

able to identify the distinctive features of the best of feminist research” (1987: 1, 3, original emphasis). Similarly, that there are no distinctive positivist methods, or interpretivist methods, or constructivist methods, and so on, though there are quite clearly feminist research *projects*, positivist *projects*, and the like. One methodological distinction that seems worth preserving is that between quantitative and qualitative methods. This division concerns the type of data or level of measurement upon which the method relies (Collier, Seawright, and Brady, 2003). We distinguish between qualitative and quantitative methods because we find it useful to distinguish among the different types of data we collect and analyze. But we do not distinguish between positivist method and (say) feminist method because the difference between positivist projects and feminist ones cannot be grasped through contrasts in method.

In closing, it is worth noting that the distinction between method and methodology helps explain the logic behind the creation of the new Qualitative Methods section of the American Political Science Association (APSA-QM). Some observers have wondered how a new methods section could be justified when the APSA already had a section called “Political Methodology.” The answer lies at least in part in the definitions offered above. The new section has been created around a distinctive set of *methods* dealing centrally with qualitative data. Its focus is not the logic of inference writ large, which is the domain of *methodology*. It is instead the use of procedures and techniques of qualitative empirical analysis that are distinctive and numerous enough to warrant study outside the sphere of large-N quantitative research. Members of APSA-QM will of course take up methodological issues. They cannot talk sensibly and systematically about qualitative methods without doing so. But the section is not organized around a qualitative *methodology*. Political methodology covers the domain of qualitative research as much as it does that of quantitative analysis.³

References

- Almond, Gabriel A. and Stephen J. Genco. 1977. “Clouds, Clocks, and the Study of Politics.” *World Politics* 29:4 (July) 489-522.
- Apel, Karl-Otto. 1984. *Understanding and Explanation: A Transcendental-Pragmatic Perspective*. Trans. Georgia Warnke. Cambridge: MIT Press.
- Axelrod, Robert. 1997. *The Complexity of Cooperation: Agent-Based Models of Competition and Collaboration*. Princeton: Princeton University Press.
- Geertz, Clifford. 1973. “Thick Description: Toward an Interpretive Theory of Culture.” in Geertz, *The Interpretation of Cultures*. New York: Basic Books. 3-30
- Harding, Sandra. 1987. “Introduction: Is There a Feminist Method?” in Harding, ed. *Feminism and Methodology*. Milton Keynes: Open University Press. 1-14
- King, Gary, Robert O. Keohane, and Sidney Verba. 1994. *Designing Social Inquiry*. Princeton: Princeton University Press.
- Kolakowski, Leszek. 1972. *Positivist Philosophy: From Hume to the Vienna Circle*. Harmondsworth: Penguin.
- Kratochwil, Friedrich and John Gerard Ruggie. 1986. “International Organization: A State of the Art on an Art of the State.” *International Organization* 40:4 (Autumn) 753-75.

- Lave, Charles A. and James G. March. 1993. *An Introduction to Models in the Social Sciences*. Lanham, MD: University Press of America.
- Moon, J. Donald. 1975. “The Logic of Political Inquiry: A Synthesis of Opposed Perspectives.” in Fred I. Greenstein and Nelson W. Polsby, eds. *Political Science: Scope and Theory. Handbook of Political Science*. Volume 1. Menlo Park, CA: Addison-Wesley. 131-228
- Rabinow, Paul and William M. Sullivan, eds. 1979. *Interpretive Social Science: A Reader*. Berkeley: University of California Press.
- Skinner, Quentin. 1972. “‘Social Meaning’ and the Explanation of Social Action.” In Peter Laslett, W.G. Runciman and Quentin Skinner, eds. *Philosophy, Politics, and Society*. Oxford: Basil Blackwell. 136-57
- Taylor, Charles. 1979. “Interpretation and the Sciences of Man.” in Paul Rabinow and William M. Sullivan, eds., *Interpretive Social Science: A Reader*. Berkeley: University of California Press. 25-71
- von Wright, Georg Henrik. 1971. *Explanation and Understanding*. Ithaca: Cornell University Press.
- Weber, Max. 1947. *The Theory of Social and Economic Organization*. Trans. A.M. Henderson and Talcott Parsons. Glencoe: The Free Press.
- Winch, Peter. 1958. *The Idea of a Social Science and Its Relation to Philosophy*. London: Routledge & Kegan Paul.
- Wohlfloth, William C. 1998. “Reality Check: Revising Theories of International Politics in Response to the End of the Cold War.” *World Politics* 50:4 (July) 650-80.

Endnotes

¹Some of the more important works on interpretivism are Winch (1958), Taylor (1971), Skinner (1972), and Rabinow and Sullivan (1979).

²Definitions in the Oxford English Dictionary suggest the close relationship of epistemology and methodology. The OED defines methodology as “[t]he science of method. . . Also, the study of the direction and implications of empirical research, or of the suitability of the techniques employed in it.” Epistemology is defined as “[t]he theory or science of the method or grounds of knowledge.”

³The problem here may lie simply in the incompleteness of the section names. If the first section were named “Political Methodology and Quantitative Methods” and the second called “Political Methodology and Qualitative Methods,” the logic of their fit might be clearer.

Interview with Clifford Geertz

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Clifford Geertz is Professor Emeritus of Social Science, Institute for Advanced Study, Princeton New Jersey. John Gerring was a Member of the School of Social Science at the Institute, 2002-03. This interview was conducted on July 25, 2003, at the Institute. Transcription by Jennifer Jefferis; editing by John Gerring.

Gerring: It is perhaps fair to say that the quantitative side of political science has taken most of its cues from economics, while the qualitative side of the discipline has taken its cues from anthropology and history. For the latter group, there is

no tradition more esteemed than interpretivism, and there is no one more esteemed within the interpretivist tradition than you. Your work is assigned routinely in courses and referenced continually in the political science literature. So it is of enormous interest to the discipline, what you think about all this. The question we're struggling with is how to understand interpretivism in the context of political science and the social sciences more generally. That's the question I will be circling around one way or another as we go through this interview.

I'll say one other thing by way of preface. As the discipline of political science has become more self-consciously scientific, more quantitative, and so forth, I think there is a sense on the part of people, even people who do quantitative work, that something has been lost in the process. But I don't think we're very clear on what exactly it is. Let's start on this question, then. You've lived through a lot of the history. It seems to me that there was a time back in the fifties and sixties when the different social sciences really were talking to each other quite frequently. The SSRC served as a focus for much of this interdisciplinary discussion, which I believe you were a part of . . .

Geertz: I think the inter-disciplinarity of the 1940s, 1950s, and 1960s owed a great deal to the war-time experience. During the war (I was just an ordinary seaman in the Navy), most of the faculty of the social sciences – the leading people, almost to a man and woman — were involved in one capacity or another. And so for the first time they were thrown together. This was particularly dramatic for anthropologists, because they were thrown in with political scientists and economists and engineers and everyone else. You would get someone working on propaganda, or some other thing involving the war effort, and it would be an anthropologist, a sociologist, a psychoanalyst, you know. And for anthropologists, particularly, that was a radical change, because before that it was a very self-enclosed discipline concerning tribes and pre-history and so forth. All of a sudden, it was this general ferment. The war was short, three years or so for most people, so they came back and this experience changed their whole notion of what social science ought to be about. It was a very lively period, and I went to Harvard just as everyone came back to the academy. Interdisciplinarity was blooming forth all over. This was also the time when the big foundations – Ford, Rockefeller, Carnegie, and so forth – were beginning to support social science on a major scale. SSRC was part of that but was not the only center of it. So this led to a tremendous concern with team research and the task of relating quantitative and qualitative work.

The Social Relations department at Harvard is a good example of this. Sam Stouffer was there, along with Talcott Parsons, Harold Lasswell, Clyde Kluckhohn, Jakobson, and many others. Norbert Weiner was down the street at MIT. I don't say there was no abrasiveness, but they were all in the same business. People were interacting, and it transformed the social sciences. At that point, everything was mixed together. Not that individuals didn't go off every once in a while, but they could also get along. It was a period of great opti-

mism. Social science was about to begin. And a great deal of it was interdisciplinary research. So it was a great period. And that's where I was formed. I came in through philosophy and the humanities. There was plenty of room for that sort of thing at the time.

Later on, when I moved to Chicago, the interdisciplinary, inter-method, conversation continued. David Easton was there, David Apter, Leo Strauss, Hans Morgenthau, the Rudolfs. They were all prima donnas. Everybody was doing his or her stuff. In fact, it was not so bad. They got along in a way because they did not have to deal with one another very often. The department was entirely run by a woman who was secretary of the department. She gave out the grants, and she just made sure that all the prima donnas got their share of the loot. And the prima donnas just taunted and held forth, and they were big figures.

Gerring: It was a heroic time. I'm very fascinated by that moment in time. It just seems so . . . I don't know, so rich, so fertile in many ways compared with the segmentation of disciplines and sub-disciplines today. I'm curious whether you think that that moment of ferment was made possible by an underlying theoretical consensus, e.g., the theory of modernization, or something else.

Geertz: Parsons of course always wanted to have a general theory, but I don't really know how much it animated the others. I think it had more to do with the war-time experience of working together. Academic life before the war, long before your time, was a very protected environment. Scholars did their work without being really in the world. The war brought them into the world and they never left it after that. You could no longer just sit in an office and know all there was to know about the Navajo.

Gerring: Subsequently, in the 1960s, it does seem to me that part of what happened was a de-legitimation of social science and a suspicion of the heretofore rather tight connection between government and the academy.

Geertz: Some of that is true, and the Cold War and Vietnam really did that. But it didn't lead back to the old system. People didn't retreat back to the ivory tower. Instead, we are faced with the question of scientism, which is another issue altogether, about which I don't quite know what to say. People had always thought of themselves as scientists, but they had a rather capacious notion of science. Now the difference between people is not whether they think they're scientists or not, but what they think science is. To some people it's only statistical tests, and that's it. For other people, anything is science. Just go out and talk to people and come back and say what they said; that's science. So the whole conception of what science is became problematic. Then you get to the 1970s and you get post-modernism and all that. By then the American anthropologists have 86 sections, psychology has 105 — it's total fragmentation, except that everybody doesn't stay in the fragments. I mean there's almost no anthropologist that is

only an anthropologist. People like myself, Mary Douglas, we do all sorts of things. Anybody in every field is sort of all-purpose. It's very rare that you find someone that's just in one of those little chapters.

Gerring: I think that's true. However, by reference to the past, the career paths that you find to day are relatively narrow. Let's talk a little about interpretivism. This label, which now is very common, is actually fairly new, isn't it?

Geertz: Well, I myself never talk about interpretivism, but that's all right, it doesn't matter. It's hermeneutics that we're really talking about. That's what it comes out of. I think the clearest statement is Charles Taylor's, in a famous essay in the *Review of Metaphysics* ["Interpretation and the Sciences of Man," *Review of Metaphysics*, 25 (1971), 3-51] in which he talks about the tradition of biblical interpretation. As Chuck says in this piece, the hermeneutic approach starts with the notion of something as unclear and tries to bring it into clarity. When I work in the field on anything, whether it's something sort of airy-fairy like religion or something more concrete like a market, I start with the notion that I don't understand it. Then, I try to understand it better by tacking back and forth between large and little things. And that's what you really do when you "interpret." It is a little like when you learn a language that you don't know. During the first few days you get a few sentences and that's about it. As you talk, get corrected, and correct others, you really begin to get into it, until eventually you master the language. And the same thing is true when you're trying to understand foreign enterprises.

I didn't understand why people were so fascinated with cockfights in Bali, because they're really rather boring to a Westerner. It's just two chickens pecking at each other. And you have eight fights in an afternoon everyday. It's one of those situations where if you've seen one you've seen them all. The cocks just go at it, eventually one is dead, and there's blood all over the place. It's exciting for the first two or three times maybe. But why are these people so absorbed in it? So the question that somebody like me asks, whether I'm an interpretivist or whatever the heck I am, is *What is going on here?* Something is going on here that I don't see, that I can't understand. What sort of story can I tell about this apparently meaningless activity? Well, perhaps this has to do with masculine competition, and so forth. That becomes the model. It's the same process whether one is working with a marketplace, a ritual, or family life. The notion is that you start with something about which you have a slight grasp, like you do with a foreign language, but you really don't understand it.

I studied a number of languages and the two that I think about all the time are Javanese and Arabic. They're really quite different experiences. To make it simple-minded, if you're learning Indonesian, it gives the impression of being easy. In a couple of lessons you can go out in the street and begin to talk. But it's a very subtle language and a lot of people who think they speak it well do not. They don't understand what is really going on. Arabic is the opposite. It's so morphologically complex that although there are almost no irregu-

larities you can't really say anything for the first year because you always get it wrong and it doesn't make any sense. However, anyone who has the patience to learn to speak Arabic correctly speaks it pretty well by the time they actually get to that point. Whereas a lot of people in Indonesian never get beyond the primary stage because they don't realize they're not very good at it. The point is, these are two different experiences. However, in both situations you're starting with something you don't understand and slowly trying to learn to speak. That's the model I think of as emblematic of what you call interpretivism.

Gerring: So, interpreting an action or a set of events is like learning a language in a sense. Of course there is always the question of whether you know how to speak it or not and that's . . .

Geertz: The way you learn how to speak it or not is also whether you can *communicate*, whether when you speak, people understand. It takes a long time to be able to tell a joke in Arabic, and if you get a laugh, well . . . And the same things true about working with cockfights. If you really can act so that you get intelligible responses from your informants; if you say things about the cockfight or the market or the ritual that they regard as intelligent, then you are obviously beginning to get a hold of it.

Gerring: I think telling a successful joke would be a good example of a fusion of horizons, in Gadamer's sense. So here's a question related to that. I think it's true that when we think about interpretivism we often think of rendering the exotic familiar, or making sense of something that is ostensibly nonsensical. How does interpretivism deal with situations that seem commonsensical? I'm thinking about the question of what a political anthropology of American politics might look like.

Geertz: I was just reading a book by Sherry Ortner, an anthropologist, who worked on the graduating class of 1958 [*New Jersey Dreaming: Capital, Culture, and the Class of '58*, Duke University Press, 2003]. I myself have never worked in the United States. There's no question that it's harder. You take so much for granted when you study your own kind. You have to *de-familiarize* yourself. You have to get the distance. You have to realize that you *don't* understand. It's obviously hard to put yourself in the frame of "I don't get this." But otherwise there's no real difference. It's just that, somehow, you have to artificially make it strange. For political scientists, I think they just have to learn how to see, not the mystification of reality, but mystery in the sense that there's a lot more there than ordinary concepts might suggest. I mean you can just read the papers and realize that.

Gerring: What do you find left out of standard social scientific accounts of contemporary politics that a more anthropological effort might illuminate?

Geertz: One example of this Putnam's book on Italy, where he talks about civic traditions [*Making Democracy Work: Civic Traditions in Modern Italy*, Princeton University Press, 1993]. He is after something that most political scientists don't even think about. I'm afraid that "political anthropology" doesn't produce an image in my mind of anything in particular.

Gerring: Yet, the appeal of Putnam to political science, and to sociology and economics, is the general theory of social capital. It's not what he has to say about Emilia Romano. I raise that only because it raises a question that we've talked about before, you and I, about whether or to what extent an ethnographic/anthropological/interpretive approach generalizes, wants to generalize, can generalize — or whether it's more of a differentiating art.

Geertz: I don't think that's the opposition I would construct. It isn't a matter of whether you're just going to do particular studies or general studies. It doesn't work that way, at least for me. The enterprise of social science is inherently comparative. What you learn about one case you then try to look at another to illuminate both the differences and similarities. Let's say you're interested in bazaar-like markets, which I first studied in Morocco. Now, there's a whole range of studies of markets around the world, including Ted Bestor's recent work on fish markets in Japan [*Tokyo's Marketplace: Culture and Trade in the Tsukiji Wholesale Market*, University of California Press, forthcoming]. So there's a beginning of a critical mass of studies of a general topic in particular settings — a big fish market in Tokyo, a clothing market in a small town in Morocco, and so forth. These are not going to be exactly the same, but they are very similar and they have to do with communication and the symmetry of markets. (I could go on for hours, there's a whole theory on this.) As these cases are written, one begins to see similarities and differences among markets and it deepens one's ability to grasp the one situation one started out looking at. I think Ted would say he'd learned something from reading my stuff on Morocco, and I have certainly learned something new from him. You don't start out with some general abstract theory of markets (or stratification) that holds everywhere. What you get is a richer picture of the variations and the similarities, the continuities and the differences, and some principles of markets and stratification systems that, even if they don't apply everywhere, apply often enough to be of some use. You don't have to explain everything to explain anything.

Gerring: What would be a good example of a generalization or a principle of that nature?

Geertz: Well, again we come back to the market example. I tried to argue that there's a difference between a market search across a firm economy and in a bazaar. In the latter, the action is not between buyers or between sellers. It's between a buyer and a seller who bargain with each other. In a firm economy, that's not the case. You have advertising, you have set prices and the competition comes between the producers. So, the

difference is that the information problem is different. The search problem is different. You have an intensive search rather than an extensive search. The used car market is like a bazaar. You have to know a lot about the car and the guy that sold it to you, because you have to pursue that particular case. If you're buying a new refrigerator, a washing machine, or a box of cereal in a firm economy you simply compare prices, look it up in consumer digest and see which one's the best, and you have advertising and the prices are the same. It's not negotiated. To go and buy breakfast in Morocco means a trip to the market and negotiating with the negotiating party.

Gerring: So there is a general phenomenon which is instantiated in different ways across cultures. Let's move onto a question that is often raised in the context of interpretivism, the question of causal explanation. You know it's sometimes said that interpretivism is about describing things, or offering interpretations, which is a certain kind of explanation, but not causal explanation.

Geertz: This presupposes certain philosophical conclusions about what causation means. That aside, if you get interpretation right, I believe the causes will fall out. If you understand the cockfight, you'll understand why people are engaging in it, why things are happening the way they are happening. It must be clear, of course, whether you're talking about a cause or a causal law. There's a big difference. Everything is caused. On the molar level there is no uncaused behavior. If I look at the cockfight and something happens, I don't say "There's no cause for this, it just happened." I don't write that way, and no one really writes that way. So that isn't what we're doing. But the question about causal laws gets more complicated. There's one issue concerning the difference between causality and determinism. If you are familiar with Elizabeth Anscombe's work, you will understand that the search for causes is close to detective work. You come in and you find the pitcher has fallen to the floor and there's glass. Did the cat push it, or did the wind blow it over? The one thing you know is that there is a cause. It's on the floor and there's milk all over. But whether the cat did it or the wind did it, or you put it down in a way that made it tumble later on — there are evidently lots of possibilities. The point is, you need to have the story of what happened. But you don't have to have a causal law. There's no causal law that cats tip over milk.

To be sure, you can correlate behavior. But this doesn't usually get you very far. An interpretivist tends not to ask that sort of question first. One is trying to get a story, a meaning frame to provide an understanding of what is going on. You want to understand what it is that's motivating people, or cats, to do these (unaccountable) things. So we look for a motive and feelings and emotions and ideas and concepts and all that jazz, which you don't need to do if you take 70,000 people and see how their movements correlate with each other. I mean you could do that for traffic flows. I'm not denying that this can be done, or that it is not useful to do it, if you really want to figure out the traffic flows in New York. I don't think you should spend a lot of time asking each individual driver what

they were doing. It might help to give some understanding but, in general, I would agree the way to do it is to pick a place and measure the number of cars that go by and correlate it with the time of day and find out how the traffic flow works. So it's not an opposition of that sort – correlation versus causation. It's just that a sheer correlation between two people's behavior is not usually very interesting.

Gerring: Let me move on to a much bigger question. Can't we all live together? I mean, those who do interpretive work, and those who do a different kind of work. I'm not sure how this works in anthropology, but in our field people from different methodological fields are oftentimes at each other's throats.

Geertz: Yeah, that's what this school (the School of Social Science) is dedicated to overcome. I think there has grown a kind of militancy in the social sciences. You see it in the evolutionary psychologists, you see it in the evolution theorists, you see it in economics. Here, people adopt a position and try to take things over. Once you get any group of people who eliminate everything else then it polarizes the situation, the opposition organizes, and you get civil wars. I would say you need a historical interpretivist approach to understand why it is there has been this methodological movement and where it comes from. My own sense is that it comes from a kind of utopian vision of science — that somehow science is about to begin and they're going to finally set it all straight. Obviously, I'm not persuaded by this, but I don't try to stop them from doing it. They try to stop everybody else — well, at any rate, some of them do. (A lot of them are really quite tolerant.) As a friend of mine says, the problem with the rational choice people is not what they do but that they practice it inside departments. I mean they just choose their own people and they make rational choices in terms of aggrandizing their own power, and I think that's bad for academia, regardless of the field. I have some questions about rational choice theory (game theory), but I acknowledge its place in the academy. We have lots of people who do that sort of work here. But to say, "If you're not doing game theory you're not doing science," well that tends to make people unhappy. I don't think it's necessarily that much worse than in the past, but there has been a kind of militancy in the last ten years or so that I don't fully understand the reason for. The level of tolerance has declined a bit. I'm talking about scientism, science as an ideology. It's still not clear what happens to turn good scientists into scientistics.

Gerring: Let me ask you one more question. This is a very open ended question and you may answer of course in any way that you wish. This newsletter is written primarily for political scientists and I'm wondering if you have any advice or thoughts on the study of politics.

Geertz: Well I've done a lot of study on it. I've written a book and I'm about to write another one. I think this is, again, a question of scientism. I would suggest that at least some

people in political science ought to get away from toy problems and start addressing real ones. For example, can there be a functioning multiethnic state such as India or Nigeria? What do we know about this from 50 years of experience with extremist states? Certainly, not as much as we ought to. Political scientists should engage problems that are there. I don't mean practical problems in the sense that one should take up social work. But they ought to take problems as they come from the world. Take Adam Ashforth's recent work as an example [Ashforth, a visiting member at the School of Social Science, is at work on a book on witchcraft in southern Africa]. He goes to South Africa and finds that people have been driven to witchcraft everywhere — including Mandela, Tutu, and the politics of AIDS — and he attacks that problem. I would suggest at least some of your people begin to do that more than they have. This means you have to be able to accept a lot more ambiguity, a lot more uncertainty. You never know quite what you'll find and when you'll be entirely wrong about what you thought was true. It is hard. It's more complicated. There are no ready devices off the shelf that you can use. You have to make them up for yourself. You have to try to interpret the evidence, to understand what the hell they're saying when they talk about witchcraft. But that's the advice I would give, to engage with the political world that one confronts. That's what Putnam did [in Italy], and [Robert] Dahl did in New Haven. They tried to talk about what's going on there. It doesn't mean that none of the sharpened tools of social science won't be useful, but I am always more concerned about arbitrarily simplified accounts that make it possible for me to show the exercising of some particular methodological skill. I think that doing something because you can do it or because there's a technique for it or a program for it — this strikes me as a very bad way to spend a life.

Book Notes

Note: Jennifer Jefferis compiled this list of books from a variety of databases. Descriptions were drawn from jacket blurbs and publisher's web sites. We tried hard to find those books (published after January 2000, our arbitrary start-date) that had interesting methodological approaches or explicit commentary on methodological issues — excluding qualitative methods textbooks (covered in the last issue) and books devoted to purely statistical issues. We are quite sure that we missed many valuable contributions to the broadly defined genre of qualitative methods. Therefore, we beseech readers to send us suggestions of additional books that we can include in future book notes (self-nominations are welcome).

Abbott, Andrew. 2001. *Time Matters: On Theory and Method*. Chicago: University of Chicago Press. \$25.00

This text focuses particularly on questions of time, events and causality. The author grounds each essay in straightforward examinations of social scientific analysis.