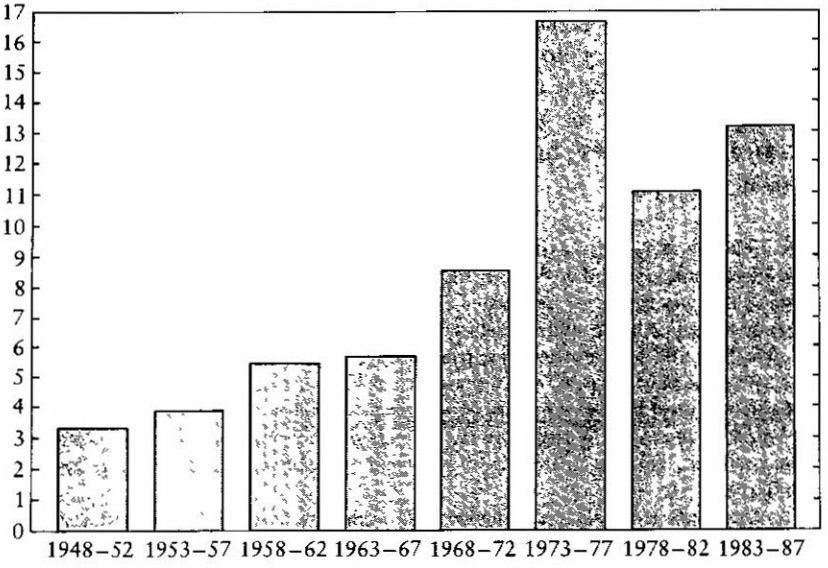

Fig. 2. *American Journal of Sociology,* Mean Citations per Article, 1948 to 1987.

Anthropology

Institutionally, anthropology has shared perhaps the closest relationship with sociology. Anthropology and sociology were taught in a single department in many American colleges and universities until the 1960s or 1970s.[12] Anthropology was the discipline most closely implicated in Talcott Parsons's and colleagues' efforts to develop a general theory of society (and, along with social psychology, figured centrally in the effort to create an integrated Department of Social Relations at Harvard). It shared a broadly functionalist orientation with sociology for many years, and a broadly evolutionary one before that (and in some quarters of each discipline, afterwards). Given this closeness, the overall rate of citation to anthropology journals seems surprisingly low (though this may be partly because anthropology is a field in which books, rather than articles, are the central form of scholarly publication).

The typical account of the division of labor between anthropology and sociology emphasizes that anthropologists deal with "primitive," "preindustrial," or "non-Western" societies. This is sometimes supplemented by the accounts that anthropologists use participant observation methods, and deal with culture, while sociologists use statistics and focus on social structure. The first two accounts are widely offered by both anthropologists and sociologists, while the third (culture versus social structure) is more often, but not exclusively, given by sociologists.[13] All three claims have an element of truth, in terms of statistical frequencies, but little or no standing as intellectual principles of division.[14]

12. Though some departments split earlier, a number—especially in smaller schools—are still joint today. The American pattern is the inverse of the British, where anthropology was the more established and larger discipline several decades ago, and sociology the subordinate sibling. Such prominent British sociologists as Peter Worsley, J. C. Mitchell, and John Barnes were trained in anthropology. Oxford University still has no professorship in sociology, although it has long had one in social anthropology (and although such distinguished sociologists as Mitchell, A. H. Halsey, Steven Lukes, Frank Parkin, and Bryan Wilson have taught there in other positions at advanced stages of their careers). The first professor of sociology at Cambridge was an anthropologist, John Barnes.

13. Anthropology itself has sometimes been split by debates over the priority of culture vs. social structure. The division is associated with that between American cultural anthropologists and British social anthropologists, with Leslie White and A. R. Radcliffe-Brown serving as standard bearers for at least one major confrontation.

14. Discussion in this text focuses entirely on the relationship of sociology to sociocultural anthropology. This minimizes one further impediment to intellectual integration between sociology and anthropology—the inclusion of physical anthropology, ar-

The first proposed principle of division would be better interpreted as making anthropology and sociology two branches of a highly interdependent undertaking. If the object is an understanding of human social arrangements, for example, then presumably both Western and non-Western, industrial and preindustrial, modern and primitive societies must be studied, and those studies related to each other. Of course, one might also call into question the intellectual merits of those distinctions. The primitive-modern typology has been especially widely questioned, largely because of its implied unilineal evolutionism (in fact, nearly every binary opposition is also used to distinguish elements within single societies as more advanced or archaic, as well as to divide up the world's societies). Such distinctions, along with most binary distinctions of overall social patterns (*gemeinschaft-gesellschaft,* status-contract, holistic-individualistic, etc.), also have been criticized as embodying a tendency to divide the world into "us" and "them." This is characteristic, for example, of the classical "orientalism" (Said 1976) that appropriated information about "Eastern" societies primarily in order to make contrasts with the West (whether negative, as in Montesquieu's idea of Persian tyranny; or positive, as in various Romantics' use of Third World societies to pose criticisms of modern Europe). It must be asked whether "preindustrial," "non-Western" and "premodern" do not group together a far greater range of heterogeneous social arrangements than do the countertypes against which they are defined. It is hard to imagine them being adequately rendered as positive rather than negative descriptions. In any case, a sociology that willingly accepts the exclusion of most of the world's societies from its purview must be considered both ethnocen-

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chaeology, and linguistics in the later discipline. Giving a coherent account of this "four fields" approach in contemporary anthropology is becoming increasingly difficult, which helps account for problems anthropology departments sometimes have in wresting positions from deans and developing plans for future development. Though a variety of individual lines of work in anthropology are thriving at present, the discipline as a whole is in something of a protracted identity crisis. A hint of this can be seen in the move a few years ago to create a "Section on General Anthropology" as a subfield within the American Anthropological Association—a remarkable testimony to the fragmentation of the latter. One possible scenario is for increasing division of physical anthropology (which maintains close ties to ecology, evolutionary biology, and anatomy) and possibly archaeology from the rest of the field (Duke University has recently reassigned physical anthropologists from Arts and Sciences to its Medical Campus, distributed archaeologists to various departments and reconstituted anthropology as a Department of Cultural Anthropology). Such actions both achieve a greater internal integration within anthropology departments and remove the portion of anthropology that sociologists find most foreign (though in some cases, they also would leave numerically weak and thereby often politically disadvantaged anthropology departments).

tric and intellectually suspect. Yet that is precisely what American sociology has done, at least implicitly, aided and abetted by the separation of anthropology into a distinct discipline.

The meaning of this separation has been further challenged in recent years from the anthropological side by an increasing attention to "modern, Western, industrial" societies. This has been motivated partly by the disappearance or transformation of many of the traditional small-scale societies with low-productivity technologies that once formed the mainstay of anthropological research. Many anthropologists have accordingly turned their attention to relatively small-scale groupings within large-scale industrial societies—youth gangs, classrooms, intentional communities. A similar shift of attention has also occurred among anthropologists still working in the Third World. It has been motivated significantly by theoretical recognition of the arbitrariness and misleading nature of traditional notions of the self-contained primitive society or "tribe." Where canonical anthropological studies of forty years ago ignored the impact of Western colonialism, long-distance slave trade, and nascent state formation on the putatively small-scale and self-contained social groups they described, more recent anthropological research has been concerned with precisely those impacts. The very notion of tribe has been all but completely rejected, especially with regard to Africa. The interrelations of neighboring peoples within regions and their common subjection to colonial or state power have become central objects of study. Anthropologists working in the Third World today are more likely to participate in the interdisciplinary discourse of political economy as they address state level phenomena, the impact of international trade or capitalist businesses, and questions of the relative capacity of different groups to determine the conditions of their own lives.[15] Anthropologists have challenged the connotations of terms like "primitive," both in general insofar as they carry illegitimate or misleading value judgments, and in particular where the societies gaining anthropological attention are complex civilizations with long written histories, such as China or India.

From the sociological side, the notion of a neat separation of anthropology on the basis of its primitive, non-Western, or preindustrial subject matter has been challenged by a renewed involvement of sociologists in area studies programs, the resurgence of comparative his-

15. Of course, such anthropologists, and especially anthropologists looking at political economic issues in nonexotic societies, are prone to a certain amount of suspicion from more traditional anthropologists who view such work as "too sociological."

torical sociology, and the work of sociologists in development studies. It is still true, of course, that American sociology is remarkably biased towards American society, and prone to draw putatively universal conclusions from work in this single setting. Nonetheless, the comparative dimension is significant. In fact, a substantial comparative dimension was earlier at work under a rubric closely related to the proposed basis for separating sociology from anthropology. During the 1950s and especially the 1960s, "modernization" research and theory was an important part of the discipline. While it did lead sociologists to pay attention to non-Western societies, too often the paradigmatic approach led them to collapse variation into a model of unilinear development. The recent wave of comparative-historical sociology has been more skeptical of such overarching schemes, and more devoted to concrete patterns of variation among specific histories. Comparative sociology is split, however, between those whose work fits this model of contrasting a small number of specific historical cases, and those who pursue large-scale quantitative analyses of many cases. In the later model especially, the cases nearly always represent static snapshots of nation-state indicators.

The methodological argument for a division between sociology and anthropology seems even more suspect, though that does not stop it from being frequently voiced. Indeed, participant observation fieldwork is central to anthropological practice. Some anthropologists do, of course, work with historical documents and collect statistical information. Nonetheless, extended fieldwork remains central and largely definitive of the anthropological enterprise in the minds of both insiders and outsiders. It is understood by anthropologists not only as producing a rich variety of data, but as leading to a deep confrontation with "otherness" that is itself intellectually salutary. An important intellectual tradition is represented by the minority of sociologists who engage in participant observation fieldwork, though relatively few take on projects of the duration or intensity characteristic of anthropologists, at least on the classical model, and fieldwork or participant-observation oriented sociologists seem no more likely to work outside the U.S. (or more generally, their own society) than do others. Some other sociologists, to be sure, come close to reading these fieldworkers and practitioners of qualitative methods out of sociology, and qualitative methods have a second class status compared to quantitative ones in most major graduate programs in sociology. Nonetheless, defining sociology in terms of statistical methods would create so many anomalies as to be impossible. And while ethnography is central to anthropology, it is hardly exclusive to it.

Two other factors, both somewhat associated with the participant observation tradition, may account for a good deal of the impact it has on maintaining disciplinary difference.[16] The first has to do with a distinctive approach to learning the disciplinary craft. Anthropology, (like literature, law, and psychoanalysis) relies very heavily on case studies or classic works as exemplars; sociology relies much more on methodological recipes or strictures. A canonical series of ethnographies plays a central role in learning anthropology. These works—for example, Evans-Pritchard's *The Nuer,* or Malinowski's *Argonauts of the Western Pacific*—are subjected to continuing reanalysis. New theoretical approaches prove themselves, in part, by their ability to shed new light on or make better sense of such classic works, or contrasts among them. Though sociology has its classics, neither a case study method nor the use of such works as exemplars figures anywhere near as prominently as in anthropology.

The second distinguishing factor stems from the idea of confrontation with otherness, the deep recognition of difference.[17] Sociological work tends both to assume a high level of universal applicability for its generalizations, and to focus little attention on sharp lines of difference. This has been evident not only in the neglect of cross-cultural comparison, but in the failure to come to grips better and sooner with gender. Even when, under the influence of the feminist movement, sociologists do take gender seriously, it is often simply by adding a single variable to their analyses, not by considering the theoretical significance of gender as a basic category of consciousness (Harding 1987). The problem of making meaningful statements across lines of cultural difference is a central one for anthropology that has, however, at-

16. That is, while not providing an adequate intellectual rationale for a disciplinary division of labor, these two factors help to account for the persistence of disciplinary differences even as the topical contrast between anthropologists working in preindustrial societies and sociologists studying industrial ones is reduced. Halliman Winsborough (in discussion at the conference from which this volume stems) suggested that a factor that unifies the four fields of anthropology and separates them from sociology is the characteristic "round of life" of each field. Where sociologists worry about the availability of computers and large data sets, anthropologists worry about travel to remote locations and shipment of artifacts. I think there is something to this, though a range of changes from the spread of microcomputers to the increasing ease with which anthropologists can travel to and from field sites are probably reducing its impact.

17. It is true that this is not always the preponderant anthropological message. There is also an implicit argument as to the unity of mankind running through most anthropological work, and sometimes taking the upper hand. Kurt Vonnegut once commented that he studied anthropology at the University of Chicago twice, before and after his military service. The first time they taught him that everyone was the same, and the second time that everyone was different. The two themes meet, of course, in the notion of "finding oneself in the other."

tracted little sociological interest, especially in the United States.[18] This takes us into the realm of the third proposed rationale for the division of labor between anthropology and sociology.

It is not entirely clear why, but the study of culture has been strikingly marginalized in American sociology and, at least until very recently, regarded as more appropriate to anthropologists. Residues of this attitude can be found in most American introductory sociology textbooks. Culture is compartmentalized as the topic of a single chapter, seldom mentioned elsewhere in the book. Religion, for example, is apt to be discussed with little or no reference to the concepts introduced in the culture chapter. In such culture chapters, discussion is based largely on the work of Kroeber, Linton, Mead, and other anthropologists whose canonical works are two or three generations past.[19] In most books there is no mention of Lévi-Strauss, let alone of Sahlins, Geertz, and other leading contemporary cultural anthropologists. Likewise, there is no mention of recent work in literary criticism, philosophy, and history that ought to be seen as central to social studies of culture—for example, the writings of Foucault, Derrida, Lacan, Said, Jameson, or others sometimes lumped together (a little misleadingly) under the label "postmodernists." Of course, texts are biased indicators because their authors are generally forced to avoid theoretical complexity. This tends to minimize not only the attention

18. This actually suggests another significant difference between anthropology and sociology. In the last decade, anthropologists have been far more receptive to the various currents of "poststructuralist" and "postmodernist" thought—partly, in fact, because of their earlier greater involvement in cultural structuralism, but partly also for other reasons, including a general disciplinary receptivity to moral relativism, to the claims that knowledge is essentially power and that all particular intellectual orientations are equally arbitrary, and to a sense of the difficulty or inappropriateness of trying to assimilate the specificity of existence to any general theory.

19. Perhaps one should not blame the authors alone. Aiming for the mass market, publishers have created and the profession has accepted a lowest common denominator approach to introductory texts. This enforces a high degree of similarity and conventionality in offerings. Though all successful books are revised frequently to support the claim that they are up to date, publishers are highly resistant to major changes (and it has to be said that the sociologists whom publishers hire as reviewers tend also to be extremely conservative in their definition of appropriate contents). A publisher's staffer often checks other texts to ensure that precisely the same topics are covered. For example, in my own text, the publisher required that the culture chapter include a discussion of sociobiology—despite my insistence that this had little to do with contemporary sociology of culture. The grounds were that most of the other texts have something on sociobiology in the culture chapter. And, to be sure, at least one reviewer pointed out the absence of such a feature in my draft. At the same time, the publisher ruled out substantial expansion and updating of the section on language, minimized the treatment of new theoretical, anthropological and comparative historical work on culture and asked that I be sure that Margaret Mead and her recent critics stayed in.

paid to the sort of thinkers noted above, but the seriousness with which contemporary sociology of culture is addressed. Few books on the market, for example, feature the work of Pierre Bourdieu in any serious way; fewer still mention Raymond Williams. Nonetheless, the textbooks do tell us something about the discipline.

Especially in the U.S., sociologists have worked with a division of labor in mind that relegated the study of culture with a capital "C" (or "K") to specialists in literature, art, music, etc., and with a small "c" to anthropology. The first part of this division of labor involved a tacit assumption that the specificities of high culture were beyond sociological explanation, were simply matters of opinion or interpretation. In other words, sociologists bought (perhaps unconsciously and even in contradiction to what would have been their explicit, considered judgment) into one of the general, individualistic, self-understandings of modern Western culture, the ideology of artistic genius.

The second part of this implicit notion of a division of labor was enshrined in Talcott Parsons's (1951; Parsons, Bales, and Shils, 1953) division of the realm of human action into the three domains of personality, culture, and society. In a way, this was ironic for Parsons since he came much closer to developing a genuine cultural sociology than did any other major American sociologist of his generation. In Parsons's later work, especially, culture figures as central to the explanation of the continuity, coherence, and change of the social system. In any case, Parsons meant for studies of personality, culture, and society to be constantly interpenetrating (as they were in his own work). While he accepted a disciplinary division of labor, he sought to avoid the kind of separation and purported autonomy of psychology, anthropology, and sociology that has become the norm. Under Parsons's leadership, the Harvard Social Relations Department was oriented to producing students who could engage in what I am told (by Robert Wilson) Charles Dollard once described as the best kind of interdisciplinary collaboration—that which occurs when you have two disciplines inside one skull. On the other hand, Parsons tended, especially in his later work, to make culture into something of an overarching compendium of values, ideas, and orientations to action, a kind of general explanation for everything. This approach to culture did not encourage making it the focus of research so much as the most fundamental of independent variables.[20] In any case, the sociology of cul-

20. Nonetheless, some functionalists produced cultural studies that were much more concrete and historically specific than Parsons's own work (e.g., those of Eisenstadt 1973; Bellah 1957; Geertz 1973). And sociology offered other alternatives: phenome-

ture did not lack foundations on which to build, but it remained a remarkably isolated subfield. It is hard to imagine a sociologist declaring studies of social structure to be simply one topical subfield among many, yet this attitude dogged studies of culture and limited their impact on the discipline as a whole. "Mainstream" sociologists might follow Parsons in describing culture as a sort of general independent variable that exerted a determining force over social life "in the last instance," but they did not make it the object of their studies. The issue was less a shortage of theories raising issues of cultural analysis than the lack of a strong empirical research tradition closely linked both to those theories and to other problem areas in sociology. Some sociologists went out of their way to distinguish themselves from their "softer" brethren who took culture and interpretative research more seriously. Perhaps the foremost example of this is the attempt by the University of Wisconsin department to expunge the theoretical and cultural orientation of Gerth and establish a positivist, primarily quantitative sociology.

For whatever reasons, the sociology of culture became a small and not very active subfield, and until recently thought about culture figured only very slightly or in very general ways in the most influential lines of sociological work. The substantial attention accorded cultural issues by many of the theoretical founders of modern sociology, from

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nology had a long minority following and included distinguished students of culture (e.g., Berger and Luckmann 1966; Berger 1969). American sociologists under the influence of Weber (including especially a number of Gerth's students from Wisconsin) conducted a wide range of culturally-focused research, including a good part of the community studies tradition (e.g., Vidich and Bensman 1968). Sociologists from Bell (1973, 1976) and Stanley (1978) to Riesman (1950), Slater (1970), and Bellah, et al. (1985) have offered cultural criticism of some note. Elias (1978, 1982) produced monumental historical analyses of cultural patterns and their change, though it took the revival of comparative historical sociology as well as of cultural sociology for them to become well-known in the U.S. Marxist sociologists and those influenced by them maintained a vital tradition of cultural analysis. Lukacs, though not formally a sociologist, wrote important social analyses of literature; Goldmann (1964) and Lowenthal (1961) kept his legacy alive. Hauser's monumental *Sociology of Art* (1982) was an effort to bridge Marxist historical and sociological analysis, though it has had little impact in American sociology. The Frankfurt school produced a host of major studies of culture, including both institutional analyses and formal and content-oriented critiques. The works of Adorno and Benjamin figure perhaps most prominently in this regard, but Horkheimer, Lowenthal, and Marcuse all published significant studies. Indeed, it would not be too much to say that more important work on the sociology of culture was done within the Marxist discourse than within that of the sociological discipline proper. The readership of Raymond Williams (e.g., 1958, 1981), for example, has been much wider in literary and Marxist fields than in sociology—especially in the U.S.

Comte through Parsons, was largely forgotten. Selective reading was powerful, especially when aided by a well-defined canon and a reliance on edited snippets and predigested summaries rather than serious primary source study. Two quick examples will suffice, since the aim here is only to be suggestive. First, Durkheim, arguably the most influential of all the founding figures, was for decades read almost without dissent among American sociologists as advocating a "pure sociology" that would be entirely objectivist and positivist, as foreign to problems of cultural interpretation as to individualistic psychology. This was plausible for the Durkheim of *The Rules of Sociological Method, Suicide,* and *The Division of Labor,* which figured centrally in the canon, but quite at odds with the Durkheim of *Primitive Classifications* and *The Elementary Forms of Religious Life.* An understanding of Durkheim's thought either in terms of its internal tensions or in terms of its development over his career had to be sacrificed in this treatment, as Jeffrey Alexander (1982) has recently shown (see also Alexander ed. 1988). Similarly, it remains possible for as sophisticated and important a theorist as Peter Blau (1986) to invoke Simmel as his unambiguous ally in opposing a purely structural sociology to cultural studies. This is because Simmel has been read (in sociology) almost entirely through certain small portions of his work, those focused on forms of social relationships, and especially on those forms that might be universal. Simmel's substantial inquiries into modern culture, like the more philosophical side of his intellectual work, have been largely ignored (see Frisby 1985). Considering its prestige at one time, it is remarkable how little a mark has been left on contemporary sociology by Sorokin's monumental attempt (1937–41) to explain overarching historical patterns of culture. It seems relegated, along with the efforts of Toynbee, Spengler, and Mumford, to some dark corner where we keep, without respect, the relics of grand historical syntheses. That Sorokin's were sophisticated sociological analyses, not just syntheses, seems to be forgotten.[21]

There may even have been deeper reasons for this sociological avoidance of the study of culture. To take culture very seriously might have meant an implicit challenge to the positivist self-understanding and the dominance of objectivist research techniques in sociology. Something of this was suggested in the debates over rationality and cross-cultural studies that followed the publication of Winch's *The Idea of a Social Science* (1958; see Wilson, ed. 1970; Hollis and

21. For more discussion of the sociology of culture, including more on its relationship to anthropology, see Calhoun (1989) and sources cited therein.

Lukes, eds. 1982). An extreme emphasis on cultural particularity and the internal, self-referential nature of linguistic meaning seemed to rule out most of the very project of cross-cultural analysis.[22] The majority of participants rejected such extreme conclusions, but never completely refuted the arguments on which they were based. Of course, the kind of analysis that Winch developed, following the later Wittgenstein, is not the only way of taking culture seriously. Anthropology offered both the *Kulturwissenschaft* tradition and the newer school of structuralism, for example, neither of which posed such intractable problems. Serious social studies of culture were undertaken by literary critics (e.g., Jameson 1981; Eagleton 1984), art critics (e.g., Gablik 1984) and historians (e.g., Thompson 1955, 1968; Maravall 1986, among many). There were exemplars from the established fields of cultural studies, particularly in the humanities. And indeed, various sociologists had been taking culture seriously all along (though often not under that name.)[23]

If it is hard to find good reasons for sociologists to ignore culture, it is correspondingly problematic that some anthropologists minimize their attention to the sociological and historical. But it should be said that by no means all anthropology—even excluding linguistics, archaeology, and physical anthropology—focuses predominantly on the cultural. There is a great deal of sociological work done under the disciplinary rubric of anthropology. Historically, this was particularly true of the British tradition of social anthropology, led by exemplars such as Radcliffe-Brown, Evans-Pritchard, Fortes, and Gluckman. It is true also of substantial segments of American anthropology, despite its more specifically cultural orientation, and, for that matter, of other national traditions.

In sum, differences between anthropology and sociology are quite real at the level of statistical patterns of styles of work and geographical distributions of work sites. Intellectually, however, sociology and anthropology seem only impoverished by any attempt to exclude domains as belonging to the other. If anything, current trends in the

22. Though it seems not to trouble most adherents, something of the same problem seems to beset much of the postmodernist and deconstructionist literature (see Calhoun, forthcoming). At its best, however, the poststructuralist emphasis is on identity *and* difference, and thereby would seem to require attention to cross-cultural relations.

23. A good deal of what attitude surveys attempt to describe and measure must be considered culture. See Wuthnow (1987, chap. 1) for a discussion of the affinity of this methodology with a subjectivist, individualist understanding of culture that he finds in the classical tradition of sociology (though perhaps he should see this as more an aspect than the whole of the work of Marx, Weber, and Durkheim).

disciplines seem to call the legitimacy of the strict division of labor argument still further into doubt.[24]

History

A generation ago, sociologists were apt to contrast their work to that of historians as scientific analysis opposed to mere description. In language descended from the *methodenstreit,* sociology was a nomothetic undertaking while history was idiographic. These accounts, and the philosophy of social science on which they were based, went out of fashion even before the recent resurgence of historical sociology and the rise of systematic and often quantitative "social science history." Nonetheless, they left an enduring mark on the implicit understandings of many sociologists.[25] Quite likely, they also contribute to the remarkably small number of citations we find to historical works.[26]

The minimal involvement of sociologists with history is all the more remarkable given the historical training and concerns of many of the founding theorists of sociology—notably Marx and Weber. Of course, there have been some historical sociologists at all stages of the discipline's history.[27] The fact remains that well under 1 percent of citations

24. I have in mind the rise of comparative historical sociology, on the one hand, and of anthropological work at state level and international phenomena on the other (e.g., the work of Eric Wolf 1983) as well as, simply, the growing number of anthropological studies of Western societies. It is not clear to me that there has been any great increase in studies of specific social settings within the Third World (as distinct from general discussions of the Third World) by American sociologists, though internationally there has been, and there has also been the rise of indigenous Third World sociology in many settings.

25. Echoes of the old opposition between idiographic and nomothetic sciences can still be heard in efforts to distinguish sociology from history. Many sociologists dismiss historians as engaged in mere description and storytelling (the nomothetic discourse always tending to reject the notion that narrative can be both a systematic and an analytic mode). The reliance on narrative was in fact attacked by "social science historians" in the 1960s. It has enjoyed a resurgence recently, however, particularly as it has been linked to notions of empowerment. Narrative not only gives voice to subaltern groups from the past, it encourages contemporary people to see themselves in these narratives of the poor and oppressed, to see not only poverty and oppression but humanity and the prospect for successful organizing and struggle.

26. Though once again, as in the case of anthropology, we are considering a discipline in which books, rather than articles, are the major form of scholarly communication.

27. This tends to be forgotten by historical sociologists convinced of the novelty of their endeavor. What is new, however, is not historical sociology but its institutionalization and a disciplinary view that treats it as something other than an idiosyncrasy. Before Skocpol, Wallerstein, and Tilly there were Moore, Bendix, and Smelser. Merton

in our sample were to history journals, and the vast majority of them were in the last fifteen years. It was during this period that historical sociology came into prominence. The rise of historical sociology depended in part on the prior flourishing of the "new social history," and the somewhat separate movement towards a "social science history." These were interdisciplinary undertakings from the start, though historians played the central roles. Senior statesmen in each discipline urged on the interdisciplinary communication.[28] E. H. Carr issued one of the more prominent and forceful calls for collaboration: "the more sociological history becomes and the more historical sociology becomes, the better for both. Let the frontier between them be kept open for two-way traffic" (1961:84).

This and similar calls were taken up by both historians and sociologists. Neil Smelser's *Social Change in the Industrial Revolution* (1958) was noteworthy for its attempt to link abstract sociological theory not only to historical narrative but to concrete analysis of primary source historical data.[29] Shortly thereafter the historian George Rudé (1964) used Smelser's theory of collective behavior to provide the theoretical context and conceptual framework for his pioneering investigation into eighteenth- and early nineteenth-century political crowds and rioters.[30] Without always drawing very explicitly on sociology, a number of other historians began to produce work that cer-

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did major historical work on science; in approximately the same generation Elias stands as the most centrally historical sociologist; before them there was Sorokin and so forth back to the discipline's very historical founders.

28. History, Lloyd Kramer has recently suggested, has always been an appropriating discipline, ideology notwithstanding, so this was only one phase in a longstanding pattern: "The dominant institutional pattern has been the tendency of historians to define themselves along the increasingly precise lines of academic departments, limited specializations, and disciplinary boundaries. At the same time, however, much of the intellectual innovation among modern historians has resulted from their willingness to draw on other academic disciplines for theoretical and methodological insights, which has led to an expansion and redefinition of the political orientation of traditional historiography" (1989:97).

29. In this, it is interesting to note, Smelser's work was different from that of his Harvard predecessor George Homans. Homans's historical work was of great distinction, but it represented more of a parallel track to his sociology, a sort of second career with few direct linkages though some obvious shared tastes and styles. Homans never portrayed (nor I think conceived) his historical work to be an occasion for "applying" or "testing" his sociological theories.

30. Smelser's and Rudé's books shared a somewhat unfortunate common feature. In both, the theory tended to appear rather as abstract bread in opening and closing discussions, while the more satisfying meat of the sandwich was the concrete historical account in between.

tainly counts intellectually (if not disciplinarily) as historical sociology: Eric Hobsbawm, E. P. Thompson, and Keith Thomas in Britain; Herbert Guttman and Eugene Genovese in the United States. For the most part, these scholars remained essentially historians, as Smelser, Moore, Bendix, Eisenstadt, and others remained essentially sociologists, although they took up historical problems. Charles Tilly, in a slightly younger generation than most of those mentioned, was one of the first modern figures to achieve a distinguished position in both disciplines simultaneously and for the same work (that is, not like his teacher George Homans for two parallel lines of work).

This burgeoning of a new sort of work burst disciplinary boundaries, but it was not always a matter of cross-disciplinary borrowings or inspirations. It was the product largely of extra-academic forces and of changes in each discipline's internal preoccupations. Something of the variety of sources for the new lines of work can be seen in the various names under which they have traveled. The new social history had a great deal to do with the politics of the 1960s and the academic rehabilitation of Marxism. Social science history was a somewhat more staid affair, predicated on the borrowing of analytic techniques (and to a much lesser extent theories) from conventional nonhistorical social science for application to historical research problems. Historical (and comparative) sociology grew in the United States largely as a part of an internal struggle against a research tradition driven by technical advances as much as substantive concerns, against the extraordinary ethnocentrism of 1950s-style American functionalism, and against the neglect of struggle itself as a factor and radical change as a possibility in social life.[31]

The prominence of strife and polemic in the relations between the emerging comparative historical sociologists of the 1970s and the "establishment" of the profession should not obscure how much of a role—both positive and negative—more senior sociologists played in the development of comparative historical sociology. The field began

31. Only in a few cases, unfortunately, did this struggle take the form of systematic comparative and historical research which began as a minority even within the oppositional minority. Initially more prominent were abstractly theoretical, often epistemological, polemics. One of the attractions of comparative historical work was the opportunity to undertake empirical research tackling questions of the large scale common to theoretical discourse in the traditions of Marx and Weber. Most American sociological research at that time (as now) takes up much narrower questions. Historical and comparative sociologists sought to step into the breach between "abstracted empiricism" and "grand theory" described by C. Wright Mills (1959). Among other things, they sought to show the limits within which generalizations (commonly put forward at that time as universal laws) might hold.

to develop in its modern form in the 1950s. Early entries were largely concerned with assimilating foreign cultures and past times to a universal model of social functioning or change or both. Smelser's (1958) attempt to study a single course of historical social change in some detail was relatively unusual. More common were efforts to arrange contemporary societies in a hierarchical model of putative stages of modernization.[32]

Modernization theory gave double impetus to the development of modern historical sociology. First, beginning in the 1950s, it sparked a number of research projects that themselves produced works of some significance. From Robert Bellah's (1957) reexamination of the "Protestant ethic" thesis in Tokugawa Japan to more general, comparative studies like Shmuel Eisenstadt's (1963) work on empires, modernization theory produced major research. Modernization studies also tied historical sociology to the older tradition of economic history. It was in large part out of economic history, particularly studies of the early modern era and of the industrial revolution, that social history was emerging as a distinct subfield and, indeed, an increasingly central one in history departments (see Burke 1980:22–27). But modernization theory had an equally substantial *indirect* impact on historical sociology. By the late 1960s and early 1970s, it had sparked an intense reaction among researchers concerned to show the possibility of other paths of change, the autonomy of other cultures, and the different external and internal circumstances facing postcolonial and other currently less-developed countries as compared to the archetypical cases of Western development. Modernization theory made a major contribution to historical sociology by providing a very stimulating foil for critique and new research during the period of its collapse.

At about the same time, there was a reaction among younger historians against approaches that saw history as a narrative of the deeds of great men, the dates of battles, and the impact of abstract ideas. During the 1960s, the new wave of historians sought the recovery of a lost past in as much detail as possible. One faction of this thrust (what I have called "social science history") turned towards American sociology and demography, econometrics, and statistics, using computers to analyze records from parish registers and censuses to property-holding and voting patterns. Slaves became objects of cliometrics (Fogel and Engermann 1974) and rioters became objects of

32. See the critique in Skocpol and Somers (1980), and Skocpol (1984). Modernization accounts varied in the extent to which they treated the arrangement of contemporary societies into stages as a sort of pseudohistory of human evolution.

"the statistical analysis of contentious gatherings" (Tilly, Tilly and Tilly 1974). This quantitative group was largely American, though the Cambridge historical demography group was a major exception. At the same time, another faction, in which British and European historians loomed larger, turned to anthropology rather than statistics for inspiration.[33] Its efforts aimed at the recovery of past cultures, of people's ways of life seen from their own perspectives and as much as possible reported in their own words. Respect for those studied and a refusal to turn them into mere "objects of research" were central tenets from the work of E. P. Thompson through to the *History Workshop* group. Both statistical studies—perhaps the truest approach to Marxism's masses—and historical ethnography could claim to be "history from bottom up" (in the slogan of the *Journal of Social History*).

The achievements of what Bernard Cohn (1980) has called "proctological history" have been undeniably great. A wide range of source materials has been used to produce an enormous body of information. Nonetheless, the new social and social science histories often have yielded to an illusion of the pure resurrection of the past, forgetting the essential constitutive role of theory (see Selbourne 1980). The best studies, of course, went beyond mere discovery to analysis, explanation, and interpretation. But E. P. Thompson, for example, could deny and submerge the theoretical dimension of *The Making of the English Working Class,* claiming only to represent various lost voices, to eschew sociological categories, and to show us "real people and in a real context" (1968:9).[34] In general, theory seemed to many of the

33. One of the younger members of the Cambridge historical demography group, Alan Macfarlane (1981), is notable for combining fruitfully an anthropological approach with statistical research, a mixture that runs against the grain of interdisciplinary work in social history.

34. As Thompson phrased his aspiration: "I am seeking to rescue the poor stockinger, the Luddite cropper, the 'obsolete' hand-loom weaver, the 'utopian' artisan, and even the deluded follower of Joanna Southcott, from the enormous condescension of posterity. Their crafts and traditions may have been dying. Their hostility to the new industrialism may have been backward-looking. Their communitarian ideals may have been fantasies. Their insurrectionary conspiracies may have been foolhardy. But they lived through these times of acute social disturbance, and we did not. Their aspirations were valid in terms of their own experience; and if, they were casualties of history, they remain, condemned in their own lives, as casualties." (1963: 13). Thompson succeeds admirably in this goal, but he also relies on and develops implicit theory. The theory is sometimes subtle and helpful, sometimes problematic, but his effort to deny it and write as though it did not exist makes discerning the overall significance of his work more difficult, both in terms of broad patterns of historical change and in terms of comparative relevance to other instances of popular radicalism (see discussion in Calhoun 1982; Scott 1988).

new social historians to be at best a move of great abstraction away from the concrete lives they studied (echoes of the idiographic/nomothetic divide), and at worst a form of symbolic violence done to this concrete human world. This view was encouraged by the extremely abstract and antihumanist Althusserian structuralism much in vogue at the time. Indeed, Althusserian structuralism did almost as much to make general theory seem irrelevant to researchers in the Marxist tradition as Parsonsian functionalism did to those in mainstream sociology. The response of historians is represented, perhaps in extreme form, by E. P. Thompson's brilliantly witty if unfair polemic, *The Poverty of Theory* (1975). More generally, even where structuralism was not involved, the British and to a lesser extent American social historians (many of whom found inspiration in anthropology) often had an antipathetic relationship to sociology (see, e.g., Jones 1976; Samuel and Jones 1976; Thompson 1972).

The notion of theory as a form of symbolic violence, as virtually a continuation of the domination of elites over ordinary people by refusing to let the latter speak in their own voices, paralleled a theme of "poststructuralist" thought. Foucault in particular argued that theorizing as such was always an exercise of power (1977). At the same time, the notion of recovering lost voices prefigured a prominent feminist theme. Recovery of the vantage point of women, and of the voices of their own experience, has been a major and important project itself stretching across several disciplines.[35] Unlike the new social history, however, much of this feminist work has embraced theory as a central part of its intellectual work. Recently, literary theory has contributed substantially to this discourse on the interrelation of multiple voices, particularly through the work of Mikhail Bakhtin (1981) and others inspired by his writings on dialogicality. Those historians who have become involved with theory have been particularly likely to look to Bakhtin, the poststructuralists, and others outside the sociological mainstream.

The relationship between sociology and history has receded in the last few years; social history has been consolidated within the discipline, but cultural history has replaced it as the exciting new trend

35. The citation analysis gives an indication that feminist work has been a bit slow to penetrate the mainstream journals of sociology. The influential and highly visible journal *Signs*, edited in part by sociologists, is cited only a handful of times. Interdisciplinary periodicals on gender and women's studies account for a nearly negligible part of the overall citation pattern. Feminist work in sociology has often been the study of social circumstances and problems of women in sociologically fairly conventional ways, rather than an occasion for more basic reconsideration and reconstruction of disciplinary orientations.

(Hunt, ed. 1989). Anthropology and literary theory have moved into the ascendancy among interdisciplinary influences on history. In the case of anthropology, this is partly a continuation of longer-standing influences—for example, through the work of Keith Thomas and E. P. Thompson. In their work, the anthropological influence was very diffuse, more an orientation of attention to certain aspects of social life than direct use of any anthropological theory or analytic methods. During the 1970s, Natalie Zemon Davis began to publish a series of important works heavily influenced by the analyses of ritual and symbolism of Victor Turner, Max Gluckman, and Mary Douglas (see Desan 1989 on Davis and Thompson). It is perhaps no accident that her work lay in French history, for among French historians (both in France and abroad) a turn to the study of *mentalités* had shifted the focus of *Annales* school historians and a growing anthropological influence could be seen in a wide range of work—that of Darnton and Ladurie, for example (see Hunt 1986). At the same time, American historians (including Darnton 1984) began to be influenced by Clifford Geertz's anthropological writings, particularly his argument that cultural performances or systems constitute texts to be read. By the 1980s, the Geertzian influence began to be pervasive.[36]

The turn to a cultural history involved not just cultural anthropology but the rapidly diffusing influence of French poststructuralists, particularly Foucault (who is perhaps somewhat ambiguously classified thus) and Derrida (who is perhaps the paradigm for the label "poststructuralist"). Where Geertz had been criticized for the functionalism of his notions of cultural as a system (e.g., Chartier 1985), the poststructuralists emphasized internal contradictions and difference. Chartier, for example, argued for "a definition of history primarily sensitive to inequalities in the appropriation of common materials or practices"—for example, texts and rituals (1985:688). In both Geertzian and poststructuralist modes, however, the new cultural history was one that disputed the primacy—and perhaps even the stable existence—of society as an object of study. Its rejection of accounts of culture as mere reflection of social structure was extreme enough to distance it from the general project of a sociological theory

36. Indeed, Geertz's influence may have been greater outside of anthropology than within it. This is a not uncommon pattern of interdisciplinary influence. Allan Megill (1987:120) observes that the most cited historians (in the *Social Science Citation Index* and the *Arts and Humanities Citation Index*) are Michel Foucault, Erwin Panofsky, Ernst Gombrich, Frances Yates, Thomas Kuhn, and Mircea Eliade—all figures from the margins of professional history; none, indeed, was employed in an academic department or institute of history.

of culture and often resulted in a presentation of culture as a more or less autonomous, free-floating object of study. At the same time, for many in this discourse, culture was essentially an arena of contest for political struggle over meaning.

Some of the most influential work in the new vein came from intellectual history, which had been recast in part as cultural history, and which was particularly open to theoretical discussion. Hayden White and Dominick LaCapra played central roles. They argued that critical theorists and the sort of critical historians they saw themselves speaking for, "should recover those lost or repressed strands of Western culture that might challenge the reigning epistemological and ontological orthodoxies of our time" (Kramer 1989:100). Thus, their work was self-reflexive, taking up tensions within history, as well as oriented to tensions, repressions, and ambiguities in records from past times. They placed special emphasis on thinking about historical records as texts (especially LaCapra, 1983, chap. 1, where the emphasis is on the complexity of textual interpretation in a deconstructionist mode), and on pondering the nature of modern historical writing, especially narrative form (especially White 1987). Both White and LaCapra have challenged what they regard as problematic disciplinary habits. For White, the fundamental problem with historians' self understanding was their presentation of history as "a discipline that purports to serve as custodian of realism in political and social thinking" (1987:61). White has been influenced considerably by Foucault, who himself jousted with the disciplinary identity of history. An interrogator described him as "'a barbarous knight,' galloping across the historical terrain, recklessly abandoning in his histories of prisons, of medicine, of hospitals, careful and meticulous research" (O'Brien 1989:29, quoting Jacques Léonard). Foucault replied in kind, describing the stereotypical historian as: "the virtuous knight of accuracy ('I don't have many ideas but at least what I say is true'), the doctor of inexhaustible information ('You haven't said anything about this thing or that, or even that which I know about and you are certainly ignorant of'); the great witness of Reality ('No grand systems but life, real life, with all its contradictory riches'); the heartbroken scholar who weeps over his little piece of earth just pillaged by barbarians: just as if after Attila the grass would not grow again" (quoted by O'Brien 1989:30, from Perrot ed., *L'Impossible prison*).

Sociologists have often felt that historians responded to their work with the same sorts of defenses of disciplinary identity. White summed up a Foucauldian as well as Nietzschean view a decade earlier, when he wrote: "Every discipline . . . is, as Nietzsche saw most clearly, con-

stituted by what it *forbids* its practitioners to do. Every discipline is made up of a set of restrictions on thought and imagination, and none is more hedged about with taboos than professional historiography (1978:126).

One of the longstanding taboos for historians has been against self-conscious theorizing; even reflection on the categories used in historical writing has been substantially repressed. In recent years, this taboo has been broken not only by social history borrowing from social science, but by intellectual history borrowing from literary theory and philosophy. As Philip Abrams says, "the really significant development of the past twenty years has been the publication of a solid body of theoretically self-conscious historical work which has progressively made nonsense of earlier conceptions of history as somehow, in principle, not engaged in the theoretical world of the social sciences (1982:300).[37]

Prominent sociological theorists have offered encouragement to the joining of historical and sociological work:

> What history is, or should be, cannot be analyzed in separation from what the social sciences are, or should be [and] there simply are no logical or even methodological distinctions between the social sciences and history—appropriately conceived. (Giddens 1979:230)
>
> Suffice it to say that the separation of sociology and history is a disastrous division and one totally devoid of epistemological justification: all sociology should be historical and all history sociological. (Pierre Bourdieu in Wacquant 1989)[38]

Such prominent historians as Eric Hobsbawm (1971) also argued that accepting that there is no principled reason for division between history and social science is only a first step toward the development of a truly adequate history of society (or sociological history). Fernand

37. Elsewhere Abrams suggests "that a long collective tussle with immediate matters of historical explanation has also been a way of discovering the problematic of structuring [his gloss on Giddens' structuration] and realising its capacity to integrate history and sociology as a single unified programme of analysis" (1982:xviii). There have certainly been some strongly dissenting voices among historians, notably G. R. Elton (1984) who finds little use for sociological history, even when done from primary sources by members of his own disciplines, and who would extend his attack to sociology and the other social sciences generally. At the same time, of course, there are a good many sociologists who neither understand the turn to historical work of many of their colleagues nor approve of it—especially if it means a turn away from the most sophisticated quantitative methods.

38. Charles Tilly, who frequently has cast himself in the role of explainer of sociology to historians and history to sociologists, ultimately suggests that the differences are more stylistic than substantive (see Tilly 1982).

Braudel (1980:69) wrote similarly that history and sociology are "one single intellectual adventure," and he has done an enormous amount to exemplify that unity in his empirical work. As Gareth Stedman Jones has commented, however, "there is no distinction in principle between history and any of the other 'social sciences.' The distinction is not that between theory and non-theory, but between the adequacy or inadequacy of the theory brought to bear" (1976:295).

Theory may be inescapable (as well as indispensable) to those who would interpret or explain history, though it may be left implicit, and thus harder to identify, challenge, and improve.[39] Sociologists should not assume, however, that it is automatically their own theories on which historians will draw. The relative weakness of sociology's involvement in the interdisciplinary discourse of cultural theory (especially in the United States) has weakened its disciplinary influence as well as its receptivity to significant new work.

At the same time, most theory that does not take care to be historically specific, and substantial about its relationship to time and place, limits itself to addressing formal conditions or possibilities for social life with no purchase on its actualities.[40] Yet if history and social theory need each other, they also challenge each other. To join them is harder than simply "applying" one to the other, or bringing different methods to bear on a given object of research. Objects of research are never "given" in any strong sense. When disciplines are brought together seriously, the taken-for-granted conceptual frameworks of each are challenged. Debate over interdisciplinary work in this as in other cases has turned on the dangers some groups of people felt and the sense of others that basic disciplinary problems would be solved simply by the mere existence of interdisciplinary work, rather than requiring serious rethinking of received disciplinary categories.

Partly for each of these reasons, there is a strong tendency for dif-

39. I use "theory" in a fairly broad sense here, including within its reference explicitly anti-theoretical positions such as those of Foucault and Derrida. The former made a number of declarations against the totalizing tendencies of theory, while the latter has gone to great lengths to avoid giving a clear-cut theory (or method) for deconstruction, and to avoid using any of his terms frequently enough in the same analytic sense so as to give them the status of "master" concept or key that opens all locks (see LaCapra 1983:152).

40. Both Weber and Simmel suggested something of this distinction between historically concrete actualities (full of content, multidimensional, and complexly determined) and the simplified and more abstract ideal types and forms that could be the objects of a universalizing sociology. Such forms describe a range of possibilities for social life, they specify conditions under which social life may take place, but they do not as such describe its concrete events or relationships.

ferent disciplines to "recapture" the subfields that make the most fruitful contact with neighboring disciplines. Thus, the Social Science History Association was founded in large part by historians seeking social science methods and social scientists seeking involvement in historical explanation. Over time, ironically, the quantitative historians and the (largely qualitative) sociologists moved past each other. While the former group was seeking something of an escape from conventional history, the latter group was seeking an escape from conventional sociology. Only for a while did they find both together. At the same time, each group grew large enough that it did not have the same need for extradisciplinary (and therefore somewhat unsettling) outsiders in order to establish a discourse. The same thing happened to the new social history. It ceased to be a radical alternative to mainstream history, and became one part of the mainstream. It also grew dramatically in numbers to the point where it was well able to sustain its own discourse without much involvement of nonhistorians.[41] In fact, social history itself became part of the "old guard," the object of attacks from poststructuralist cultural historians (see, e.g., Hunt, ed. 1989, and especially O'Brien 1989, on the Foucauldian dimension to this).

Historical sociology (or comparative historical sociology) has grown substantially, but it increasingly seems destined to be (a) contained within sociology, and (b) compartmentalized within the discipline.[42] Rather than challenging the limits of conventional sociology, suggesting the myopia implicit in ahistorical understandings of nearly every sociological topic, historical sociologists seem to have settled for acceptance as simply another special area within the discipline. The actual (rather than potential) relationship of sociology to history is thus not a general one, but rather, for the most part, a relationship

41. This is not to say, of course, that there are not such dialogues continuing and bearing considerable intellectual fruit. Rather, the point is that the definition and self-conceptualization of social history no longer involves any particular reaching outside of history as a discipline.

42. Its growth should not be overestimated, as the rate of citations to history journals suggests. The ASA's section on comparative-historical sociology is very eclectic; its membership far exceeds, for example, the number of sociologists actually doing primarily historical research, and especially doing it in ways which are in close relationship to the work of social historians. Despite the prominence of some of its practitioners, illustrated, for example, in the disproportionate number of works of comparative or historical sociology that have won the ASA's award for a distinguished contribution to scholarship in the last fifteen years, comparative historical sociology remains very much a minority orientation within the discipline. It is also probably undercounted by citation analysis techniques, because its practitioners are likely to write a smaller number of longer pieces than in many other specialties of sociology.

only on the part of a subset of sociologists specializing on historical topics. All too often, moreover, these sociologists simply draw data (or authoritative descriptions) from published historical works in order to construct what are often relatively conventional sociological analyses.[43] Many of these are not historical in any sense except being about phenomena that occurred in the past—that is, they are not about patterns of continuity and change over time. The real test of whether sociology has overcome its impoverishing self-distancing from history lies in whether sociologists in general *think historically,* as well as recognize the historical context of their work and use historical data. Does a sense of historical structuration and change become a basic part of the way we conceptualize the social world? Such a sense must affect the way we conceive of the relationships between structure and action, and between function and power.

The notion that history is idiographic while sociology is nomothetic shares a good deal with the notion that anthropology can be distinguished from sociology by its reliance on participant observation studies. These distinctions are meaningful as characterizations of styles of research. Historians and anthropologists are both more likely than sociologists to confront themselves with rich, dense data on many aspects of people's lives in concrete settings. This may make them sometimes resistant to theories and methods that demand great abstraction from such concrete specificity. It also makes them sensitive to issues of difference among peoples and ways of life. None of this, however, makes the disciplinary distinction a matter of principle. Indeed, the very notion of a purely idiographic discipline involves a misleading assumption that pure descriptive facticity is possible, that there can be descriptions untainted by theory.[44] A good social history without some

43. Though many historians profess a radically empiricist ideology, few things inflame the self-consciousness of historians at large more than being told that they are a class of empirical underlaborers piling up facts that sociologists will use to construct theoretically significant analyses. Such a description of the relationship between the two disciplines both neglects the very substantial work of systematic analysis undertaken by historians and, even more importantly, reifies facticity, losing sight of the work of interpretation and informed scholarship that goes into establishing sound historical judgement as to the facts. Such a view is hermeneutically naive in quite fundamental ways, whether presented by historians or sociologists.

44. In the same sense, Glaser and Strauss's (1966) reinvention of ethnography in the notion of grounded theory makes sense primarily as a rejection of excessively formalistic deductive theorizing. As a positive account of an approach it is quite problematic. Not least of all is an element of naive empiricism and an assumption that microsociological phenomena were somehow more immediately real than macrosociological ones. The general point, however, is well taken: nearly all the best sociological theory has been empirical work at the same time. This was true, for example, of Marx, Weber, Durk-

form of explicit or implicit sociology is no more imaginable than a good sociology with no sensitivity to specificities of time, place, or patterns of continuity and change (though goodness knows a fair amount of such sociology has been published).

Psychology

It is, of course, widely considered that psychology is not a social science at all, but either a behavioral science or a biomedical science, depending on the informant. Nonetheless, it is both grouped into the social sciences by many university administrations and creators of library cataloguing systems and is the most frequently cited of cognate disciplines to sociology. Part of this close relationship depends on the long-shared occupation of the field of social psychology. The proportionate role of psychology in this joint endeavor has increased over the last twenty years, particularly with the declining prominence of attitude survey methods. Nonetheless, a considerable number of sociologists both consider themselves social psychologists and (perhaps in smaller numbers) relate their work closely to that of colleagues in psychology departments. This linkage is all the more accentuated for social psychologists working in such topical interdisciplinary fields as human development and life-course studies, family studies, etc.[45]

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heim, and Simmel, as of many theorists today, perhaps most notably Pierre Bourdieu. A case could be made also that Habermas's *The Structural Transformation of the Public Sphere* (1989) is among his most important statements precisely because it does include a substantial amount of concrete empirical analysis. Even where Habermas's theory is abstract, it is not simply deductive. Likewise, there is some empirical substance to much of Giddens' theorizing (though also a great deal of abstract prolegomenal discourse on definitions and relations among concepts); and his partially empirical *The Nation State and Violence* (1985) is among his more attractive works.

45. Though, conversely, in some such fields which were once clearly joint provinces of psychologists and sociologists, the sociologists have all but disappeared. Small group research is a good example. In the 1960s, not only were sociologists such as Bales and his colleagues prominent, they were in some cases pioneers in developing approaches most of us would now consider exclusively psychological—e.g., Phillip Slater (1967) in his use of Wilfred Bion's psychoanalytic approach to changing patterns of group relations. By the late 1970s, social psychology was thriving in psychology departments, but was on the decline in sociology. It has enjoyed an apparent partial resurgence particularly where its label has been used as a covering term for all microsociological studies, including those of symbolic interactionists (symbolic interactionism itself often being less a theoretical orientation than a euphemism for a mix of fieldwork, interpretive methods, and microsociology). And new attention to life-course studies and the emotions have helped to revive sociological social psychology.

Within sociology, social psychology has become something of a catchall category. In many curricula, it lumps together symbolic interactionism (perhaps the core of sociological social psychology but also an alternative label for qualitative microsociology), studies of groups and interpersonal relations, much of cultural sociology, studies of the life course, and the work of both Goffman and the ethnomethodologists—though it is arguable that neither is substantially "psychological." Much of contemporary sociological research into socialization pays little or no attention to psychological issues and may, indeed, counterpose its arguments to those of psychology (e.g., Kleinman 1985).

Perhaps the most important change in the relationship of sociology to psychology lies outside the joint subfield of social psychology. This is the declining concern of general (or macro-) sociological theorists for establishing a psychological grounding for or complement to their theories. For Talcott Parsons, it was essential that a connection be established between the social and cultural systems and that of personality. The connection was socialization processes.[46] Moreover, Parsons believed (along with a number of other theorists; see, e.g., Parsons, Bales, and Shils 1953) that a strong psychology must necessarily underpin any sociological theory by contributing to its understanding of what it means to be a human being.[47] This need not have anything to do with an attempt to reduce social phenomena to derivatives of individual phenomena or to generalize from elementary principles of behaviorism (as in Homans's work, e.g., 1964).[48] In Parsons's case, in fact, the most relevant psychology came from psychoanalysis, and he joined the "interpersonal relations" school of Harry Stack Sullivan, Clara Thompson, and others in modifying the Freudian inheritance in a sociological direction (see Parsons 1967).[49]

46. Indeed, courses in "socialization and personality" were long common in sociology departments. I also taught such a course in the late 1970s until my department renamed it simply "socialization," explaining that "personality" was not a topic of sociological concern.

47. This is a question not only of psychology, of course, but of philosophical anthropology. It is symptomatic of sociology's scientism that relatively few recent sociologists (in the U.S.) have turned to philosophy for help in developing an adequate conceptualization of what it means to be human. My impression, however, is that more have done so during the last few years than at any time since the 1920s.

48. As Camic (1989:44) has pointed out, behaviorism was one of Parsons's consistent polemical foils, especially in his early work. This is not appreciated as often or as clearly as his related opposition to atomistic individualism.

49. One little noted consequence of (or accompaniment to) the decline of functionalism in sociology has been a near disappearance of psychoanalysis from sociological

The main principle proposed to distinguish sociology from psychology is a seemingly straightforward appeal to levels of analysis: psychologists study individuals, sociologists study society (which many sociologists feel compelled to argue is an emergent phenomenon). Though its status may be problematic, this is perhaps the clearest of the principles adduced to distinguish sociology from another one of the social sciences. It is correspondingly ironic, then, that psychology is the other discipline by far most often cited by sociologists at all stages of the discipline's postwar history. The reason may lie, quite simply, in a shared ideology of science that unites psychology and sociology not in substantive work, but in the notion of reciprocal claims to distinct phenomena. However much sociologists may continue the Durkheimian tradition of inveighing against psychological reductionism and calling for a sharp division of labor, psychology shares much of the same understanding of science as mainstream American sociology. Both are highly empiricist. Moreover, both follow Auguste Comte's (1830–42) claim that each science must have its own distinctive subject matter, though not necessarily his more radical argument that psychology did not have such a subject matter and accordingly did not figure in his hierarchy of the sciences. Modern sociologists are inclined to be more generous and grant psychology scientific status as the study of "individuals" (or perhaps more sharply, of "mental life," or what goes on "inside" individuals).

Where Durkheim found it necessary to struggle against psychology to demonstrate the legitimacy of sociology, modern sociologists have worked in universities where the notion of disciplinary division of labor is much more firmly enshrined.[50] This does not mean that main-

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discourse (though see the work of Chodorow 1978; and Smelser 1989). It is unclear how much this reflects the simultaneous decline of psychoanalysis in the face of behaviorism within psychology (and to a lesser extent in the face of psychopharmacology and psychobiology in both psychology and psychiatry), and how much it is a specifically sociological matter. It does seem that today's "neofunctionalists" are less inclined to resuscitate psychoanalysis than some other aspects of the Parsonsian inheritance. In passing, it may also be worth noting that psychoanalysis was (and in some cases still is) closely related to functionalism's great macrosociological antagonist of the last twenty years, Marxism, particularly in Frankfurt school critical theory. Like neofunctionalists, many neo-Marxists—including a number claiming specific descent from the Frankfurt school—have been wary of too great an involvement with psychoanalysis. In both cases, this has sometimes contributed to a theoretical weakness by leading to avoidance of issues of socialization, personality, and emotional life.

50. Indeed, Durkheim's struggle against psychology on behalf of the nascent discipline of sociology was sufficiently important to him that he allowed it in many ways to distort his own work, leading him to stress the apparently objective side of society in

taining distinction from psychology and implied individualistic reductionism is not still important to many sociologists. Homans struggled against this basis for rejection throughout his career. More recently, one of the conditions of the renewal of interest in rational choice theory has been arguments (such as Becker's, see below under Economics) that rational action models do not depend on psychological accounts of rationality (or indeed any accounts of action as such), but rather on such external, supra individual factors as supply and demand. Durkheim (1895) premised his rejection of psychological foundations for sociology on the argument that social life was a phenomenon *sui generis*, an emergent phenomenon not explicable by reference to its constituent elements any more than life itself could be explained by simple reference to inert matter.[51] Blau (1986), Lenski (1987), and other contemporary macrosociologists often make similar claims.

Though not all sociologists find it necessary to be so radical, the idea of social life as an emergent phenomenon is widely repeated; it constitutes one of the main general claims sociology makes for its existence and shapes debates about the relationship of macrosociology to microsociology. Part of the underlying reason for this is simply that sociologists buy into the basic Western dualism separating individual from society and treating each as an autonomous substance. Even declarations of the mutuality of individuality and sociality tend to reproduce the substantivist dichotomy, for example, "self and society are twin-born" (Cooley 1909); though Cooley is still quoted, contemporary usage is often less subtle, substituting "individual" for "self," and seeing both society and individual as simply given. Macro/micro debates are often more or less simplistic reproductions of this dualism (see, e.g., several of the essays in Alexander et al 1987). One might think that levels of analysis should be conceived of as innumerable,

The Rules . . . (1984), for example, to the exclusion of the more subjective features. He did this even though he showed awareness of the problem of "internationalization" of social facts, and in other work evidenced a more basic (if philosophical more than psychological) concern for cognitive categories and what we would now call social psychology.

51. The claim of "emergence" is a very strong one, much stronger—and therefore more demanding if it is to be defended—than most of those who offer it seem to realize. That there are distinctive properties associated with large scale that are not present in constituent units does not demonstrate emergence. On the contrary, these are better understood as *collective* properties, not emergent ones. And whether relationship or connectedness should be considered emergent is debatable. Indeed, as Simmel (1908) suggested, there is good reason to argue that social relations are not external to individuals in the way Kant suggested that connection is external to things.

and distinguishable only on a more or less ad hoc basis, as part of specific research designs. Could we not, for example, imagine network analysis operating at the scale of interacting dyads and triads, and at every intervening "level" up to social structure on a global scale?[52] Why then does the debate always get posed in terms of a dualistic opposition? However many times it is asserted that individuals are social constructs, and that society is inconceivable without concrete human beings (though these may not always be individuals in the Western cultural sense), we seem unable to give up the opposition. The macro/micro debate characteristically dissolves into two main positions: (1) All good explanations must include reference (though not necessarily reduction) to the individual action that constitutes the social phenomenon; accordingly, macrosociology needs to rest on microsociology (Collins 1986; Coleman 1986, echoing Weber 1922);[53] and (2) macrosociology is entirely autonomous of the individual (or psychological) level of analysis (Blau 1986; Lenski 1987). I find more wisdom in the suggestion that the division is poorly posed and distorting of the issues involved (Giddens 1985a; Bourdieu 1980).

The level of analysis principle is problematic whenever it is taken to suggest some difference in substance rather than perspective. Simply observing modern disciplinary practices reveals that sociologists do not ignore individuals. Moreover, it is not clear that psychology is the study of individuals. Psychologists define a variety of objects of study: groups, interpersonal relations, intrapsychic phenomena, language understood in profoundly intersubjective ways, psychopharmacology, etc. Behavioral psychology generally takes individuals as units in the study of how operant conditioning cumulates, but usually in a purely external fashion, without much analysis of the cultural construction of individual as either autonomous or irreducible. As Richard Sennett (1979) has argued, it is quite meaningful to talk not only of social psychology, but of a "psychology of society."

In sum, the claim to a principled distinction of sociology from psychology based on the distinction of individual from society is challenged by the substantial attention that at least some sociologists pay to individuals, by difficulties in describing psychology as the study of

52. As Burt (1982) has summarized, the network perspective is premised largely on the assumption that the basic unit of analysis is the relationship; levels of analysis change with how many relationships are aggregated into the pattern under examination and must therefore be understood as involving collective rather than emergent properties (see also Nadel 1957).

53. Coleman and Collins are, of course, proponents of very different sociological theories, but both wish to place macrosociology on microfoundations.

individuals, and by difficulties in the very conceptual distinction of individual from society. As some sociologists turn their attention increasingly to problems of "agency" or "action," the implausibility of the disciplinary distinction grows (though talk of agency often produces a new dualism between it and "structure"). Sociologists have long studied the constructed quality of individuality and subjective dimensions of society. With regard to the latter, important strains of thought have maintained the importance of seeing subjectivity not simply in individual but in intersubjective terms (cf. Habermas 1988). A growing interpretive sociology of culture further erodes the claimed principle for distinguishing disciplines, because of its attention to subjectivity, even though it involves little reference to psychology.

Political Science

In some ways it is surprising that political science journals are not more often cited in our sample. They are cited more frequently towards the end of the period studied, which suggests perhaps the growing prominence of political sociology as a subfield of sociology. In addition, it probably reflects first the so-called behavioral revolution in which political scientists turned to largely quantitative empirical methods and, second, the growing attention in both political science and sociology to state formation and related problems. Nonetheless, political sociology has never been entirely absent as a subfield in sociology (though it was only very recently formed into a section within the ASA). For a number of years, however—roughly between World War Two and the middle 1960s—sociologists worked rather hard at maintaining the fiction of distinction between the objects of social and political analysis. Though it was not much explored at the time, some rationale for this might have been found in the classic oppositions of state and society, government and people. Of course, those oppositions were interesting precisely because of the questions they raised about relations *among* government, public discourse, and social integration. And such questions could not very readily be taken up when the division was used as the basis for a dichotomization of disciplines.

Internally, political science has been much more sharply divided into subfields than sociology. In particular, the four major subfields of U.S. political science—American politics (which includes also the judiciary and constitutional and public law as quasi-autonomous sub-sub-fields), comparative politics, international relations, and political theory—have almost nothing to do with each other. One of the oddities of this is that political theory as a speciality has almost entirely to

do with the history of political thought, and especially normative theory, while the empirical theory that guides research (to whatever extent it does) is often quite separate (and at points quite likely to be heavily influenced by sociology). Relatively little attempt is made to relate normative and empirical theory—though more than in sociology, where normative theory is hardly discussed. One of the obstacles to bringing political theorists and sociological theorists into fruitful discussion is the fact that sociologists are not generally knowledgeable about classical social and political theory (e.g., Hobbes, Locke, Rousseau, Bentham, Mill), let alone about the ancients. This means that key reference points in normative theoretical discussion are lost to sociologists. This problem is often reinforced by the formal philosophical style of argumentation common in political theory but alien to sociologists. If there is to be better communication between the disciplines, it will depend significantly on sociologists learning more about social theory written before the invention of sociology as a discipline and before the work of our canonical founding fathers (whose work is greatly illuminated by such knowledge).

The differences between political scientists and sociologists cannot be said to inhere in contrasting styles of work as much as can those between sociologists and both historians and anthropologists. Especially in recent years, many political scientists have used analytic techniques similar to those used by sociologists. This has been true especially in American politics, where the "behavioral revolution" took deepest root. Indeed, the field of American politics is in practice defined rather oddly. It tends to exclude political economy of the U.S. and, to a large extent, the study of nonelectoral political movements. American politics is taken by many powerful senior political scientists to mean essentially (a) the study of voting behavior, and (b) the study of political institutions—e.g., congressional committees, patterns of bureaucratic appointments, etc. To a lesser extent, comparative politics at one time involved taking up the same sorts of questions cross-nationally. Perhaps simply because a look abroad inhibits myopia of all sorts, comparative politics became much more open to contrasting intellectual approaches and a diversity of problems of study. There have been substantial relations between comparative politics and comparative sociology, especially with regard to such issues as the role of trade unions, social movements, and nationalism.

From the 1940s to the 1960s, many sociologists worked hard to maintain a double distinction of their work from politics. First, it was not political because they were "value-neutral" social *scientists*.[54] Sec-

54. Though this phrase of Weber's was commonly cited in explaining the distancing from politics held to be appropriate to sociologists, the substantive conceptualization

ond, their work was not political because that was a dimension of society that they delegated to political scientists. The former was a misleading ideology, of course (which is not to say that recognizing political biases and engagement means discarding entirely the ideal of distinguishing them from empirical findings). Enough has been said on that subject, I think, to let it rest.

The second aspect of the distinction is equally significant and less immediately recognized. Quite simply, it means that the very conception of society with which sociologists worked was apt to be distorted by the attempt to define politics as a removable institutional area within it. Much the same sort of thing was done with economic activity. Both were treated as separable institutional areas, segmentable along with religion, education, health care, and the family. In fact, because they were largely to be delegated to cognate disciplines, they often received even less attention.[55] What was obscured from consideration under such a plan was the role of political institutions (the state) and economic institutions (capitalism) in constituting what we mean by society in the modern world. The very fact that nation-states were the units in most comparative analyses went virtually unremarked and its implications nearly unexamined. The central role of capitalism was obscured by conceptions like "industrial society" and generally ignored.[56] It may be argued that family and religion were or are in some cases equally salient to defining and constituting in practice some social units, but the sense in which society came to be used as a singular noun in ordinary sociological parlance depended specifically and crucially on the state. Only the state could explain what was meant by a single society having multiple "peoples" within it, and admitting of dramatically different concrete forms of social organization at the community, ethnic, religious, or familial level (though reliance on the notion of society during the heyday of functionalism tended to reduce attention to such internal diversity by focusing on the presumed generally normative status of the mores of the dominant group).

Whatever the claims about division of labor, sociology and political

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was more Durkheimian. As Aron (1968) has noted, it was really Durkheim and Comte who pioneered the apolitical conception of society.

55. In introductory sociology textbooks, for example, politics and economy were at most likely to receive attention in a single combined chapter, while other social institutions received full chapters. Even religion and education—clearly dealt with by other disciplines—were canonically held to need a full chapter each, though politics and economics did not.

56. Since the publication of a substantial literature of praising capitalism, and a shift in political rhetoric during the last ten years, it may be hard to recall that a generation

science are probably the most similar of social sciences in style of work. Some individual scholars—Seymour Martin Lipset, Philip Converse, Duncan Macrae, Theda Skocpol, and Ira Katznelson—have moved freely between the two fields; the boundary crossing remains very active among younger scholars.[57] Statistical methods, empirical theories, and theoretical orientations have been shared. Perhaps the most striking difference is the absence of a strong tradition of normative theory in sociology (and along with it the much shorter temporal depth of most sociologists' historical studies in social theory). As policy analysis has grown as a field it has frequently linked political science and sociology (see Macrae 1985), though economics remains political science's more common partner in that venture. Political science and sociology shared both the growth and the crises of the 1960s and early 1970s. They have shared in the revival of both academic Marxism and rational choice theory.

Interrelationships may grow still further in new areas. The revitalization of the theory of the state and its link to new avenues of empirical research is one of the most important (see Evans, Rueschemeyer, and Skocpol 1987) and has already been alluded to (see also Kurtz, this volume, chap. 2). The long overdue growth of sociological study of peace and war suggests increasing relationship to the field of international relations. Attempts to bolster democratic theory with a developed conception of critical discourse and the public sphere have come from both disciplines (Habermas 1989; Keane 1987; Calhoun ed. forthcoming). Development studies (or more generally, studies of the Third World) draw on both political science and sociology as well as economics. Though political science is more prominently represented in area studies programs, this is another meeting ground. Finally, "poststructuralist" work offering claims about the ubiquity of power in all social relationships and even in knowledge—Foucault is the most important—is shaping a range of interdisciplinary discourses in which both sociologists and political scientists (among others) are playing active roles. The most important instance of this is probably feminist scholarship (though the relative absence—or nonprominence—of feminist theory and theorists in sociology is striking; feminist theory is much more visible in political science, perhaps because of the greater emphasis on normative theory).

ago "capitalist" and "capitalism" were words which smacked of Marxism and at least implied criticism. Euphemisms like "the free enterprise system" were more common.

57. It seems more common for sociology Ph.D.'s to be employed in political science departments than the reverse (though I cannot say with any certainty that this is so).

Important as all these growing interdisciplinary discourses are, it remains remarkable how much the separation of political science from sociology has been maintained. There are journals such as *Politics and Society* and various broadly Marxist journals that try explicitly to join the fields. Graduate students cross departmental boundaries to take courses. But, if our citation analysis is to be believed, disciplinary literatures remain substantially separate (see also Crane and Small's findings, this volume, chap. 5).

Economics

One of the striking findings from our citation analysis is the substantial increase in references to economic literature since the late 1940s and 1950s. The earlier low figures are also surprising in themselves. Economic concerns had of course figured centrally for Marx, Weber, and Simmel, and only somewhat less centrally for Spencer and Durkheim. They had loomed large in the early work of Parsons (Camic 1989) but as Parsons's theory developed, and especially as it became more cultural and evolutionary, it turned away from economics.[58] Early American sociology had not been as heavily economic in orientation as that of Germany. Nonetheless, economic life figured centrally in Sumner's social Darwinism, in Giddings's accounts of community life, and in the work of other key figures in early American sociology. In his lifetime, Veblen was probably as influential in sociology as in any other discipline. By the postwar period, however, sociologists seemed to accept a disciplinary division of labor. The increasingly technical nature of economics may have had something to do with this. There was also, however, the sociologists' willingness to define their concern as having to do with the consequences of economic phenomena (e.g., the experience and social organization of poverty) rather than with the study of economic activity as such.

There are three dimensions to change in this pattern. First, a substantial and growing number of sociologists have turned to rational action or rational choice models as a primary theoretical orientation (e.g., Hechter 1987; Coleman 1986, 1990; see discussion in Wacquant and Calhoun 1989). These models have been most central to economics, and indeed in their characteristic form involve economistic assumptions about the nature of human action, though that is not strictly essential. Figures like Gary Becker have in fact made a partial

58. Parsons and Smelser's (1956) *Economy and Society* is perhaps the most neglected and underappreciated of Parsons's books, as Parsons himself once commented.

crossing from economics to sociology with their extension of economic rational choice models to sociological problems from marriage to fertility behavior, residential location to social movements.[59] In this sense, the current extensions of rational choice theory are making economic reasoning into a much more general approach to social life (as distinct from an approach to a restricted segment of social life); economic rationality is staking a claim to be a theory of comparable capacity for general understanding to the other classic social theories of sociology's heritage.

Second, many sociologists have become involved in the analysis of economic activity as a social process. For some, this has meant the long overdue development of the study of business as an institutional arena (and thus distinct from the search for general laws of organization; see Hirsch 1986; Jackall 1988; Calhoun ed. 1990). For others this has meant studies of markets (White 1983; White and Leifer 1987). For still others it has meant involvement in questions raised by the "new institutional economics"—for example, questions about the relationships between markets and organizational hierarchies (Williamson 1982; Powell and DiMaggio eds. forthcoming). This last is perhaps especially important, and has already involved the creation of a substantial arena of interdisciplinary discourse and a number of powerful analyses. It has direct, if older, sociological roots in the work of Selznick (1960), for example, and it recovers a pre-World War II closeness of relations between economists and sociologists. Though a fairly idiosyncratic work, Stinchcombe's *Economic Sociology* (1984) is something of a bridge between these first two dimensions to the growth of relations between sociology and economics. It combines rational choice theory with attention to ecological and other material conditions of social life, and to the institutional organization of economic activity.

Third, there has been the rise of political economy and development studies—two closely related fields in which sociologists have been central participants in recent years, not just borrowers from or supplementers to economics. Development studies obviously has a direct lineage back to modernization research, though it developed as part of the reaction to that school. It is an area in which sociologists are

59. Becker (1976) also contributed a key argument which helped convince many sociologists that rational choice theory did not involve a capitulation to *psychological* reductionism. He argued that the basic condition of scarcity, which made laws of supply and demand operative, allowed one to derive all the essential features of rational economic behavior without recourse to psychological accounts of individual procedural rationality.

linked to anthropologists and to some extent political scientists, as well as to economists. The rise of political economy and, in part, of development studies also owes a good deal to the politics of the 1960s (including the internationalism that manifested itself in such contradictory ways as the Peace Corps and the Vietnam War).

The reformulation of modernization theory started out as a challenge from Latin American economists, notably Raul Prebisch and his UNCTAD colleagues. From an early point, however, sociologists joined actively in the project of accounting for dependency and underdevelopment. One central figure, Andre Gunder Frank (e.g. 1967), was quite difficult to classify as either sociologist or economist (trained in part at Chicago in the interdisciplinary circle of Hoselitz and the journal *Economic Development and Cultural Change*). Sociological factors were at the core of *dependista* arguments. Perhaps most notable was the suggestion that Latin American countries, for example (almost all the early dependency theorists focused on Latin America), were not developing strong capitalist classes of modern industrial sectors precisely because of their links to the already rich countries. Instead of autonomous capitalists, countries like Brazil and Argentina had their best businessmen working as agents of American and European companies. Their political leaders were primarily intermediaries between their own countries and the governments of rich foreign countries on whose aid they depended. Governments in the poorer countries of the world had to be at least as attentive to the foreigners who gave them aid as to the wishes and needs of their own people. This made democracy—one of the supposed elements of modernization—hard to develop.

Dependency theory was not a final solution to problems of development nor a long-reigning dominant force in development studies. Nonetheless, its challenge to modernization theory was deep and effective. The way in which the contest was played out, however, with culturalist modernization theory as the main antagonist of a very sociological, class-oriented dependency theory, itself obscured some important issues. Certain material factors are crucial to development, even if they were not as sufficient as once was thought. Roads and other developments in physical infrastructure, for example, still matter, dependency or no dependency (Rostow 1980).

Development studies continues to involve sociologists in a broad interdisciplinary discourse with economists and others. Studies of peasant societies and of Third World states have been particularly active terrains for sociological work (see Alavi and Shanin eds. 1983). The most important sociological line of work in this field, however,

has been the world systems theory of Immanuel Wallerstein (1974, 1980, 1989) and his colleagues. Initially an Africanist, Wallerstein turned his attention to a reexamination of the Europe-centered process of capital accumulation during the early modern period. His account of the modern world system drew most heavily on historical research, and it has remained significant in historical discourse as well as political economy. A more or less Marxist account ("more or less" because of its heavy stress on trade relations rather than relations of production), world systems theory was part of a dramatic revival in Marxist discourse during the late 1960s and 1970s.

The academic rehabilitation of Marxism was most influential in sociology of all the social science disciplines. Economics remained one of the most conservative. Yet, of course, Marxism involved economic analysis. Once sociologists began to take seriously the notion of capitalism as either a kind of social arrangement or a crucial force shaping social arrangements, even those who were not strict Marxists or who had left the Marxist fold found previously ignored economic elements to be central. Of course, classical sociological theory also offered important non-Marxist theoretical foundations for economic sociology, notably the work of Weber and Simmel.

Attempts by sociologists to adduce intellectual principles to explain their disciplinary division from economics are fairly rare. As in the case of political science, sociology is simply confronted with a larger, arguably older, and better organized field that will not go away simply because of sociological claims to include its subject matter. Economists are the least likely of all social scientists (unless psychology is counted as a social science; Rigney and Barnes 1980) to cite work outside their discipline, partly, it would seem, because of their large numbers and high prestige, and partly because of the relatively strong governance of economics by a paradigm that renders much of the rest of social science (and of the social context and organization of economic activity) irrelevant or at least distant.[60]

One area of interdisciplinary work that would seem to touch closely on economics, but often has not, is worth mentioning in this respect. This is the cluster of activity and publications that I have loosely grouped as "organizations, administration, management and labor."[61]

60. There are exceptions, of course. A notable one is Hicks's (1942) attempt to reformulate economics as a science of social welfare and to some extent social organization (a well-known, if not necessarily altogether successful venture, and one in which Hicks found little need to enlist the aid of sociology).

61. It could easily be argued that I have grouped together too heterogenous a set of publications and fields. How close, after all, are the linkages among the *Administrative Science Quarterly* and the *Monthly Labor Review*? One thing they do have in common

Journals and teaching in this area (or areas) are often linked to business schools or schools of industrial and labor relations, administration, and similar interdisciplinary hybrids. The prominence of such fields in general, and particularly in sociology, has grown substantially. Where work on the old problematic of bureaucracy was largely contained within sociology (and to some extent political science), the new field of organizational studies is increasingly dominated by work conducted in business schools (although often by sociology Ph.D.'s employed by them).[62] Interdisciplinary journals serving this cluster of activity now account for one of the largest sources of citations outside sociology proper. The very absence of strong attempts to distinguish in principle among sociology, economics, and political science may be one of the reasons why this area could flourish so readily across disciplinary borders. Of course, compared to the more uneasy tolerance of cultural studies or social history, it also has the advantage of a high-level of use of advanced statistical techniques and a general association with high-prestige academics whose status as social *scientists* is not in doubt. The relative prestige of journals like the *ASQ*, and their acceptance as genuine sociological publications, contrasts sharply with the way anthropological and historical journals are treated (e.g. by tenure review committees).

This does not have to do simply with the fact that organizational concerns were central to classical sociological theory and research, for so were historical and anthropological topics. It does not have to do with a long history of sociological linkage to economics and political science, it would appear, for those fields were cited less than anthropology in the early years of our sample, though by the later years they far exceeded it. In a sentence, then, my conclusion is that similar styles, compatible ideologies of scientism, and the rationalization of these by proclaimed principles of exclusion played the central role in minimizing the respectability of some areas of interdisciplinary work while elevating the status of this particular one.

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is links not only to economics but to the concerns of business management, particularly as incorporated in the various programs of the university business schools which have risen to new prominence in recent years, especially, but not exclusively, in the U.S.

62. Ironically, though, sociologists teaching organizational studies in business schools have seldom been pioneers in developing an institutional sociology of business (see Calhoun ed. 1990). They have often been committed to a "scientific" paradigm for organizational studies, which narrowed its subject matter greatly, and sometimes, as in the case of population ecology, made it rather distant from managerial concerns and especially ideas of managerial action. The old problematic of bureaucracy has actually shifted in large part away from organizational sociology (except in textbooks) and toward political sociology (to the extent that it remains active in the research literature).

The Project of a General Understanding of Social Life (by Way of a Conclusion)

In reviewing sociology's relations with several other disciplines in the social sciences, I have argued that there are no coherent, principled bases for justifying the disciplinary division of labor. That does not mean that the disciplines do not differ substantially; they do. Nor does it mean that having some disciplinary division of labor is simply a bad thing and that we should have only social science in general. Rather, given the scale of the social science enterprise today, some internal differentiation seems unavoidable. Moreover, there is something gained from the very defense of pluralism that disciplinary divisions produces. I should not want, for example, to see anthropology swallowed by sociology, especially if that meant a denial of resources to the kind of work done by anthropologists because it was such a minority tradition. Nor, in the same vein, would I want the larger and, in many quarters, more powerful disciplines of history, economics, and political science to have the opportunity to overwhelm or expunge the particular internal pluralism which is one of the attractive features of sociology. What I would argue is that elevating the divisions among disciplines to matters of principle discourages the recurrent reshuffling of boundaries and formation of interdisciplinary lines of work that are part and parcel of flourishing intellectual life. Moreover, each of the disciplines is itself impoverished by making a principled exclusion of that which is the turf of another. Our conception of society is damaged if we allow politics, or the economy, or culture, to be cut from it. Not only is it rent by gaps, but it is distorted in its fundamental nature. And conversely, work on politics, or the economy, or culture, is distorted and impoverished if those topics are reified and abstracted from the context of a general account of social life.

While the disciplinary division of labor does protect a certain pluralism, it ironically encourages an intradisciplinary monism—that is, an attempt to give a singular and unitary definition to sociology or any other discipline. For the most part, I think sociologists intuitively recognize this, and have come to tolerate a high level of internal pluralism. The tension between this and the idea that in principle there must be some principled unity to the field, however, often leads us to keep a certain amount of bad faith with ourselves. We make insupportable declarations to undergraduates; we devalue some subfields as "not really sociology"; we structure requirements in graduate programs as though there were some consensus about the minimum essential knowledge required to be a sociologist; we discourage cross-

disciplinary work by both graduate students and young faculty still subject to tenure evaluations.

In fact, we act on a peculiar notion of the division of labor. With Durkheim, one might expect modern sociologists to see the division of labor as a source of interdependence and unity. But such interdependence would depend on a constant exchange—in this case of ideas—among the various units. Instead, we are like industrialists who, having established a division of labor among the producers of various components—wheels, axles, seats, engines—see no need for an assembly process in which the various components are brought into relationship with each other. On this analogy, the "car" we would attempt to create from the products of diverse labors within and without our discipline would be a general understanding of social life.

Strangely, given the very name of our discipline and the rationales we offer to defend its integrity, we do not seem really to care very much about a general understanding of social life. General sociology has been relegated primarily to introductory textbooks, and to a lesser extent to a sort of social theory that most practicing sociologists use but little in their work (and which draws too little on empirical research). Even the way generality is pursued in introductory textbooks indicates a telling problem. We divide up the discipline into a number of more or less discrete topics, each the subject of a chapter. These chapters are minimally integrated.[63] So the sense in which sociology claims to offer students a general understanding of social life is that it

63. That is, especially, they are not very much substantively integrated. Publishers introduce a variety of stylistic integrations—boxes with a running heading and other devices of format, for example. Authors provide a scheme (almost always the patently unrealistic notion that there are three basic theoretical orientations in sociology—functionalism, conflict theory, and symbolic interactionism) by which to make recurrent reference to schools of thought. But there is little attempt to link up what is said about the economy in its chapter to the way economic factors affect the family, or to the role of capitalism in producing social change, or to patterns of criminal activity and varying responses to them. Moreover, during the last fifteen years the number of chapters in a typical book has grown from about fifteen (dictated not by intellectual considerations but by the number of weeks in a semester) to more than twenty. This reflects a further fragmentation of the discipline, on the one hand, as more and more subfields require attention. On the other hand, it reflects our inability to recombine subfields. We cannot drop a chapter from the canonical table of contents. At one level, this is simply a matter of marketing; the publishers are afraid of losing customers among the highly conservative teachers of introductory courses. But it is also a matter of disciplinary consciousness. We are, quite simply, *habituated* to the idea that there will be separate chapters on socialization, family, and education; if we are now to add "life course" to them, there will just have to be an additional chapter. How much current work is being done in each subfield, or its relationship to any overall way of integrating the wealth of work in the discipline, is irrelevant.

offers them superficial information on a very wide range of topics concerning social life.

The difference from a typical history or anthropology course is instructive. The history course will usually focus on a particular period and place—in one sense a highly restricted approach to social life. It will not be called "Introduction to History" but "Colonial American History" or "Twentieth Century U.S. History" or, more broadly, "Western Civilization, Part II (since 1500)."[64] Within the definition of scope thus offered, the course will attempt to tie together aspects of political, social, economic, and intellectual/cultural history. Obviously, particular instructors, departments, or the whole field at certain times may have biases (e.g., traditionally minimizing economic and especially social history). Some courses may be topically defined as "labor history" or "women's history."[65] Nonetheless, the student is apt to learn about some instances of the phenomenon of "social life" or "human life" in a fairly rich way. He or she will be led to try to make connections among different aspects of such life, and to note contrasts and similarities between the historical instance studied and his or her own life. Similarly, in a typical anthropology class, the student will be presented with several ethnographic cases—the Swazi, say, the Tikopia, and the Tiwi—and perhaps studies that go beyond local groups to consider regions or states in China, Ghana, or Peru. The student will be asked to try to relate kinship to ecology, economy, religion, and power relations. Once again, much may be left out. Even so, what both these stereotypical history and anthropology courses offer is a confrontation with relatively rich concrete subject matter in a way that encourages the student to think about it in at least something of its fullness. This, I would suggest, is a central reason why history and anthropology succeed in winning more devoted undergraduate students, from among the most talented students, even in periods when the job prospects for new Ph.D.'s in those fields are much worse than for sociologists.[66]

64. The last might, at one time, have defined the broadest history course. More and more often, however, history departments (and the setters of liberal arts B.A. requirements) are offering courses in "World History" (still usually demarcated by dates suggesting the beginning of modernity or the end of the previous epoch). It is not clear whether such an approach does justice to the diversity of the world's peoples, but colleges seem unwilling to require non-Western history alongside Western.

65. In fact, increasing specialization within history is expanding the latter sort of offering—making history more like sociology—at the expense of the earlier idea of integration of different aspects of life in a period. This is partly, but by no means entirely, an effort to ensure attention to people or themes left out of more traditional courses.

66. At the conference on which this volume is based, William D'Antonio (see chap. 3, Tables 7 and 8) presented statistics showing the relative weakness of the GRE scores

Obviously the relative prestige of fields is also an issue, as is their perceived usability as pre-professional school majors. Sociologists should ask themselves why sociology is not seen as a more attractive prebusiness and prelaw major, and should consider whether the relative prestige of fields is not greatly influenced by the way that they are taught at the beginner's level.[67] I would suggest that history and anthropology, for example, present themselves in a particularly favorable way.[68] With regard to the dimension under consideration here, both do more to present a sense of the fullness of social life than is evident in the work of most historians (who are apt to write about highly specific features of a time and place—not nineteenth-century France, but "marriage patterns among industrialists in Lyon during the Second Empire") or most anthropologists (who still do write gen-

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of sociology graduate students. There are several possible reasons for this: both sociology majors and sociology graduate students may be located disproportionately at non elite schools, sociology students may include a disproportionate number of those for whom GREs are poor indicators of ability, or a larger percentage of sociology majors may take the test. Be this as it may, it would appear clear that sociology is not attracting as many of the best students as it should or as other social sciences do. Not only are sociology students' GREs low, they have declined further from the discipline's peak than is the case for other disciplines (especially in the natural and physical sciences); we are thus losing ground relatively, not simply suffering a common decline. Even in the period 1977–87 when job prospects were much worse in anthropology and history than in sociology, anthropology students scored an average verbal and quantitative total of 1077; history students scored 1079; sociology students scored only 972. Students in anthropology and history even scored substantially higher on the *quantitative* half of the aptitude test. We delude ourselves, in short, to think that good undergraduates are not going into sociology only because they are careerists headed for professional schools or "harder sciences." Good students, oriented toward academic careers, interested in social issues, are simply choosing other fields in disproportionate numbers.

67. Even economics and political science, which do not generally confront social life in the rich fullness characteristic of anthropology or history, do teach introductory courses in a way decisively different from sociology. Political scientists teach about politics and public issues (even though they may do research, especially in American politics, which is remarkably depoliticized); economists teach about the economy (even though the work of most research economists is highly technical and abstractly removed from the day to day functioning of the economy). In economics particularly, the writing of an introductory textbook is far more of an effort of synthesis than in sociology, and correspondingly textbooks also serve as reference books within the discipline and are more often written by major figures than is the case in sociology. Of course economics has the benefit that students are prepared to face harder courses because they have a prior belief in the seriousness and practicality of the discipline. To some extent, sociology's attractive pluralism and internal diversity are the enemies of synthesis.

68. By "favorable" I do not mean that courses are simply more popular, but that they give a view of the discipline as more intellectually serious and satisfying. Sociologists—as indeed stereotypes suggest—often achieve superficial popularity—high enrollment levels—at the expense of intellectual substance and accordingly portray their field in a very unflattering light.

eral ethnographies, though much less often than in the past, being now more likely to write works focused on an aspect of or problem in the ethnography of a particular people, say, "linguistic indexicality and sense of self among the Bororo").[69] The very superficiality we impose upon ourselves in the attempt to convey generality through taking up so many different issues, rather than by relating aspects of a given instance of social life to each other, may contribute substantially to the way we are seen by powerful decision makers in government and the university community. Most people only know sociology through an introductory course, after all, not through research and theoretical works. And we often present ourselves at our worst in such courses.

Much the same can be said for the ways in which sociology is presented to the general public and informs public discourse. Sociology does a poor job in producing general, synthetic works that present research and theory to the educated public in an attractive form. Of course, this does happen occasionally and very successfully, as in the long tradition of sociological critiques of individualism, including those of Riesman (1950), Slater (1970), and Bellah et al. (1985). But these works only point to the difference between the sociological writing that attracts public attention and that which earns prestige and publication in the major journals. There are other exceptions, more closely related to disciplinary research agendas—for example, Coleman's *Adolescent Society* (1964), or Wilson's *The Declining Significance of Race* (1978), but not the number and range for which we should hope.

Our poor disciplinary self-presentation is not just willful and not entirely accidental. It is linked back to the general issue I have been discussing throughout this paper. Our urge to claim a specific place for sociology within the social sciences, and thus to proceed by distinguishing ourselves in a principled fashion from our competitors, is largely at fault. Why do we continue this effort that so impoverishes our self-understanding and work? I think, in closing, that a central reason lies in scientism. This begins with the notion to which we have

69. A related difference between anthropology and history, on the one hand, and sociology on the other is that sociologists are for the most part much less likely to see scholarship as a central criterion of evaluation distinct from research. That is, there is less emphasis in sociology on knowing the literature of a certain field which extends well beyond one's own specific research interest in it. Ironically, perhaps, the two specialties in sociology for which this is least true are theory and methods (and it is significant that both are conceived as specialist undertakings, not generalist knowledge). Unfortunately, perhaps, standards of "objective" research productivity are gaining ground in history and anthropology at the expense of direct respect for scholarship.

adhered since Comte that every science must have a distinct subject matter. This (as well as academic politics) is what informed Durkheim's overstated effort to distinguish sociology from psychology. It is, in the contemporary discipline, a key reason why we break our own field up in terms of topics and try to break up the social sciences in the same way. Scientism urges on us a belief in the objective certainty of empirical knowledge and makes us fearful of the suggestion that all knowledge is rooted in the perspective of the knower. Recognizing this does not obligate us to abandon the idea of truth or the project of empirical research. But we do need to see that truth—knowledge—depends on understanding, not just data. And surely, I hope, we want to understand social life in its richness and complexity. If so, we should focus less on disciplinary boundaries and recognize that both within and across disciplines there are different perspectives that shed light on an inherently common subject matter—human life—which is always social, yet always admits of certain tensions between individuality and sociality, which is always historical, and which is always culturally rooted and specific.

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