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Autor: Sica, Alan

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THE DIRE NEED FOR HISTORY: AMNESIA AND SOCIOLOGY IN THE U. S.

Alan Sica

Pennsylvania State University

In Response to Other Voices

The five other historians of sociology, whose statements were written before mine, each made important points, and there is remarkably little in their papers which I would contest – even though some of them might well disagree with one another. Like Camic, I was amazed to learn from Hirschhorn that histories of sociology are the rage in France, and students are now expected to read them in the same way that, according to Turner, American psychology graduate students routinely take courses in the history of their field. Hirschhorn's report that Weber and Simmel have lately been rediscovered in France is good news, but I am fairly skeptical, as was Camic, about her concluding argument that studying the history of sociology will somehow help us invent a new form of analysis; possible, but not likely or even necessary as a way to legitimize historical research.

Valade's comments reminded me how, nearly 20 years ago, Giovanni Busino kindly sent a huge volume of Pareto's work in Italian to my office in Kansas, after learning of my interest in resuscitating the latter's reputation among U. S. theorists. Valade believes that sociology's "crisis" ("which one?" we might ask) led scholars back to its origins in an effort to find out what went wrong, and from this archaeology, Condorcet, Tocqueville, Simmel, Weber, Pareto, and others have finally seen the light of day in France. It had been systematically hidden by Durkheimians over the last 70 years or so: "Durkheim and his followers ... deliberately eliminated works and currents not in harmony or at variance with a given orientation". A harsh verdict, but given Durkheim's humorless and dogmatic nature (cf. Jones' various papers), one that seems entirely plausible. Like Valade, I am especially intrigued by Pareto's removal from the field of theoretical play, and wonder when he, too, will come back into his own.

With Jones' remarks about irony we move into a frame of reference that is also close to my heart, and not only because I studied literary criticism with

unusual receptivity as an undergraduate, and continue to read critics like George Steiner, who blend social, cultural, and literary criticism on the same palette. Jones's "resonance" with my own views is also due, I am sure, to his self-removal from a sociology department (also the case with Turner), thus allowing him to evaluate his former academic identity with a healthy disregard for trivial obsessions over "turf". But more profoundly, and borrowed in part from Rorty's and Skinner's arguments about what in an earlier day might have been called "vocabularies of motive", Jones accuses conventional sociologists of lacking epistemological self-reflection and, perhaps more dangerously, a sense of humor. The Social Thought Program which I began at Penn State keys its promotional literature to the phrase "conversation among disciplines", so Jones's invocation of this same idea gladdens the interdisciplinary heart, as well as that of the historian. Yet the very difficulties of sustaining true interdisciplinarity in today's U. S. academic environment seem also to dog the work of sociology's historians.

Moreover, Jones is asking those who have invested considerable effort in becoming social "scientists" (as in National "Science" Foundation) to step outside the professional identity they have struggled to attain, and in the interests of which they have banished and sublimated from their consciousnesses components of humanness, like humor, which are normally taken for granted. When a physicist asks for money from the NSF to study sub-atomic particles, he or she does not admit to epistemological insecurity: a theory, some data, and suitable methods are joined following certain rules and, *voilà*, "science" happens – or so it should under ideal (or idealized) conditions. How could Jones, therefore, be surprised when "a demographer sneered" at his suggestion that history be examined in search of "the contingency and fragility of [a] final vocabulary" of analysis? Learning to sneer at what is not obviously pertinent to method is part of the training that "mainstream" sociologists endure and accept during graduate study. In fact, "learning to sneer at the right moments" might well be used as an "empirical" measure of disciplinary indoctrination. Put another way, to ask sociologists to become ironists – to use history as a way of seeing the temporary significance of any given analytic vocabulary or method – is asking a great deal of people who, by and large, are not in the same league with Weber, Durkheim, Tocqueville, or Condorcet when it comes to interdisciplinary exploration or methodological experimentation.

Camic's remarks cause me throughout to nod in agreement, and then to borrow his tidy bibliography should some deviant student ask me what to read in the history of sociology that has lately been published. His point, though, that historians within sociology departments are what might be called "persons

without a country”¹ is as important as it is troubling for those who insist upon pursuing the past rather than claiming to illuminate the present through “empirical” methods. And note his interesting and accurate comparison between the U. S. and France, showing that despite substantial productivity among historians of sociology in the States over the last decade, neither undergraduates, graduate students, nor non-historically minded sociologists, feel the least need to read this rapidly growing body of work, or to adjust their professional self-estimation accordingly. Nor even, it seems, do colleagues in the historians’ arbor keep up with each other’s work. Put in Weber’s terms, it would seem that the scholars who honorably people Camic’s bibliography are spending their time in “irrational” pursuits, for the “payoff” of their incredible labors is very small indeed.

Finally, Turner’s longer and more elaborate meditation deserves comment. As one of our best-informed and least diplomatic historians, and, like Jones, one who has left a sociology department in favor of another, his viewpoint is somewhat different from those of scholars who stayed behind; he more approximates the “free-floating intellectual” Mannheim recommended for “objective” knowledge. It is fascinating to learn from him that Alfred Weber and Vögelin both used Rockefeller money, and that in the 30s Charles Ellwood (totally forgotten) and Sorokin (barely remembered) wrote books of compelling theoretical argument which Parsons’ first book, and the sociological epistemology it announced, eventually buried. But more important is his theoretical observation that “history is a weapon”, and one which sociologists married to scientistic self-definitions would like to defuse. It is, of course, highly instructive once again to consider the ideological war between Ward and Sumner, or to dust off one’s collection of Ernest Becker’s hits from the 60s, in order to find out what might constitute an “alternative” sociology. But it is indeed risky, at some basic existential level, to learn what “people in the past thought”, as Turner puts it, or to discover that the currently dominant way of doing sociology is but one of many possibilities, and is not “the only path that could [have been] taken”. As Weber often pointed out, people want not only to know they have won, but also that they *deserved* to win. But real history does not serve up these justifications willingly or often.

Turner’s most important reflection, based on many years of historical investigation of both American and German sociology, seconds critical remarks made by the popular historian, Page Smith, in 1990, when the latter argued that

1 Cf. Edward Everett Hale, *The Man Without a Country*, first published in *The Atlantic Monthly* (December, 1863), and as a brochure in 1865, a terrifying story of ostracism with an almost Durkheimian tone regarding the “sanctity” of the collective consciousness, and the penalty deviants pay for violating its immense self-regard.

only sociology's founders had something interesting to say to today's world, and that current sociology amounts to journalism with a bad vocabulary. As Turner himself put it, "The past is an embarrassment precisely because it is better, its thinkers more serious and profound, and its concerns deeper". This, as my demographic colleagues would quickly point out, is "an empirical question", and needs justification before everyone in the ASA can be persuaded to rush to the Grand Canyon in Arizona, and throw themselves into the abyss, lemming-like, because they recognize the futility of their collective endeavor. And in the interests of just such empiricism, I will now turn to "my own stuff".

Some Previously Unrecorded Sociological History

After about five years of effort, the joint desire of Bernard Barber, Stephen Turner, myself, and others will finally be realized, when the official American Sociological Association/National Sociology Archive begins to receive documents in the main library of Penn State University. This is only the most recent of my own efforts to demonstrate that history matters to professional sociologists, both their own discipline's as well as of related areas of inquiry. An early publication of mine showed, through some low-level bibliographic detective work, that Weber and Pareto *must* have known of each other's work, despite their refusal to acknowledge this in their writings, and was published nearly 20 years ago in the first volume of *The Journal of the History of Sociology*.²

In 1983 I was asked to take over the journal myself, as publisher and co-editor, renaming it *History of Sociology*, and, as Camic remembers, it survived only a few years and then went under for lack of funding or a prospective commercial publisher. Neither my university at the time, nor the ASA, nor any other discoverable scholarly entity wanted to pay for such a journal, though subscriptions were rising in number, as was the quality of the published work.³ Articles from the journal continue to be cited and reprinted. I go into this detail not to parade my dedication to history, but to contextualize the following "data", and also to show why pursuing the history of social science is intrinsically worthwhile, and need not *ever* serve any presentist demand for "practical" utility.

2 Alan Sica (1979), Received Wisdom Versus Historical Fact: On the Mutual Awareness of Weber and Pareto. *Journal of the History of Sociology*, 1:2 (Spring), 17–34.

3 More detail can be found about this discouraging, yet worthwhile, experience as a "publisher" in: Alan Sica (1989), On Running a Little Journal, *Perspectives* (ASA Theory Section Newsletter), 12:3 (July), 3–4.

The tale

Within a year or two of moving from Massachusetts to the University of Kansas, probably around 1980, I opened the door of an obscure closet off the main corridor of the sociology department's suite of offices, looking for stationery. On the floor beneath the usual accumulation of detritus sat an old cardboard box of modest size which was not securely closed. In it lay the forgotten remnants of a career. In addition to published papers and books, I discovered a collection of diaries from January 21, 1931 until December 15, 1935, 18 volumes of them, tied together with a frayed crimson ribbon. Each was 120 pages long, neatly written in pencil, dated, sometimes revised, covering thousands of numbered pages. They discoursed in earnest detail, not, as one would expect today, on the writers's love-life or existential anxieties, but rather, about intensely serious questions of *sociological* concern. They were work-books for future research and teaching, the thoughts of a hard-working scholar. (Along with them I found a postcard mailed March 6, 1924 from Unity Church, Wilson M. Backus, Minister, announcing a sermon at 11:00 Sunday entitled "The University and Religion", written in elegant black ink, with "A welcome to all").

Naturally, I was entranced, and before long the afternoon disappeared as I read the private thoughts, delivered in excellent prose, of one Seba Eldridge, born in 1885, died in 1953, long-time professor of sociology at the University of Kansas, series editor for Thomas Crowell Publishers, and social-democrat. Eldridge first worked in New York City in the welfare bureaucracy, publishing "Suggestions for a Social Program for Greater New York, with a Directory of Speakers on Municipal Problems" in 1914 and *Problems of Community Life* the next year, a 180-page monograph.⁴ He took his Ph. D. at Columbia with a dissertation in 1925 that immediately saw print as *The Organization of Life: A revaluation of evidence relative to the primary factors in the activity and evolution of living organisms, including a factorial analysis of human behavior and experience*,⁵ (which in the Penn State Library is catalogued under "evolutionary biology"). In addition to supplementing a poor salary at Kansas by editing many books for the Crowell sociology series (he was able to teach only half-time through this arrangement, which angered some of his less entrepreneurial colleagues), he also published a string of substantial works over the next 25 years. A partial list includes *Political Action: A Naturalistic Interpretation of the Labor Movement in Relation to the State* (1924), 382 pp.;

4 The report was published by the Department of Social Betterment, Brooklyn Bureau of Charities, 20 pp., a copy of which now exists in microfilm at Columbia University library; the book was brought out by Thomas Y. Crowell.

5 Thomas Y. Crowell (1925), New York, 470 pp.

Major Problems of Democracy: A Study of Social Conditions in the United States (1928), with his colleague at Kansas, Carroll Clark, 585 pp.; *The New Citizenship: A Study of American Politics* (1929), 357 pp.; *Public Intelligence: A Study of the Attitudes and Opinions of Voters* (1935), 101 pp.; *New Social Horizons: Designs for a Personality-Centered Culture* (1941), 444 pp.; *Development of Collective Enterprise: Dynamics of an Emergent Economy* (1943), 577 pp.; and *The Dynamics of Social Action* (1952), 119 pp.⁶

In order to prepare a conference paper on Eldridge⁷ (for which there was then, as now, no publishing outlet), I read his dissertation, *The Organization of Life*, the diaries, a few letters held in the University's archives, and interviewed several retired members of the department and their spouses who remembered him in the flesh. As happens with all historians, even amateurs like myself, I soon was swept entirely into another social and intellectual world, one in which sociology was taken literally as a *Beruf* in the strictest Weberian sense. Were I to tell my colleagues and students today who are fascinated by *The Bell Curve* and those many other quasi-intellectual excrescences falling roughly within the socio-biology orbit, that a great many delusions associated with genetic interpretations of human life could be clarified for them if they read Seba Eldridge's Columbia dissertation from 1925, they would surely "sneer" just as knowingly as did Robert Jones' colleague at Illinois not so long ago. And I would not blame them, really, because they have been socialized when to sneer, and become good at it.

What is this sneering about? Surely any intellectual would find the Eldridge diaries intriguing, for the same reason, if nothing else, that biographies are now one of the major staples of commercial publishing. Gossip is gossip, and when prettied up as "authorized biography", few readers can resist it. Even the science-oriented olympian, Robert K. Merton, developed a youthful zest for biography, and read 6,034 of them when preparing to write his dissertation.⁸ But other points of resistance develop. In order, for example, to enter Seba Eldridge's mind circa 1925, one must know something of Columbia University

6 The publishers were as follows: *Political Action* (J. B. Lippincott); *Major Problems* (Century Company); *New Citizenship* (Thomas Y. Crowell); *Public Intelligence* (University of Kansas Journalism School); *New Social Horizons* (Appleton-Century); *Development of Collective Enterprise* (University of Kansas Press), and *Dynamics of Social Action* (Public Affairs Press, DC).

7 "Sociology and Social Life in Kansas: The Case of Seba Eldridge". "What is Midwest Sociology" session, Midwest Sociological Society annual meeting (April, 1981).

8 See Morton M. Hunt's celebrated "Profile" of Merton, "How Does It Come to Be So?", in *The New Yorker* (January 28, 1961), p. 56, and also Merton's reminiscences of his youth in *A Life of Learning* (1994), reprinted in his *On Social Structure and Science* (1996), ed. and introduced by Piotr Sztompka (Heritage of Sociology Series), Chicago: University of Chicago Press, p. 343.

at the time, and the urgency of debates then going on between biologists and social scientists in the nature/nurture wars. One must perform that hermeneutic miracle about which Schleiermacher and Dilthey wrote more insightfully, and practiced more skillfully, than has anyone else: one must enter another's consciousness despite all the historical limitations of place and circumstance in order to "bring it back alive".⁹ This takes – as my five historian colleagues know so well – enormous investments of self, time, concern, intelligence, and professional "capital". The reason we learn to sneer at what we do not know is *not*, for the most part, due to a complex arrangement of competing methodologies or ultimate value commitments, but for the simplest of human dilemmas: "Would love to, but don't have the time".

And although it may seem inelegant and sophomorically unprofound, this, I think, is the most dangerous enemy to the development of historical consciousness among sociologists and their colleagues in related fields. The expected pace of publication, particularly among junior scholars, has been set now at such a high level that they cannot risk spending time reading historical material which may not promise speedy publication. The safer route, to grind precollected data through accepted formulae and handy programs, thus pretending to advance "science", is the road most often taken, as well it should for smart young people who are "rational" (in the well circumscribed meaning of that term as it applies to the academic reward system).

But returning for a brief moment to Pareto: is it not obvious to anyone who has read even a little of his *Trattato* that the excuses we give for our behavior ("derivations") are as limitless as human guile is deep, but that, as Marx argued, the "real foundations" of our lives (for Pareto, the "residues") cannot be escaped for long, but only temporarily concealed? In statistics the unexplained portion of phenomena, the "residual variance", is almost always much larger than what a model can explain using standard rules of inference. Similarly, in Paretian social theory, residues are the unknowable, the noumenal stuff that propels our lives in directions we cannot control. And it is, I would argue, by studying carefully the history of our predecessors in sociology that we are most likely to gain access to this more elusive region of meaningful social action – the place the sneerers cannot find, even if they suspect it exists.

9 See Alan Sica (1981), *Hermeneutics and Social Theory: The Contemporary Conversation. Current Perspectives in Social Theory*, 2, 39–54; and the two bibliographies in Gary Shapiro and Alan Sica (1984; 1988, pb. ed.), *Hermeneutics: Questions and Prospects*, Amherst: University of Massachusetts Press.

Author's address:

Alan Sica
Department of Sociology
Penn State University
201 Oswald Tower
University Park PA 16802 USA