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*Sociological Perspectives* 2003 46: 21

DOI: 10.1525/sop.2003.46.1.21

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# CONTINUITY AND CHANGE IN HOWARD S. BECKER'S WORK

## An Interview with Howard S. Becker

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**ABSTRACT:** *Howard S. Becker is one of the foremost sociologists of the second half of the twentieth century. Although he is perhaps best known for research on deviance and his book *Outsiders*, this constitutes only a very small fraction of his earliest work. This interview looks at some of the continuities and cores of his work over fifty years. Becker highlights how his work maintains the same core concerns, although new interests have been added over time. At the core is a concern with "work" and "doing things together." Becker provides many concrete stories from the past and also raises issues about the nature of doing theory and research, how he writes and produces his studies, and the problems attached to the professionalization of sociology. His writing on art and culture can be seen as assuming a major position in his later work, but he does not identify with either post-modernism or cultural studies.*

Howard S. Becker did his undergraduate and graduate work at the University of Chicago and received his Ph.D. in 1951. He held various research jobs until he became Professor of Sociology at Northwestern University in 1965, where he remained until 1991. He has also taught at the University of Washington, the University of California, Santa Barbara, the Visual Studies Workshop, and elsewhere. He is the author of *Outsiders*, *Art Worlds*, *Writing for Social Scientists*, and *Tricks of the Trade*. He now lives and works in San Francisco.

This interview took place in Santa Barbara in January 2002 at the home of Harvey Molotch. Becker has given a number of interviews over the years, and these are listed at the end of the references.

Ken Plummer: Howie, you've had a sociological life for over half a century, and I'd like to ask you where you see sociology has come to, and what its pitfalls are now. Is there any thing that has held you together in your sociological life for the past fifty years? Actually I think there is a very strong underlying

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Sociological Perspectives, Volume 46, Number 1, pages 21–39.

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Journals Division, 2000 Center St., Ste. 303, Berkeley, CA 94704-1223.

ISSN: 0731-1214; online ISSN: 1533-8673

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continuity to all your work, but you may not see that. It's those sorts of things that I'd like to get into.

Howard S. Becker: Well, let's start with the last one. I feel that that it is completely continuous.

KP: It's all from one cloth.

HB: Oh yes, absolutely. I mean I don't think I've changed the way I do things very much. You know, I've added things, quite a lot of things, cause I'm kind of a magpie—you know, I just pick up new things, but they always get incorporated into the way I do stuff.

KP: So lots of different areas you've looked at?

HB: Yes.

KP: But always the same approach basically?

HB: Yes.

KP: Because a lot of people still think of you as a deviancy specialist.

HB: Yes. A lot of people think of my earlier work as mainly about deviance, but it's not.

KP: It's a very limited period of your work, isn't it?

HB: Well, I don't know if I had periods, I never really did work on deviance as such. What happened was I did my master's thesis on musicians. After I got out of school, I did the marijuana study, which I wanted to do because I read Alfred Lindesmith's book on opium addiction and thought, This is really terrific and I could do the same thing with marijuana. It'll be interesting because it's not addictive. It'll be kind of an interesting comparison. And at that same time, when I was writing the marijuana stuff up, I sat down and wrote ninety pages about deviance. This is in 1953 or 1954, and I wrote ninety pages, too long for an article, not long enough for a book. My friend Erving Goffman would have figured out a way to make a book out of it right away, which I didn't. Maybe ten years later I found this draft in a file and said, "Hey, this isn't bad," and I sent it to Irwin Deutcher, who I knew from Kansas City, and he said, "You ought to publish this. You ought to make a book out of this, this is pretty interesting." And then I got the idea of sandwiching the marijuana stuff and the musician stuff in between parts of the essay on deviance. One of the reviewers, I think it was Kai Erickson, pointed out that there was a certain lack of coherence in this volume, which was absolutely right. But that was my involvement with deviance.

KP: Well, and a few little bits. There's the book *The Other Side*?

HB: True. I was editing the journal *Social Problems* and the year after *Outsiders* I collected some papers from the journal for that book. Then I wrote another chapter for the British Sociological Association conference, in 1974, which became the tenth chapter of the book as it is now. And I wrote a few pieces later on about drugs, when LSD happened, because I thought I knew something that would be interesting.

KP: "The Culture of Civility"—about San Francisco—and those pieces?

HB: Yeah, right, I wrote that piece with Horowitz.

KP: But if you can go right back—I don't know what was your first article, but the piano jazz band stuff, I think that was one of your earlier pieces?

HB: That was my first article.

KP: In a sense you can see straightway a continuity with the more recent *Art Worlds* in that they both deal with cultural creative worlds.

HB: Yes, the continuity is probably Hughesian. Everett Hughes. All his classes were the same, whatever he called them. One class would be called "Race and Ethnic Contacts," another would be "Institutions," and the other one he taught a lot was "The Sociology of occupations and professions." They were all the same class, and usually dealt with whatever he was reading that week.

KP: And they all said what?

HB: He started one of these classes I took by saying, "Everything that happens in society is somebody's work," So you could always study what's going on someplace by looking at it as somebody's work. That's probably the most basic thing about everything I do .

KP: Interesting you see it that way . . .

HB: Let's say you want to study the phenomenon of death. Robert Habenstein wrote his dissertation on funeral directors. So, when I thought about deviance, it's quite obvious that this is somebody's work.

KP: You couldn't say that all social life is work?

HB: Sure you can. There's a challenge!

KP: Well, it's a metaphor which you can push and you can say having sex is work, but it's not so obvious as, say, many of the areas that you've looked at.

HB: Not every person involved in any of these activities is working. The dead person is not working, but work is being done about this. It's somebody's work. So sex is obviously a lot of people's work. It's pornographers' work, it's policemen's work, it's doctors' work. It's therapists' work.

KP: But not all of it. There are bits, which touch each other.

HB: No, not all of anything is work, but it's an approach to any area of activity.

KP: That does make sense as an organizing metaphor for all your work. And of course one of your edited collections, *Sociological Work*, actually puts it in the title. But there's a sense in which you are also a symbolic interactionist, yet I never see you use the term.

HB: No. I don't know what it means. I mean it's like all those "school" titles, you know, labels—they're appropriated by all kinds of people for all kinds of reasons. So when I look at the journal *Symbolic Interaction*, I don't recognise what's in there as being remotely connected to anything I'm interested in.

KP: But I mean you are firmly located in symbolic interactionism by other people whether you like it or not.

HB: I can't help what other people think.

KP: Goffman's treated the same. But he denounced symbolic interactionism as far as I can see. . .

HB: Erving's lineage is quite different from mine.

KP: But you were in the same class together

HB: Oh yes, we were quite close. We used to walk our babies together down Fifty-third Street, they're the same age. But you know, my lineage is Simmel, Robert Park, Everett Hughes. I think that Erving's lineage was Durkheim, Radcliffe Brown, Lloyd Warner.

KP: That's interesting. I would also see Simmel as being influential on Goffman too. I mean, you both have this kind of preoccupation with what is called mini concepts and little phrases that sensitize you to how the world works. You both do that.

HB: It's not that having his lineage he never heard of Simmel. Of course not. But that's not his lineage. He is not a descendant of Robert Park.

KP: Right. That's interesting.

HB: We both worked very closely with Lloyd Warner who these days never gets the attention he deserves. He was quite influential for a lot of people.

KP: And you both have in common the essay mode of writing, the Simmelian mode. Perhaps with the exception of *Art Worlds*, you tend to write in fragments—you write in little pieces rather than big pieces.

HB: Oh no. *Art Worlds* is the stitching together of a lot of shorter pieces. Back in 1970, I had a year off, I was . . . most of the work I'd been doing till then was in the sociology of education. We had this grant to study a medical school, then we studied an undergraduate college, then we studied *people* who were of college age who didn't go to college.

KP: This is *Boys in White* and *Making the Grade* and those books.

HB: Right, and the third one was actually *Learning the Ropes* edited by Blanche Geer.

KP: Ah, that's much less known.

HB: Much less known. Graduate students did these studies and wrote them up. They were studies of places like a barber college, a beauty college, apprenticeship in the meat cutting trade, things like that. So that sort of educational study was what I'd been doing for ten or twelve years, including my dissertation on schoolteachers. I got to a place where it was boring. Because I knew I could go to study another educational institution and—it's not that I knew how to penetrate its secrets immediately, but I knew how to study it as the kind of thing I knew how to study, if you see what I mean. And three days after I got there, I knew what the book would look like three years from that.

So it was boring and I didn't want to do it anymore, and I was teaching at Northwestern by then. Until then, I had subsisted on grants, research that was funded by raising money somewhere, and now I didn't have to do that anymore. So I was quite free to do any damn thing I wanted to instead of looking for a topic where we could find some money.

I thought at the time that the sociology of art was a really underdeveloped area, because it was mostly in the hands of people who were pretty much aestheticians. You know, like Adorno, Lukacs, and Lucien Goldmann, whose work was a thinly disguised way of making and justifying judgments of value in various arts. And I thought that this was not really all that could be done. I spent the year reading very widely: books by film editors, books about Florentine art, books about Victorian novelists, and I came away with a lot of raw material. I wrote a piece called "Art as Collective Action," which appeared in the *ASR*, I think it was 1974. That became the first chapter of the book.

But about that time an odd thing happened. I was out in California, and I got a call from the head of my department, Bob Winch, who said, "Look,

something's come up. There's a new professor of ethnomusicology here, a man named Klaus Wachsmann, and he's quite surprised that we don't have a class in the sociology of art—he says he needs it for his Ph.D. students." It was kind of a joke, because nobody had a course in the sociology of art then. "Would you be interested in teaching it?" So I said, "Well, that's a message from someplace. OK." And I taught that class and I think the first or second year I taught it, I recorded the lectures and got them transcribed, and they became the basis of the chapters of the book. Then I'd write a piece, somebody would ask me to do something for a conference and I'd write a piece about something, and that became part of a chapter or whatever, and I just wrote a lot of pieces and eventually I had eight or ten of them and laid them all out on the ground and said, "OK, what's missing?"

KP: Well, that's the same as *Outsiders* then in a way, isn't it?

HB: Even more so. Every book I've written is like that.

KP: So it's little bits that emerge. It's like a kind of collage which eventually takes on some coherence.

HB: All in the service of a controlling idea, which of course changes from time to time. But the idea of art as something that a lot of people do together, and that it is a matter of convention which one gets to be called the artist, that was right there pretty much from the beginning.

KP: So we've got the notion of work. You keep saying collective action and you keep saying, here in the case of *Art Worlds*, a lot of people doing things together. So that's another kind of major preoccupation—doing things together. That's how deviance works too. Doing things together. Are there any other kind of mini ideas—mini concepts that are really organizing frames for everything you do? I mean, these are the ones you've written about. I can hear them all the time. Are there other ones you think are around?

HB: Well, the other one that is really connected to that is the idea of process. If you explore this notion fully, which for a long time I didn't, not really, not in some abstract sense, it starts to put pay to the idea of cause and establishing causal relationships. I mean all kinds of work does that, treats things as—here are the causes and there are the results. How you get from one to the other is not explored, it's just, the causes cause the effect.

KP: So it's "how" questions. I mean that to me is very Blumerian.

HB: It's extremely Blumerian. Because the key criticism Blumer made of everything, of every theory that proposed to explain human conduct, was that they act as though there's some automatic connection between stimulus and action, between instinct and action, between culture and action. Like, "Why did you do that?" "Well, it's my culture." People do that, whatever it is you want to explain, because it's in their culture. He said, "No, bullshit. The way it works is there are all kinds of things in the environment, people are active not passive, they're not sitting there waiting to be stimulated to do something, or being forced to do something, being coerced by their instincts or their culture. They're busy doing things and they're actively searching the environment. They are not responding to stimuli, they're creating stimuli, looking in the environment for what they can use," It's very Deweyish.

Blumer always insisted that there is always this step between what's in the environment and the behavior that comes out, which is the stage of reflection. And he used to give us this exercise which was really quite wonderful, because he would rave on and on about what was wrong with instinct theory and what was wrong with stimulus response theory, etc., and then ask us to do this exercise: take any ten minutes of your own behavior and explain what went on in those ten minutes using one of those theories. And you know, really and truly, you can't do it. The theories aren't up to it. Whereas the whole notion of an internal dialogue, you can observe that, you really can observe it.

You're talking all the time in your head, you're always thinking of things, and you're paying attention to what the other one says. One of the implications of that is that there's no telling where it'll end up. You can't make predictions. I've been quoting David Mamet, the American playwright who says somewhere, but I can't find it, he says, "In every scene in a play all the people who are in the scene are there because they want something. If they didn't want something they wouldn't be there, so by definition if they're there, they want something. And the scene develops by each of them pursuing what they want in the light of what other people are willing to do and what results from everybody pursuing what they want, and having to adjust to each other, is something none of them might have wanted and certainly that you could never have guessed in advance." It results from their interactions.

KP: But that is the original symbolic interactionist stance. It seems to me that a lot of interactionists have moved on into a lot of other things and have dropped what is in fact the core of it.

HB: That's the only part worth having. There's that great paper of Blumer's. He said the same thing over and over again, and the one paper where it is best summarized is the piece called "The Sociological Implications of the Thought of George Herbert Mead."

KP: It breaks into five different parts.

HB: Yes, and "Collective Action" is one of them. That's the part that seems to me central, because if you have that image of one person acting that way and then all the people in the scene acting together to produce whatever happens in the scene, you just extend that.

KP: Yes, but it isn't just true of people, it's true of objects too. It reminds me of that article where you talk about overhead projectors in classrooms and you take that as an object and then you go to all the people involved in making it work and making it happen. On its own, the object is nothing, but it requires that collective action round it.

HB: Yes. I've found the way Bruno Latour makes a great deal out of nonhuman actions very useful. He speaks of nonhuman actors, machines. Latour defines a machine as when you get a bunch of independent parts and tie them together in such a way that they begin to control each other's behavior, like the governor on an engine. That's a machine. Well, he says, "The people are tied into the machine and the machine is tied into what they're doing and you can think of that as a bigger machine." It's another way of talking about the same thing.



It's absolutely fascinating to me that Latour who, I don't think, probably to this day, has read a word of John Dewey, or George Herbert Mead, I can't imagine him reading that, but it's exactly a pragmatist position. It's all sociology of science and it's brilliantly original.

KP: I'm still trying to get at key themes in your work. And one way in getting at them is also to think about if there have been any significant shifts. We said you've more or less been doing the same thing for fifty years in different areas of inquiry. From writing to everything you think about that comes out this way. So would you say over half a century there has been anything that's kind of dramatically made you go through some transformation in the way you think about the world? Or perhaps there really hasn't been anything like that? Given the fads and foibles of sociology, with one trend after another, you must have seen so many by now. Has there been anything in any of that that's kind of hit you and made you say, "Now look, I've got to rethink this."

HB: I don't think that I've ever thought I had to rethink things. I've added all kinds of stuff. And in the course of adding, you know, you change the main house when you put the addition on. And there are lots of things like that. When I began to read in art history and literary theory and what not, when I started doing *Art Worlds*, I just picked up all kinds of ideas that were so useful to me. To be sure I twisted them so that they'd fit into what I was doing, but they were new stuff.

KP: But that is something that makes you a very different kind of sociologist. The fact that as your work progresses, you become more and more interested in literature and drama and you have more and more references to this. More and more concerns about writing, of being intelligible, of being literate, which actually, for a large number of sociologists doesn't even come into the game at all.

HB: Well, those were always interests of mine. It was always just a question of working them in. I tell this story in my writing book. When Jim Carper and I wrote this piece about occupational identities and sent it to the *AJS*, Everett Hughes, who was the editor, he wrote this scathing letter. I got a letter back from Helen Hughes, his wife, who was the managing editor, saying, "Howie, you know that Everett loves you, please read this with that in mind." And he was just raving. "What happened to you, you used to write decent English. Good God, this sounds like it was translated from German, word for word."

KP: But it does seem to be a credential these days to be a good sociologist. I'm not talking Parsonian stuff. I'm just suggesting that students are almost trained to write incomprehensibly. And it is a feature of your work all the way through, that it's always intelligible and everybody says that about it.

HB: Sometimes critically.

KP: Yes, well of course the trouble with it is, it makes it sound too obvious and too simple because, you know, you can understand what you're saying.

HB: Yes, that's what they say. It is a fault. I remember when I read Leonard Meyer's book *Emotion and Meaning in Music*. This book gives you kind of a gestalt theory of how music carries or creates emotional meaning, based on the idea of tension and release. But the key idea is that this is all convention-



alized because you have come to expect that a dominant seventh chord will be resolved into a tonic chord, which is like a fundamental piece of Western musical practice. Because listeners have come to expect that, you can really create an enormous tension by not resolving the dominant seventh chord that way.

You create tension by not doing what people expect. It is connected to the idea of inertia. And there's lots of stuff that people have learned to expect. That this chord will lead to that chord, for instance. I used to do this thing in class, it was a lot of fun. I'd put a musical staff on the board, write down middle C. Then I'd say, "OK, this is the first note of the melody," and I'd sing it, "mmmm." "O.K. what's the second note?" So they'd look at me like "What are you talking about?" I'd say, "Come on, what is the second note? You have to guess." So somebody would finally say E, you know, because everybody's learned that much music in school. "So you mean C E," I'd sing it, it's a major triad, right? Anybody have a different idea? Somebody would say D. C D, second note of the scale. "Well, what do you think? Which one is it?" So then people would begin to guess wildly. Finally I'd say, "OK, I can see you can't solve this with the information I've given you. OK, the second note is E. He was right, C E. What's the third note?" If I did it right the whole class would sing "G." Because it was obvious. Why was it obvious? Because you'd indicated that this was going to be a major triad. If I'd said no, it was D—C, D, E—everybody knew that that was the next note. That was the fundamental idea of Meyer's book. It's a very powerful idea, and then I read a number of other things about the idea of convention, which he made such good use of. It's all through discussions of art, literature, and everything else.

KP: It's bound in narrative structures and all conventions. It makes me ponder where you stand on another "ism," which is postmodernism. This is supposed to be a theory which breaks down those conventions.

HB: I don't know. I haven't read that much.

KP: I've never seen you proclaim on it.

HB: No, no. I didn't think they were talking about anything I was interested in. Coming back to conventions, I soon saw that the idea of convention was the same idea as the sociologist's idea of norms, shared understandings, any of those words we habitually use. If that's true, then you can import all the work people do on conventions in all these various fields and it's sociology.

KP: You wouldn't use words like "institutions" for that, would you? It's not part of your language.

HB: No, but it was part of Everett's, Everett Hughes. He was a wonderful essayist. But that idea was always kind of vague to me. Everett essentially used it interchangeably with organization, I think. An institution was an organization that lasted longer than some other organizations. But he didn't set great store on distinctions like that. So the idea of conventions was something that had a big effect on me, but I wouldn't say it transformed my way of thinking. It added to my repertoire.

Similarly, when I started reading Latour's work, I thought, this is a soul buddy, you know, this guy is simpatico. I met him in San Francisco years ago

and I read *Laboratory Life* and then I read everything else Bruno had put out. It was about the time I'd started reading French, so I would read his books, in French, painfully, but then after a while I realized that everything he does now is going to be translated, so I stopped that because it takes me a lot longer to read a French book. But I read *Les Microbes*, which is the book that came out in English as *The Pasteurisation of Society*. *Science in Action* came out in English originally. Bruno's work is very powerful, very original. So that's given me a lot.

When I worked with Charles Ragin at Northwestern, Charles and I talked together quite a bit. He's developed this method, what he called for a while the Boolean method, now he calls it qualitative comparative analysis, and that's had a big influence on my thinking. It originally appeared in his book called *The Comparative Method*. Now he's got a second book, which is really a winner. It's called *Fuzzy Set Social Science*. It's just terrific. It's something all qualitative sociologists should have under their belts. But they take one look at it, and it looks like formal logic, which it is, and "Oops—can't deal with that."

KP: Right. I haven't read it.

HB: Well, you should. You really should. It's so useful, as a way of thinking, you can't believe it.

KP: "Ways of thinking"? One of the things that strike me about your work is that by and large, you don't talk theory and you don't talk method. I mean you do talk a lot of theory and you do talk a lot of method, but you don't! Method comes up all over the place, how you do sociology, but you don't write those formal papers and handbooks on interviewing, and all the rest of it. *Tricks of the Trade* is the book, isn't it? I mean, it's called *Tricks of the Trade*, it's not called *A Primer of Sociological Qualitative Methodology*. And the same is true of theory. You haven't written your theoretical magnum opus, because it drips out from everything you actually write about. Is there any reason why you never felt the urge to develop a formal theory?

HB: I may have felt the urge, but it's like Robert Hutchins, who was the president of the University of Chicago when I was a student there, who used to say, "Whenever I feel the urge to exercise, I lie down till it goes away."

KP: There could be a whole Becker school. I mean, you actually train many graduate students, etc., but there isn't really a Becker school of sociology.

HB: Praise the Lord!

KP: [laughing] Hope that comes out.

HB: Donald Campbell the social psychologist was another person who had quite an influence on me. When I went to Northwestern, there was an interdisciplinary program in social psychology, involving anthropology, sociology, and psychology. And there was a seminar, usually taught by someone from psychology and someone from sociology. The first year I was there, Raymond Mack, who was the head of the department and a truly brilliant administrator, suggested that I teach the seminar with Don, so, OK, I said yes.

He was an incredibly wonderful person, unbelievably smart, with a very sly sense of humor. He said to me, with a straight face, "You know, I once

taught a class at the University of Chicago with Herbert Blumer and I never did understand what he was talking about, and I was so traumatized by dealing with him, he was such an impressive person physically." Campbell himself was about 6'4", quite impressive himself. So he said, "Why don't we have the seminar this year on symbolic interaction?" Well, about halfway through the term, I realized that that meant I did all the work and that Don sat around making smart remarks. And the next year he had another idea like that, but I said, "No, this time you're going to do the work." But he was really an interesting guy. He had a big influence on me too. What did you ask me?

KP: It was about methods.

HB: I think the problem about methods in social sciences, when you talk methods, it's as though you could establish a method that would be independent of the situation you used it in. You know, like a questionnaire. There are principles in questionnaire construction and all that, and there's a methodology for dealing with that. But the way that works is that you have to impose on the world the structure that the questionnaire requires. Latour has this wonderful line where he says that science is an absolutely fabulous way of doing things. It works like a railroad; it runs anywhere so long as tracks have been laid for it. Science works when you make the world into the kind of place where that kind of science will work. That's the purpose of creating laboratories, in his analysis: science doesn't really work in the world because there are too many other things going on that interfere with the thing you want to make happen. In a laboratory, you purify things, you keep the natural enemies of what you are studying under control. Then it works. Well, that's the case with a lot of social science methods. It works as long as you change the world into the kind of place that method will work on.

KP: Well, this is Blumer's point too. Then you've amputated the world. It's not the world.

HB: Put another way, you can't talk about anything much, other than the world you've created. Because it doesn't work outside that, because other things are there. Thomas Kuhn makes the same kind of point in a different way. His way is really fun, because he says the only way science can make any progress is if everybody agrees to collaborate on answering the same small set of questions, using more or less the same methods. Then they make enormous progress. The only problem is that this almost immediately generates anomalies, this is the dynamics in his theory of science, because to specialize like this means that the scientists leave practically everything out. The only way to concentrate on something that way is to leave out 99 percent of what's going on out there, leave it out. Well, you can leave it out, but it's there and it will stick its head up.

KP: Doesn't it slightly depress you, looking back on fifty years' worth of work, with you taking your particular stance, and not many other people working like you. You have your particular approach. I may be wrong, but in the main most of the people are working in the scientific cast that is going down a different track.

HB: The same thing is true of doing fieldwork. We are also creating a situation in which we can do it. It's not exactly the real world. I mean, you study sex, you're not in bed with everybody.

KP: No. [laughter]

HB: And there's no way to do that. So you've got to find some way to turn what you *want* to find out about into something you *can* find something out about.

KP: Would you say that one track is more useful than the other track?

HB: The smart survey researchers, which there are some—Lazersfeld was a very smart researcher—work in a way that takes account, more or less, of things that need to be taken account of. It can be done. It's not the way I do it. It's not the only way to do it. I don't think the way I do it is the only way to do it. I was brought up in a school, my descent from Robert Park includes the idea that there are a lot of different ways to do this. They all contribute something. To me, I never doubted for a moment that with all the flaws and all the inaccuracies and everything left out of a census—it's a lot better than nothing. Am I interested in what percentage of the American population is black? Even though I know that the question in the census is "Do you consider yourself White, Black, Asian?" I mean, what kind of a measure is that? Well, not bad. Good enough for a lot of purposes. With all the inaccuracies built into it, it's plenty good enough for many, many purposes.

KP: Well, this is kind of good news actually. Because I was getting an image that you might have after half a century of sociology been hand wringing about the state of sociology. But you don't seem to be.

HB: I've been what?

KP: Hand wringing—in slight despair about the contemporary state of sociology.

HB: I am. *Despair* is a little hard. I'm not going to stay up nights worrying about sociology. Organizationally, science is also people doing stuff together and the conventionalized ways we have been doing it are very tied to universities, and universities are in terrible shape.

KP: Well, this is something you were writing about in fact. I mean you have a little piece in *Doing Things Together* which is about university education, how sociology departments are changing, and this is actually obviously written in the 1970s or the early 1980s.

HB: I think it was published in 1980.

KP: I mean that's twenty-two years ago and all those trends you talk about—specialization, fragmentation, etc.—they've all just gone on. And they've got more and more extreme now.

HB: I like that piece very much. I wrote it with Bill Rau. It's really a demographic analysis. It says, look, there are ten or fifteen times as many sociologists as there used to be. What's the consequence of that? The chief consequence, I thought, and it turns out it really is what happened, and our friends at Sage Publications are like a walking bulletin board of how it happened, is that there are enough people in every tiny subspeciality to support a bunch of journals, to support an organization, to elect a president, to give a prize, and to have a whole world so complete that they don't need anybody else. And I think that's bad for sociology, because it means that

the delinquency people don't talk to the sex people and none of them talk to the art people.

KP: And in the same volume you have something on graduate training which also is a very difficult area now. What constitutes any kind of basic training in sociology when there are so many specialties, even within methodology or within theory.

HB: I have a really Deweyish notion of education. I didn't know I did, but it turns out that's what it is. I essentially think you can't teach anybody anything. You can help them learn. If they want to learn and are willing to put the time in, you can help them teach themselves things. My model of teaching is much more like piano lessons, language lessons, where you learn to do something. Helping someone learn how to do something. I don't like to teach a course which doesn't help someone learn to do something. I love teaching fieldwork because, and this is the Deweyish part of it, you get them started and let them get into trouble. Because learning is painful. People do not like to learn new things. They like to do things they know how to do, and the only way you can get them to try anything different is to put them in a position where continuing to do what they know how to do is even more painful than learning. So the way I teach fieldwork is, "Go out and start doing it!" "Well, we don't know what to do." "I don't care. Start, go there, watch people, talk to people, write it all down, then come back." They immediately get themselves into what they regard as terrible trouble. A nice example is when they finally get somebody to agree to be interviewed and then they realize they don't know what to ask them. They hadn't got that far in their thinking. And now they really want to know. Now it's not an academic exercise. What is the right answer? "Listen, I was sitting and this person was waiting for me to interview him and I didn't know a damn thing."

KP: If you adopt that model, how would you get them to do theoretical work?

HB: You require them to make up a theory about something.

KP: Right, so you'd show them the overhead projector and you'd say develop the theory of how that works . . .

HB: I think theory really comes down to a series of activities you can do. Like the trick I describe in *Tricks of the Trade* as Bernie Beck's trick. Bernie was just fabulous with this. Students would come, they'd found this in their fieldwork or whatever and they wanted to know how to generalize it. And their idea of generalizing it would be to go read Weber and find something that they could tack this on to. They'd say, "I'm using Weber." It's a hideous expression: "I'm using Weber, I'm using Durkheim." And Bernie would say, "No, that's not how you generalize your work." Say you found out about something. "OK, Becker, you studied schools"—this is the example I have in the book—"You studied schoolteachers in Chicago and how they dealt with issues of race and class. Now tell me what you found out. But you're not allowed to use the word *Chicago*; you're not allowed to use the word *schoolteacher* or *school*. Now tell me what you've found out." That's a theoretical exercise. My model is really like learning to play the piano. When we play the piano we play scales, we learn to play chords: do this, do that. The doing

is the main thing. Reading about Weber and Durkheim is not doing anything. I mean, it is doing something, but it's not doing what you need to do to get your work done.

KP: It's doing Weber work or Marx work.

HB: Yeah, exactly.

HB: You see, what I think is wrong these days, why despair, is that everything has become so formulaic and so ritualized. You only have to look at the journals. You're a journal editor and you know that the papers you get are written to a template. When I was learning sociology, just after I got out of graduate school, I spent a couple of years among psychologists. The canonical experimental psychology paper is such a formula: theory, problem, subjects, method, results, conclusion. It was just like filling out a form.

KP: Well, a lot of Ph.D.'s in sociology are like that now. Even the Ph.D. student is made to write these silly "literature reviews." And it's called the literature review. And there's no imagination.

HB: Harvey Molotch wrote a wonderful paper called "Going Out." He talks about that, and he says, "They talk about 'the literature,' or even worse now, 'the literatures' in the plural." I mean, what the hell is that? As though the stuff comes in a little box. Here's the literature on the sociology of music, OK. But you know, maybe that's not the relevant stuff to be reading for your study, even though it involves musicians.

KP: I have a colleague, Dennis Marsden, who always says to students read it after you've done the research, don't read it before, because it would kind of cloud it. Then you do need to know what other people have said about it. But you don't need to read it at the beginning.

HB: But there is a weasel word there, Ken, you need to know what other people have said about it. Why?

KP: Well, whatever it is you are studying.

HB: Well, what is it though? Education is a wonderful example. One of Edgar Friendenberg's more snotty and brilliant things was to write about schools as though they were prisons. He looked at a school and said this is a prison in which wonderful young people are imprisoned and prevented from being the wonderful people they might be. It's the same thing Goffman is talking about, total institutions. I mean, there is a dimension of school life which is exactly like prison life. It's not all of it, but it's a very telling comparison. It's very interesting. And the thing for sure that you can't assume is that this particular place is a school. What's going on here? Education. What else? It is a school; I mean, that's what they do. Isn't it? Well, they might, but it's by no means guaranteed. There are plenty of "schools" in which no education, under any construction of that word, is going on, and there are plenty of prisons, conversely, in which a lot of education is going on. The way most of our research is done is we pick a place—this is especially with field research, but it doesn't matter, it can be any kind of research—you pick a place and you say you're going to study that place, as though you know what they did there. But what they do there has to be a finding, you can't assume that you know that. What they do there? You'll find out what they do there, that's

what your research will tell you. I remember reading in the *New York Times* once years ago (which I don't ordinarily read because I don't believe what I read there) that they had busted the warden of a prison somewhere in Soviet Russia because he had turned the prison into a sweater factory. He had had a railroad siding built and they brought wool in there. He had found knitting machines somewhere. The whole damn prison, everybody there, was working making sweaters, boxing them, putting them in railroad cars, sending them out, selling them. Well, is that a prison or is it a sweater factory? If you went to study it, what would you be studying? Supposing you did this study, should you be reading the literature on prisons, or the literature on factories, or what?

KP: This accounts for another feature of your style, because you tend not to go over the literature very much of whatever it is you're studying. You know, you're not a heavy footnote person, are you?

HB: That's another thing that bugs me about contemporary sociology. Every article you read has a list of references from here to the corner, hundreds of references for some article that doesn't need them. It's ridiculous.

KP: It's establishing your credentials in part, isn't it?

HB: Well, I don't know what it is—perhaps. I mean, what it's not doing is any useful thinking work. It's not being helpful about the problem that you study, it just gets in the way. It's mostly totally irrelevant. It's paying homage. Art Stinchcomb wrote this piece, "Should We Honor Our Sociological Fathers and Mothers," about the practice of citation. He lists seven reasons you might cite something, almost none of them appropriate to serious intellectual work. But the journals are so heavily refereed now that this might be necessary. The editor sends a paper, it's pretty standard, to three referees to read. I don't know, you're an editor, how many referees do you send a paper to?

KP: Four.

HB: Four?!

KP: It's because at least one won't bother.

HB: So you get at least three readings, they're surely not going to agree. They're bound to find things where they give divergent advice, and the editor will usually not adjudicate among them. The editor sends back all this stuff and says, "Deal with this." And then you get this kind of patchwork—OK, this paragraph will answer that, that one will answer this one. Pretty soon the paper has no coherence, no intellectual coherence at all. It's just a patchwork—that's what it is.

KP: And the journals are full of these sorts of articles.

HB: Journals are full of them and especially as the market for sociology Ph.D.'s has contracted, creating the anxiety of young folks about, Am I going to get a job? Which is quite realistic and cannot be controlled in any reasonable way, and this goes right back to Malinowski: If you can't do it in a reasonable way, you do magic. So there's an enormous amount of magical thinking and practice. "If I do it the right way it'll get published, so what do they [the editors and referees] want?" Trying to second guess the editors' reviewers gets totally in



the way of any kind of intellectual coherence. I mean, imagine Goffman trying to answer to that kind of stuff.

KP: It wouldn't get published.

HB: He would never get published. Goffman's papers—it's like the experiment of Trollope that I mentioned in *Art Worlds*, where he wrote a couple of stories under another name and sent them to the magazines that published his work routinely. Not a chance. He got letters like, If you keep at it, perhaps in five years.

KP: So what you're saying about the fate of sociology is really the institutionalization of it and the fragmentation of it and its growth which actually was inevitable?

HB: Its growth is largely a function of its growth as a subject for undergraduates to study because that's what makes the jobs for the teachers.

KP: Graduate schools have grown enormously too.

HB: Well, there are Ph.D. centers now.

KP: Ph.D.'s?

HB: Well, yeah, but the Ph.D.'s are people who—they can't all be teaching graduate students, it's like a pyramid scheme.

KP: I wanted to raise another theme that I see in your work. It's really caught in the title *Outsiders*, because I think you're a bit of an outsider—to put it mildly. You started off as an outsider, and now as I am talking to you, you're still an outsider. You've not held, for example, any major posts—you've not been president of the American Sociological Association. You don't go for any of those big glories.

HB: Well, I think that, not just sociologists, but also any respectable intellectual can't afford to sign on with blind loyalty to any organization. It's just giving too much up, because you can't trust an organization. You particularly can't trust universities. I mean universities are—I've always thought that Veblen had his finger on it in *The Higher Learning in America*. Of course, he was excessive as he always was. He may have been quite serious about it. He described the tycoons of erudition and the captains of industry who are financing them. The tycoons of erudition, college presidents and the like, have to make universities the kind of places that the captains of industry want them to be. I mean, it shouldn't be a surprise to sociologists that universities answer to very powerful people and organizations. I don't mean to say that there's this ruling class, all of one mind, but the money's got to come from some place and . . .

KP: Well, it's a slightly different situation in the U.K., of course, because they're not privately run institutions.

HB: No, not when money's coming from the government [laughter].

KP: But in the U.K., we now have the constant preoccupation with auditing everything. I don't know whether it's really taken on here to the same extent.

HB: Well, you know, this is one thing Veblen said: The captains of industry are used to cost accounting. They want to see a quantitative measure of what they're getting for their money. I know the U.K. has gone totally mad about assessment. It's completely nuts. One thing that Anselm Strauss said to me

that stopped me cold was, he said—Anselm was always looking for the key phrase to define someone's work—he said, "You're easy," he said, "It's liberty, freedom, that's what you're interested in." And the more I thought about it, the more I think he was right. That is a preoccupation. And the outsidersness is that I'm very leery, very wary of getting obligated to some institution. A book that I found very interesting and useful, I think about it a lot, is Albert Hirshman's *Exit, Voice and Loyalty*. He's an economist. What it's about is, you belong to an organization. Your organization does something you don't approve of. What are the possibilities? Exit, voice, and loyalty. Exit you quit, voice is you fight within the institution, and loyalty is you shut up and go along with it.

KP: So most of your life I suspect, you've been that "Voice."

HB: Well, I can say very often "Exit."

KP: Now, it's "Exit."

HB: No, I'm very often, you know—places, things I do, like I mean like the ASA, you know, God bless it, a . . .

KP: A big jamboree.

HB: That's what it is. It's not something you'd want to be president of. I mean, seriously, you know, because I saw friends of mine who were. The year Stan Lieberson was president, he's an old buddy of mine, I was on the program committee at Stan's request and, good God, I mean, once you're involved in the day-to-day running of things, it goes on and on . . .

KP: If you've been "exiting," where does that leave your politics over the past fifty years in relation to sociology? I know you've written about radical matters in politics; "hierarchies of credibilities" and the "Gouldner" attack and all that stuff. But have you really been a quietist in the sense of really not being a major activist for large periods of time?

HB: I've never really been politically very active. I suppose I'm a little pessimistic about that. The issue for me that is sort of like the canary in the coal mine; you know, it's the marijuana issue. It's not an important matter, really. It doesn't make any difference if the government make laws against it or doesn't. In the larger scheme, I mean it's not . . .

KP: But it is a good case of your concern with liberties, isn't it?

HB: To me, it's a good case. Also of—what good is it to do scientific research on these matters, because by now there isn't the slightest doubt from any point of view, no reasonable biologist or medical person is going to be willing to get up in public and say this is a dangerous drug. There just isn't any evidence. In this country, almost all the chiefs of police of major cities have signed on as against the marijuana laws, quite a lot of influential people.

KP: It's happening more and more in the U.K. now.

HB: It's happening all over the place. But nothing changes, it's been known by physiologists and people who do that kind of work that this is one of the most benign drugs you could possibly put in your body. It can hardly hurt you. The only way they can kill a laboratory animal with marijuana is to inject so much of it into the poor little rat that it finally bursts its organs. It's just physical. It'll put it to sleep for a while. But it's like a—I remember a

pharmacologist telling me, "It's astounding that a drug that has such obvious psychological activity, has no discernible similar physical activity." It's pretty hard to figure out how it works. It's pretty obvious that it doesn't do any harm. I mean, everybody knows that. But you can't get politicians to change any of the laws regulating it. So when it's that clear cut and there's essentially hardly any big money involved—so there's not even that preventing it—what chance is there for activism about other issues where things matter more to people with more money, more influence. And you know, I watched my friends over the years who were activists and I don't see what they've accomplished that adds up to much. Too bad. The big changes in society don't come about that way.

KP: I'm not sure I agree with you completely. When I think of some, of the lesbian and gay movements or the women's movement, they have brought about quite a few changes.

HB: I would say that they rode in on the back of a gigantic cultural drift that was going in that direction. It's like they took credit for it. I mean that was going to happen, bound to happen.

KP: Interesting.

HB: I can't think that you know, because it's not like there's a riot in a bar in New York and that's why.

KP: No, no, it's not as simple as that, but it kind of gave it the visibility that it needed and it became a potent symbol.

HB: It was like, you know—it was a match. If there hadn't been any undergrowth in the forest, nothing would have happened.

KP: Yes, yes. It didn't come out of the blue, because nothing comes out of the blue.

HB: I think these major cultural changes come about over a much longer term. Hard to see because you don't see it. All of a sudden, one day, it's like somebody says, "Hey, it's bullshit," and everybody looks and says, "Oh yeah, I guess that's right." But it's already happened.

KP: Interesting. That gives your whole theory a kind of much broader sense of cultural change.

HB: Blumer used to talk about this, not very clearly, but he said "cultural drift." A good example of it is what you see in Stanley Lieberson's book about the first names given to children. Where over quite a long period these shifts occur, but nobody is trying to orchestrate it, nobody's proselytizing on behalf of biblical names, and somehow over fifty years, they've become more important, more common.

KP: But this is the background to all your little human beings actively doing things together in small situations, which cumulatively drift into bigger social change.

HB: Yes.

KP: One final thing I would like to talk about—given the popularity of Cultural Studies these days—is your view of culture. I wondered also about how you felt about the rise in cultural studies because you've obviously been working in the field of culture, you actually have an area of cultural studies attached to you in a way, but you're very different from all the cultural studies folk.

HB: Yeah. Cultural studies is a phenomenon of academic politics, much more than an intellectual movement. I have a very simple view of culture. There's really two sources for me. One is William Graham Sumner talking about folkways and mores. When you get a lot of people together and they all have pretty much the same difficulty and they talk to each other, they're very likely to arrive at a solution in common, and begin to treat that as the way we do it. That's culture. That's part of it. The other part is Redfield's definition, which is one of the 153 definitions that Kroeber, Kluckhohn, and Parsons agreed on or whatever. Redfield's is "shared understandings made manifest in act and artifact," I like that. It's an interesting remark and you don't need to add anything to it. Culture to me is, hey, this is the way we do it and I know it, I know that you know it and I know that you know that I know it. So I can act that way with pretty good assurance that when I do, you'll say how right he's doing X, that's the way we do it.

KP: And that's missing from another common definition, which I think is "designs for living." It doesn't have that notion of "sharedness" about it.

HB: Well, the "sharedness" is what makes it work.

KP: It is crucial.

HB: "Sharedness" is what says I can go ahead and act this way knowing that everybody else is going to fall in line. It'll fit. We don't have to sit down and every day begin from the beginning. What sounds shall we use today when we talk? And what shall we have them mean? I can speak to you as I am, and feel more or less that you'll know what I'm talking about. More or less is the qualification.

KP: Yes, but then you have academic cultures, which do the same thing. So if you say cultural studies, you can only understand that if you're a member of that group, but that's a subculture.

HB: My real problem with cultural studies is, and I haven't really done a deep study of this because I have lots to do, is that it struck me as they're not very empirical.

KP: Well they're not. I think you're right. But they are empirical in the sense that they take a film or they take one cultural artifact and then they give it multiple meanings.

HB: There's a tremendous amount that they assert that they don't know to be true. If I say to them I don't believe you, the answer to that is, well you should believe because look, here is the evidence. There isn't any evidence; it's just their interpretations. You see, if you say here is the film, it means this, the first question is, to who does it mean that? And does it mean really that to those people?

I just don't take this stuff too seriously because it's mostly, you know, jerk-off fantasies.

KP: Howard Becker. Thank you.

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### Interviews with Howard S. Becker

Over the years, Howard Becker has been interviewed a number of times. This is a listing of his main interviews.

- "Dialogue with Howard S. Becker." *Issues in Criminology* 5 (Summer 1970):159–79. Reprinted in Howard S. Becker, *Doing Things Together*, 25–46. Evanston: Northwestern University Press, 1986.
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