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## SOCIOLOGY AND ITS USES\*

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### SOCIOLOGY AND ITS USES

Sociologists deal with questions of greatest practical importance, such as the conditions of the stability of political systems; the causes and consequences of social inequality, or the pre-requisites of modernisation. But neither sociologists, nor their actual and potential clients have been satisfied with the applicability of the answers provided by sociology.

The main criticism leveled against sociology is that it does not provide hard and fast solutions of a technical kind, such as are popularly attributed to the physical and biological sciences. There is a feeling that sociologists do not really come to grips with practical reality, only talk about it more or less interestingly.

This criticism is widely accepted among sociologists, and they tend to react to it in different ways. Some accept the limited usefulness of sociology for practical purposes as a fact of life, and are content to practice sociology for purely intellectual purposes, as a kind of pure science or branch of philosophy. Others try to remedy the situation by following more closely the ways in which research in the natural sciences is used in the solution of technical problems. Finally, there is a third group, which rejects all analogy with the natural sciences, since it does not believe in the possibility of separating facts from value judgements in sociology. They think of sociology as indistinguishable from myth and ideology, which, of course, are very practical endeavours, but not applied science.

The purpose of this paper is to show that the criticism is misplaced, and none of the three alternatives is acceptable. Sociology as practiced today has many potentially useful applications, but because of various misconceptions these are frequently unrecognised, or not made proper use of. In the two subsequent chapters of the paper an attempt will be made to identify these misconceptions and suggest proper uses of sociological inquiry. Following these there will be a discussion of the problem of value neutrality and objectivity in social research under present day conditions.

### WHAT KIND OF SCIENCE IS SOCIOLOGY ?

Among those who accept sociology as an empirical science, there is much confusion about the question of what kind of science it is. Usually it is regarded as a kind of basic discipline the purpose of which is to generate theories about social

\*I am indebted to Lia Greenfeld for her help with the research and comments on the first draft of this paper.

phenomena, irrespective of their uses. Therefore, in order to make sociology more useful for society, and more lucrative for its practitioners, they advocate the development of an applied sociology, in order to focus on questions of immediate practical importance, adopt more effective ways of investigation, and concentrate not only on the diagnosis of social problems, but also on finding practical means for their solution.

This view is based on a confusion between “basic science,” “applied science,” and “development”. Both basic and applied science are concerned with the discovery of new systematic knowledge, and not with immediate applicability. The distinction between them is in the choice of questions. In basic science questions arise from purely intellectual interests, such as investigations on the age of the universe; in applied science from practical concerns, such as a desire to understand the immune system of the body with the ultimate purpose of preventing and curing illness. Because of this practical starting point, there is an assumption that the results will be eventually of practical value. But only in exceptional, and usually unpredictable cases, are the results immediately usable. Immediate use is expected only from “development,” which is a very different process from scientific discovery. It uses available scientific knowledge, conventional wisdom, practical experience and intuition in order to invent and produce specific products or processes which are economically marketable and politically acceptable<sup>1</sup>. This latter is a very different kind of work from scientific research, and is frequently done by people with limited background in research.

According to this classification sociology is, and has always been, overwhelmingly an applied field. Even historical questions, such as that about the relationship between economic activity and religious beliefs and practices, or anthropological ones, such as attempts at making sense of the differences in the incest taboo between different societies, are practically motivated. They do not arise out of pure intellectual curiosity, but out of an interest in such practical concerns as the differences in the functioning of the economies of different societies, or the ways sexual relationships structure and reinforce solidarity between kinship and other kinds of groups.

Answers to such questions are potentially applicable to dealing with practical problems, but the difficulties of creating theories capable of dealing with complex and changing social reality, and the subsequent application of those theories in practice are very great and poorly understood. An attempt will be made to clarify these difficulties through two examples; the study of the relationship between religious ethos and economic development, and racial integration of schools in the United States. The first is the story of a search for theory with potential applications; the second is an attempt at the actual application of an existing theory with obvious practical implications.

The effect of religious ethos on economic development was first studied by Max Weber early in this century (Weber, 1952, 1958, 1960, 1968). His problem

<sup>1</sup> These definitions of basic and applied science, and development are adapted from OECD, 1967, p. 10.

derived from the widespread practical concern with the characteristics and workings of the capitalist economy. The context of his work was as much political and social as theoretical. To understand the unique characteristics of Western capitalism was important not only for comparative history – to explain the emergence of this unique phenomenon – but also for the practical politician who had to devise economic and social policies in societies in which capitalism was attacked both by representatives of pre-capitalist groups and world views, and by socialists who regarded capitalism as a transitory phase in history approaching to its inevitable decline and fall.

In the 1950s, there was a revival of interest in Weber's ideas, again due largely to practical concerns. This time the context was "modernisation," namely the attempt at industrializing and converting to a scientific world view all the primitive and traditional societies of the world within a short span of time. Weber's account of how this happened in the Western societies, where all this development towards "modernity" began, seemed to be an important starting point for a rational approach to this problem.

An attempt to apply Weber's ideas in a technical sense was implied in the work of David McClelland (McClelland, 1961, 1966). He tried to break down the Protestant ethos into its motivational and behavioral elements, and determine the conditions under which these elements arose. He hypothesised that the most important element in this ethos was the "achievement motive," a general psychological tendency to excel, which can be empirically verified through projective tests (Thematic Apperception Test). He, furthermore, tried to show that this motivation is fostered by certain kinds of child rearing practices. In order to test his hypothesis, he attempted to relate the prevalence of achievement motivation in different populations to economic growth.

This attempt at straightforward "development" of Weber's ideas into a directly applicable technique failed, partly because McClelland's findings on the positive economic effects of achievement motivation were not accepted (Blaug, 1972) and partly because the implementation of his technique – changing child rearing practices – would have been extremely difficult anyway.

Weber's work has become an important part of present day thinking on the problems of modernization through another attempt at the application of his theories in a new context, that of Robert N. Bellah (1957). This was one of the first attempts at the systematic explanation of the question of why the Japanese experiment in industrialization succeeded while others failed. According to Bellah, different strands in Tokugawa religiousness were functional equivalents of the Protestant ethos in the creation of a spirit of economic discipline, thus predisposing significant parts of the Japanese people to the successful adoption of modern ways of government and technology. This was no mere application of Weber's theory to a new case, but also a modification of that theory in three important respects:

- 1) Originally the theory was designed to explain the first emergence, but not the diffusion of modern capitalism; now it has come to be used in the explanation of its diffusion.

2) Confucian and syncretistic Far Eastern sects, the kinds which Weber found to have been incompatible with the spirit of capitalism, were shown to contain elements positively predisposing people to a certain kind of capitalism.

3) The capitalism implanted to Japan was not identical with the Western model; it was subjected to political controls and assumed organizational forms which had been unforeseen by Weber, and perhaps inconsistent with his conception of the phenomenon. This modification of the theory allows to view modern capitalism as a variety of patterns of industrial and governmental organization, consistent with a variety of social ethos rooted in different religious-ideological backgrounds (Eisenstadt, 1973, 11-115, 231 -307). This has not turned Weber's theory into a ready made technology of modernization, but has turned it into a concept of potential practical value which allows for the exploration, and perhaps deliberate engineering, of new types of economic modernization fitted to a variety (but probably still a limited variety) of value systems and ways of life.

This case shows the tortuous and unpredictable ways through which findings of potential practical importance reach the point at which practical application can actually be considered. This is not to say that every case has to be like this one, but it illustrates an important kind of difficulty encountered in the practical application of social theories. In order to understand this difficulty, it will be useful to analyze the case in some detail.

Sociologists want to understand the structure of important phenomena at, or near the point of their emergence. In the case of "capitalism" and other features of "modernity", this occurred in the 19th century, when the "modernization" of Europe took place. As has been pointed out, such understanding was of great practical importance, because the spread of "capitalism" (which is usually a designation for "modernity" as understood in the West, namely a set of loosely interrelated phenomena, such as private economic enterprise, liberal democracy and religious tolerance) gave rise to social disorientation which had to be dealt with. The urgency of the need produced instant theories ("ideologies"), such as utilitarianism, positivism, and the different schools of socialism. Because the phenomenon was still new, and the need for orientation acute, the theories were inevitably short on observation and long on speculation, and, therefore, of limited practical value. When dogmatically applied, they could be outrightly dangerous. Like the application of clinically untested medical theories, the application of untested social theories usually produces results that are greatly inferior to those of a pragmatic trial and error approach. Weber's work on capitalism was a determined effort at breaking the *impasse* of ideological dogmatism through looking at the phenomenon with a degree of detachment and moving the discussion from the emotionally charged context of modernity versus traditionalism represented by warring social groups, to the context of comparative world history. This made possible the discovery of analytical variables, such as religious and economic ethos, and a non-evaluative interpretation of capitalism and modernity in general. "Capitalism" was no longer an inevitable stage in a predetermined evolutionary sequence – "higher" than the preceding and "lower" than the subsequent ones – encompassing all human societies, but a particular concatenation

of conditions which occurred at a given place and time. In principle, this opened the way towards constructive practical thinking about the phenomenon. Whereas the evolutionary theories only allowed such "practical" questions as "what side one has to take in the political and ideological struggle in order to ensure that one will be on the winning side?"; following Weber, one could ask questions, such as "what elements are missing in a situation in order to produce capitalism?" or "how the capitalist phenomenon could be partially modified in order to produce different, but still "modern" social systems?" In practice, these questions were not asked, because modern society was still in its first stages of development, and the phenomenon of modernity was of a single capitalistic type and limited to a small number of societies sharing a common cultural background. Therefore, although the analytical tools for a constructive practical approach to the phenomenon were there, they could not be taken advantage of. It was possible to explain the emergence of capitalism as a peculiar historical phenomenon, but once it emerged it looked as if it had possessed a monolithic structure and an uncontrollable tendency to spread, a view not very different from that of various kinds of evolutionary dialectic. Only with the emergence of successful alternatives to Western capitalism and a great many "failures" in modernization (or one may speak of "successful cases of resistance to modernization") could the theory be modified as done by Bellah, and become an element in the practical thinking on, although perhaps still not in practical policies of, modernization.

In this case the difficulty of practical application of the theory was the result of the complex historical nature of the phenomenon. It was virtually impossible to analyse "capitalism" or "modernity" without some time perspective and the emergence of some variation in the broadly defined phenomenon. Theory could not have created either the time perspective or the variation. Only after these emerged in the pragmatic, trial and error world of everyday life, could theory use them and arrive at formulations which, in their turn, may conceivably be of use in practice. Thus in this case, arriving at a theory of potential practical applications, one had to go in a roundabout way, forget altogether about applications, and be guided only by the cognitive need to break down the complex phenomena of capitalism and modernity in general into logically coherent elements which make possible a rational understanding, and eventually also a degree of practical mastery of the phenomenon. This abandonment of immediate applicability did not eliminate the applied nature of this inquiry. In fact, it was a pre-condition of eventual applicability, since attempts at producing immediately applicable theories, produced only intellectual bias and political propaganda.

The second case, that of school integration illustrates — as has been pointed out — the difficulties involved in the actual application of a potentially applicable theory. Since the 1950's the courts in the United States faced the question whether *de facto* segregation of black and white children constituted an infringement of the right to equal educational opportunity. They decided, partly on the basis of sociological evidence, that it did, and ordered the elimination of such segregation through busing children from district to district, so as to create racially desegregated schools.

The sociological evidence that next to the pupil's own family background, the

strongest influence on his achievement is the family background of his school peers, is convincing, and supports the idea of social and racial integration of schools<sup>2</sup>. Yet desegregation through busing has been a highly controversial arrangement, and is being considered as causing more harm than good even by many people who identify with the cause of racial equality. In any event, follow up studies have failed to show any improvement in the educational performance of black children as a result of desegregation (John, 1975; Gerald & Miller, 1975; Rist, 1979).

There is no inconsistency in these attitudes and findings; the theory that racially mixed classes improve education may be right, but its application may be wrong. Effective application would have required the taking of steps similar to those taken by clinicians before the adoption of a new procedure or medicine. One ought to have found out what kinds and ratios of mixture have the desired effect. For example, a white majority and a black minority might have an entirely different effect than a combination of black majority, with a white minority. The attitudes of teachers and pupils towards integration, and their ability to handle misunderstandings and conflicts which arise in inter-racial contact, are another important condition of success.

One would also have had to consider conditions, such as the costs and benefits of alternative educational inputs, like additional tuition, or better textbooks; and even such details as traffic conditions and weather. Such careful exploration of the conditions of applicability of a new discovery in the natural sciences are undertaken as a matter of course, prior to large scale application. This was not done in the present case, partly because the matter was treated as a political rather than educational issue, but partly because there is no clear understanding of the problems involved in the practical application of sociological knowledge. Thus instead of exploring the conditions of application in advance as necessary development work, the exploration has been done after the adoption of desegregation policies as "evaluation research" (Rist, 1979). The resulting damage has been considerable.

This case shows that "development" is an unavoidable step in the application of sociological knowledge. Research, which produces such knowledge, always deals with a limited number of controlled variables, while practice takes place in an environment of uncontrolled variables the number and characters of which are difficult to predict. Science always has to apply to an entire category of cases, while practical solutions have to take into consideration the peculiarities of every single instance. These can be ascertained only through "development".

While these two cases probably do not exhaust the problem, they clarify two important aspects of the relationship between social research and its application :

1) Applied research deals with problems of practical importance, but its aim is not to produce actual solutions, only to contribute towards them by the discovery of the logical structure of the phenomena of practical interest. In order to do so, applied research has to be systematic and guided by criteria of cognitive validity,

<sup>2</sup> That the norms of white middle class majority may have important beneficial influence on the black minority in a classroom was suggested by the analysis of survey data, see U.S. Commission on Civil Rights, 1969. These findings were well grounded in theories on the way social influence occurs in groups.

which during many parts of the research, may temporarily lead it away from the practical issues.

2) The discovery of the logical structure of phenomena and events which one wants to influence in a practical way, does not provide a prescription of how to proceed in practice. This will always depend on the particulars of the situation which are usually not (and do not have to be) disclosed by research, and professional research workers. Research findings may be crucially important for this “development” work, but research (including applied research) and “development” are conceptually, and as a rule also socially, two distinct processes, not to be confused with each other.

### *What Kind of Ends Can Sociology Be Applied To ?*

Having dealt with the question of how sociological knowledge applicable to practical ends is produced, a few words have to be said about the kinds of practical ends which this knowledge can serve.

The popular expectation from sociology is to produce techniques for the manipulation of people, such as how to increase the effectiveness of advertisement and propoganda; how to “treat” delinquents; or how to “modernize” people who have no desire to become modern.

The assumption behind this kind of use is that people are part of “nature” and sociology is part of “science”, and that the task of science is to create “transitive” theories, which make possible the mastery and manipulation of nature, implying also society, by man. Carried to its logical conclusion, this would mean that those who know and are able to manipulate belong to a different moral and metaphysical plane than those who don’t know and can be manipulated; a horrifying conception of society and social science.

This conception, rarely verbalized, but all too frequently implied in the practice of, and attitudes towards, social research, is not only morally unacceptable, but also inconsistent with the logic of sociological inquiry. Social research, like all research, tries to shed light on puzzling phenomena, or obscure events and situations. Its purpose is to provide a new cognitive map, which, like a geographic map, enables its user to orient himself, or which, like a blueprint, shows how things are constructed and how they work. This knowledge can be used to manipulate things and people. But there is a basic difference between things and people : Things can never know, but people can. In order to use sociological knowledge for the manipulation of people, they have to be kept ignorant.

To be used for manipulation, sociology would have to be turned into the secret love of priveleged groups, and not public knowledge available to everyone who cares and is capable to study it. Under such conditions of secrecy it would soon turn into some kind of non-science, since secrecy would deprive it from the ongoing re-examination and correction by a succession of researchers and critics with changing interests and points of view, which give science its progressive and universalistic character.

Therefore, social research as a public scientific pursuit has limited use as a tool for the manipulation of people. Its proper and effective use is to serve as a cognitive

map, which reveals to people hidden connections and unintended consequences of their actions, and helps them to make better and more responsible choices, and resist manipulation by others. In other words, it is a tool of limited usefulness for oppression, but one of considerable significance for the intelligent and responsible exercise of freedom.<sup>3</sup>

### *The Value Neutrality of Social Research*

The final question to be discussed is that of value neutrality. One of the principle arguments against social sciences is that it is impossible to keep in them value judgements apart from investigation of facts. According to this criticism, results of social research are subjective and have no claim for scientific validity or technical effectiveness.

The classic rebuttal of this view is that of Max Weber. He distinguished between the choice of problems for research and the process of investigation. Problem choice, according to Weber, is always influenced by the values of the investigator. This cannot be otherwise in a field that investigates social phenomena, because the meaning and significance of these are culturally determined.<sup>4</sup>

But the research process which tries to provide the answers to the problems can, and has to be conducted and judged according to strictly universalistic and value free methods and criteria. Therefore, good research will reduce or eliminate the bias implied in the choice of problems. This view had been widely, although not universally, accepted by sociologists, until the late sixties. Since then there has opened a rift between those who still accept, and those who reject it. The rejectionists point of view rests on a mixture of philosophical, and political-moral arguments. They argue that there is nothing in an answer that is not included already in the question. Therefore, if there is bias in the question, there can be no value neutrality either in the process of investigation, or the answer. The logical conclusion of this sort of criticism is that indeed sociology is not distinguishable from ideology, or, perhaps even from religion, and that this fact has to be consciously and programmatically accepted. As a matter of fact, some would argue that natural science is not distinguishable either from ideology and religion, and that the whole endeavor of setting up science as a category of enquiry sharply separated from religious thought is itself just another prejudice, the prejudice of a scientific "religion". Those influenced by Marxism would add that this 'religion' – like the more traditionalistic ones – only serves to protect entrenched interests of ruling classes and imperialist nations (Becker, 1966; Dolby, 1971; Marcuse, 1968, 144-170; Stammer, 1971, 51-78). This latter kind of criticism implied that the idea of value neutrality is not only mistaken, but also a morally reprehensible and sinister distortion of reality.

The rationale of this criticism is that because there are points of overlap between social science (or science in general) on the one hand, and magical, religious

<sup>3</sup> This is similar to the practical function attributed to sociology by Edwards Shils (1961, 1430-32) and Morris Janowitz (1972).

<sup>4</sup> It is evident that Weber's conception of social science is that of an "applied science" in the sense defined in this paper.

and ideological thought on the other, therefore it cannot be asserted that science as objective knowledge is clearly distinguishable from these other kinds of knowledge, which are subjective. This reasoning is based on the fallacious premise, that objectivity and bias are dichotomous phenomena, whereas in fact, they are continuous ones. All systems of belief, including science, are a mixture of bias and objectivity. But the majority of them are institutionalized in a way which is bias-preserving, while science is institutionalized in a bias-reducing way.

Paradoxically, the best evidence for this is provided by some instances usually considered as evidence against the value neutrality of science. There has been lately a growing literature, partly stimulated by the views critical of the value neutrality idea, showing that, indeed, in many cases scientists made judgements and decision out of prejudice, rather than on the intrinsic merits of the case; or that magical and religious belief systems contain important elements of scientific knowledge.<sup>5</sup> Superficially, these findings cast doubt on the value neutrality of science and its demarcability from magic and ideology. However, this is only the case when the distinction between objective and biased is perceived as a dichotomy. Then of course any single instance is enough to contradict the value neutrality view. When the same instances are looked at from a point of view of value neutrality as a continuum, then these very cases become the best evidence of neutrality. They show that bias, even if deeply ingrained and old standing, is always open to correction in science, as one expects from its bias-reducing mechanisms and methods. Far from providing a ground for doubts in the clear demarcability of science from magic, religion and ideology, these discoveries of bias in science through scientific investigation are a striking evidence of such demarcability. Religious, magical and ideological thought would never have led to or tolerated such self-critical disclosures. Thus, far from being contradicted, the distinction of Weber between problem choice which is always biased, and the research process, which is objective, or — as suggested here — bias-reducing, is actually reinforced by the very evidence brought up against it.

Arguments of allegedly systematic bias based on sociological, rather than philosophical grounds can be similarly refuted. Much of the attempt at the exposure of bias in sociology is based on the belief that class and ethnic conflict are systematic and ubiquitous sources of sociological prejudice. However, this belief is contradicted by evidence.

A well-known example of alleged class bias is Talcott Parson's structural functional theory. It has been frequently argued that this theory was but a camouflage for the justification and celebration of the American system of production and government of his times (Dahrendorf, 1958; Lockwood, 1950; Mills, 1959, 44-49; Gouldner, 1979, 167-338). One of the most criticized points in this theory was the postulate of value integration, namely that there were universally shared values cutting across class lines in modern societies, which maintain allegiance to the system. This postulate flatly contradicted the Marxist postulate, according to which all ideo-

<sup>5</sup> For cases in which the scientific community did not observe the norm of value neutrality, see Stephen G. Brush (1974) and I. Mitroff (1974). For the scientific characteristics of magical thought, see R. Horton (1967, 50-71 and 155-87).

logies were rooted in class conflict. Since class and ideological conflict were in Marxist theory the roots of social change, Parsons' idea about value integration was interpreted as ruling class ideology disguising upper class interest in the status quo as shared social values, and preventing thereby the emergence of conflicting class ideologies and social change. However, all this criticism overlooks the fact that the logic of value integration could lead to an interpretation of the class situation in a manner which is in principle no less revolutionary than Marxism. Merton's reformulation of *anomie* theory was in fact such an interpretation, showing how and why certain classes may become alienated from society, because the shared system of values leads to the adoption of the same goals by all people, but does not provide all of them with the means to achieve those goals (Merton, 1957). This shows that whatever the motivation behind the formulation of a question for research (it is conceivable, although impossible to ascertain, that Parsons questions were influenced by his attachment to the *status quo* in the United States), once a theory is formulated, it becomes subject to confrontation with empirical evidence and logical analysis which are independent of the original bias. Thus when Merton tried to apply the idea of value integration to the attitudes and behavior of different classes in the United States, he perceived in it logical implications, which could not conceivably have served the interest of maintaining the *status quo*, and could actually have been used as a justification of subversion (irrespective of any intention of Merton).

A recent paper of James Coleman goes even further, and makes a plausible agreement supported by evidence from his own involvement in government-sponsored policy research — that research results can *legitimate a challenge* to policy, and thus to the authority system that makes the policy (Coleman, 1979). His research was not used by the Office of Education, which actually sponsored it, because its conclusions were mainly negative from the point of view of current government policies. Instead, it came to be widely used by protagonists in a conflict involving educational policy. In fact, he concludes, the very uncertainty of research results makes policy research a very disconcerting tool for policy makers.

A second area of allegedly systematic prejudice is that of ethnic conflict. The argument is that in multi-racial situations dominant ethnic groups usually have a negative prejudice about the low status groups, partly as a legitimation of the oppression and exploitation of the latter. This prejudice is said to be shared by investigators stemming from the high status groups, and to influence their research on the low status group. These investigations are, therefore, deeply imbued with insidious prejudice, and, hence, become a source of prejudice themselves and legitimation of oppression.<sup>6</sup>

<sup>6</sup> For the allegation that there was a correspondence of interests between the colonial rulers and the presentation of the Africans by Western anthropologists, see Gutorm Gjessing (1968); D. Lewis (1973); J. Maquet (1964); B. Magubane (1971). A most extreme statement of the view attributing intended and unintended malice to research done by outsiders in a field related to anthropology is Edward Said (1978). In Sociology the argument against outsiders was raised mainly by some black and women sociologists in the United States. These later have been described, placed in perspective and discussed by Robert R. Merton (1973, 99-136).

Although one has to reject the postulate that all members of a group in which there is a negative stereotype about another group have to share this prejudice, it is true that they frequently do, and that their questions may be influenced by it. But it is not true that the investigations so instigated necessarily or even usually lead to a re-inforcement of the stereotype. There can be little doubt that the work of anthropologists have on the whole reduced the bias against so called primitive and savage people.

The main thrust of this work has been to translate into Western concepts the ideas of primitive thought, and interpret in a universally accessible manner the logic of primitive institutions. Thus, Ruth Benedict's *Patterns of Culture* and Margaret Mead's *Coming of Age in New Guinea*, and *Growing Up in Samoa* brought home to a wide reading public the meaning and integrity of primitive cultures, and compared institutions of primitive peoples with Western ones in a manner which was usually favourable to the former. And the work of Evans Pritchard and Robin Horton have changed the view which regarded African religion and magic as the childish constructs of inferior minds to one which regards them as a sophisticated, and in many ways self-sufficient system of thought (Mead, 1928, 1931; Benedict, 1934; Evans-Pritchard, 1934, Horton, 1967).<sup>7</sup>

In all these cases the investigators were, probably, favorably disposed towards the subjects of their investigations. But the bias-reducing effect of investigation also holds true in cases in which the investigator is motivated by negative prejudice. An extreme example of this is the work on Jews produced in institutes for the study of Judaism established by the Nazi regime in Germany during the 1930's. Spreading prejudice about Jews was the purpose of these institutions, and their very constitution was incompatible with the elementary norms of science. Still, those working in these institutions were, at least in some cases, professionally trained historians, with high standards of scholarship. Even if they were Nazis, they tried to produce work acceptable by those standards. As a result, apart from some invectives and obviously distorted interpretations, their work, in several cases also contains a great deal of good objective scholarship, which can be used by all scholars, including Jews, to this very day.<sup>8</sup>

Of course, there is no justification for invectives in scientific work, and to undertake so called scientific work with the expressed purpose of causing harm to the subjects of the research, is an insult to science. But, if we consider the results, it is obvious that even limited acceptance of scientific standards produced work which was incomparably more objective and intellectually honest than any work produced by Nazi intellectuals who did not care about scholarly standards. This is not to deny the existence of conscious and unconscious group prejudice in many kinds of social research, nor to justify those who do not do their best to control their prejudices. It is only to show that even if the attempt at controlling prejudice is less than com-

<sup>7</sup> For a detailed description of the actual relationships that prevailed between colonial administrators and anthropologists, see Adam Kuper (1973, 123-149).

<sup>8</sup> Some examples of publications of considerable historical value by Nazi Authors are W. Frank (1939); W. Grau (1934).

pletely successful, or even in the deplorable cases of an absence of such effort, disciplined scholarly investigation will usually lead to a reduction of prejudice.

Therefore, the recent fashion of exposing the prejudices of present and past investigators of foreign cultures and societies, and using this as a ground for denying their scientific value, is completely misleading. The critics begin with the disclosure of bias in scientific publications and proceed to attribute the negative stereotypes prevailing in society to the bias in these publications. Actually, of course, the negative stereotype in society is pre-existent to the scientific investigation and is the source of the prejudice of the investigator.<sup>9</sup> Negative prejudice does not stem from scientific research, but either arises spontaneously from conflicts between two groups, or is deliberately created by priests or ideologists. The bias reflected in scholarly writings is usually a reduced and qualified remnant of the popular, dogmatic and ideological stereotype.

Attributing such remnants of prejudice to research is, therefore, erroneous as well as dangerous. The condemnation and suppression of research by outsiders, because of suspected bias, would not be a safeguard against the creation of negative ethnic stereotypes, but would eliminate the probably most effective means for reducing them.<sup>10</sup>

In view of all this it seems that Weber's idea about the possibility of disjunction between bias in problem choice and (relative) objectivity in the research process, stands up well against criticism in all the contexts in which such criticism has been raised. The question is whether there are other contexts unperceived, or unidentified by the critics, which require modification or complementation of the Weber approach. In the next section an attempt will be made to identify such a context.

### *The Cartelization of Research Specialities as a Source of Systematic Bias*

Weber's disjunction between problem choice and research process was based on the assumption that research workers are by and large engaged in and rewarded for the discovery of truth. He was perfectly aware that there were deviants among research workers, but seemed to have thought that this could be dealt with by the usual mechanisms of moral control, namely exposure and exhortation.<sup>11</sup> The possibility that the entire community of scholars in a given field might develop a systematic

<sup>9</sup> However, it has to be emphasized, there is no evidence that outsider bias is unavoidable (except in the trivial sense that every hypothesis is a bias), and – more importantly – that such bias is systematic, so that it has the same character and direction among all members of a given outgroup.

<sup>10</sup> This question is related to, but not identical with the one discussed in Robert R. Merton (1973). Merton deals with the antecedents which give rise to suspicion of outsider bias, and analyses the cognitive contributions of insider and outsider perspectives. My purpose is to show that irrespective of kinds and degrees of bias, as long as there is a scientific community capable of enforcing scholarly standards in a field, there will be a trend towards reduction of bias in the professional literature.

<sup>11</sup> Weber's views on value neutrality are based on philosophical and methodological arguments. However, these are consistent with and perhaps to some extent imply a sociological view of the scientific community of a kind described many years later by Michael Polanyi (1951, 3-90).

bias, without ostensibly deviating from methodological norms did not occur to him. However, such possibilities may arise, and — it seems — have arisen today.

I shall try to illustrate this point through the case of research on the social and economic correlates of advanced education and research since the 1950's. In sociology, much of this research was closely related to the study of social mobility, namely the investigation of the question as to what extent was the social status of people determined by the status of their parents. The hypothesis guiding these studies was that there was a great deal of social injustice and inefficiency due to unequal opportunity for members of different classes, and that the measurement and public exposure of these inequalities would contribute to their correction. It was hypothesised that in this process of status allocation, the educational system played a critically important role, since depending on how education was organised and distributed, it could serve either as a mechanism of perpetuation of existing status differences, or a mechanism of re-distribution of statuses according to purely universalistic standards.<sup>12</sup>

These ideas provided a framework for the comparative study of educational mobility. Rates of mobility were internationally compared, and differences were related to differences in the organization of education as a cause, and differences in occupational and manpower structure as an effect. School systems, such as the American, with a high compulsory school-leaving age, and a stress on electives in both secondary and higher education, were more equitable, and produced an occupational system with a higher fraction of people in professional and technical occupations than countries with more selective and prescriptive systems.

These sociological investigations were logically and politically related to studies in the economics of education. Relative openness of the educational system did not only benefit the individual by providing him with easier access to better occupations, but also benefited the economy as a whole. A famous study by E.F. Denison found that a large part of the economic growth in the United States between 1910-1960 (23 per cent of the annual growth rate) was attributable to the diffusion of education.<sup>12</sup> And there was a great deal of other evidence suggesting a positive relationship between growth of education and knowledge (research, inventions) on the one hand and personal income and economic growth on the other. It seemed that exploration of the growth and structure of education and scientific research would be of central importance in understanding, and eventually providing a key to influencing the mechanisms of economic growth and class formation. From this background emerged a world-wide research effort consisting of comparative studies of educational mobility, educational and research systems, and in economics, of returns to education and the growth of knowledge (Denison, 1962; Vaizey, 1964).

Until the sixties this was a typical case of systematic applied research as the term is used in this paper. The starting point of the investigators was the highly practical concern with social justice and efficiency, but they had no clear idea of

<sup>12</sup> D. Glass (1959) and Natalie Rogoff (1953) were the first major publications on the subject. They served as starting points and models for a still continuing literature on mobility. For a recent publication reflecting the present state of the art, see John H. Goldthorpe (1980).

how and when their work will actually contribute towards these ends. They did not work for governments, or in co-operation with civil servants. The original work of Glass and associates which initiated this line of research was funded by the Nuffield and Rockefeller Foundations, which probably welcomed its implications for policy, but were not themselves implementing any policies.

The circumstances changed in the early sixties. The theoretical framework provided by the studies on educational mobility and related problems seemed to provide a consistent answer to the world-wide search for accelerated economic growth, and in particular to the world-wide desire of catching up with, or at least not to fall far behind, the United States.

This gave rise to an unprecedented situation. Much of the research in the field came to be supported by governments, or intergovernmental agencies, such as OECD or UNESCO, and there developed close personal and institutional ties between civil servants and social scientists. Social scientists provided a doctrine that made sense and gave greater coherence and clearer direction to policies devised by the civil service, and on many occasions actually participated in the formulation of policies, such as the extension of educational opportunity; structural changes in education, like comprehensive schools; elimination of early streaming in schools; or the substitution of departments for "chairs" in the continental European and Japanese Universities.<sup>13</sup>

Intellectually, this co-ordinated effort in the exploration of the relationship between mobility, education, scientific research and economic growth has been one of the most successful research undertakings in the history of social science. Much of the work was of high quality, and original. It involved sociology and economics, was sustained, and coherent. In addition, it had a significant political and social impact. The findings were widely diffused and deeply affected the perception of the educational system and educational policies. Opportunities for entry into secondary and higher education were rapidly expanded.<sup>14</sup> Schools opened up for much greater fractions of the population than ever before. This was accompanied by school reforms on all levels, Curricula were broadened and diversified so as to cater to the abilities and interests of masses of relatively unselected students.

So far, this story may appear as an ideal case of interaction between research and practice. However, about the mid-sixties there began to appear signs, which suggested that there was also an insidious negative side to these developments. There was an accumulation of evidence that the possibility of reducing educational and social inequalities through educational expansion was much more limited than generally assumed. The well-known paper of C. Arnold Anderson, "A Skeptical Note on

<sup>13</sup> Much of the relevant research on the subject during the sixties is summarized in R.H. Halsey et al. (1961); J. Ben-David (1963-64); R. Poignant (1969). For the state of research in economics, see M. Blaug (1972).

<sup>14</sup> Perhaps the best known and most effective example of research in preparation of policy in the field of higher education was the "Robbins Report", see United Kingdom, Committee on Higher Education (1963, Cmnd 2154 and Appendices I-IV, Cmnd 2154 I-IV).

Education and Mobility" casting serious doubt on the effect of education on mobility, appeared as early as 1961 (Halsey et al., 1964, p. 164-179). The Coleman Report of 1966 showed the limitations of formal education in overcoming educational deficiencies due to poor cultural background, and the fact that the relatively high rates of mobility, attributed partly to the educational system, could occur without a significant reduction of the dependence of the status of sons on that of their fathers, was noted by Blau and Duncan in 1967 (Coleman, Campbell et al., 1966; Blau and Duncan, 1967).

The limitations of education and growth of knowledge as economic assets also became clear during the mid-sixties. Derek de Solla Price showed in 1963 that the expansion of science training at the then current rate was impossible to sustain. Denison himself provided evidence in 1967 that his findings about the importance of investment in education in American economic growth was not paralleled in European economic growth rates since the 1950's.

However, while research thus provided growing evidence about the need for a reappraisal of the views about education, science, mobility, and economic growth, and the policies ensuing from such views, there was in actual fact no such re-appraisal. Sociologists and economists did not treat the new findings as requiring a systematic revision of the accepted view on education and mobility, such as was done later by Raymond Boudon, Christopher Jencks and others (Jencks, 1972; Boudon, 1973). Mark Blaug's survey of the economics of education shows that in spite of the inconclusiveness of the evidence, there was a great reluctance to the revision of the hypothesis about the positive effect of education on income as well as economic growth throughout the 1960's.<sup>15</sup>

This reluctance was even more marked in policies. The importance attached to educational (and scientific) expansion for the furtherance of mobility and economic growth remained the guiding principle of governmental and intergovernmental policy during the sixties. Only in the seventies, when economic difficulties made the continuation of expansionist policies impossible, was there a re-appraisal of these policies.

It is difficult to assert with any certainty that this 5-10 years of lag between the discovery of evidence for the need for re-appraisal of educational thinking and policies, and the actual re-appraisal, was the result of systematic bias among research workers and administrators. It may be argued, that it always takes time to conceptualise negative evidence and shape it into a new theory. But my impression from reading the literature and as a participant in this research effort at that time is that there was a kind of bias. The negative evidence was not difficult to conceptualise, and there was considerable awareness of its conceptual implications. But research workers had simply no time to engage in research, the practical implication of which would have been to disclose the limitations of current educational policies. They were

<sup>15</sup> M. Blaug (1972, 61-100); however, the change of emphasis became evident in the seventies, manifesting itself in the renewed salience given to the question of demand and supply for educated manpower, see R.B. Freeman (1971).

deeply involved in “policy research” on such issues as the planning and establishment of new universities, and “centers of excellence”; identifying areas of research in which scientific and technological break-throughs could be expected, and exploring through research, the optimal structures for the support and organisation of research in these fields; or describing, and devising policies for stemming the “brain drain” (migration of scientists and technologists from all over the world to the United States).

Focussing on the systematic implications of the negative findings would have required a disengagement of the social scientists from these ongoing policy concerns, which they themselves partly initiated, and which they still found important and legitimate. After all the new evidence did not show that there was anything amiss with the promotion of education and research, but only that such promotion would not have all the effects originally expected from it. Therefore, postponement of the systematic elaboration of the new findings seemed as quite a justifiable strategy.

This case by no means constitutes a negative evidence for Weber. As assumed by him, research was bias-reducing in this case too, and produced evidence which went against the initial prejudices of the researchers and their supporters. Still the disjunction between the questions rooted in evaluative attitudes, and the answers provided by research performed according to universalistic standards, was not complete. The negative evidence was only registered, but not followed up to its logical conclusions, not because the research community in the field could not overcome its bias, but because it was involved in research supporting practical policies and accorded higher priority to these latter than to the intellectual challenge presented by the new evidence.

This suggests that the logical disjunction between formulating questions and obtaining answers posited by Weber, has some institutional pre-requisites.

The fact that research may lead to results which suggest unanticipated new problems, or actually contradict the initial beliefs and prejudices of the investigators is not enough. The results have to be conceptualised, confronted with existing views, and elaborated into new conceptual frameworks. Weber took it for granted that this would be done “automatically”, since he assumed that taking advantage of new results was part of the *raison d'être* of the scientific community. The present case suggests that under circumstances when an entire community active in a field becomes involved with policy research, this process of reconceptualisation may be delayed by considerations of policy.

This conclusion reinforces the one reached above on the utility of separating systematic applied research from “development” or “policy research” in sociology. There the argument in favor of such separation rested on the recognition that practical applications involve consideration of particular circumstances, and may require intuitive judgements alien to the systematic approach of the scientific investigator. The present case shows that such separation is also necessary to ensure the relative value neutrality of science. This value neutrality is nothing else but the existence of a set of motivations among communities of investigators to evaluate and use research results according to immanently cognitive criteria. If the barriers between scientists and administrators or other practical people are eliminated, these motivations may

be considerably weakened. Paradoxically, this would impair not only the intrinsic scientific quality of research, but also its practical utility.

The practical utility of social research consists not only of finding means to achieve stated social ends, but also of discovering unanticipated consequences and ramifications of policies and other social actions. Systematic social research guided by relatively independent scientific considerations is more likely to disclose these latter, than research directly linked to the execution of programs and policies.

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