

5-24-2009

At the End of That First Year or So I Would Say That I Became as Close to Erving as Anyone Else in the Sociology Department

Neil Smelser
University of California - Berkeley

Follow this and additional works at: https://digitalscholarship.unlv.edu/goffman_archives



Part of the [Politics and Social Change Commons](#), and the [Social Psychology and Interaction Commons](#)

Repository Citation

Smelser, N. (2009). At the End of That First Year or So I Would Say That I Became as Close to Erving as Anyone Else in the Sociology Department. In Dmitri N. Shalin, *Bios Sociologicus: The Erving Goffman Archives* 1-10.

Available at: https://digitalscholarship.unlv.edu/goffman_archives/65

This Correspondence is protected by copyright and/or related rights. It has been brought to you by Digital Scholarship@UNLV with permission from the rights-holder(s). You are free to use this Correspondence in any way that is permitted by the copyright and related rights legislation that applies to your use. For other uses you need to obtain permission from the rights-holder(s) directly, unless additional rights are indicated by a Creative Commons license in the record and/or on the work itself.

This Correspondence has been accepted for inclusion in Bios Sociologicus: The Erving Goffman Archives by an authorized administrator of Digital Scholarship@UNLV. For more information, please contact digitalscholarship@unlv.edu.

Remembering Erving Goffman and Herbert Blumer

Neil Smelser:

At the End of That First Year or So I Would Say That I Became as Close to Erving as Anyone Else in the Sociology Department

Dr. Neil Smelser, professor emeritus of sociology at the University of California Berkley, wrote these memoirs at the request of Dmitri Shalin and gave his permission to post them in the Erving Goffman Archives.

[Posted 05-24-09]

May 24, 2009

Dear Dimitri Shalin -

I have drawn up some recollections of the years of my friendship with Erving Goffman and attach them for your archives, because as far as I know there is some material in my notes that is not produced elsewhere. The material is attached.

Thanks for giving me the opportunity to reflect on Erving after so many years.

Neil Smelser

I first met Erving Goffman at an informal departmental lunch in the fall of 1958, when I first came to Berkeley as an assistant professor of sociology. We sat next to one another and talked for most of the time. This first interchange was not a pleasant occasion. Erving immediately went on the offensive, making one aggressive remark after another about Harvard (from where I had just come), Talcott Parsons (with whom I had co-authored a volume and who was my principal thesis supervisor), and other topics on which he apparently thought I was vulnerable. It was clear he had heard about my coming to Berkeley and he had heard a few essential facts about me. His comments were sharp but rather stereotyped. I was annoyed at this behavior, but I didn't rise to the bait and let the insults roll off my back in that initial meeting.

In subsequent meetings his aggressive style continued. I had heard in the meantime that this was a general type of come-on for Erving, and I also witnessed similar assaults on others. I began to counter this style by asking him why he was continuing this kind of baiting, and on some occasions I returned fire. I don't know whether my style had anything to do with it, but over time, during that first year of our acquaintance his style began to mellow—and, as a result, mine did, too—and we began to progress toward a better relationship, first a more civil one, and moving toward one of honest, frank, and respectful intellectual discussions our one another's work, of social theory, and then to the development of an outright friendship. At the end of that first year or so I would say that I became as close to Erving as anyone else in the sociology department. My sense was that Harold Wilensky and I became his two best friends. In that new context we came to be able to criticize one another without playing games or bringing any complex baggage to our conversations. I remember on one occasion I asked Erving—in connection with his intended study of gambling in the Las Vegas casinos—why he seemed to have gone out of his way to select such an “odd”, far-out setting and suggested that he would get much more intellectual mileage and more serious attention if he turned his approach to the stock market and other more legitimate institutions. My questions set off, not a sharp exchange as might be expected with Erving, but a serious intellectual discussion of the selection of research sites and how sociological research could be made more credible or less credible. For his entire remaining time at Berkeley, Erving and I remained good friends. We did so afterwards as well, though I saw him only rarely after he went to Pennsylvania. About a year before his death he and Gillian came to Berkeley for a prolonged visit and lived only a few houses away from us; we saw one another several times during that visit, and it was especially gratifying to renew our relationship one more time before his death. I always felt it a major tragedy that Erving was not able to deliver his Presidential Address at the American Sociological Association a few months after that.

I mention two special moments to signify how close we became in the first five or six years of our acquaintance. In 1963, when my first marriage was dissolving in a sea of conflict and unhappiness,

Erving volunteered to testify as a witness on my behalf in what was looming as a bitter court fight over property and child custody; I did not ask him to do so. He knew my first wife, and I think he possessed some incriminating facts about her, though he was very straightforward with me as to what he was willing to say and what he was not willing to say in court. As it turned out, there was not a court fight; the two attorneys engineered a settlement that avoided that unpleasantness. But I was forever grateful to Erving for his kindness and support. The second involved his wife. In befriending Erving I also befriended Schuyler (Skye) as well. She was a very troubled person, and his marriage was a troubled one. In particular, she went into some kind of psychological tailspin after the assassination of John Kennedy in November of 1963. That in turn drifted into a kind of hyper-manic stage, in which she developed a fix on the idea that she, using the money in her family, could, with the help with a number of us (myself included), launch into some kind of world-saving enterprise. I did not let myself get drawn into her scheme, which I regarded as hopelessly unrealistic, but I maintained a friendly and supportive relationship to her and Erving throughout. When that mania turned into deep depression, it was not long before her psychological state propelled her into that dramatic suicide leap off the Richmond Bridge (I never drive over that bridge without remembering that moment). After the event a number of those closest to Irving (Fred Davis from San Diego, David Schneider from Chicago, Hal Wilensky and a few others I cannot remember) gathered and stayed at Erving's house continuously for a couple of days. I took on the special (unplanned) role of spending long periods of time with their son, Tom, during those days. It is also notable that Erving opened up to me about the problems of his marriage during that unhappy time, and I know that this kind of intimacy was rare for Erving. I have never been able to understand how Erving and I became close; we had very different personalities and personal styles; we were very different kinds of sociologists; we had different cultural backgrounds (I came from Protestant background and grew up in Phoenix, Arizona); and we had different conceptions of human nature. Despite this lack of initial bases for mutual understanding and friendship, I consider the relationship to be one of the truly gratifying ones of my life. I

always felt that Erving did not look down on me (except at the beginning) and I did not look down on him.

For a year or two Erving and I were in a poker group with a number of other individuals—Irving Piliavin, Henry Miller, Bill Kornhauser, Hal Wilensky, David Matza, for a while Ernest Becker, and a couple of others. We played every two weeks. Erving turned out to be a very poor poker player. Most of the time he lost money in our friendly game. An ironic twist was that he also turned out to be very unimpressive as an impression-manager. He was far from being a poker-face. I used to joke that if he were dealt as much as a pair of deuces his hands would begin to tremble and his face would begin to flush. Given his work and his pride in his insights about the manipulation of human situations, one would have expected Erving to be a Mr. Cool, a good bluffer, and a good strategist. He displayed none of these characteristics, and I suspect that his notable lack of success led to his relatively short tenure as member of the group.

I always regarded Erving as a truly brilliant man and a wonderful sociologist, deserving all of the awards and reputation he enjoyed during his lifetime and after his death. On a few occasions I assigned his work in my graduate theory course and analyzed it in my lectures. I noted central themes in his work, such as a very remote variety of rational choice theory—with the actor treated as a rational one, given the master principle of the protection and projection of self-image. I also noted a remote paranoid streak in the writing, evident in his treatment of the actor as navigating in the midst of others' manipulative and phony behaviors. Erving's brilliance, however, was his very own, and not transferable to others. He had very few graduate students at Berkeley, none of whom matched him in intellectual ability or quality. Students were somewhat afraid of him, in my recollection, because he did not like teaching very much and could be impatient and downputting. Erving was an average citizen with respect to his commitment to and participation in the Department of Sociology. He came across as something of a cynic.

It so happened that I sat next to Erving in the very historic massive meeting of the Academic Senate on December 8, 1964, following the massive sit-in in Sproul Hall by the Free Speech Movement. It was at that meeting that the faculty voted, by something like an 8-1 margin, to urge liberalization of the rules regarding political activity on the Berkeley campus (thereby siding with the student activists and repudiating the Chancellor, Edward Strong, who subsequently was eased from office by the Board of Regents). Erving and I voted with the majority. During the whole meeting Erving carried on a running commentary on the meeting and its speeches and arguments, largely poking fun at the proceedings and treating the meeting as something as a drama of the absurd. That kind of demeanor was what earned him that reputation as a cynic and a loner. That reputation was confirmed in an incident at the meeting of the American Sociological Association, at which Erving won the Sorokin prize, I think for his book on The Presentation of Self. Though he was in the convention hotel at the time, he would not show up for the presentation.

Charles Glock has written about the circumstances of Erving's departure for Penn. I was often with Erving (along with Hal Wilensky) during the weeks before his decision to leave. We were both very dismayed at the prospect of his leaving, and worked as hard as we could to present arguments and reasons for why he should stay at Berkeley. But the situation became a tough one for everybody involved. Erving was putting a demand on the Department that he be permitted to teach half-time but at full salary. Such arrangements were not unheard of at Berkeley, but in all cases the faculty member had to seek relief by securing external research support and thus "buying off" his time or by securing a research appointment in an organized research unit (I myself was subsequently given a permanent half-time salary on state funds by the Institute of International Studies and remained on a half-time research appointment for the remainder of my active career; Bob Bellah also got a similar appointment on Ford Foundation funds). But Erving would have none of that kind of understanding; he wanted the half-time arrangement as an outright gift. This put the Department in an almost impossible position. When Hal and I heard about this demand, we continued to urge Erving to stay at

Berkeley, but we both agreed that this particular demand was unreasonable, unfair to his colleagues, and impossible; furthermore, we expressed this opinion to him directly. Erving would not budge from his position, and his departure was thereby almost guaranteed. I was extremely disappointed with his leaving, but I have never questioned the rightness of the position that Hal and I (and ultimately the Department) took.

May 25, 2009

Dear Dimitri -

I told you yesterday that I was uncertain about writing anything down about Herb Blumer because my relationship with him was remote. However, your request set me thinking (and I suppose I was in a memoir mood after writing about Erving), so I seized the moment and put down my recollections. They are attached. The piece is about the same length as the one on Erving, but is probably less revealing because there was less of a relationship there. At the same time, I suppose most of what is going into the Blumer recollections is of a more positive and intimate nature--I know he had many admirers and grateful students--so my piece may at least have some uniqueness.

Onward and upward,

Neil

[Posted 05-25-09]

Memorandum
Herbert Blumer
Submitted by Neil Smelser

The first time I met Herbert Blumer was when I visited the University of California, Berkeley in the winter of 1957, when I first came to the campus for job interviews. I was just finishing my doctoral dissertation at Harvard and had come on to the job

market. I had not been offered the job as yet, but I was confident, both because the market for young sociologists was very strong at the time, and Talcott Parsons, my thesis supervisor (and with whom I had completed co-authoring Economy and Society) had given me good press to the Berkeley campus. Ultimately I received an offer from Berkeley (the day after my visit) and competing offers at the Assistant Professor level at Michigan and Harvard.

As chair of the department, Blumer had me into his office during the one-and-one-half day visit. He was quite formal and distant, officially describing the position and asking me some questions about my work, but revealing little else about Berkeley or about himself (all I really knew about him at the time of the visit was that I had read his work on collective behavior; his work on symbolic interactionism was not really in the intellectual culture of Harvard during my graduate-school years). Several other faculty members—especially Marty Lipset, but also Reinhard Bendix, Hannan Selvin, and Leo Lowenthal—were actively welcoming and extremely sociable (Lowenthal even told me during the first day of the visit that I was surely going to be offered the job). I attributed Blumer's aloofness to the fact that he was officially the chair and that, because I hadn't actually been offered the job. But it also struck me as odd that, on the day after the visit when I was visiting the Center for Advanced Study in the Behavioral Sciences (Stanford), it was not Blumer but Lipset who telephoned me and said the job was mine if I wanted it. (It was a comical phone call, because I talked to Lipset on the phone in the presence of Talcott Parsons [my mentor and a Fellow at the Center at the time] who was standing beside me and whispering urgently again and again, "Don't say yes!", because he knew a Harvard position was going to be offered to me). I accepted the Berkeley offer a few weeks later, but in keeping with my reading of my visit, I informed Lipset, not Blumer of my decision, and nobody asked me to inform Blumer).

When I arrived at Berkeley the following fall, Bendix had just come into the chairmanship, so all official arrangements were handled through him. I didn't really see much of Blumer during the first year, and he didn't seek me out at all. During the second year, however, I took the initiative in setting up a meeting with him. We

had a lunch at the faculty club at my invitation. The reason for this was that I had been working very hard since I had come to Berkeley on what would become Theory of Collective Behavior. I know that Blumer was one of the figures in that field, and I wanted feedback on chapters that I was drafting on the panic, the craze, and the hostile outburst. I asked Herb if he would read one of the chapters (on panic, I believe) and give me feedback and he agreed. We arranged to have another lunch a couple of weeks later. During that lunch Herb obviously showed that he had read the material. He gave no overall judgment—either encouraging or discouraging—but seemed to land on one major point, and he repeated that point again and again in different ways. The essence of his objection was that I was giving too much emphasis to structural determinants of the panic process and too little attention to the perceptions and meanings and interpretations of the actors involved. This point was of course consistent with his symbolic-interactionist perspective, and also consistent with his ongoing critique of Parsons and other functionalists who, he believed, regarded the actor as the passive vessel through which structural forces passed and determine the actor's behavior. I heard Blumer and I think his message led me to think somewhat differently about the kinds of interpretations I was developing, but in a way his objection, if I had taken it seriously, would have undermined my entire theoretical perspective and would have led me to write an entirely different book. So I can say that I responded only partially—and, I suppose, minimally in Herb's opinion—to his critical approach.

A month or so later I sent him a copy of the next chapter on crazes, asking him for feedback on that one, too. This time we did not have a lunch. Instead, he wrote me a very long letter (six or seven single-spaced pages, as I recall). In this letter he developed the very same line of objections as he had put forward in the lunch, and the letter was as repetitive as the luncheon conversation had been. I wrote an equally long letter back to him, explaining and defending my position and implicitly criticizing his. I still remained undaunted, and in the end sent him almost every chapter of the book. He responded to all of them by letter, but after a while the correspondence became boring, because his points were always the

same, and my responses tended to be the same, too. There must have accumulated almost fifty pages of correspondence. I gave him credit in the Preface of the book, but in reading those words recently, I think I was more generous than I really felt.

Herb and I served together on a number of orals examinations for graduate students, I examining usually in social theory, he in social psychology. I didn't like his style very much, and was embarrassed by it. He would always ask the same questions, focusing on George Herbert Mead's theory of personal interaction. He would ask the students to reproduce Mead's point of view, and if the students didn't use the right words, he would continue to ask until the student would get it right. I felt it was demeaning to the students, and more about Herb's insistence on things that in revealing the students' command of material and analytic abilities.

We also appeared together on panels at scholarly conferences and ASA meetings. I remember one line of exchange that was especially striking. I ventured a theoretical critique of the symbolic interactionist approach—I thought it was civil enough—namely that with the insistence on the idiosyncrasy of the meanings that guided actors' behaviors—the approach (a variant of phenomenology) was caught in a position of not being able to generalize but only to tell descriptive stories, and for that reason found itself difficult to measure up to the scientific canon of seeking generalizations about human behavior. Herb reacted very strongly to this point, because he prided himself on the empiricist characteristic of the symbolic-interactionist approach ("you have to dig for the facts" was his phrase), and resented any suggestion that it was non-scientific or anti-scientific.

Between 1962 and 1965 I was editor-in-chief of the American Sociological Review. It was a rewarding experience, but one of the more difficult side issues was that quite a number of Berkeley colleagues sent me manuscripts, thinking, perhaps, that this younger colleague (though I had been promoted to Full Professor in 1962) would be an easier touch than another editor. Herb was one of those who contacted me. He submitted a manuscript on economic development, which he had prepared in the course of an

academic visitorship in Brazil. The essence of his argument was that development was a pragmatic, seeking process, and difficult to generalize about. I sent the ms. out for review, and the reviews were generally negative. I had to reject the manuscript and tried to do so diplomatically, but Herb was quite gruff about the decision, attributing it to reviewers' (and presumably my) intellectual rigidity. That decision didn't help our relationship at all, though the situation didn't develop into any kind of fight.

I seldom saw Herb socially at parties and other kinds of gatherings. When we did meet, we were always civil but distant from one another, finding it difficult to find things to talk about. I often wondered why we never really broke through the aloofness. After all, we had a lot in common: we were both Missouri boys (I was born in northern Missouri, and though I grew up in Phoenix, Arizona, my family and I visited the state numerous times, and I felt Missouri roots); we were of common ethnic stock; we both were sports fans. But we never came to the point of having a personal relationship, much less a friendship. I felt he was shy, certainly more so than I, because I did develop collegial and friendly relationships with others. At one point I even entertained the implausible thought that Herb might be afraid of me, because that seemed to be consistent with his behavior; but it was implausible because he was twice my age and was certainly an established scholar and sociologist. In the end I suppose it was because of a theoretical impasse. Herb regarded me as hopelessly wrong-headed and beyond his influence (he once introduced me to an academic in what I suppose he thought were flattering terms, saying that Talcott Parsons regarded me as his best student; he couldn't give his own opinion but quoted that of a theoretical foe). I regarded him as hopelessly dogmatic. We couldn't get beyond that impasse, and what might have developed as an intellectual and even a personal relationship never had a chance to do so.