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WHO'S AFRAID OF THE HISTORY OF SOCIOLOGY?

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Laszek Kolakowski once pointed out that there was an internal contradiction in the Marxist critique of “utopians”. Good Stalinists, and their academic friends, held that utopian thinking was “politically ineffective”, in contrast to Marxism. But, as Kolakowski noted, “if the alleged utopians were only a group of visionaries weaving a perfect world out of the threads of their fantasies, no one would take the trouble to criticize them. If criticism of utopian doctrines becomes, or is considered, an important social problem, this ... is not a criticism of the social ineffectiveness of utopian thought, but of its effectiveness” (1961, 369). Something similar may be said about the peculiar status of the history of sociology within sociology.

If the history of sociology was of such trifling importance, there would be no need for, or interest in, attacking it. Yet, as Robert Alun Jones has noted in a previous contribution to this discussion, quantitative sociologists in the United States are today quite dismissive of the history of sociology. They regard it as a waste of time, and at the same time regard themselves as the vehicles through which sociology will realize its future. Jones might very well have given a history within American sociology of opposition to the history of sociology, an opposition which is of long-standing, and curiously bitter, pervasive, and associated not only with quantitative sociology but also with the kind of system-building or as Merton put it “systematic” sociological theory of the generation of Merton and Parsons. A recent attempt to create a section for history of sociology within the ASA has run into opposition. And it is of course not just American sociologists who have had an antipathy to the history of sociology, though the antipathies take different forms in different national contexts, and blend or combine with other antipathies.

The History of the History of Sociology

It is striking, especially for Americans, to contrast the treatment of the history of sociology within sociology and the history of psychology within psychology. In psychology, “history” courses are taught routinely. History of psychology is a recognized specialty within the profession, and now has its own official journal within the framework of the journal system of the American Psychological

Association. Psychologists behave, sometimes in peculiar and embarrassing ways, in ways that show that they have a strong sense that their research is history-making, and of the historical basis of their own efforts.¹ Like natural scientists, they are concerned with the history of their field because they believe themselves to be contributing to its continuing development, and are concerned that there be a field of history that exists to recognize and recall these contributions. There are no campaigns against history of psychology with the American Psychological Association, just as there are no campaigns against the history of science as such within science² nor campaigns against the history of politics by statesmen, who wish, as do scientists, to fare well in the eyes of historians who celebrate their deeds.

The difference, at least in the case of American sociology, can be understood in part through history. The role of “history of social thought” in the early teaching of sociology in the United States, especially before the first World War, was very large. Typically a department would have begun its instruction in sociology with two or three courses, one of which was devoted to the precursors and founders of sociology and their ideas, notably their theoretical ideas. A mastery of this material was basic to an education in sociology. Knowing this history meant knowing about a great many explanatory ideas that could be used to explain social life. The list of explanatory ideas included such topics as environmental influences, geography, and a great many other topics that were subsequently excluded from sociology proper. This kind of history of sociology lasted a long time. It is still evident in such works as Barnes and Becker ([1938]1961) and Barnes (1948).

Hostility to history understood in this way was a matter in part of the rise of a new conception of the project of sociology, especially after the second World War. But there were many antecedents. In the twenties there were already expressions of hostility to theory on the part of “empirical” sociologists, and Rockefeller funding for sociology, which was very lavish, was specifically directed away from theory, especially in the United States. In Europe, matters were different, in part because the Rockefeller advisors were concerned with recruiting and supporting potential leaders and with developing the social sciences as academic disciplines rather than with re-directing already existing activity. Some of the personal judgments made by these advisors were very good, and

1 For example, psychologists may name factors they discover with an initial representing a revered researcher in the area, just as statisticians (who are also historically sensitive) name formulae (e. g. Yule’s Q for Quetelet) or scientists name measurements after previous researchers such as Mach and Ohm.

2 Although there has been a campaign against social constructivist sociology of science, which has come to influence the history of science.

very broad-minded. The Stockholm School of Economics, for example, was supported by Rockefeller money, as was Alfred Weber, for a time. And of course Paul Lazarsfeld and Erich Vögelin each received one of the many travel fellowships to the United States.

Merton and Parsons were at the forefront of the attempt to replace a historical approach to theory with a systematic one, and their motives were quite clear. They considered themselves to be the generation that would at last make sociology scientific, and considered the historians of social thought who preceded them to be obstacles to the scientific attitude. In fact, several of them were strong critics of scientization, such as Charles Ellwood and P. A. Sorokin, and, more equivocally, MacIver. The hostility of quantitative American sociologists to the older history of social thought was less intellectualized. Like Henry Ford, they believed that history was bunk.

But the history of sociology did not die, and I would like to explain why it did not and why it will not. Students who received degrees in the early postwar period and who had an interest in the history of social thought, such as Roscoe Hinkle, were told by their advisors that this label would prevent from ever being employed, and that they should redefine themselves as having the specialty of "social change". Through this and other stratagems those with a personal interest in the history of social theory and the history of sociology generally did in fact survive. To be sure they were marginalized, and figures such as Merton and Parsons who were then ascendent did their best to keep the history of sociology from returning to its former status.

History as a Weapon

Most history writing is motivated, at least unconsciously, by present concerns. Sometimes the motivations are quite conscious. Parsons' *The Structure of Social Action* ([1937]1968) was an attempt to write a history of a kind familiar from Hegel, in which the forces of reason in history culminate in the views of the author. Parsons' famous convergence thesis, which held that his predecessors, especially Pareto, Durkheim and Weber, were converging on a single model of action where there was a special and necessary role for normativity, simply got everything backwards. The authors he considered started from a more or less common point, namely the "social" critique of utilitarianism exemplified by Ihering and Spencer. Parsons collected the traces of Ihering and Spencer that remained in various forms in their various critiques, such as the problem of the irreducibility of "the normative" to the useful. He then put these elements together to produce a new view of action, which he then pronounced to be the

point toward which they were converging. It would be more precise to say that they were rejecting this implicit model of action, and proceeding in radically different directions away from it.

The convergence thesis is history with a point. The point is to validate a present view by showing that it follows directly from and is superior to past views. And in a sense this is a very useful kind of history, for it tames the past and assures us that the path from the past to the present that we have actually taken is the only path that could be taken. A different kind of history might make a different point: that the present conventional wisdom is not the only possible outcome of the past, and that much that people in the past thought of perhaps ought not to be excluded today. Put differently, history can make the point that the situation of the present day is a product of decisions that might just as well have been made differently.

History is thus a weapon, or provides the material to make a weapon. In the thirties, precisely at the time where Parsons was writing *The Structure of Social Action*, Ellwood was writing the *History of Social Philosophy* (1938), which made a quite different point: that the conflicts of the thirties between the advocates of planned social intervention and their opponents was deeply rooted in the history of social theory, and exemplified in the American context by the conflict between Lester F. Ward and William Graham Sumner. Ellwood took sides – he was a reformer of the Wardian “interventionist” kind – but presented the conflict as a genuine and fundamental intellectual issue that was central to the public politics of the day as well. Later, in the sixties, Ernest Becker, in a number of books read widely outside of academic sociology, also attempted to revive the heritage of Ward, “the lost science of man” as he called it, by identifying the normative core of this “science” (1971). These texts were meant as a reproach to the scientistic sociology of their times, and as a reminder of the intellectual depth and moral significance of the reformist roots of American sociology.

The power of history as a weapon remains today. The question that needs to be asked is why this weapon is so potent in sociology, and why history is so frequently written with this kind of point in mind, and so infrequently written as a story of continuous advance in the fashion of traditional history of science. Part of the answer is the role of disciplinary “politics” in the history of sociology. Organized intellectual life, such as the life of a discipline like sociology, is the result of collective or political actions, such as the exclusion of certain kinds of writing from standard sociology journals.³ Most of the decisions have indirect

³ A useful discussion of the notion of forsaken alternatives and contingencies is to be found in Camic (1994).

effects, effects that are difficult to “prove”. But the fact that sociology is an organized activity in which decisions are made means that we can at least point to the people involved, understand the decisions, and reflect historically upon them in terms of their consequences. If we can give some grounds for thinking that these decisions were wrong, we are at the same time giving grounds for rejecting the inevitability and the legitimacy of the outcomes of these decisions, such as a discipline with particular cognitive values.

Some of these “collective decisions” are largely notional. Feminist writing on the history of sociology, like feminist writing on the history of science, has concentrated on questions involving the exclusion of women’s voices. In some hypothetical sense, sociology could “have rejected masculine ideals and then been genuinely gender neutral” in 1900. But in a practical historical sense this was beyond the power of any collectivity of the time. Nevertheless, even these very abstract historical counterfactuals have a powerful reflexive significance. If the way sociology is today is in fact the result of the past exclusion of women, this justifies the reconsideration of those features of sociology ordinarily taken for granted (or even value highly) that can be said to be the product of a distant and hypothetical historical fact.

Many other examples might be added to this list. An intense discussion of American sociology and the historical circumstances and accident of its post-war influence over German sociology has been promoted by Friedrich Tenbruck and carried out by his students. The discussion raises an extremely important reflexive question. Does present German sociology have the shape it should have, or is its shape the product of the historical accident of the outcome of the war and the consequent intrusion of an alien idiosyncratic sociological tradition which cloaks itself in an appearance of neutrality and universality? This is a very good question indeed, and it is one which, in some other forms, has motivated a great deal of historical scholarship by Europeans on the effects of north American philanthropy, notably that of the Ford and Rockefeller foundations, on European sociology.

Another area in which the role of history in reflection is of considerable importance is the question of the origins, especially within American sociology, of the dominance of the quantitative ideal, and of the influence of quantitatively oriented elites within sociology. These questions are the occasion of considerable bitterness, a fact to which I can myself attest. My own book with Jonathan Turner, *The Impossible Science*, which detailed the long history of the dependence of quantitative sociology on preferential funding from foundations and other sources, earned the contemptuous derision of the then editor of the *American Sociological Review* in a meeting of the publications committee of the American Sociological Association, so I am told. These were exactly the kinds of books

he didn't like to see published. And why not? One suspects that it is precisely because they call into question the intellectual legitimacy of the disciplinary dominance of quantitative sociologists.

Why is history such a powerful tool for raising these questions? If we want to question the present institutional and disciplinary form of sociology in these countries, it is highly relevant to ask whether these disciplinary features are the product of intellectual merit or of something else. The question of intellectual merit is there already: it is a question we ought to ask ourselves anyway. Historical scholarship provides a means by which we can reflect on the origins and nature of our own standards and intellectual preferences, including the standards and intellectual preferences that we apply in order to make these judgments.

The idea of notional or hypothetical decisions is worth exploring briefly. It applies to any collectivity that makes historical decisions of one sort or another, or that can be thought of as having made or failed to make decisions. In the case of nations the image of decision is especially appropriate because there are actual decisions made by people acting as representatives. The same goes for disciplines, but the realities of disciplinary life are quite different. There is no single sovereign exerting authority. There are no simple cases of representatives making actual decisions on behalf of the discipline with clearly definable historical consequences. Nevertheless there are plenty of decisions, made on behalf and in the name of disciplines, in the name of standards of adequacy, and so forth, which do in the aggregate shape disciplines: decisions to appoint people, to grant degrees, to publish, to fund, and so forth. These individual decisions made on behalf of sociology are enough to make the notion that things "could have been otherwise" meaningful.

Collective reflection then becomes similar in kind to personal biographical reflection. The fact that not only institutional structures but also the very basis for evaluations are in some sense the product of past decisions that could have been otherwise gives the past and its interpretation primacy. It is this primacy, the primacy of cause, that makes historical reflection so powerful, and makes historical scholarship such a tempting path for those who wish to challenge the outcomes of history, or simply to open them up as problematic.

Collective Responsibility

Where there is collective decision, there is collective responsibility, and it is here that the history of sociology has perhaps its greatest importance – as the bad conscience of sociology. Sociology today as an institutional structure which has elevated certain kinds of scholarship and promoted certain kinds of careers and careerism is the product of a century of collective effort. We may reasonably ask what this effort, starting from the founding of the *American Journal of Sociology*, the German Sociological Society, the Social Science Research Council, and so on, has produced.

We are unlikely to agree on the answer to this question. But it can be dealt with “objectively”. An important paper published by the main research analyst of the Institute for Scientific Information, Henry Small, and Diana Crane, a prominent American sociologist of science, shows quite clearly that the discipline of sociology changed radically between the sixties and the eighties. The major change, which they document, is indicated by the bibliometric structure of citations in sociology. Sociology once was a normal discipline, with citation clusters in the center of the discipline. In the eighties, these clusters simply disappeared. Citations went instead to clusters outside of sociology. Sociology ceased to exist as a discipline, at least in the standard bibliometric sense (1992).

The history of sociology is the history of an intellectual adventure that was full of promise. The classic texts of sociology, whatever their failings, pointed to the possibility of a deeper understanding of the social world. Even the critics of sociology acknowledge this. Page Smith, an American academic administrator and historian, wrote a blistering critique of American university education a few years ago. In it he commented that in sociology:

... it is clear that it shares the most acute problems of the other social sciences. It is not a science and is never going to be one. Moreover, it lacks a clear vision of its mission as a less pretentious “study” ... Unlike history, it has no real body of literature to fall back on. After a student has read Max Weber, R. H. Tawney, Emile Durkheim, C. Wright Mills, and a few others, he or she has pretty much exhausted “the literature”.

Page Smith, 1990, p. 232

The cruel accuracy of this judgment is a sign of the real reason why the history of sociology is resented.

The best of sociology *is* in its past. The history of sociology is a continuous reproach to the sociology of the present. The past is an embarrassment precisely

because it is better: its thinkers are more serious and profound, its concerns deeper, and it is far more worthwhile to study. The formal discipline that was created with such effort over the last century has been a fiasco. The people who have responsibility for its present form have good reason to be afraid of its historians. But they resent the conservators of the past when they should examine their own failings.

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