

Postscript 2009: Corrections and Second Thoughts

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“My department [or school] was actually a lot more important than you acknowledge.” This was the most common complaint voiced to me about my intellectual and political history of the Michigan faculty, 1938-1988. But I hasten to emphasize that most reactions to this lecture were non-defensive and wonderfully generous. I was relieved not to have provoked a greater number of complaints, even of this predictable variety. Doing full justice to every program over a 50-year time-span was beyond me, but I did the best I could on the basis of studies of the history of the various disciplines and of conventional indices such as academy memberships and national rankings by reputable agencies. Yet the point of this postscript is to correct the errors I did make, to confront the most important of the criticisms I received, and to make some observations more easily voiced now than two decades ago.

I do wish I had mentioned Philip Converse’s article of 1964, “The Nature of Belief Systems in Mass Publics,” as published in David Apter, ed., *Ideology and Discontent* (New York, 1964), 206-261. A prominent theme in my discussion of Michigan’s achievements in social science was the overwhelmingly empirical character of these contributions, in contrast with the more theoretical contributions at some other universities, especially Columbia. But Converse’s essay was a major contribution of exactly the sort I had in mind, and it was all the more important because its theoretical

arguments were firmly grounded in “Michigan style” empirical work, notably the most famous mid-century product of Michigan social science, *The American Voter* (to which I do devote attention in my lecture), of which Converse was a co-author. Converse’s essay of 1964 remains a pivotal work. More than forty years after its publication it is not only cited by political sociologists, but in semi-popular writings. It was the centerpiece in 2004 of Louis Menand’s *New Yorker* (August 30, 2004) commentary on the electorate in 21st century American presidential elections. Menand developed Converse’s central point that most people don’t care much about politics and don’t have much in the way of political ideas. Converse himself did not complain that I had failed to mention his famous article, but Howard Shuman and colleagues of Converse’s complained, and they were right to do so.

Elizabeth Douvan was one of several Michigan colleagues who cautioned me about something else: I may have exaggerated the security and standing of Angus Campbell at the time of the hearings he chaired that eventuated in the termination of mathematician Chandler Davis during the McCarthy Era. Campbell’s stiffness in dealing with Davis might have derived, in part, from his eagerness to protect the Institute of Social Research, which in 1954 had not yet attained the standing that I may have retrospectively attributed to it. This is a speculative point, and while I will not push it here I do want to acknowledge the plausibility of the criticism.

Davis himself, while not actually contesting my account of his termination and of the dynamics of McCarthyism on the Michigan campus, told me that the purging of leftists from the Michigan faculty was largely pre-determined by the structure of politics at the time. My attention to the details of the debates within the Smith Committee and the

Campbell Committee implies that these deliberations mattered, and that more than one outcome was possible. Davis doubts this. I am not persuaded that Davis is right, but I want to acknowledge his demurral from my interpretation and to refer readers to Davis's own discussion of this episode in "The Purge," in *A Century of Mathematics in America*, Part I (American Mathematical Society, 1988), 413-428.

Of the various complaints that I had underestimated the achievements of this or that department or school, the most sustained and response-worthy were offered by William McKeachie and James Gindin in relation to the departments of psychology and English, respectively. In 1990 I deposited in the Michigan Historical Collections both of these critiques, and my letters responding in detail, so that others can consult these exchanges if they wish. I did this in the interests of fairness, but ultimately I was not persuaded by either of them, nor am I now upon rereading them after two decades. My lecture gave extensive recognition to the role played by psychologists in Michigan's rise to national leadership in the social sciences. I believe McKeachie has no substantive quarrel with me. Gindin's complaints focused more on the value of standard indicators, such as National Research Council rankings of graduate departments. I did rely on these rankings, extensively. Gindin may be correct that these indicators are less important than I implied. But Gindin's criticisms are pertinent to many units other than the Department of English.

Several correspondents cautioned that anti-Semitism and anti-black racism were more widespread at Michigan during the early decades of my study than I had allowed. The evidence presented to me to support this complaint, however, had more to do with student life than with the faculty-centered aspects of the campus, which were my focus.

Perhaps I should have pointed out that only three years before the beginning date of my study President Ruthven had kept a black player out of the lineup of a football game in order to please a visiting team from George Tech. Four students who protested this action were later expelled by Ruthven, who in a letter described the four (all Jewish) as belonging “to one particular persuasion.” The details of this incident and the documentation for it were called to my attention by Robert Cohen of New York University.

Yet in regard to anti-Semitism within the faculty, there is more to say. In the key case of Lawrence Klein, I came across evidence that I was unable to use on account of a restriction that has since expired. In a then-restricted section of the transcript of Marjorie Brazer’s 1979 interview with Gardner Ackley, Ackley described in some detail the statements made to him by William Paton, whose successful opposition to Klein is central to my discussion of the McCarthy Era at Michigan. I cited (endnote 89) William Haber’s belief that Paton’s opposition to Klein was anti-Semitic in character, a charge contested by Paton’s son, William A. Paton, Jr., who came to see me after my lecture was published in *Rackham Reports*. But the portion of the Ackley interview I was unable to quote actually supports Haber’s belief, and undercuts the younger Paton’s defense of his father. Ackley declared in 1979 that Paton told him at the time of the quarrel over Klein that the effort to keep Klein at Michigan was part of a conspiracy to enable Jews to take over the Economics Department. The same box in the Michigan Historical Collections cited in endnote 59 and 89, containing Brazer’s interviews with Ackley and Haber, also contains a transcript of Brazer’s 1979 interview with George Katona in which Katona

insisted that the tape recorder be turned off when he was answering Brazer's questions about Paton and Klein.

By far the most substantive critique of my lecture was offered the very evening of its delivery by Converse himself, and was published in *Rackham Reports* along with my lecture and the commentaries of several other colleagues. I did not then respond to Converse's critique, but this postscript provides a welcome opportunity to do so. Converse, amid generous observations about my lecture, took issue with the terms in which I asserted that "mainstream professionalism" characterized Michigan in contrast to other leading universities of the period, especially Columbia. I called attention to Columbia's greater number of faculty who wrote important theoretical vindications of social science, and who wrote successfully and well for a large public beyond the particular disciplines in which they had been trained and in which they practiced. I attributed to Michigan a distinction that took place, by and large, within a frame of "suspicion of grand theory and of epistemological quibbling, a preference for concrete and clearly manageable projects, a penchant for technical methodological refinements, and above all, attention to aspects of the social sciences and humanities least likely to be mistaken for political advocacy, cultural criticism, or journalism."

Converse declares that "the road not taken" of "public intellectualism" leaves him "with neither regrets nor apologies." Travelers on that road, he observes, "tend to rely on personal values and moralizing rather than on any evidentiary base, either ignoring the need for the latter or exercising stringent selection of facts to bolster the argument." Converse allows that he came out of this "bellevue" background, but abandoned it because he felt it was "pure ephemera." Converse is welcome to his own opinions, but I

am eager to remind readers that the Columbia examples I named as worthy of respect were Richard Hofstadter, Lionel Trilling, Jacques Barzun, Daniel Bell, Charles Frankel, Henry Steele Commager, John Herman Randall, Jr., Robert S. Lynd, Gilbert Highet, I. I. Rabi, Moses Hadas, and Meyer Shapiro.

Did the widely appreciated works of these Columbia scholars constitute “pure ephemera”? Perhaps. But to impose such a precious standard on these individuals would surely carry with it an obligation to impose on practitioners of empirically rigorous social science a standard by which a number of books and articles in that domain would be found to amount to very little. Triviality and ephemera are widely distributed among the varieties of academe, and by no means confined to the more interpretive side of the social sciences and humanities. Converse may have no regrets that Michigan between 1938 and 1988 produced so few historians like Hofstadter, so few literary scholars like Trilling, and so few sociologists like Bell, but I, and many others, have been enriched by reading the works of such scholars.

Converse is also uncomfortable with my contrast between Michigan’s social scientists and the Columbia theorists whom I credit with endowing “their generation of American intellectuals with a language in which to talk about empirical research” (Robert K. Merton, Ernest Nagel, Paul Lazarsfeld, and David Truman) and in which to criticize the limits of such research (C. Wright Mills). Here, Converse makes what at first appears to be a strong point, which is a chronological distinction between the period from “the late 1930s to the mid 1950s,” when Columbia was at its most creative, and the “somewhat later” period of ISR’s major contributions. Hence, in Converse’s view, the

differences between the two campuses follow not from local campus cultures, but from the “orderly succession of stages in the development of modern social science research.”

I see little evidentiary basis for Converse’s claim that social science research developed in such orderly stages. Converse seems to believe that ISR’s era of greatness is to be understood in relation to a period-specific “large and self-conscious movement” attempting to transform “social studies” into “something more worthy of the name, ‘social sciences’,” but in fact this movement was well launched at the turn of the twentieth century. Countless carefully documented monographs on the history of American social science abundantly warrant this. The 1950s and 1960s indeed witnessed a prominent episode in this movement, but the scientific aspirations of that period were not nearly as distinctive as Converse implies.

But even if we were to accept Converse’s claim about chronology, the claim begs the question. Why should it have been Columbia, rather than Michigan, that produced the leading works of the earlier period, and Michigan, rather than Columbia, those of the later period? I see no reason to revise my insistence that during the full half-century under review, the “marks on the world of social science” made by Michigan faculty “as theorists and critics were not nearly so deep as those they made as practitioners.” I am eager here to repeat one of my most important claims: “...there issued from Michigan no theoretical works of the stature of Merton’s *Social Theory and Social Structure* and Nagel’s *The Structure of Science*, no manifestos for social research as widely quoted as Lazarsfeld’s papers, no theoretical synthesis of behaviorist political science as influential as David Truman’s *The Governmental Process*, nor any critiques of the whole enterprise comparable in bite and in influence to Mills’s *The Sociological Imagination*.” The

closest thing to an exception is Converse's own paper of 1964, to which I have referred at the opening of this postscript.

Converse's most convincing argument about the issues taken up in my lecture is his methodological clarification of the National Research Council's *Assessment of Research-Doctorate Programs in the United States*, on which I relied for many points, including my observation that Michigan had excelled even more decisively in the quantity of publications than in their quality. Converse, who was among the designers of the NRC study, offers a number of caveats that do indeed enable one to read that document more discerningly than I did. Converse persuades me that I was mistaken to hint that Michigan was turning out too much stuff that was less than first rate. I take it back. But it is a compelling confirmation of my argument about the special strengths of Michigan social science that this methodological refinement should emerge as the most telling of Converse's criticisms of my lecture.

I want also to acknowledge the existence of a half-dozen minor factual errors that I have taken this opportunity to correct (e.g., the name of the Canadian university to which Mark Nickerson went when he left Michigan) in the text as reprinted here. Also, I have slightly revised endnote number 97 concerning Michigan faculty who were Fellows at the Center for Advanced Study in the Behavioral Sciences. The information that I had collected and reported on the high representation of Michigan social scientists among Center Fellows was correct, but I implied that my discovery was new and that the Center had sought to conceal the heavy representation of several universities among its Fellows, when in fact, I later learned, the Center itself had published a report in 1982 providing the same information. Otherwise, the text of the lecture here is just as published in *Rackham*

Reports in 1989, and as reprinted in my own book of 1996, *Science, Jews, and Secular Culture: Studies in Mid-Twentieth Century American Intellectual History* (Princeton University Press).

Finally, a disclaimer and a note of thanks. The disclaimer is that I here make no claim whatsoever about academic culture at Michigan since 1988. The pattern of strengths that I identified for an earlier period may or may not have much relation to the way the University of Michigan has developed in the last two decades. I left Michigan in 1992, and have followed its workings only occasionally since my departure. And that brings me to my note of gratitude. I was very pleased to be a member of the faculty at Michigan for fifteen years. And I am honored now to have my study of a major phase of that university's history reprinted. I am especially grateful to Francis X. Blouin of the Michigan Historical Collections for facilitating this reprinting.

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