An Interview with Erving Goffman, 1980

by Jef C. VERHOEVEN[1]

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[After a short presentation of JV's background, the interview starts]

JV: …. I am studying symbolic interactionism and for that reason, I started to make contact with the third generation of people trained at Chicago, and some of the fourth generation in Berkeley.

But your situation is a little bit difficult for me. I thought that it was important for me to visit you. Because you see in a lot of books, an indication - Let's put it this way: they mention you as a symbolic interactionist. But for me the problem is, do you see yourself as a symbolic interactionist?

EG: Well, if I said I didn't it would depend on your understanding of my feeling about the label. If people insist on using the label like that. I guess I'm as much what you call a symbolic interactionist as anyone else. But I'm also a structural functionalist in the traditional sense, so if I can't answer that question it's because I don't believe the label really covers anything.

I don't know what your feelings are on this, but mine have been, coming from Chicago, that there was the tradition of George Herbert Mead to provide the social psychological underpinnings or background for any study. From there one could go in all kinds of directions one of which is the one [Everett] Hughes developed: a sort of occupational sociology and basically urban ethnography. And what I did up to a few years ago before I got somewhat more interested in sociolinguistics was a version of urban ethnography with Meadian social psychology. But that Meadian social psychology with a social psychological underpinning for a large amount of the work in American sociology and

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could, sort of, be taken for granted as just part of basic sociology.

So, I've never felt that a label was necessary. If I had to be labelled at all it would have been as a Hughesian urban ethnographer. And what happened about, I suppose it was, six or seven years ago, was a movement in sociology for persons to classify themselves. On the social psychological side it was probably stimulated as a response to ethnomethodologists, who labelled themselves. They were in the social psychological side, I suppose, the first group that oriented to a label that excluded and included. I always felt that the reduction of the term, symbolic interactionism, as a label for some sort of group was a response of people to tendencies in sociology to fracture and fragment and for some of the persons in the fragments to make a "club" of their profession. So I've never treated the label very seriously. I don't think it applies very much.

Now we have in American sociology a handful of persons who are professional social psychologists from the sociological side, like Anselm Strauss to a degree. Although lots of his stuff was later influenced by Hughes and he also became an occupational sociologist. A pure case is Gregory Stone at Minnesota. Another one is [Bernard] Meltzer. Both of these individuals had an interest in social psychology as a specialty. Now I think they would have the right to call themselves symbolic interactionists if they wanted to - if that was their concern. But the people who ordinarily label themselves symbolic interactionists, who are so labelled, are persons much like myself, like Fred Davis, Howie Becker, people like that. They are basically Hughesian sociologists who employ a quite general Meadian frame of reference that everybody of that period employed.

Expanding the group to include those sorts of persons makes of the name something that doesn't signify too much. It would be more accurate to call them sociologists of small scale entities like occupations, things like that, with a Hughesian, qualitative, ethnographic perspective. So if we had to choose a label, Hughesian sociology would be more accurate one than symbolic interactionism. But it was all one group in terms of friendship links and origins at Chicago and that sort of thing.

JV: Were there scientific links ?

EG: Oh yes, they were all graduate students.

JV: At the same time?

EG: Same time. There was a huge graduate student group in the late 40s at Chicago. They all got

or, at least, a section of them got Hughes and [Herbert] Blumer. Blumer was not around too much because he was employed in the government as a conciliator, a labor arbitrator. Hughes wasn't there all the time. Part of this goes back to the history of Chicago, and various things that happened to that generation of students.

[Joseph] Gusfield was another example. Elliot Friedson was another. All of these persons would call themselves, or would be called, symbolic interactionists. But that would be a very allusive term for them than could otherwise be found. On the other hand, persons like Gregory Prentice Stone, to a degree Anselm Strauss, and Nelson [Foot], they took social psychology as their substantive domain. If they wanted to call themselves symbolic interactionists they could.

The name was resurrected out of a footnote from Blumer, seven or eight years ago.

JV: Now, sometimes your work is called dramaturgical because you give an important place to the dramatic movement in society. Is that a label you prefer, or is it just a term given by other people?

EG: That came along partly in response to the labelling. You know, [Thomas] Kuhn [1970] wrote this book on the organization of science and then a character came along and wrote a book, [Nickolas] Mullins [1973], on schools of sociology that got to be influential. Well if you come to an understanding of all this from that, from now as opposed to before, then you come to think of these things as real. But if you look at historically, if you are involved in the history of it over the last thirty years, then it becomes apparent, I think, that labelling of sociologists, breaking them up into categories, the formation of sections in the national meetings, and that sort of thing, is a response, not necessarily to the nature of the field but to other kinds of social circumstances. Its only about 8 or 9 years old; so considering the length of time I have been in the business, I can't take any of that labelling very seriously.

The term dramaturgy I can't take all that seriously. If one comes to think from the outside, say like [Alvin] Gouldner [1970] did with the history of American sociology, you get one view. If you come to it as somebody who has lived through it, you get quite a different view. I think it’s going to be very hard and increasingly so for an outsider to get any picture, other than the sorts of pictures which are generated by picture makers. Which you have helped to do when you make your picture.

And that's gets to be the reality of the history of the profession. That’s fine, providing somebody hasn't been around when the history was occurring. But it doesn't provide a very satisfactory version of it for those who were involved with it. I feel.

JV: Now if I may go back to the point you mentioned a time ago about the influences. I have the impression that Blumer was not of big importance to you?

EG: Well, I actually didn't take - he wasn't around much when I was there. Hughes was much more of an influence and Hughes came later also. He wasn't around all the time I was at Chicago. But I found Blumer's [1969b] writings very congenial. And his early papers criticizing the lack of concepts, certain kinds of methodology and the like, I found very sympathetic. And I was a colleague of his for a decade. So there was that kind of influence but basically my sociology is a much more traditional kind. It wasn't social psychological in character. I came to write social psychology because early on I wrote papers without having research projects. At the time I began my career professors got big grants and students worked for them. And I tried to do it without grants, so my first couple of papers were just sort of, papers I wrote off the top of my head. Well, it's easier to write social psychology that way than it is other things I have found. So my work got to have that social psychological slant, but it was itself rooted in the circumstances of its production.

The dramaturgy was partly just a name people applied. Burke, Kenneth Burke, was an influence in somewhat the same way. Louis Wirth, at the time we were all students in Chicago, felt that *Permanence and Change* [Burke, 1935/1954] was the most important book in social psychology. So we all read that and that was a real influence on all of us I think. Burke's later work somewhat less so. But then there was interactive process - one looks around when writing one's stuff for reference for authentication, authority, and the like, and so one dips into things that one might affiliate oneself with. My main influences were [Loyd] Warner and [Â.R.] Radcliffe-Brown, [Emile] Durkheim and Hughes. Maybe [Max] Weber.

JV: Was there a place for [William] Thomas and [Florian] Znaniecki?

EG: Well, Thomas and Znaniecki, we all read them but it was really kind of vulgar social organization. While Znaniecki's [1934] book on methodology was a very fine little effort, paralleling [Talcott] Parsons in many ways. The big thing, *The Polish Peasant* [Thomas & Znaniecki, 1919-1921/1958], was an influence more on Blumer than myself and Blumer's [1939] critique of it was a thing we all read, of course. So there's that kind of influence. But not Thomas and Znaniecki so much as Robert Park. He was sort of the founder of the whole Hughesian tradition. He had a lot of influence on us I think. My teachers were Park, Burgess and Louis Wirth. And then later on Everett

Hughes, but the person I worked for initially, was Lloyd Warner. I was oriented to social anthropology at the time. The woman I was living with was a social anthropologist.

So Radcliffe Brown was central to my interest and concern - and a model for writing papers - more than any sociologist.

JV: So you call yourself more of a sociologist than a social psychologist than traditionally someone who is a symbolic interactionist?

EG: Yes. Outside of that I don't see that any name applies. I've never belonged to a section, in the [American Sociological] Association. You know there are sections on theory and all that. I've never belonged to any of those. I can see that there should be substantive distinctions in the field, and divisions, but once you pick a domain I think that partly determines what your method should be. And so, while I'm in favor of a substantive division of the discipline, I'm not much in favor of a methodological one.

JV: Now there is in your work, for instance in *Frame Analysis* [1974]. On page 13 you say at a certain moment that "society to be first in every way and any individual's current involvement to be second…" But in other places in your work you point immediately to the individual as a central point, the main way to come to society. Can I say that in your approach the individual is the most important starting point for sociologists?

INTERRUPTION FOR PHONE CALL

EG: What an individual says he does, or what he likes that he does, has very little bearing very often to what he actually does. It seems to me that you can't get a picture of anyone's work by asking them what they do, or by reading explicit statements in their texts about what they do. Because that's by and large all doctrine and ideology. You have to get it by doing a literary kind of analysis of the corpus of their work. I don't think it's worthwhile to do that, for a sociologist by and large, I don't think it merits that kind of effort. But it's an effort in its right. It's my feeling, anyway; that if you just take person's versions of what they do, you will end up with a very superficial view of what goes on and furthermore, you will then be contributing a statement that itself will act as a barrier to anyone else finding out what goes on.

So I'm perfectly prepared to give my view on these matters but I would like to caution you that

there is no reason for taking them seriously. My ideological view is that what I'm doing is the structural social psychology that is required, or is natural for, sociology. That is, given sociology is a central thrust, what can we say about the individual? Not that the individual is the central unit that permits the study of society but if you take society as the basic and substantive unit you can still ask yourself the question - given social organization as the central reality - what is it about individuals, what is it we have to assume about individuals so that they can be used, or be usable socially? That's the kind of sociology that I do. Now it ends me up in what looks like social psychology because I am always looking at the individual. I'm always talking about the individual because I am an ethnographer of small entities, and I don't work in terms of large grants employing anybody. I've never employed anyone in my life. So I very often end up talking about the individual, but by that I don't mean to raise the individual into the first position. It's just my piece of the whole - the whole being social organization. That's the central thing.

JV: Can I say it this way? You work through the individual as a representative of what's going on?

EG: No. I don't think that as a representative at all. Because very often individuals have very partial and narrow roles to play in the whole. I don't think you can go from the individual to society. Given society, society has got to make use of individuals or constitute individuals in such a fashion that social organization can be sustained. That doesn't mean to say that the way they are constituted is going to reflect society in any direct, or holistic fashion. That's what I say explicitly. What I actually do when I write things is another question. I don't know that, really. Somebody else has got to analyze that as you would a literary text. In that line of endeavor, what other people have said won't do you any good but only do you harm. There isn't an easy way to do this work and I'm not sure it's worthwhile doing it in a realistic and responsible fashion. That's why I encourage your efforts.

JV: Right. Another point I want to mention is that when I go through your work - I have not yet read all of your work.

EG: I'm sorry there are so many.

JV: Yes. The impression I have is that when you are looking to reality, you see reality as a construction by mankind. If I may say it this way, in *Frame Analysis* [1974], you speak about keying, framing, fabrication and different other things. May I say that social reality is not a given reality but a

product of man. May I put it that way?

EG: Well. If you did you would have to sort of... frame it properly. Because there has come to be a lot of interest in what is called the social construction of reality. Well, sociologists in some way have always believed in the social construction of reality. The issue is, at what level is the reality constructed. Is it individual? A small group? Or somehow the amorphous cross working of overall social processes that no one really knows too much about. I believe, of course, that the social environment is largely socially constructed, although I am sure there are some biological matters which have to be taken into consideration.

But where I differ from social constructionists is that I don't think the individual himself or herself does much of the constructing. He rather comes to a world, already in some sense or other, established. So there I would differ from persons who use in their writing the social construction of reality. I am therefore on that side, closer to the structural functionalists. Like Parsons or [Robert] Merton. Just as they were closer to initial functionalist anthropology.

I'm a cultural relativist in that I believe societies can differ in almost every way. Knowing about one society one has to be very, very careful indeed to generalize from there to another one. That's a generational feature. These days lots of people are less culturally relativistic than I am.

JV: Do you mean that the different stances that you take in your books are more in relation to the American society than to the European?

EG: Well, given the sorts of things that I end up studying. Face to face interaction and the like. Given those things the line can't really be very clearly drawn between American society and any of the European. It's rather a difference between Western societies and others. But that line has not been closely studied very much in any sort of technical way. It's very hard to say just what the universal application of any of one's findings are. There are some very marked regional differences within the United States but there are lots of things that transcend the English language as you well know. There are quite common - you can manage yourself on the street here with gestures and signs which will be understood quite perfectly by persons who don't know that if you talk to them they will hear an accent. So the dimensions or extensions of our gestural language, our gestural behavior, has not been mapped much. There is one book which has to do with some gestures recently by Desmond Morris and his colleagues [Morris, Collett, & Marsh, 1979]. But that's about the only one. We know something about language boundaries - that too is a flimsy sort of thing in many respects.

So while I've got no grounds for giving the - and that's one of the weaknesses of the things I do - while I've got no grounds for describing the perimeters, the outside boundaries of the conduct I talk about – it's usually broader than a small section of the middle class in America. I hedge my bets usually by saying that I'm only talking about that group. But that's just a verbal device. No one really knows the boundaries of these things. That's one of the problems of dealing with this kind of work. Or I suppose any sort of sociology.

JV: Sometimes you brought in information from Europe, from England.

EG: Yes. I've never had any experience of an alien culture. I wish I had. Although it's getting a little late in life to do so. That's, I think, a weakness in what I do.

JV: You said a moment ago that the influence on your work perhaps comes more from persons like Durkheim?

EG: Well and social anthropology generally.

JV: And also you mentioned Parsons?

EG: Parsons on the - again you know, it's hard to take yourself back to the periods when one was subject to influence. These realities change so much but Parsons [1937/1968] provided, in the *Structure of Social Action*, at the time, the first statement that it was reasonable to be thinking in theoretical terms which I'm sure we have come to think too much of. Secondly, he provided something of an epistemology that I've always found congenial and reasonable. I'm a positivist basically, I guess. The epistemological realism espoused in that volume I found congenial and it provided the best statement of Weber and Durkheim available in the English language. In those days there weren't many translations. Having access to these simple texts was a significant thing. It's very hard for a European to understand circumstances where there would be a few professors who had access to the language and none of their students. Those were the circumstances I went to school in.

JV: That was true of your generation. When I compare that with this generation of younger scholars, I have the impression that knowledge of other languages is rather bad. Your generation is more familiar I thought with..

EG: No. Not at all. This generation has more access because there are more translations available. In my time at Chicago *The elementary forms* [Durkheim, 1912/1965] was available, *Suicide* [Durkheim, 1897/1951] wasn't.] *The Rules of Sociological Method* [Durkheim, 1895/1982 wasn't. A small bit of Weber [1904-1905/1976], *The Protestant Ethic* was available. A few articles of [Georg] Simmel had been translated by Park and Hughes [1921/1969, pp. 348-361, 553], and that was about the extent of it. Then gradually partly with the introduction of the Free Press, translations of some things, gradually this material became available. But there were very few, in my generation, who could read in any effective sense German or French. Probably there is more of that. Now certainly they are more conversant in French, German. But with the war that sort of put a damper on things for a while. But the group who are in sociology has increased; it's very large, three times I suppose what it was then or more. By virtue of that there are now persons who have command of the languages, so there are always a few people around that work in that fashion. There's enough in translation.

JV: So I may say that Durkheim's view of science interested you. You said also that you are a positivist? How must I see that. Must I see that in the straight logical positivistic stance? Or must I see it in the trend of critical rationalism?

EG: Well, I suppose that one can work towards a value-free social science. Or that's a realistic ideal to have. That one can be wrong in one's findings. That one statement can be closer, or more accurate, more valid than another and that things have a structure. Now, I'm not certain of any of that - but that's my operating hypothesis.

INTERRUPTION BY TELEPHONE

EG: Objective experience is a simple part of some domains of sociology such as the one I'm in. It doesn't seem to me that those subjective experiences are any less factual than anything else in the world. I've never found cause to have to change those kind of fundamental beliefs in science even though there has been a great deal of legitimate criticism of ethnography as such. And it's very difficult these days to do ethnography because one has become so critical of any sentence or utterance that one makes. I don't believe it's possible perhaps to provide an overall picture of anything but I believe it's possible to catch aspects of things and touching on aspects of things and we are doing no

more or less than people in the hard sciences do. Some sociologists think we have closer access to our facts. Some less so. Some people argue that these facts become part of conscious life and they are always subject to change, whereas the facts of the physical world aren't. But I don't think that's always critical.

So I'm old fashioned, conservative and unsophisticated in my epistemology. I have no strong belief in what I espouse. I claim it's a reasonable operating hypothesis and if people take a different view it ends them up in other kinds of problems. So even though what I do could be called symbolic interactionist and the like, it's still done from the conventional, conservative traditional perspective on believing that one could maybe not have a science of society but maybe come closer to it than persons who are less instructed, and that some concepts may be more valid than others, and that concepts' validity depend not merely on some practical use that one wants to put it to, but upon the state of the field at the time, the character of the behavior and that sort of thing.

So I don't take a radical, evaluational, subjectivist view. I'm not an ethnomethodologist by any means.

JV: When I look to the kind of reasoning in your work, can I formulate it that way, that you have particular idea, it can be hypothesis, and you look at society through different examples to find a confirmation of this particular an hypothesis. So I mean that your reasoning, is a reasoning of inductive thinking. Is that a right description?

INTERRUPTION BY TELEPHONE

JV: In relationship to your positivistic stance my question was if -.

EG: Oh yes. I guess I would go along with that except I wouldn't use the term hypothesis. I think that's rather optimistic. Mostly we have concepts, leads, but ... generalizations that relate to concepts, to each other, in analytical law in the manner of the hard sciences, I don't see this, and I don't think we have too much of it. So when people use the language of the hard sciences like hypothesis and the like, I get to be a little restive. I think that's mostly done by persons who are trained in literary undertaking and have too much sense of what the hard sciences do.. It seems to me we are in a primitive - my area of sociology is in a more primitive state. We are just trying to get reasonable classifications, one or two useful concepts, ways of touching on and describing processes and practices and my approach as you say, is largely inductive. I don't think we have anything like a

theory of anything yet. Mostly in literature what one gets is scientistic talk and very little relationship to science. So I'm not embarrassed at all by the crude and primitive character of my work. I try to think of concepts that I employ, as much an anybody thinks about the concepts they employ. I try to draw the lot whether they survive after being thought about, or tested, or applied, or used. But I think it's very much an exploratory, tentative undertaking. If we have low expectations about our achievement we can act with more confidence and assurance in what we do than if we think we are developing theories and hypotheses, then I think we are kidding ourselves. I'm not embarrassed by the humbleness of our product.

I would be were it institutionalized more. That is whether we should have control of very many resources is another question. I don't know how much teaching we should be doing. Teaching at the lower levels get to be pretty doctrinaire. Doctrine inculcating activity. But I suppose it isn't us inculcating, it will be some other group. So my worry about sociology is not the limits of what it has produced and I think it's very gravely limited indeed. But rather the claims, the institutional claims that are made for it.

JV: Now you said that we have to be self-confident and we have to be humble to the aims sociology can reach. In positivistic thinking, looking to the philosophy of science, there is a movement at present, I think, to go to a kind of unified science. So there is a fundamental view that doing research, looking to experiences must give at the end the possibility to formulate a unified science. My question is, do you agree with that stance of this positivistic thinking?

I have the feeling you will say that it's impossible for sociology. We have not to worry about it.

EG: Well, it really isn't a concern of mine. It seems to me so far afield. So alien to what we have accomplished so far that I just don't think along those lines. The issue is that when we make a small generalization or even a description of something around us, can we say that we are doing other than arbitrarily extracting one little piece that interests us - the interest of which we don't explicate much or become self-conscious of - then the whole process consists of just ideology, or doctrine. That would be, I would take it, the anti-positivistic position. We can make some generalized descriptions, of elements, or components of behavior. If we try to make a picture of the whole then we do end up making an arbitrary selection of the features to talk about. So I have very limited expectations, but I approach those expectations as a naturalist would. If you can do that as well as a botanist can or anybody else who has a classificatory science.

JV: Some time ago speaking about - you used the words conservative, positivistic stance - you said I believe in fact that sociology can be done from a value-free stance. Can you explain that a little bit more? What's your interpretation of this value-free stance, because there are a lot of interpretations possible.

EG: Well, I guess, I just follow the traditional, early Weberian one that one can see something about one's political and social life and do something about overcoming that, in a limited sort of way, and that persons of slightly different social backgrounds and political commitments can still, nonetheless, come to some degree of agreement about an array of social facts. And, that that is an ideal and a goal that we can aim for. Now there are lots of problems with that position. On the other hand, the other position has lots of problems too, because the other position fails to come up with a theory of good proof, of what constitutes a valid statement. And then there is the reflexive issue of why put weight on an argument when the argument is no argument, is other than political bias; then what's ... the bias of that argument? You know, the old circular issue. So, I don't think that any of these basic arguments are foolproof. I pick the one that - with all its limits and prejudices and biases, with a full understanding that what we deal with is just an aspect of things somewhat arbitrarily selected, that we don't provide grounds for our selection of the part we study - I still believe, that given what one studies, one can come up with something that wasn't in one's head but was in the data, within limits. That it isn't just a creation of the student. It is partly that but only partly that. I believe there is some social order and organization in the data that is accessible to us. Otherwise there wouldn't be much reason to continue in the business except as a livelihood. It would be just a question of who could paint a picture that would sell. Maybe to some degree it comes to be that but I don't ..., I think in some sense most sociologists espouse that position. That's their working hypothesis - whatever they say they believe, in fact, that's - their conduct presupposes that one can move toward something like an objective study of society, some elements of society some features of society.

For example, social class is very little studied in this country and the argument is its very little studied because of social reasons, which is probably true. But that doesn't mean to say that things that are studied...

BEGINNING TAPE LABELLED GOFFMAN - PART 2.

[EG started questioning JV about his opinion of symbolic interactionism and the ideas of the scholars he had spoken with. JV's reply ended as follows.]

JV: … what is called symbolic interactionism puts a lot of people together who were perhaps once symbolic interactionists. They had been trained in Chicago, perhaps that's one of the connections you mentioned also in the beginning. But apart from that, you have a lot of different stances, different approaches.

EG: You know there is a buried issue behind all this. You know the real issue was not symbolic interactionism and the other things; the real division up to the time when labelling began, labelling of categories in sociology began, Was the difference between persons who took - who studied things quantitatively and those who studied things qualitatively. And behind that was the persons who were interested in ethnography and persons who were interested in the sorts of studies that you would use an instrument for, of another kind, like a questionnaire. That to me was the major breakdown. These schools of thought now obscure what was a big political split in sociology. The persons who are symbolic interactionists, especially the Chicago ones, are by and large on the qualitative or ethnographic side. But a more accurate description would be to call them social ethnographers. That's really, in some sense, what they share. And then, Blumer doesn't qualify there - he was never interested in ethnography, never engaged in any of that undertaking. He wrote essays that were of a very general, and abstract kind. You could put persons like Gregory Prentice Stone in that group. Strauss bridges the two, he did both ethnography and pure social psychology. Blumer's social psychology is such, while it seems to me, it's all - goes in the right direction relative to the other available ways of doing sociology with questionnaires and that sort of thing, and his critique of that sort of thing was part of the basis of a lot of our orientation. But when he got down to doing work himself, he didn't end up studying anything, in effect. So the pure symbolic interactionist position is without substance. It doesn't take a domain for analysis.

Now, there were a few things that were done. One was Orin Klapp [1962]. You ought to see him, if you haven't, or at least read his work. He wrote a book on heroes and things like that. He was a Blumerian in the central sense and he did have something of a substantive area. And that was an illustration of where you could go if you wanted to get substantive. Another area was what was called collective behavior. Collective outbursts of various kinds. And these persons were centrally influenced by Blumer [1969a] and his early statement of social movements is very fine, I thought, conceptual analysis. It's very good study but very broadly absorbed into sociology.

So, that sort of thing led in some social psychological directions: collective behavior, things like the study of heroes and the like, a few things like that. But most of us are involved in the study of

occupations or their social organization into institutions and establishments.

JV: More influences of Hughes

EG: Yes. yes.

JV: I see. What was the reason for the foundation of the Society for the Study of Symbolic Interaction?

EG: I think it was persons, in my analysis anyway, they were being crowded by other persons joining organizations, especially the ethnomethodologists. So since these persons were already organized - this was a response, a defensive reaction. There was already a group which included all of these people. The sociological study, or whatever it was called, of social problems. Social problems was the first, one of the first anyway, if not the first named sort of sections in the [Ameican Sociological Association]. They have a journal, Social Problems, and they have their meeting a day or so before the national one. So the same people go. Now all the persons who call themselves symbolic interactionists were involved centrally in the society - it was called the Society for the Study of Social Problems. Again I was never a member of that organization. With their own journal. It's more central in some ways to symbolic interactionism than the actual organization of symbolic interactionists.

JV: But I have the feeling that some of the people just joined that organization. Some explanations are this way: I joined this organization to support young people. So, for me, I am just trying to find out what the real meaning is, of that society.

EG: Well, part of it was to honor Blumer and Hughes. It was done at a meeting at San Francisco and we had a great time at the first meeting and all that.

Blumer was alone for a long while as a critical voice against quantitative questionnaire type of sociology. I myself, believe that there is nothing wrong at all in counting bodies, or houses, but that any study involving attitudes is likely to be worthless. So I myself have never been against quantification, but only against the use of quantification linked to instruments that purported to get at subjective experiences. Well that stance was largely Blumerian, in that he was the one who provided early on in the late 50s and 60s the clearest statements of the shortcomings of quantitative sociology.

There were, of course, other ways and other sources of that kind of criticism.

One was social anthropology, later a second one got to be ethomethodology like Aaron Cicourel [1964]. So that message was sent from different places at different times. But for lots of sociologists Blumer was the one identified with it. That work he did was very fine, very critical, very solid and his own conceptual work was - say social movements- was great but that occurred like 30 years ago. So it's pretty much in the past. There was then a revival of Blumer's writings and his papers came out and that sort of thing. Those papers were then read by persons like yourself and others whose business it was to talk about sociology.

Well, that's a newish sort of thing. So you have got to unwrap, and see the various ways that activity was involved. Criticism that Blumer initially raised against quantification, that Hughes sustained in his substantive work, that we all got involved was one thing. Then quite later on, the ideology of looking back to a few selective published sources as the ground and basis for a definition, a way of labelling oneself was a later sort of thing and I don't put too much stock on that later thing.

[JV observed that at Chicago there were more views on the matter than Blumer's. EG commented on specific names mentioned and then continued.]

EG: … When I was at Chicago in the 40s one could still combine lots of different things. Ecology and social organization uhh, class analysis with Warner and the like. But later on when Columbia took over and got to be the dominant university - It got to be dominant through [Paul] Lazersfeld. Lazerfeldian methodology got to be the central thrust in American sociology . A good part of Chicago went along with that, and then Chicago broke up into different kind of factions: persons who wouldn't touch the quantitative side, and persons who wouldn't touch the qualitative side. In the mid 40s, however, everybody did everything. Everybody read all the articles in the journals and one took courses across the board and, one didn't draw those sorts of lines. They came later on with the introduction of large research grants (in the early 50s, and it continued to go on with the Lazerfeldian kind of sociology and then Chicago got to be more and more quantitative in character as it is today. But there were always some people who were social organizational in orientation like [Morris] Janowitz and the like who was trained at Chicago.

But Everett Hughes who stands for ethnographic sociology himself used numbers whenever he had the chance. If you look at his book on *French Canada in Transition* [1943], it's got such quantitative data as he can bring to bear. So that line wasn't very sharp. Blumer has never used any numbers I don't think. I never have because I'm lazy but I'm not ideologically disposed against them.

I am only disposed against the use of those instruments attached to inventories of so-called, subjective states. There, I think, it's a waste of time.

JV: What is in your opinion, the weakest point in the symbolic interactionist - or let's say the Blumerian approach?

EG: Well it stands as a good corrective to the excesses of quantitative sociology. But, in itself, it doesn't provide you with the structure of organization for the substantive area you are studying. It is "anti' system, that is "anti" any systemic kind of finding. It seems to me. In that sense it's a very sort of, merely critical, primitive approach, and in that sense I don't think it goes anywhere. I don't see how it can satisfy you unless you set yourself up in the business of being a critic of sociology, of other kinds of sociology. And being an analyst of what other persons have said in sociology, and there are persons like that. I suppose for that it's fine. But in the last analysis when you get down to studying something, well, you are interested in showing that there is some sort of organization, or structure to it. Otherwise you haven't found anything presumably.

Well, there is nothing in symbolic interactionism which suggests there is going to be a structure or an organization to anything. The ideal for lots of us is the kinship system in a pre-literate society which is a functional whole. Well, one doesn't expect to find that in contemporary society but one expects to be able to say something about the processes. As soon as you relate and identify a couple of processes, or try to relate them, or provide a systematic picture of something, you have to pull away from the very abstract approach of symbolic interactionism which doesn't provide any patterns. The arguments would be that there are no patterns, that patterns emerge through persons taking each other into consideration in moving in and around each other. But there is no structure of the way they are supposed to interpenetrate into each other's course of action. So Blumer's fine in providing a very broad and general perspective to social action. But not to anything in the next stage down of an organizational character. So it seems to me meaningless at that level. Why anybody would want to use that as a label except as a corrective relevant to other kinds of schools of thought. I just don't know.

They are all my best friends, I know them all very well and I've known them all very well for thirty years, you see. So it's not a question of any kind of personal animus. They are the only persons I eat with at the meetings and all that sort of thing. I just don't understand what, why... unless it's a self-defensive process, you know.

JV: I see.

EG: You have to end up with a natural history of something, with phases, structures, patterns, or you haven't said anything. Or you haven't analyzed it. And none of those things are really part of symbolic interactionism.

Now, those analytical processes, the ways in which people take each other into consideration, the ways in which they construct joint action, all that, is no doubt, perfectly valid and a good basis for looking at society. But if you stop there you've got the most abstract view in the world of what goes on there is no way of concretizing it. If you start concretizing it, your mission is to create order for what you've seen, and that order means structure, or a natural sequence of phases, or whatever. There you are into, into structural sociology of one kind or another, it seems to me.

JV: Can you say that symbolic interactionism at the end is going so far back that they are just analyzing individuals, in a certain sense, producing sociology from a kind of psychology?

EG: Well, you could move from the individual say, to the nation state, acting through its representatives and see conflict as management of the intention of one organized group upon another, in those sorts of individual terms. It isn't the human being but some kind of acting agency that would be central. But is does deal with that level, and I myself dealt somewhat at that level especially with a book called *Strategic Interaction* [1969].

But, you know, its fine if you want something to write about and talk about. It's not a study of anything, it's not substantive at all. Hughes has never been hung up on that direction. He takes his subject matter and tries to study it and he's never been given, I think, the credit he deserves. He started occupational sociology - he started the sociology of the professions and all that. On the social pathological side of it, which was strong with the social problems group, [Edwin] Lemert [1951] is very central and important. And behind Lemert, a book on Opiate Addiction by [Alfred] Lindesmith [19471968]. Lindesmith was, of course, a Blumer student, so there is that way into the thing. A lot of this is tied up with this Society for Social Problems, and in some way, that would be a much more natural entity to study as representing a group of American sociologists and symbolic interactionists. The overlap is very great between those two groups.

JV: Now by the way, can you agree with the stance of Lindesmith in relation to analytical induction just like [John Maynard] Keynes used it?

EG: Well, I think it would be wonderful if analytical induction worked. But I think it's very optimistic. It's a nice model but to have things work out that neatly and patly is asking too much.

JV: This analysis, this formulation of Lindesmith's, can it be considered the main explanatary paradigm of symbolic interactionism? It's only a hypothesis.

EG: I don't think so. I think better you should go back to George Herbert Mead and *Mind, Self and Society* [1934/1974], and the issue about the way we are constructed socially the individuals by taking to ourselves the opinion and attitude of the other. Pragmatism and John Dewey is strong in this business too. That is, it could have derived from Dewey almost as well as from George Herbert Mead. I think there is a lot to Meadian studies. That is Mead was a very smart fellow. You can learn a lot from reading his books. At least get a lot of leads - what are now called sensitizing concepts, directions for enquiry. Cooley ,as far as I was concerned, was very significant in all this too. Charles Horton Cooley, and he would be called a symbolic interactionist now, but the name wasn't around when he wrote. He is very, very important.

JV: Is there any influence on your work coming from the pragmatist thinking in American philosophy?

EG: Sure. Dewey, I suppose. Again I don't know if it's a matter of espousing something after the fact but I read Dewey earlier on. He wrote a book called *Quest for Certainty* [1929/1960] that had an impact on me. I thought it was a very fine analysis. But all of that pragmatism, the Meadian analysis and Blumer doesn't provide me with substance. That was introduced by social anthropology and Hughes, that is the study of something. Trying to find out its features and characteristics, as opposed to sitting down and pulling out from an examination of social life in general and one's own experience, some basic processes and practices. That's what Dewey did, that's what Cooley did, that's what Mead did and that's what Blumer does. That's a whole different approach. I have done quite a bit of it as in that book *Strategic Interaction* [1969]. But there is an ethnographic side, you couldn't get a reasonable picture of the workings of an occupation or a group just by sitting down and thinking of those basic processes. You have to do what is called participant-observation, you see, so there is a whole observer movement paralleling social anthropology and even in sociology. The presupposition of it, is that, you can have the best general themes and principles in the world but it won't tell you about the

character of an occupation, for that you have to study it.

That empirical approach was very much fostered by Chicago. It had that push away from mere criticism. So there is an interaction of these various sorts of forces and people who label themselves symbolic interactionists sometimes forget that their work itself, is informed by these other influences, as much or more. I don't know in the last analysis whether they take the label all that seriously. It's partly an occupational term so they can make a claim to other people as to what they are. I don't know whether they believe all that all that much.

JV: You mentioned a time ago…

EG: I would help somewhat if I came out strongly in favor of the meaningfulness of the concept, I think. Other persons like me would help, but you know, it isn't all that uniform. Howie Becker is very important in the development of deviance and the like. He was really Hughes' chief student. I don't know how strongly he calls himself a symbolic interactionist or how concerned he is to push that label.

JV: I will see him next month

EG: Yes. Very good. It will be interesting to see what his view on that matter is.

JV: In relation to the methods, to the techniques of research, earlier you said " I prefer different approaches, I can use counting, I can use participant observation, I can use different things" But you said also that - perhaps it was because you were lazy, whatever that means- but you didn't use figures. What are, according to you, the main techniques of doing research in sociology? I think you have a very special feeling for in participant observation.

EG: You mean the main methods of sociology in general, or the methods that I would employ?

JV: That you employ.

EG: Well, I suppose its participant observation in the main, some sort of deduction from one's data. But I haven't done much participant observation over the last decade. I have been mostly involved in writing papers on conversation and that sort of thing, face-to-face interaction in general. I still have to

write up a study I did of casino activity. I don't know when I will get to finish that.

JV: Is it in relation to Las Vegas?

EG: Yes.

JV: You use also in your work different reports, and you have a lot of things in your book which normally we are not used to finding in let's say the tradition of scholarly works, like newspapers, biographies, novels, comics, cartoon, accidental experiences. Now...

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EG: What I have been doing recently is not what you would call research, its more scholarship. Its taking some concepts or variables which I think have some relevance, significance, and reviewing various kinds of findings, of researches, that sort of thing, in terms of it. So it is a sort of a freewheeling, literary kind of a thing. The only difference between that, I suppose, and what symbolic interactionists do, is that I usually collect a larger number of illustrations and think about those maybe in a more systematic way, I hope, than would characterize persons who are merely scholars. But that stuff is stuff one does when one isn't doing participant-observation, isn't doing a natural study. I don't know where that stuff goes. It's theory, on some crude and vulgar level. It's a claim that one has access to, or one can introduce some concepts, or variables, or terms, that will have some utility. And one introduces them by showing how a wide variety of relatively different sorts of material can be discussed in terms of it. That's, I suppose, one of the techniques that I employ but I also employ that technique in actual ethnography. What those methods are, I'm not the best person to say. That is, somebody would have to analyze what I do. My version would be what I said before, just doctorine, and not really telling you.

JV: Now when you use these cartoons, these newspaper stories, these biographies, novels, comics, at the end you come to a certain pattern of social behavior, but aren't there problems in relation to the reliability of the information you got there? Or is your stance that reliability of the text, of a particular experience doesn't matter? How must I interpret these things?

EG: Well, I think in many cases it's impossible to say what really went on if you take a reported

event. The issue is not to have to depend on very much on being able to say for certain what went on in any particular case, rather to be able to use particular cases to illuminate things that must go on in some cases but one isn't absolutely sure in which cases it goes on. And all that ought to be presented tentatively, that is, not as confirmed findings but merely as suggestions. The proof always comes from whether individuals find it useful to use those concepts and terms and what they do. Now that itself may be a very fallible sort of thing because people can pick up the wrong things. Things that aren't basically fruitful or valid but, but writings of that kind, it seems to me, are presented under the guise of being just possibilities that might be useful or might work out. And that's the way some of that writing is, especially *Frame Analysis* [1974]. Frame analysis has been considerably criticized in part because of the fact that the data it uses are just from newspapers and the like. The problem there is not that you can't get valid material from snippets of newspapers but that it limits you to the kinds of social facts that you talk about a great deal. You don't end up talking about broad social processes - rationalization of industry, you know, the rise of the third world, other sorts of things, the changing relationship of the social classes to each other. So you end up in a sort of a personalistic social psychological material which that book *Frame Analysis* [1974], explicitly does. Just as, in a way does, *Strategic Interaction* [1969] and the first book, *Presentation of Self* [1959]. So its weakness comes from dealing with only a segment or a phase of social life. Not from the triviality of the data because life is trivial by and large in lots of ways. But that one's committed to omitting some very large issues. Historical ones, ones dealing with the whole society, that kind of thing. Thats life.

JV: Now, when you bring these things together, it is not a problem for you to use these things without any knowledge about representativeness I think.

EG: Well that's a problem. Doing a traditional sample to ensure representativeness does not ensure representativeness. All it does is provide you some information about the sample itself but it doesn't tell you very much about the universe. So traditional sampling itself has its problems. I think sampling is always required. That is the statistical approach to social facts is central and critical. There is no way really around it. But in itself it’s a criterion which is in no way easy to satisfy even when you are a statistician.

I present my stuff, tentatively. When I do these studies I take a large number of illustrations variously obtained and put them all before my eyes in one form or another and try to get a formulation that is compatible with all of them so there is a check upon just making wild imputations. Now somehow or other, one can manage to uncover something that has validity that is more broadly based

that one has good grounds for hoping, given the way one undertook it. So I can't say what it is that gives such validity and representativeness; for example, I studied by and large only one mental hospital. I presupposed that what I said about one had some quite general and even close application to what went on in others, but there is no way I could provide proof of that. Yet, it turns out that it is the case, that what I found out about one by and large applies to others. Now, had I taken the family as a unit and studied one family it is unlikely that what I would find out about one family would be as closely applicable to other families. So, somehow or other, there is something in what one studies that ends one up being able to say something, without being able to prove it, but nonetheless being able to say something that turns out to have validity more broadly based.

Now the criticism of that is, well, how does one know whether what you are saying will carry that validity or not? I don't know - its partly the art of the process, I suppose. There may be in some areas like, for example, pedestrian traffic, some systemic forces such that there has got to be orderliness, or in couldn't go on and, therefore, finding one or two of the bits, that allows one to make some statements about some of the other parts, you see. Other aspects of life may be less intensively orderly. I like to try get at those parts that have that orderly organization, so indeed one can make valid statements. But this is a mysterious [phenomenon], and not a process that I can explicate at all. You see.

I would rather sort of leave it open. It can be done well, and it can be done badly and I can't provide the rules for doing it well. It isn't necessarily an art, it's a method of some sort, I just don't trust so far, anybody's explications of what that method is. In the last analysis you end up saying something that isn't valid. It's hopeless. That is, the only way to succeed in this business is somehow or other, to say things that hold, or have some truth. Otherwise, it doesn't last very long, you see. And I know how I proceed. I collect lots of different things and force myself to try to find the formulation that's relevant and consistent with it. But that's not analytical induction, in a strict and rigorous fashion; it's much more freewheeling and crude - than analytical induction strictly speaking. Who knows how strict analytical induction was? I mean people never report what they actually do and all that sort of thing, you see.

JV: In the introduction of *Framework* [1974], you said that your analysis was about the 'organization of experience', the organization of experience which the individual person has in mind, not about the organization of society. In that sense you say, I think, that you are not writing really a fundamental treatise of sociology.

EG: No, this is more cognitive than sociology would have to be. And is again the individual's cognitive intention upon social life, but it was also argued in the preface that, I don't think people get that just out of their heads. That is, they have to come to assess the situation around them as it is, by and large, or they would come a cropper. That is, they would soon have to change their views. So it's not a social constructionist view in the sense that anybody can, at any moment, define the world around them. The world around an individual is, by and large, defined at any moment. It comes to be defined through society at large in a constructed way. And that book has to do with the way one comes to understand how the world around oneself is defined, ought to be defined and can get to be misdefined. So it is again the individual's intention on the world that at this time, his cognitive impingement on the world and that part of the organization of the world, that underpins, or informs his cognitive response to it.

Again it's very partial and is presented as such, and it's not been very widely, widely accepted in sociology. I suppose a few people have read it but not too many. It's a long, very long book. I hope more people will read it. I don't know if they will or not. But I spent a lot of time on it.

JV: I have two other questions, to conclude. The first one - you mention at a certain moment [Alfred] Schutz. What is the meaning of Schutz for your work?

EG: Well, again it was a late sort of thing, but the last book on Frame Analysis [1974] was influenced by him. [Gregory] Bateson quite a bit, but Schutz's [1967] paper on multiple realities was an influence. Schutz is continuing to be something of an influence. His stuff on, on the corpus of experience and that sort of thing. There are some ways in which he impinges upon sociolinguistic concerns, but I can't profess to be a close student.

Again I think Schutz has wonderful leads, but that Schutz himself doesn't carry one very far in any one direction. I part strict company with scholars who take one book as central and then see all other books, all other writings as not as - as falling short of the basic treatment. This has recently become very strong in American sociology. [Ludwig] Wittgenstein gets to be a writer whose writings are held up as the touchstone for what ought to be done. It seems to me there is no way that Wittgenstein could know anything about the organization of an occupation, or things like that. Schutz has come to have something of that status, of course, for ethnomethodologists.

I think all of these people that write, provide us with leads, and are interesting to read and worth reading, but to establish these books as canons of some kind, as things we should be returning to and not be parting from, is extremely misguided. It changes the nature of sociology into some sort

of ritualistic, cabbalistic, literary undertaking.

Mind you it's easy for me to talk that way now. When I was younger I thought The *Elementary Forms* [Durkheim, 1912/1965] was really a very, very central book. Much as people now think that Schutz was, or Wittgenstein, or whatever. Or [Maurice] Merleau-Ponty.

But this tenor of analysis of where the whole analysis consists of showing how a current writing departs from and falls short of what, say Schutz said, well I don't think Schutz said enough to inform any particular studies sufficiently. That is, it's just a set of leads of possibilities. So also with William James, or anybody else you can go back to, or Gregory Bateson. I think that's plain bad hero worship.

Europeans have this much more than Americans have it. And now Americans are acquiring it - treating the corpus of a scholar's writings as the ultimate data of social life. I'm always struck by the facts that in Europe, whether you are a Marxist, or somebody on the other political side, whether you are in Germany or France and the major countries, ultimate reality for social science is not social life but the writings of some character like Karl Mannheim or Karl Marx or Max Weber, or anybody else like that. To me that is a very sad thing. I don't think they will ever get anywhere if they follow that course because the corpus of man's writings isn't reality and isn't society.

JV: But can't it give a way to look to society?

EG: Oh sure, but then you to have go and uncover something. The French now have it worse than the Germans. The Germans are learning to be empirical and I think they will perhaps succeed. But the French are totally locked up in a world that consists of persons who have written things. An entirely literary world. And that's what happening to some American sociologists who are getting too old to do studies that they weren't really interested in doing. They are spending their time now, writing. (laughter) As you are!

[JV explained why he thought that it made sense to study the influence of important ideas on the development of sociology. EG then replied.]

EG: You can study institutionally, for example, if you can show how people at some stage in the game, get to be very interesting, and call themselves something. That's the study of the sociology of science, basically, or the sociology of education or the sociology of educators basically.
That's an empirical undertaking. It just may not be all that significant if you are dealing with sociolo-

gists, that's all. There might be other persons who are more important.

JV: That's right. But nevertheless sociology is a business on this moment. You have a lot of departments. The average faculty of a department in the States as far as I could see now, is 20-22 persons. And how many students are paying money, the State is paying money for that.

[A discussion followed about the quality of undergraduate training in sociology, comparing European with American curricula. EG then resumed his thesis that it is very hard, if not impossible, to form a picture of the development of sociology by relying on books because "all the writers write about what other writers wrote about these books."]

EG: So it seems to me what one has to do is to go back to the corpus of the work and get a critique of it that is fresh, that isn't informed by other people's critiques but comes just from yourself, on the one hand, and, on the other, find out about the real, institutional history of these things. Like, critical in symbolic interactionism was the fact that there was a war on. And Herbert Blumer was employed for a large number of years, seven or five or something, as an arbitrator. He was away from teaching a generation of students who got to be symbolic interactionists. Another feature was increasing specialization. Which made it impossible to acquire competence across the board. So you got a split, for example, at Chicago between persons doing ecology and persons doing other sorts of things. Before, everybody did everything.

Or the fact that Hughes was an outsider at Chicago to some degree. Outside of the main power base, which was Louis Wirth, Ernest Burgess, and maybe [William] Ogburn at the time he was there. But centrally, Louis Wirth. All of these [people] influenced what happened. So the persons who weren't taking traditional sociology, which was then already quantitative, gravitated to Everett Hughes, who had, I don't know where he got it from, except for Park, an institutional way of studying things. At that time in Chicago, anthropology was close to sociology, people knew each other fairly well. Radcliffe Brown had been there three or four years. So all of these influences are institutional ones, and the history of what came to be symbolic interaction was really related to them.

As is the rise of ethnomethodology. Which was unique in that it was the most groupy of any of the splinters in American sociology. That is, they set themselves apart. Spoke only to each other, read only each other's stuff and that sort of thing. And introduced into American sociology, successfully for them, a sense of where you could get by travelling under a label.

I can remember arguing with Harold [Garfinkel] not to use ethnomethodology as a term, that

he should just introduce his criticism of ordinary methods into sociology at large. He would have nothing to do with that and I think rightly so, politically, I think, for the group.

But the introduction of ethnomethodology as a label that was meant to label just the students who attached themselves socially and institutionally to Harold and his group, that was a very novel thing in American sociology. Symbolic Interactionism as we know it today, is in part, the unstated response, unadmitted response, to a very novel kind of self-labeling that the "ethnomethodologists" engaged in.

They really started in some sense, self-labelling. Calling oneself something. It wasn't that they started ethnomethodology in part; in part they started the calling of oneself something. Well that's the institutional history of it, you see, you can't get that from critical papers that are written about this and that.

JV: So that's the reason why I am visiting these different scholars.

EG: Well, the point is at issue, is whether these different scholars are going to talk to you about it, or not.

JV: Yes, of course. They will not say everything I suppose. But nevertheless, I have your information, I can speak to them and Tom Shibutani.

EG: Oh yes. Tom, Tom by the way is a social psychologist in the classical sense, namely in the Gregory Prentice Stone sense, and I shouldn't have omitted him. He was a direct, lineal descendant of Blumer's, and he did work at the purely social psychological, very general and broad level. He had students who were also influenced in a second or third generational way by Hughes. Like Arlene Daniels, for example, who was close to Shibutani and then got influenced by the Hughesians, and then by the ethnomethodologist to a degree. He's an interesting guy Tom Shibutani. You've talked to him?

I: Yes, I'm there now at Santa Barbara since the end of January.

[The interview ended with some remarks on the weather in California, and arrangements were made for JV to come and find his bag at EG's house after he went to visit Independence Hall. EG wouldn't be at home, and gave JV his key.]

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