SHOULD SOCIOLOGISTS FORGET THEIR MOTHERS AND FATHERS*

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Sociological classics serve six distinct functions: (1) touchstones, examples of beautiful and possible ways of doing scientific work, (2) developmental tasks to induce complexity of mind in a student, to replace the clichés of Sociology 1, (3) intellectual badges for the first footnotes of a paper to identify broad features of a style of work, (4) sources of fundamental ideas, root concerns of sociology, (5) routine science, as sources of puzzles and hypotheses for empirical work, (6) rituals to express the solidarity and common concerns of sociology as a discipline. Much confusion about the roles of classic books in the education of sociologists stems from confusion of these functions, and particularly from not noticing that a classic that fails at one of the functions may serve well at another.

The Uses of Classic Books or Papers

I would like to discuss separately a number of uses of the classics. It is quite possible, for example, that one would not extract hypotheses about Australian religion from Durkheim's *Elementary Forms . . .*, and yet might read it for some other purpose. Let me specify these functions with the catchwords: (1) touchstones, (2) developmental tasks, (3) intellectual small coinage, (4) fundamental ideas, (5) routine science, (6) rituals. Let me specify briefly what I mean by each before analyzing them separately.

By a "touchstone" function I mean the sort of thing Claude Levi-Strauss was talking about in his autobiography when he said he read a few pages of *The 18th Brumaire* before sitting down to write something himself. *The 18th Brumaire* was an example of excellence, showing the way a sociological study ought to sound.

I used to advise students to think of ten books in sociology they would most like to have written, then to analyze those ten to figure out what virtues they would have to develop in order to do the kind of work they admired. Classics as models of good work is the original sense of Thomas Kuhn's much-abused notion of a "paradigm." A paradigm is a case of a beautiful *and* possible way of doing one's scientific work. A touchstone then is a concrete example of the virtues a scientific work might have, in a combination that shows what work should look like in order to contribute to the discipline.

By a "developmental task" I mean that advanced students need something more complicated than the clichés of elementary textbooks, in order to persuade them to make their minds more complex. For example, before people are ready to tackle the question of what is the most strategic way to study how spiritual goals affect earthly goals, they need to have gotten used to thinking that people can want spiritual objectives in different ways. Reading Weber's *Protestant Ethic . . .* will not teach graduate students much about the causes of capitalism, because they rarely know enough economic history to have any judgment of their own. But the notion that how one pursues salvation may affect how one pursues savings is a source of complexity of thought. I might mention here that the fashion of giving people just enough of Weber so that they come away with the notion, "Religion is also important," undermines rather than encourages this mind-complexifying function.

The "small coinage" function is that of using a few citations to the appropriate literature to indicate generally in what tradition one is working. A paper may have a general, innocuous title, mainly consisting of the word "deviance," for example. (There are some half dozen sections at the annual sociological meetings with such a title, and a stranger cannot tell them apart.) But if the first footnote to a

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paper with a vague title cites Parsons, or George Herbert Mead, or Thrasher, or Wolfgang, or R. D. Laing, or Lemert, one soon knows what general sort of beast is being tracked on this particular hunt. Our mystification with the half dozen sections on deviance could be cleared up if the title of the section had to give two references in a footnote. Classics, then, serve shorthand functions, for communicating with knowledgeable people what sort of thing one is up to, and, therefore, what standards should be applied. No doubt some of the work that cites Laing would be improved by Wolfgang's cohort analysis, but one would only write that on a referee's report for a journal for the sake of contentiousness. For the small coinage function one wants simplification, so the little snippet which says "George Herbert Mead is interested in definitions of the self" is good enough. If we really kept in mind *all* of *Mind, Self and Society*, it might indicate that Mead also was interested in gangs, like Thrasher, and in the relation of means and ends like Parsons, and so on.

The fourth function, "fundamental ideas," is the one we usually emphasize in theory courses. It is this which explains the Coles's finding that heavily cited papers in the real sciences are more likely themselves to cite heavily cited papers, and the classics to cite other classics, than are lesser papers by the same distinguished authors. If in a paper one modifies an idea nearer to the main trunk of a science, one is more likely to be addressing questions that the great minds of the past also have addressed, and to find their orientation useful. In the case of Einstein's first paper on relativity, this tendency went so far that Einstein simply ignored experimental results that flatly contradicted his theory, because they made it all too messy. (It turned out some years later that the experimental results were the result of a leak in the experimental instrument, but Einstein didn't know that.) Einstein wanted to show how Newton and Lorenz could be unified, not how some messy little fact could be explained. In this case we praise the classics for being both unique and fundamental, rather than for being fine work as in the touchstone function, for being complex as in the developmental tasks function, or for being symbols with agreed-on meanings for the small coinage function.

The "routine science" function of classics is the same as the routine science function of ordinary papers and books. Besides being a touchstone of quantitative reasoning, more complex than Soc. 1, small coinage to show one is a pure sociologist, and a source of fundamental thought on how normlessness works, Durkheim's *Suicide* also has a bunch of hypotheses about suicide. One could easily imagine asking whether crack troops in the Israeli army kill themselves more than reserves, the same way it happened in France and Italy. One can think of a lot of differences in what it means to be an elite soldier in the two circumstances that might alter the self-destructive propensity. Classic scientists could usually still get promoted nowadays for their routine science. When Marx, for example, tells us about how piecework wages work, we still imagine he could teach a lot of industrial sociologists something. It is this function which accounts for the famous advice, I believe of Thurstone, that if you wanted to write a classic you should build into the center of it a fundamental, but subtle, flaw. Then hordes of graduate students for generations would write dissertations refuting it, and some of them would find out new things to contribute to the discipline. Only if the classic also serves as a source of puzzles for daily scientific work would this advice be true.

The "ritual function" of classical writers is typified by the advice Jim Davis used to give to graduate students, that they had to find a dead German who said it first before they could publish a finding (positive or negative) on the subject. We define what holds us together as sociologists in part by having a common history. So ritual myths about Max Weber's staring at a wall in nervous prostration, Georg Simmel being kept from a professorship for being Jewish, Thorstein Veblen refusing the Presidency of the Economics Association because it wasn't offered when he needed it (it is perhaps worthwhile to point out that he wasn't offered the Presidency of the Sociological Society), Parsons's dis-

sertation on some obscure German's ideas about capitalism, all serve the functions that the cherry tree and the Gettysburg address written on the back of an envelope do in American history. And like the cherry tree and envelope myths, the fact that I don't really know whether any of them are true indicates less about the quality of my scholarship than it does about the ritual function of these classics.

So what I propose is that the question of the uses of the classics is really six questions, which all can have separate and contradictory answers. We can ask: (1) Are old models of excellence in sociological craftsmanship still close enough to what we do, so that *The 18th Brumaire,* for example, can show us what political sociology really should look like? (2) Are classics of sociology tough enough for advanced students to cut their teeth on, to replace clichés in the student's mind with complex and flexible patterns of thought, or is their complexity too obscure, too irrelevant to the science as it is practiced, to be useful? (3) Are the classic symbols of style of work in sociology really producers of sectarianism rather than division of labor—does the use of classics as small change debase the currency, so we fight over simple symbols rather than the real intellectual issues? (4) Is there still creative theoretical work to be done in developing the fundamental ideas of a Tocqueville, a Trotsky, a Weber, or even maybe Durkheim? (5) Is there a fund of unexplored important hypotheses in the classics to turn graduate students loose on, or to fill out the first paragraphs of empirical papers in *ASR, AJS, Social Forces,* and *Social Problems?* (6) Does the fact that you and I cite the same dead Germans (namely the ones who have been translated) hold us together in a common solidarity so that we can monopolize jobs in sociology departments, make people pay their dues to ASA in order to come to the convention to give papers to each other, and otherwise to serve as an intellectual community for each other?

The Touchstone Function

If one is looking just for intellectual excellence, to remind oneself or students what the real thing looks like, I think one does as well with Erving Goffman as with Georg Simmel, with Paul Veyne's *Le pain et le cirque* on ancient patterns of charity as with Max Weber's *The Protestant Ethic,* with Lipset, Trow, and Coleman's *Union Democracy* as with Emile Durkheim's *Suicide*, with Immanuel Wallerstein's Volume I of sixteenth century history as with Volume I of Pitrim Sorokin's *Social and Cultural Dynamics*.

That is, there are several ways of being excellent in sociology, from exact description of interpersonal processes in Goffman and Simmel, brilliantly sharp theory illuminating rather disjoint historical processes in Veyne and Weber, quantitative exploration of social psychological processes producing structural patterns in *Union Democracy* and *Suicide,* and massive learning held together with a suspicious theoretical superstructure in Wallerstein and Sorokin. The scale is one of excellence of a particular kind, rather than one of historical origins.

The only reason we tend to use older works as touchstones of excellence is that our geniuses are rare, and have to be made to last at least until we get the next one. Now that we have Paul Veyne, I guess it is O.K. to forget Max Weber, at least for those who read French. But in the meantime we needed an example of theoretical precision and fertility in a disorganized field of historical particularity: the particularity disappears into a simplified theory in Durkheim's historical work; and the theoretical precision in Sorokin is not what I would advocate imitating. That is, the touchstones of one kind of excellence cannot serve for the other kinds, so not every contemporary genius replaces all the originals. Even if we all agree that Erving Goffman is genius enough so that it doesn't matter whether we read Simmel or Goffman, we all, I believe, have the intuition that it matters whether we read Goffman or Weber. Similarly it matters among contemporaries; it matters whether we read Paul Veyne or Erving Goffman.

But what exactly is the function of those touchstones? When Claude Levi-Strauss says in his autobiography that he reads a few pages of *The 18th Brumaire* before sitting down to write, it clearly is not to

derive hypotheses from Marx's theory. Hardly anyone who reads Simmel's essay on the stranger then writes a survey questionnaire about the last five strangers you've met. The touchstone function is to furnish the mind with intellectual standards, not to furnish it with hypotheses.

I believe that the reason we need such touchstones is that first class science functions with aesthetic standards as well as with logical and empirical standards. These standards are not defensible by the positivist or the Marxist or the symbolic interactionist philosophies of science. No philosophy of science tells you where the chill of excitement at the beauty of the thing comes from. We may not ourselves know how to produce the beauty we admire, which is why we cannot really write a philosophy of science to tell us why Weber was great, Sombart only first class. But if we embed the examples of excellence in our minds, as concrete manifestations of aesthetic principles we want to respect in our own work, and use them as touchstones to filter out that part we throw away and that part we keep, we may very well manage to work at a level higher than we can teach. For we work by the standards embedded in the touchstone, standards we cannot formulate but can perceive if we use a paired comparison—is this piece as good as Simmel?

And if we cannot formulate and teach the aesthetic principles embedded in the touchstones, we can at least expose students to a leisurely inspection of what constitutes excellence. If students are exposed to *The Protestant Ethic* only as a causal problem: "Which came first, capitalism or Calvinism?" they are deprived of its main value. As a hypothesis it's kind of dull, and probably wrong. But as a piece of work it is beautiful. If we can persuade students to combine good philosophy of science (so our hypotheses will be true) and the standards of intellectual beauty of *The Protestant Ethic*, we will have taught them to do better than ourselves.

Let me now make a brief aside on conflicting aesthetic principles. I was criticized recently by Wally Goldfrank for not writing a conclusion to my book, *Theoretical Methods in Social History*, and by Theda Skocpol for being a Weberian. Both of these are true observations, and I would like to argue that they are connected. Let me use the touchstone method to analyze a bit why Weber and I do not write conclusions. A conclusion is a short essay version of the meaning of world history as a whole, which Sorokin and Wallerstein, for example, write, while Weber does not. I really agree with the Sorokin/Wallerstein aesthetic principle at stake here; if you really understand something you should be able to state the central thesis in a sentence, or if you are a little more prolix, in a short essay on altruism, or on the world system. I have affirmed that aesthetic principle, by quoting Selznick to that effect, in the preface to my *Constructing Social Theories*.

But obviously I do not follow it as well as Sorokin or Wallerstein do. And while Weber had the excuse of dying before he was finished, I dare any of you to draft a conclusion for *Economy and Society*. While I am well aware of how far I fall short of Weber's standard, I would like to argue that I fall short along the same dimension, and that Sorokin and Wallerstein are working with a different aesthetic standard, along a different dimension.

No doubt it would be more satisfactory if we had a short essay of what *Economy and Society* all added up to. Parsons and Bendix have tried to write such essays, and it is an illuminating fact *both* that they felt pushed to do so, *and* that they wrote two completely different essays, Bendix on authority and Parsons on values. Roth wrote still a third essay as an introduction to the English translation, in which the book was mainly an essay on the nature of constitutional law, and I wrote a brief review essay in *AJS* saying it was a book about historical approximations to the assumptions of classical economics. So we have at least four radically different summary chapters for *Economy and Society*. This shows that even Weberians, in their weak moments at least, respect the aesthetic impulse that lead Sorokin and Wallerstein to give brief summaries of world history. Note however that nothing in any of our philosophies of science leads us to expect that history can be summarized in

a few principles, with the possible exception of Marxism.

But I would argue that the summarizing standard really is inappropriate to the material we are working with. If Sorokin and Wallerstein were as lucky as the Greek sculptors, and had the gilding and paint of the grand theory washed off by centuries as the gilding and paint were washed off the statues, we would be left with their massive scholarship. As it is, poor Sorokin is remembered for a rather foolish summary about cycles of values, and I suppose Wallerstein, an equal time after his retirement, will be reduced to a slogan about world systems. Because they wrote their summaries themselves, there will not be four different ones.

And the reason is that all of world history, or even all the origin of capitalism, really cannot be summarized in twenty pages or four graphs. If you try, you get your book translated into lots of languages quickly, and then you are forgotten. I believe the forgetting is unfair, because the detailed interpretation of masses of evidence gets lost as well. But it comes from following an aesthetic standard, that of writing conclusions to historical works, which is inherently unattainable. And like the gilt and paint on Greek statues, a conclusion ruins the beauty of good historical work.

My general point here is that this all has nothing to do with any substantive disagreement between Weber and Wallerstein. I see nothing in Wallerstein that contradicts Weber, though I suppose if Weber addressed the question he would give a different interpretation of the failure of North Italian capitalism to industrialize. Instead, the difference is in where they locate the "Aha!" experience, the feeling of aesthetic completion. Weber locates it in clean analyses of historical configurations, Wallerstein in summaries of the main historical drift of a given period. I would like to believe that the aesthetic experience sought by Wallerstein was possible to achieve without intellectual sloppiness. I do not believe it is.

Classics as Developmental Tasks

Perhaps the best way to pose the question of the contribution of classics to making one's mind complex is to try to imagine Max Weber or Claude Levi-Strauss writing an introductory textbook. One can sort of imagine fitting Durkheim's *Rules* into the mind of an intelligent sophomore, but even when Weber tries, as in the first section of *Economy and Society,* he is just too complicated. One might use the clean elegance of Durkheim's *Division of Labor* as a touchstone of simple unified treatment—I would say of false simplicity and unity—but one would not use it to increase the variety in the mind of the student.

What one wants, to induce complexity and flexibility into the mind, is found in those thinkers where we suspect we are getting only half the argument the first time through. Clifford Geertz, John Dewey (in the original rather than in the symbolic interactionist cutdown version), the Karl Marx of the part of *Capital* that analyzes 19th century England, Paul Veyne whose *Bread and Circuses* I mentioned earlier, Jon Elster's *Logic and Society,* Parsons's *Social System,* all stretch the mind, show a new way of looking at things and then another new way a few pages later.

This all is fairly autonomous from the question of whether the work is scientifically valuable. For example, when I, at 20, read John Dewey's *Logic: A Theory of Inquiry,* I thought it fundamentally misconceived. I am somewhat less confident of that now, with all the advances in ethnomethodology that tend to support Dewey, but I still think it basically starts from the wrong end. But I think I came out of reading it with a deeper grasp of the problem, so that, compared with most positivists at least, I was better prepared for ethnomethodology. The point then is that even not agreeing with Dewey, the book stretched my 20-year-old mind, tempered a bit the sophomoric dogmatism of that mind, opened it to new sorts of evidence on cognitive social psychology. Similarly I think much of Levi-Strauss is flawed in the middle, that he has no real causal mechanisms in the mind, demonstrable by other means than mythical analysis, to make the whole thing go. But, in the first place, I'm not sure, because I'm not sure I understand him. And, in the second place, I think if I could grasp what

he saw as the problem, I would stretch my 48-year-old mind.

While excellence and complexity of mind probably are correlated, we would no doubt give different ranks to different classics on the two dimensions. Geertz for example clearly is more challenging than imitable. I suspect even Levi-Strauss might start writing with a sense of inferiority and bewilderment if he read a few pages of "Deep Play" rather than a few pages of *The 18th Brumaire*. But even if the same pieces serve both functions, the functions are different, teaching aesthetic standards in the first case, teaching variety in the mind in the second case. And again there is no reason to prefer ancient to modern examples of complexity of mind.

Classics as Intellectual Small Change

Now we come to a function which can better be served by older pieces than newer ones, that of serving as intellectual badges. Imagine if our badges for the convention had our names, our institutions, and our favorite classic writer. So mine might read "Stinchcombe, University of Arizona, Max Weber." Suppose now, in a fit of preciousness, I write instead, "Stinchcombe, University of Arizona, Paul Veyne." He is right now the person I am most intellectually excited about, and embodies the same virtues as Max Weber. But 90-odd percent of the people I met would not know who I was talking about, so would not learn anything about the set of prejudices and intuitions to which I was declaring my loyalty.

But what do we need to know about the classics for this function to be served? If you know about Weber that he emphasized subjective phenomena, that he was interested in the economy and in governmental authority, and that he did historical research, you probably would know enough to identify me, to know whether you wanted to talk to me. Similarly Herbert Blumer's cut down version of the pragmatist philosophers is perhaps better than the originals for the purpose of identifying a symbolic interactionist, and a rather vague reference to Chomsky suffices to update it.

The important point to note about this function is that it serves to differentiate us. By choosing Max Weber I am, most obviously, *not* choosing Marx. This does not mean, of course, that I am not an admirer of Marx (as was Weber). But it means that I am unlikely to be interested in the tortuous paths of Marxist epistemology or explications of the Marxist texts; it means that I will enjoy reading those Marxists who rarely quote Marx, like Gramsci or Trotsky, rather than those like Lenin or Cohen who are always trying to be textually accurate (rather than historically accurate, if necessary). And in fact it means I will look with interest rather than with dismay at a theory of why the profit rate and the capital intensity of industry tend to remain the same rather than the profit rate to fall and intensity to rise.

Thus, by saying Weber, I enter into contention, generally at a childish level, I'm afraid, with those who would write Marx on their name tags.

The problem here is that we really need simple guidelines to choose people we want to read and to talk to. There are far too many things written for us to keep track of them all, and no one would seriously propose to enter into serious dialogue with all 14,000 members of ASA.

But just as those bibliographies from librarians never tell us which are the good books, and just as the index of a book does not tell us which of its arguments are coherent, so the small change use of the classics is *nearly* as deceptive as the use of session titles at ASA conventions. Even if we had the first three footnotes of all the papers in the deviance sections, we could not reliably find the session with the good papers. Our prejudices are not good guides to intellectual quality. The use of classics as identifying badges tends to produce sects rather than open intellectual communities. The badges tend to become boundaries rather than guides.

Fundamental Ideas and Where to Find Them

These are not the usual reasons we are given in graduate school for studying our intellectual parents. Instead the rationale is in the form of a genealogy of ideas. Certain fundamental ideas about, say, social causation of rates of voluntary be-

havior were formulated first in *Suicide*. Within the masterpiece itself there are certain first branches. Some of these branches have been fruitful, such as the anomie branch, and some have died, such as the branch about women committing suicide less because they are such simple souls. But the fundamental notions that norms regulate and tame personal goals, that some social commitments are so intense as to efface the personality and reduce its value to nullity, are first expressed clearly and brought into contact with the facts in Durkheim.

So when we run into a problem of how norms and social commitments influence personal psychology, it's logical to return to the source. In general, then, the deeper the ideas in any particular piece of work, the more relevant the classical work, the genitors of the whole line of work, will be.

This picture of the relation between the basic nature of a thing and its historical origin is deeply embedded in human thought, and has been analyzed in philosophy in Kenneth Burke's *A Grammar of Motive*. The pattern found in the sciences, mentioned in the introduction, that much cited or classic papers themselves are more likely to cite other classic papers (as measured by *their* citations), indicates that in intellectual life this identification of fundamental ideas and classical origins has some truth to it.

If we imagine a developed science as looking like the evolutionary trees we used to see when we were in the tenth grade, we can see this function more clearly. Ordinarily we are working on the tip of some twig, and when we get into trouble we go back only to the first branching point to reconstruct. But if we are Einstein, we go clear back to the trunk, to Lorenz and Newton, and even flatly ignore experimental results on one of the twigs. Maybe something will show up between the trunk and the twig to explain away the contradiction. And for Einstein it did—a leak in the apparatus. Not all of us can depend on being able to ignore the facts like Einstein. But Einstein's achievement perfectly illustrates the process of ignoring the twigs for the trunk, because it is so extreme.

Of course it is true, as Kuhn has shown for several sciences, that the occasional reshaping of the trunk is a lot more important, and creates more discontinuities, than a lot of work on the twigs. So the obvious question comes up, why not work directly on the trunk, say by writing critiques of Weber, Durkheim, and Pareto or by writing a book to be called *The Structure of Social Action?* Why not work on theory itself, rather than on the twigs far removed from the theory?

But anyone who has read a pile of preliminary exams in theory knows why not—not many of us are Parsons, and it is not clear that even Parsons brought it off. The basic positivist stance, which is my stance, is that you can do something useful to the trunk of theory only if you approach it from the twigs.

To move to a more minor scale than *The Structure of Social Action,* it seems to me that a lot of essays on Michels's "Iron Law of Oligarchy" were of very little use, while *Union Democracy* made the theory more precise, more solidly supported, more empirically relevant, more useful in every way. This is because it approached the problem of the distribution of power in a voluntary association from the twigs of facts about typographers, not from the general theory. What *Union Democracy* says about the possibility of democracy in working class organizations has to be addressed by any serious socialist. What thousands of prelim answers have said on the subject does not have to be addressed.

Thus I think that the true fact that classics deal with more fundamental ideas, and so are more fruitful to think about, appears in practice mainly as a mortal temptation to skip the empirical work. Certain easy literary tricks that turn thoughts about Simmel or Durkheim into a publishable essay tend to deceive us.

As Erving Goffman once said about Parsons, at that time the chief practitioner of working directly on the trunk instead of on the twigs, "I wish social theory was as easy as Parsons thinks it is."

Classics as Underexploited Normal Science

But if it is not always wise to work only on the trunk of social knowledge, on the

most general theories, the classics contain a lot of twigs as well as the trunk. Part of the way we recognize theoretical classics is by their empirical fruitfulness. Let me elaborate a couple of examples, the first of a very direct sort, the second somewhat indirect.

We recall that the main place in modern societies where Durkheim located altruistic suicide was in elite units in the military. The idea was that the society became so strong as to obliterate the value of the personality. In the first place, it would be interesting to know for more armies whether the suicide rate of elite troops was higher than that of regular troops. But clearly it is not a very big step to imagine that elite troops with a great deal of ideological dedication might be expected to evaluate individuality even lower than "secular" elite troops. So perhaps the Nazi weaponed SS, crack Israeli commandoes, the survivors of the long march in the Chinese communist army, should have even higher suicide rates, while troops that are elite only by being near an unprincipled dictator, like the Shah of Iran's guard or Idi Amin's secret police, might not be distinct from the mass of soldiery. With only a slight extension of Durkheim's theory, we should be able to measure the social-psychological intervening variable, the devaluation of individuality, in elite troops.

These are examples at the level that Durkheim himself worked at. Now let me take an example from Max Weber's *Protestant Ethic* that requires just a bit of thought, but still would be within the range I would call "normal science." We already have had the very normal science of seeing whether Protestants did indeed start capitalist industries, and whether they did indeed avoid adventure capitalism in the form of the slave trade or of conquering gold mines or whatnot. Now I would like to show what happens when we stretch Weber a little.

One part of Weber's argument says that *whatever* the ethical system of a religious group, it will be more powerful in governing the behavior of laymen if the religion is organized as a sect. Perhaps the crucial sect feature for Weber was adult baptism, that one chooses sect membership only with an adult conversion. But various other features include the lack of monks and monasteries, lack of magical practices or rituals of any kind for the forgiveness of individual sin, congregational control of appointment of pastors, the institution of lay preaching by those who felt the call, and so on. The general idea was that all these features call upon all the faithful to be saints, while leaving them to live a secular life within this world.

It seems to me that it would not take a very great deal of ingenuity to develop measures of whether the *different* ethics preached by *different* religious groups were more salient to members if the group were organized as a sect than if it were organized as a church.

That is, Weber was interested in a particular ethic, that of popular Calvinism. He held that that a particular ethic was more likely to govern the behavior of members when a Calvinist-type religious current maintained a sect-like organization, than when it had become a state church or a a tamed American denomination.

But suppose we take instead socialist ethics, which show some variety, but which surely always include donating time to working class organizations, writing socialist pamphlets, egalitarian behavior toward women and toward minority races and ethnic groups, trying to make sense of conflicts between socialists and others in such countries as Afghanistan, and of conflicts between variants of socialism in places like Cambodia, reading socialist classics, attending meetings of socialist groups, and so on.

It does not seem to me to be stretching Weber very far to predict that socialist morality should be more characteristic of members of socialist sects—defined the way Weber defined sects, as far as this is possible—than of social *ecclesiae*.

The general point here is that the puzzles one can find in classic works often are more interesting than the puzzle of entering another variable in a model of status attainment. The twigs we investigate in routine science can be nearer to or farther from the trunk. One way to find twigs nearer to the trunk is to examine the puz-

zles that still have not been investigated in classic works.

Ritual Use of Classics

It is the fact that we have all read these classics, or at least answered preliminary examination questions on them, that binds us together into an intellectual community.

But that brings up the question of how far the classics still sing to us, how far they symbolize what we are really all about. It seems to me that there is a broad correlation between a quantitative style in sociology and a cynical attitude toward the classics.

I think this is because there is teachable innovation in the quantitative branches of social science. That is, there are quite a few things that a run-of-the-mill quantitative social scientist can do now that Ogburn or Chapin or Durkheim could not do. There are not many things that a run-of-the-mill historical sociologist can do that Weber could not.

But there is a more serious problem. The classics of quantitative or mathematical social science are hard for non-quantitative types to read, while the reverse is not really true. Or perhaps better, it is harder to *show* that someone really has not been able to read Simmel than to show that they have not been able to read von Neuman and Morgenstern, so people of the most variable command of Simmel can participate in the ritual. The impatience of quantitative people with classics is perhaps the central challenge to our feeling of being a moral community.

My own opinion is that a lot of the supermodernism of quantitative sociologists is beside the point. For example, I have never found a difference between the decisions I make on a cross-classification table when I use the method of my youth, which involves treating combinations of proportions as combinations of normal variables after the manner of Goodman in his Stouffer-Dorn-Tibbets paper, and decisions based on the log linear method now in fashion. It would, of course, seriously challenge our confidence in Goodman's current opinion if he had been radically wrong in *his* youth. But you can hardly be respectable and analyze percentage tables by non-log linear methods nowadays. What has happened here, I would say, is that the ideology that a science always uses the most modern methods has caused us to exaggerate greatly the virtues of the advances we have made lately.

It is still true that with all our advances in statistical methods, the main determinant of the value of a table is whether you have measured the right variables in a study with a good sample of the relevant population. If you have done that, then statistical efficiency does not matter much. That does not mean that I favor statistical inefficiency, of course, anymore than I favor mistranslating Weber from the German. It just means that I think the ritual emphasis on modern methods has got out of hand, leading to a species of methodological scholasticism that is as bad as all the scholasticism of the textual analyses of Marx, Lenin, and Mao that we also have been burdened with.

But I do think that the quantitative cynicism toward the classics has one very strong healthy element in it. A central feature of any symbol of solidarity for sociologists should be, I think, that we be concerned for whether it is true or not. One of the things we tend to lose sight of, given all the other uses of classics, is that our central symbols ought to be tested for truth, as well as for intellectual beauty, complexity of thought, recognizability as intellectual small change, empirical fruitfulness, and the other virtues I have discussed.

When Dudley Duncan many years ago criticized me for using Durkheim's *Suicide* as a methodological example in *Constructing Social Theories,* he said something like, "We can surely do better than that now." Whitney Pope has shown in some detail that we *could* do better now, and that it is quite doubtful whether Durkheim's theory is true or not. It surely is a bad thing for sociology to have false gods to symbolize the search for truth.

Moral

I suppose the moral of all this is that it is destructive to mix up the different func-

tions that a classic can serve. We may believe that students' minds are expanded by reading Durkheim without our having to believe Durkheim has many true generalizations about the causes of suicide. George Herbert Mead can symbolize what is distinctive in symbolic interactionism even if we cannot quite figure out how to test the hypothesis of the independence of the "I" from the "me," and to turn it into a puzzle for routine science. And one can enjoy the taste of Marx's famous passage in *The 18th Brumaire* about French peasants forming a vast mass, without that beauty being undermined when we find some regions of modern France where the peasants vote Communist.

What is destructive about admiration of the classics, then, is the halo effect, the belief that because a book or article is useful for one purpose, it must have all the virtues.

INTERVIEWS, SURVEYS, AND THE PROBLEM OF ECOLOGICAL VALIDITY*

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Despite the fact that virtually all social science data are derived from some kind of discourse or textual materials, sociologists have devoted little time to establishing explicit theoretical foundations for the use of such instruments as interviews and surveys. A key problem always has been the lack of clear theoretical concepts about the interpretation of interview and survey question and answer frames. We lack a theory of comprehension and communication that can provide a foundation for the way that question-answer systems function, and the way respondents understand them. The paper briefly describes the possible relevance of linguistic and cognitive processes for improving our understanding of interviews and surveys. The theoretical foundations of interviews and surveys also must address the way that artificial circumstances become necessary to guarantee adequate study designs. These artificial circumstances often violate ecological validity, or the way interviews and survey questions are constructed, understood, and answered, as contrasted with the way that field notes and tape-recordings of natural settings are used to address the same or comparable substantive and theoretical issues.

Social scientists have relied on interviews for a long time. There is little reason to doubt their value and utility for many theoretical and practical purposes. There exists a huge literature on the virtues and drawbacks of interviews that use open-ended questions and surveys that use close-ended questions. Yet there is little in the way of theory that would link interviews and surveys to more general issues of communication and comprehension. Those researchers who are convinced that interviews and surveys are basic research tools for the sociologists are concerned about improvements in survey design and use, but see little point in challenging their routine use. In this paper I want to suggest a few cognitive and linguistic issues that can clarify our understanding of the processes and mechanisms underlying the use of interviews and surveys. I also want to suggest some theoretical ideas that can strengthen the ecological validity of interview and survey methods and findings.

The necessity of writing a brief paper does not permit me to discuss old issues about current interview and survey practices that I hope are obvious to

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