Some Animadversions on the Americanization of World Sociology

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As the latest arrival among the social sciences in the United States, sociology in effect defined its field as what the other, already established disciplines had not laid claim to. Some of these remnants, in particular the family, were of such fundamental importance that it was only by accident that they had not become, like the state or the economy, the subject matter of single academic departments. Other bits and pieces were what any scavenger might pick up — interesting sometimes, but hardly the elements of a coherent whole. And from this heterogeneous base, sociologists more recently have successfully invaded the territories of their neighbors: the difference is vague and usually slight between “political sociology” and parts of political science, or between “industrial sociology” and one area of economics, or between the “sociology of culture” and cultural anthropology.

The prime virtues of sociology as a field, thus, are negative: since it lacks boundaries, it can escape the sometimes arbitrary restrictions of a defined discipline; since its development lacks direction, it can move into areas that are off limits to more formal scholars. To define the field positively, on the other hand, is notoriously difficult. In my department, as I am sure in sociology departments throughout the country, the recurrent efforts to work out the curriculum more precisely never achieve anything more than a temporary stalemate, for almost every one of the department’s members, projecting his own point of view and manner of work to a larger perspective, arrives at a unique designation of the whole field. Given this variety, American sociology is hardly subject to overall criticism; for no matter what the critic’s bias, he will find something to praise and something utterly wrongheaded, based on false theory and pernicious methodology.

If I am correct in my impression that much of the world’s sociology is being patterned after the American image, this influence is not of one piece. Some of it in my view is all to the good. But in too many cases what has been taken over is not the best but the flashiest, and in this survey I shall concentrate on these less propitious borrowings.

I.

Let us, as a kind of sociologist’s dream, imagine a newly created country with almost every social choice freed from tradition and open to fresh analysis. In the inhabitants’ efforts to transcend their diversity — for they have immigrated from many parts of the world — and to create the basis of a new national unity, they constitute a permanent large-scale social laboratory. The government, so far from impeding social analysis, welcomes it, for its commitment to rapid social development depends on continual expert advice on how this can be realized. If such a country existed, its sociologists would be happily placed in that otherwise rare situation, simultaneously
developing theory, performing research, and guiding social policy. Or so one would suppose.

The country does exist: its name is Israel. And the country’s leading sociology department is the world center of Parsonian theory, which its chairman has even extended to a comparative analysis of social systems. It is as though seismologists lucky enough to be living on a volcano approached their subject by a analysis of the references to earthquakes in Herodotus.

In the United States, while probably all sociology students are still forced to read some of Professor Parsons’ prose, his influence has been giving way to increasing criticism. When I was a graduate student at Columbia, the department’s principal theorist, Professor Merton, set the tone against grandiose efforts and for what he later termed “middle-range” theories. In our course on the history of doctrine, it is true, we read Comte and Spencer and Marx and many other grand theorists of the past, but it was implicit that expounding great systems was, precisely, a thing of the past. Parsons’ picture of himself as the culmination of the grand tradition has always run against the strong pragmatic, “practical” bent of American social studies. If for a period his sheer prodigious effort neutralized this hostility to what he was attempting, my impression is that now the tide is turning back. One reason is the quality of the work: inspite of the excellence of The Structure of Social Action, Parsons is manifestly not of the same caliber as Marshall or even Spencer. The postulates of his general theory, when translated into plain English, are at the level of common-sense proverbs, and, like all folk wisdom, are sufficiently ambiguous to be adaptable to almost any circumstance.1) Even when such theories are valid, they impede progress. It is true, for instance, that every atom in the universe affects and is affected by every other one, but if analysts of the material world had permitted the grandeur of this structural-functional vision to dominate their thinking, there would be no modern physics.

When Parsons descends from his usual heights to examine a mundane problem, he often demonstrates, however unwittingly, that his theory is irrelevant to such an enterprise. Two examples out of many must suffice to illustrate the point. In 1946, when knowledgeable social analysts were trying to explain the baby boom, Parsons offered a quasi-Freudian — that is, completely general — explanation for a decline in fertility. The adolescent girl, competing for masculine favor, becomes insecure and therefore aggressive; and this aggression “underlies the widespread ambivalence among women toward the role of motherhood, which is a primary factor in the declining birth rate.”2) His most recent essay, on the status of the American Negro, is characterized by the usual self-indulgent lack of structure, by new barbarous neologisms (“what I shall call the societal community . . . refers to that aspect of the total society as a system, which forms a Gemeinschaft, which is the focus of solidarity or mutual loyalty of its members, and which constitutes the consensual base underlying its

political integration”), and by a total absence of original analysis. Some of its statements, moreover, are demonstrably false — for example, that “the WASP’s” (a derogatory designation for white Anglo-Saxon Protestants) “generally succumbed to the temptation to define their own role on rather aristocratic terms.” 3) In fact, from before the Civil War to the present, the struggle for civil rights has been in large measure by some Protestants of English forebears, who in this respect (as in most others) do not constitute a coherent social group.

If one tries to analyze not the well established, more or less discrete societies of the modern West, but the social world of the past or of underdeveloped countries, then Parsons is even less useful as a mentor. In such a case, to define one’s unit of study as “the social system”, which is made up of mutually interacting “subsystems”, begs the most important questions.4) Was there a “France” in 1000 A.D., or a “China” in 1700; is there a “Nigerian society” or an “Indonesian society” today? If one assumes that the answer to such questions is unambiguously yes, then one is tempted to explain away social phenomena that transgress these units (the “Chinese” who were not part of “China”) or that do not fit into them (tribal influences in Africa as “remnants”). For Eisenstadt, thus, the outcome of the struggle among Israel’s diverse peoples is predetermined, for the eventual victory of the modern Western socialist strain is assured by his very definition of “acculturation.” 5) The question, which of the subcultures, if any, will be established as the national norm, is not examined; Eisenstadt’s sociological theory is not a check to his Zionist predilections but a reinforcement of them.

What is the point of social theory? The answer that at least some of the earliest exponents of American sociology gave to this question, to establish the existence of cause-effect relations in order to be able to intervene successfully in social policy, strikes the modern ear as absurdly naive, but I would like to argue that it is correct. It has become fashionable to dismiss social causation as beyond the powers of analysis, if not indeed meaningless. Two factors, A and B, can be shown to be correlated, but whether A causes B, or B causes A, or both, or neither, is according to this theory beyond our ability to determine. I believe, on the contrary, that establishing when a correlation expresses a cause-effect relation is the key to true understanding. Three is a high correlation between bad swamp air and malaria (as the word’s etymology suggests), but should physicians have therefore reserved judgment on whether this factor caused the disease? 6)

Perhaps the best example to illustrate the crucial practical difference between a correlation and a cause-effect relation is parole prediction. In the mid-1920’, Ernest

6) I do not mean to suggest that epidemiologists see “cause of death” as a simple relation, to be facilely analyzed in every case. For a discussion of some of the complexities, see William Petersen, Population (New York: Macmillan, 1961), pp. 244-248, 587-590.
Burgess, then one of America’s leading sociologists, headed a team that undertook
to differentiate on the basis of a large number of factors between those who did and
those who did not break parole. From the systematic associations, they then constructed
prediction tables, which Illinois and subsequently other states adopted as a "scientific"
aid to the common-sense judgments of parole boards. Since most persons convicted in
American courts get indeterminate sentences, the length of their imprisonment
depends on whether, and how soon, they are granted parole, and thus under this
system on the factors found to be associated statistically with past success. In the
original tables, these included the prisoner’s age, whether his parents were native or
foreign-born, his race, his marital status, the type of neighborhood he lived in, and
so on. These items were eventually dropped because they "lacked a sufficient degree
of association with outcome, or statistical significance, or reliability, or stability." The
items continued in the Illinois tables still included the type of home the offender
came from, how interested he is in his family, whether he works regularly or not,
his "social type" ("erring citizen, marginally delinquent, ‘farmer’, socially inadequate,
ne’er-do-well, floater, socially maladjusted, drunkard, drug addict, sex deviant"), and
so on.7 In short, the consequence of this system is to subvert the first principle of
Western justice, that a man shall be punished as an individual for the crime he
personally committed, and to substitute for it punishment based in part on the group
characteristics he shares with many others. The method has also been applied, with
even less justification, to predict on the basis of their general characteristics which
children and adolescents are "predelinquent" and thus demand "treatment" before they
have committed any offense.8

In my admittedly spotty and sporadic reading of the sociological works of various
countries, I have never seen a reference to an excellent work that argues the funda­
mental importance of social causation,9 which in my view is worth more than Parsons
entire corpus. Like Parsons (as well as Florian Znaniecki and Howard Becker, who have
also been unjustly neglected) MacIver rejected simplistic positivism in part
by incorporating a theory of action into his social theory; unlike the others, he has
seen theory less as a mode of describing reality than a tool with which to analyze it.
The second premise of American sociology naïve beginnings, that one studies
society for the purpose of improving it, is also valid in my opinion — though again
in an unnaive sense. Of course, the relation of values to research is not a problem
restricted to American sociologists, but they seem to be addicted to especially sim­
plistic solutions of it. Such excellent guides as John Dewey have been ignored in
this discipline, and Max Weber’s relatively complex stance is often reduced to a

Foundation, 1951), pp. 52, 122-123.
8) See Barbara Wootton, Social Science and Social Pathology (London: Allen and Unwin, 1963),
chap. 6.
9) R. M. MacIver, Social Causation (Boston: Ginn, 1942). More recently, the best American work
on the analysis of causation has been by Paul Lazarsfeld, but, again, not in the articles for which he
is best known.
The question has been much discussed recently, and in this context it would not be appropriate to review the whole matter again. I pass over the question of whether social scientists may legitimately analyze values (a false, even a silly issue), what the values of social science properly are (e.g., to be value-free), whether social scientists typically hold values relevant to their professional work ("in nearly every sociologist there lurks a social reformer, less ambitious but today more practical than Comte"); and I concentrate on the effects of the professional canon prescribing an aloofness from moral issues on practitioners who almost to the man believe in their version of the betterment of society.

1. If social issues are not faced directly by those with the greatest relevant knowledge and experience, others deal with them. Books by such types as Vance Packard and Paul Goodman, thus, are assigned to students in American college classes; and the current interest that sociologists show in American "poverty" was stimulated by Michael Harrington's long pamphlet.

2. Those in the profession who try to change its course generally spend so much effort opposing the positivist tradition that they become shrill and, as in the later works of C. Wright Mills, sometimes lose their sense of professional responsibility. To the notion that no moral commitment may be permitted to show, no matter how pertinent it is to one's work, there is opposed the dogma that any and every license may be subsumed under academic freedom.

3. Those who maintain the positivist stance are often pushed into a more or less conscious hypocrisy. For example, at the last meeting of the American section of the International Union for the Scientific Study of Population that I attended, the question was raised whether the "scientific" in the organization's name was still appropriate, since its main activity had become to foster birth control in underdeveloped nations. The reply was not to deny the allegation but to point out that this reformist work can be done more effectively under scientific auspices than by an avowed planned-parenthood association.

How then shall one's prejudices be kept under control; how can there be research that does not simply reflect the analyst's biases? The problem is fundamental, and what I see as the proper answer has two parts.

First, every social analyst must admit, both to himself and to his readers, that he has a value orientation and that his products may be contaminated by it. This caveat emptor may be as brief as the "S.J." after an author's name or as full an exposition as the


11) A review article on the subject lists seven diverse discussions over a dozen years; see Allan W. Eister, "Informal Comment on Values, Sociology, and Sociologists", *Social Compass*, 11 : 2 (1964), 40-43. The interaction between value judgments and causal analysis has also been a recurrent theme in historians' self-criticism, and in the United States a particularly interesting focus of the recurrent debate has been the American Civil War. Representative surveys include Arthur Schlesinger, Jr., "The causes of the Civil War: A Note on Historical Sentimentalism", *Partisan Review*, October 1949, pp. 969-981; William Dray, "Some Causal Accounts of the American Civil War", *Daedalus*, Summer 1962, pp. 578-598; Fawn M. Brodie, "Who Won the Civil War, Anyway?" *New York Times Book Review*, August 5, 1962.

methodological appendix to Gunnar Myrdal's *An American Dilemma*. That such an open commitment to moral values need not compromise scientific integrity can be indicated in the United States by pointing to some individual works, but the argument can be made better by citing the whole record of classical British sociology.\(^{13}\) This developed from practical philanthropy, social administration, and surveys of problem areas; and these practical concerns are maintained by such men as Richard Titmuss, who have no precise American equivalents. Such leaders of the older generation as L. T. Hobhouse, Morris Ginsberg, and T. H. Marshall all have written with professional competence and great insight on questions of what Ginsberg has termed "social philosophy". Their concern with ethical questions intertwined with their analysis of important social issues, and they helped to set a standard in which some of the questions that plague American sociology are not so much answered as not worth asking.

Second, the model for arriving at the truth in the social disciplines can seldom be the one we are all taught to believe in — the controlled experiment performed under laboratory conditions. A more generally relevant model is the court of law, a subtle instrument to establish a valid picture of what happened in one segment of society. The confrontation of advocates who argue for opposed positions within set rules of evidence has been one means of advance in every science. Perhaps the best example of this mode of advancing knowledge in the social disciplines is the journal *Current Anthropology*, which submits all important papers to a world panel of experts and prints their comments, together with the author's reply. In American sociology, however, such procedures have been largely blocked by the tradition of simplistic positivism. Book reviews are seldom used to challenge an author's frame of reference and contrast it with a competing one; exchange by correspondence is ordinarily omitted from American journals; professional meetings do not usually encourage a formal opposition of competing truths. In fact, this competition among schools exists also in American sociology, but half-hidden, like the value orientations of sociologist; the result is not better science but only duller.

II

Theory, whether Parsonian or any other, is not really the distinguishing feature of American sociology. The characteristic by which it is best known in other countries pertains to its mode of research, community studies and particularly public-opinion polling, which has grown during the past generation from one mode of sociological research to the dominant one to just over the horizon — the only truly legitimate one. In some respects, of course, polling was nothing new. The United States census has been taken every ten years since 1790, and a permanent Bureau of the Census was established early in the present century. The most fundamental questions sociologists later posed about statistical rigor and methodological subtlety had been both asked and in part answered earlier, in the context of the Census Bureau's work. In what respect does polling represent an extension from this established base?

\(^{13}\) See ibid., chaps. 1-2.
One important difference is the standing of the researcher. The Census Bureau is subject to law; it may ask questions only if these fall within its legal province; respondents are required by law to answer questions properly put to them; and the privacy of the response is carefully guarded against inquiries even from other government bureaus. Inquiries by sociologists are subject to no limitations, whether of law, accepted professional canons, or even good taste. Students who know nothing of a field are encouraged to learn about it by subjecting the public to a survey; and if it is useful for "science", they are taught to disguise the true purpose of their inquiry and to manipulate their respondents. That the system engenders abuses is hardly to be wondered at, and some of the ethical dilemmas posed by any social research have special pointed relevance to the methods most used in the United States.

One of the more notorious cases pertains to the privacy of responses. Often persons are willing to answer questions only if they are assured that they will not be identified as the source of the information, and one frequent means of keeping this promise is to give a blanket anonymity to the whole community in which the research was done. Of course, every undergraduate knows that "Middletown" is Muncie, that "Yankee City" is Newburyport, and so on; and even when the overall disguise is effective to outsiders, it is unlikely to afford respondent protection from their friends and neighbors. This was the case in "Springdale", a village near Ithaca, N.Y., analyzed in Small Town in Mass Society. One of the authors, Vidich, was associated with a Cornell University research project, and there was a protracted dispute between its director and the authors on various issues. When the book was published it was seen as a sufficient breach of professional norms to be discussed editorially in the journal of the Society for Applied Anthropology. If we ignore peripheral matters and try to summarize the main ethical issue, even this is complex: (a) Dr. Urie Bronfenbrenner, the project director, had given a firm assurance to the village residents that no individuals would be identified in written reports, and apparently, according to their own apologia, the authors repeated this assurance whenever it was deemed necessary. (b) According to the authors' own description of their research, it "ignored all the procedures of the scientific project research,... was unintentionally unplanned, had no budget, no a priori theory, no staff, nor research stages or phases, and was not conceived as a study or a project until it was almost over". In line with this method of work, large amounts of gossip were included in the authors' speculations about the personality, motivations, and attitudes of the community residents. (c) According to a public apology by the project director, "Upon learning of Dr. Vidich's plans, we requested him to eliminate or, at least, 

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14) For a summary of some of the court cases upholding and even strengthening these principles, see William Petersen, Politics of Population (New York: Doubleday, 1964), pp. 264-267.
modify the materials in the manuscript which we felt to be most objectionable, and received his assurance that he would do so. Accordingly, we were doubly shocked to find that much of the objectionable material had been retained". In the correspondence with Vidich, Bronfenbrenner had pointed out not only that references to individuals might do them harm but that the points raised were not really central to the analysis. Vidich defended himself as a scientist, but seemingly included many juicy items for reasons extraneous to research; critics compared it with the salacious novel *Peyton Place*. The one modest conclusion that is drawn from an analysis of research ethics is that "one should refrain from publishing items of fact or conclusions that are not necessary to one's argument or that would cause suffering out of proportion to the scientific gain of making them public". 17)

The ambiguous practices of American sociology, which is unable or unwilling even to formulate a code of professional ethics, are likely to be even less salubrious when they are exported to other countries. According to one analysis, American social science is in a crisis of ethics…

Its motives, techniques, and practitioners are falling into disrepute in many parts of the emergent world … At this moment, not a single survey research study can be done in Chile. Throughout Latin America quantitative studies have halted or been impeded, and all scholars, whether in teaching or research, find their actions questioned in direct correlation with the sophistication of the persons with whom they deal. 18) The proximate cause of the crisis was the Project Camelot, a large-scale study of civil strife in Latin America supported by an initial $6 million from the U.S. Army. Cooperation from Chilean sociologists was sought by a Dr. Hugo Nuttini, who presented it as an academic project and assiduously hid the identity of the sponsoring agency. Those Chilean social scientists who thought they were being used as tools in a Yanqui espionage plot were not reassured when, in the midst of the dispute, they read of American intervention in Santo Domingo. Eventually President Eduardo Frei protested formally to the U.S. Embassy, and the project was buried.

A second important difference between a census and a survey is the range of questions that is considered appropriate. Queries about what are termed demographic variables typically have specific referents, so that there is a single, definite answer to "Where were you born?" or "How old are you?" 19) What are now commonly termed "the behavioral sciences", on the contrary, have less and less to do with observable behavior; their data consist mainly of reported opinions and reported attitudes. There is nothing inherently reprehensible in this extended view of the social world, but it does encompass new difficulties. Would one believe it possible that, even in the United States, a team of 17 researchers would spend four years and over a quarter of


19) I do not mean to suggest that such categories as birthplace or age contain no element of ambiguity. See Petersen, *Population*, chap. 3; Aaron V. Cicourel, *Method and Measurement in Sociology* (New York: Free Press of Glencoe, 1964), chap. 5.
a million dollars asking prisoners their opinions about prison life, and come up with such encouraging data that only one percent see prison as a school for more successful crime, while a much larger proportion “try to be more conscientious about religion?” 20) The resolute naiveté of this massive study underlies the whole new methodology: if one establishes something called “rapport” (which can be taught in about two weeks), then no matter what question, asked of no matter whom, will elicit a response that can be related to social reality in a meaningful way.

I am far from certain that this is a useful proposition even in a country like the United States, where fear of government oppression is minimal compared with a sizable proportion of the rest of the world, where access to homes and a view of domestic life are much less hampered than, for example, in France or the Netherlands, where communication across class or regional lines is less self-conscious than, say, in Britain. But how shall one react to the export of these American methods to other societies, which in one way or another lack this degree of openness? A Chinese proverb tells us that “before a stranger it is better to express only one third of your opinion”, and a Taiwanese sociologist who quoted this commented as follows:

In a modern society, particularly one of the Western type, the individual citizen expresses his individual or even individualistic opinion quite freely. In a traditional folk society it is almost impossible to get a clear individual opinion on many problems. What are problems for the modern are not considered problems by these people, because they are connected with unquestionable mores. Some of our queries were considered just too foolish to answer. Other questions were looked on as impolite or of bad omen. Some knowledge of the social and cultural background is necessary to understand this attitude. 21)

In short, the most difficult part of research, that calling for the greatest subtlety and “knowledge of the social and cultural background”, is the collection of the original data. In traditional anthropology, and in that portion of sociology that in this respect derives from it, personal work in the field is a sine qua non of professional training, and a dedicated analyst of society feels it necessary to keep in recurrent personal touch with his subject matter. In American survey research, on the contrary, the collection of data is typically relegated either to “professional interviewers” (generally middle-class housewives who work part-time) or to students or junior members of a research team. It is not even necessary that the amateurs who do the interviews work under professional supervision; if a body of data has been collected, say, by the officials of a voluntary organization trying to analyze its own operations, it is possible in even the best American sociology departments to get a doctoral degree with a “secondary analysis” of these presumably biased, inaccurate, and incomplete data. For, as one proponent of this system points out, “Secondary analysis allows the student to spend more time on analysis and writing, usually the most

difficult parts of research, than if he had to collect the data himself. Indeed, when there is a director of research, his function has often become largely administrative and technical — keeping the institute staff and the computer operating — so that it is deemed permissible to do research on a subject about which no one on a team is knowledgeable.

A good example of how these American methods work out in other countries is a socio-economic survey conducted by a research team at the University of Ceylon. A questionnaire was drawn up and pretested in the standard fashion; out of the 17,561 households in 58 villages over 116 square miles, a two-stage random sample was taken; under the supervision of the project directors, a batch of thirty students surveyed three villages during weekends in August 1955 and then finished up with the remaining six villages in a continuous ten-day period in September. The project underwent various difficulties (at one point it ran out of money and was bailed out by the Asia Foundation), but on the face of it it seems to be a competent study of rural Ceylon. The main difference between this research project and others of its kind, perhaps, is that it underwent a thorough criticism by an anthropologist who had spent considerably more than ten days in the field in another part of Ceylon studying the same questions. "the anthropologist", Dr. Leach asserts, "does not doubt the power of the statistical apparatus which is the sociologist's major tool; what he tend to query is the quality of the original data which are fed into the statistical apparatus". The exemplification of this elementary point makes fascinating — and for a sociologist, chastening — reading.

In order to undertake quantitative analysis, one must begin with units that in some sense can be counted; and even when this step is not palpably problematic, it may flaw everything that follows. According to Sarkar and Tambiah, thus, the agricultural holdings they surveyed fall into two clearly defined groups, with relatively high and relatively low yields per acre. Leach suggests that the reason may be the same as for a similar finding in his own work, that the size of an "acre" is about one and a half times larger in the "traditional" (purana) village lands than in the recently developed (sinakhara) plots. Another example: Sarkar and Tambiah define a "household" as those persons who cook their rice in the same pot; and out of 506 such households surveyed, 335 own no paddy land. Leach asks whether many of these landless might not be in a different sense members of other joint households — that is, young, recently married couples who are heirs to their still living parents and

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25) In the case of this study, however, the lack of a properly equipped statistical laboratory prevented the authors from testing the statistical reliability of the measures they use and held up publication of the short monograph, which is about half of the projected complete work, for a year and a half after completion of the field work.
only for that reason temporarily without a paddy of their own. Sarkar and Tambiah report that the decline in the average holding over one generation, from the respondents' parents to the respondents, was from .778 to .082 area of paddy land, and by a somewhat smaller proportion in the highlands. A comparison is made, in other words, between what is remembered from the past and what is asserted about the present, and with respect to "the very last topic", in Leach's words, "on which one can expect an informant to offer candid and straightforward information". Moreover, the whole dismal picture of fragmentation of the land is based on the division among heirs, but the traditional recombination of holdings when persons marry is ignored. In fact, with this system operating in a static population, the average size of holdings remains constant.

It should not be surprising that survey research, a method that deliberately sacrifices deep understanding to broad coverage, should often distort reality. It is more disturbing when the study itself creates the facts it analyzes. This can be done in either of two ways, both of which seem to be becoming more prevalent.

A recent analysis of American high schools, Coleman's *Adolescent Society*, is a prestigious example. The principal topic of the study is teenage elites, who were identified by asking students who they think is in "the leading crowd". But there is a fundamental difference between, say, asking persons whom they will vote for as president of the United States and asking this of high school students: we know that there will be an election, but whether there is a leading crowd is the first question the researcher must pose. Similarly, sociologists have made a number of so-called "reputational" studies of what they termed the "power elites" of various communities; but more recently it has been shown that different persons decide which books should go in the library, whether a street should be paved, whether a rise in pay for firemen should go on the ballot, and so on through all the decisions that constitute municipal life. Whether students, who have very little formal power, throw up an elite in any sense is even more dubious; and in one isolated passage Coleman says as much: . . . Who shall be called the elites in these schools? . . .

In such a situation of informal organization, people cannot be clearly devided into leaders and non-leaders, popular heroes and non-heroes. Instead, leadership resides in many people, in varying degrees.

But this touch of frankness undermines the main argument of the book. If the "leading group" is not really a model for more than an indeterminate, but probably small, proportion of the students, then a study that presumes to analyze "the adolescent society" by looking mainly at this model suffers from a fundamental inadequacy. Yet sociologists have become so accustomed to survey questions that in effect create the social world being analyzed, probably very few readers (certainly few of the reviewers) noted that this was an analysis of an entity that probably does not exist.

An even more Kafkaesque society can be ground out by the high-speed computer.

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If a research team uses its foundation or government grant to collect facts on every facet remotely related to the subject to be studied, and if this mass of data is fed, with no a priori hypotheses, into a computer that is programmed to reveal whatever correlations exist, then some of these correlations, simply on the basis of probability, will in all likelihood be statistically significant. Of course, if the study were to be repeated, the same associations would not show up; but replications are anomalous. The worst feature of American sociology to date is its indigestible mass of unrelated facts; but now that we have a machine to relate them, who will be able to distinguish the expensive, expert nonsense from social analysis?

III

At the 1965 meeting of the American Sociological Association, attended by 2,395 sociologists, these were some of the papers read: “Sign Functions: Organized Activities as Methods for Making an Invisible World Observable”, “Ethnomethodology”, “Sociological Deficiencies and Opportunities in the Study of Hyphenated Americans”, “Fairies and their Protégés in Grimm’s Tales”, “Non-rational Elements in the Sociology of Art”, “Balzac as Sociologist”, “Sociology of Antiques”, “The Political Integration of Medieval Muslim Cities”, and “Sex as a Latent Identity and Control over Professional Subordinates”. The very heterogeneity of the papers indicates that one can hardly characterize American sociology as a unit — a point made at the beginning of this paper and worth repeating for emphasis. Some of its main tendencies, and the ones seemingly most imitated abroad, appear to me to lead nowhere. But no one can stifle so lively a body. Dozens of men with solid achievements behind them are training students; hundreds of younger people are seeking better means of analysis. One can still hope, if sociology ever jells into a discipline, that their influence will prevail.

27) The list is copied from New Society, September 23, 1965. No other choice could indicate better the range of interests represented.