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Writing for Social Scientists

How to Start and
Finish Your Thesis,
Book, or Article

Howard S. Becker
with a chapter by
Pamela Richards



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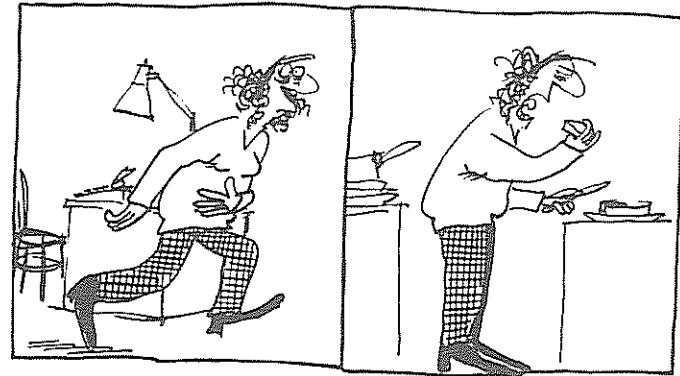
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is much worse), knowing that any organization of reality you make will likely be incorrect in some way, you don't get it organized at all? That is the deepest cause of the anxiety that strikes writers when they begin. What if we cannot, *just cannot*, make order out of that chaos? I don't know about other people, but beginning a new paper gives me anxiety's classical physical symptoms: dizziness, a sinking feeling in the pit of the stomach, a chill, maybe even a cold sweat. The dual possibilities, one as bad as the other, that the world has no real order or that, if it does, I can't find it, now or ever, are philosophically, almost religiously, frightening. The world may be a meaningless mess, but that is not a philosophical position one can live with easily. Not being able to figure out the first sentence makes that possibility palpable.

Do I have a cure for the disease I've described? Yes and no. A lot of other activities, especially sports, provoke paralyzing fears that keep people from getting started. The advice of experts in these areas is always the same. Relax and *do it!*. You cannot overcome the fear without doing the thing you are afraid of and finding out that it is not as dangerous as you imagined. So the solution for writing something that will not fully, logically, and completely master the chaos is to write it anyway and discover that the world will not end when you do. You might be able to do that by tricking yourself into thinking that what you are writing is unimportant and makes no difference—a letter to an old friend, perhaps. I know how to trick myself, but I don't know how others can trick themselves. So here is where the advice stops. You can't start swimming until you get in the water.

Eight



Terrorized by the Literature

Students (and others) often, as I said earlier, talk about “using” this or that approach—“I think I’ll use Durkheim”—as though they had a free choice of theories. In fact, by the time they begin to write about their research, they have made many seemingly unimportant choices of details that have foreclosed their choice of a theoretical approach. They decided what questions to investigate. They picked a way of gathering information. They chose between a variety of minor technical and procedural alternatives: who to interview, how to code their data, when to stop. As they made these choices from day to day, they increasingly committed themselves to one way of thinking, more or less firmly answering the theoretical questions they thought were still up for grabs.

But sociologists, and especially students, fuss about choosing a theory for a practical reason. They have to—at least they think they do—deal with the “literature”

on their topic. Scholars learn to fear the literature in graduate school. I remember Professor Louis Wirth, one of the distinguished members of the Chicago school, putting Erving Goffman, then a fellow graduate student of mine, in his place with the literature gambit. It was just what we all feared. Believing Wirth had not given sufficiently serious attention to some influential ideas about operationalism, Goffman challenged him in class with quotations from Percy Bridgeman's book on the subject. Wirth smiled and asked sardonically, "Which edition is that, Mr. Goffman?" Maybe there was an important difference between editions, though none of us believed that. We thought, instead, that we'd better be careful about the literature or *They Could Get You*. "They" included not only teachers but peers, who might welcome an opportunity to show how well they knew the literature at your expense.

Students learn that they must say something about all the people who have discussed "their" problem before them. No one wants to discover that their carefully nurtured idea was in print before they thought of it (maybe before they were born) and in a place they should have looked. (Wirth also told us that originality was the product of a faulty memory.) Students want to show the world, and all the critics who may be out there laying for them, that they have looked and that no one has had their idea before.

A good way to prove your originality is to attach your idea to a tradition in which people have already explored the literature. Hitching your work to a well-explored scholarly star helps you to assure yourself that your work doesn't redo something already done. If you "use" Weber or Durkheim or Marx or Mead, the exegetes have preceded you, laying out the terrain, specifying what the questions really are, defining what work by who will be relevant to consider—and in general providing a surefire way of dealing with the literature: "See Chaim Yankel's exhaustive review (1993) of the literature in this area." This protective ritual effectively

covers the author's ass, but works less well to produce good or interesting scholarship. The reasons, interesting in themselves, also illuminate the institutional bases of creativity and banality.

Writers should, of course, use relevant literature appropriately. Stinchcombe (1982) has pointed out six major uses. (I intend my summary of his paper to exemplify what I will describe later as a good use of the literature, to provide an already thought-through piece of an argument you need.) Although Stinchcombe writes about the narrower category of "classics," what he has to say also speaks to our problem of "the literature."

Two of the six uses he discusses relate to early phases of research and are less relevant to problems of writing. As a source of fundamental ideas, the classics are very important in the early stages of a project; but by the time you start writing, you ought to have your fundamental ideas clear. Clear or not, you already have them and they have informed your work and done their best or worst. The classics' second function, as "under-exploited normal science," as a source of empirical hypotheses, hunches, and hints, is similarly crucial in the prewriting stages. Stinchcombe also mentions an organizational function of the classics: to symbolize solidarity among people in a field. "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." He worries about this function, thinking that it leads us to admire work that time may have shown to be wrong (as, he says, Whitney Pope showed that Durkheim was wrong about suicide): "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."

Three other important uses of the classics have directly to do with getting our writing done. A classic work of scholarship serves as a touchstone: "a concrete

example of the virtues scientific work might have, in a combination that shows what work should look like in order to contribute to the discipline." As Stinchcombe says, this is what Thomas Kuhn meant when he used the term *paradigm* in the sense of an exemplar. The virtues Stinchcombe is talking about are not the ones you might expect:

[F]irst 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. . . . [I]f 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?

Stinchcombe here describes what I meant when I spoke earlier of editing by ear. If he is right, and these aesthetic standards cannot be justified "scientifically," it follows that there is no sense trying to find the One Right Way to write what you have to say. Copying well-done work (especially its organization or format), however, is a wonderful way to find possible right ways.

Classics also serve as "developmental tasks for novices," showing them how much more complicated things are than they thought and bringing them up to the level of sophistication common in their field. This function is usually what people have in mind when they talk about the benefits of studying for qualifying exams. It probably contributes to the irrational way people think about the literature, and to the mindlessly

ritualistic literature reviews that decorate so much scholarly work.

Stinchcombe calls a final use of the classics "intellectual small change." You cite Weber or Durkheim or Yankel (just as you use the catchwords of a school) to show what camp you belong to. To do this, you must use well known names:

Imagine if our badges for the convention [he is referring to the annual meeting of the American Sociological Association] 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 per cent 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] 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.

The conventional review of the literature provides evidence of the author's allegiances in this way, but authors would be briefer and less obsessive if that were their main purpose.

The classics are not the same as "the literature." Sociologists worry about the classics, but they also worry about the literature of commentary and methodological discussion, about research reporting specific findings on the topic and discussions of those findings, all of which they feel responsible for (much as students know when they are "responsible" for material on a test).

None of these are intrinsically bad ways to use the literature, but none of them answers the question of how to use the literature on your research topic.

Science and humanistic scholarship are, in fact as well as in theory, cumulative enterprises. None of us invent it all from scratch when we sit down to write. We depend on our predecessors. We couldn't do our work if we didn't use their methods, results, and ideas. Few people would be interested in our results if we didn't indicate some relationship between them and what others have said and done before us. Kuhn (1962) spoke of this mutual dependence and cumulation as "normal science." Many sociologists use "normal science" pejoratively, as though it meant "merely normal science," as though all of us could expect to produce scientific revolutions every day. That is a total misreading of Kuhn, and foolishness as well. Individual scientists don't make scientific revolutions. Those revolutions take a long time. Large numbers of people, working together, develop a new way of formulating and investigating the problems they are interested in, a way which finds a home in lasting institutions of scientific work. To imagine that your report of your project will accomplish what takes all that time and all those people is wrong-headed. It's alright to aim for the stars, but we ought to have a decent regard for what is humanly possible. If making a scientific or scholarly revolution singlehandedly is our chief goal, we are bound to fail. Better to pursue the goals of normal science: to do a piece of good work others can use, and thus increase knowledge and understanding. Since we can attain those things in our own research and writing, we don't set ourselves up for failure by aiming at the impossible.

A scholar can try to work in isolation from others and without their help, like so-called naive artists who produce paintings and constructions without reference to any of the traditions of the medium they work in. Artists who do that usually produce exceptionally

eccentric work, but their work is also free of the constraints imposed by standard ways of working. That freedom from organizational constraint sometimes allows naive artists to produce works which command the respect of an established art world and which may even eventually be absorbed into its tradition. The dialectic of constraint and opportunity that naive artists illustrate affects all of us as we write our dissertations, papers, and books. That dialectic suggests two questions: how we can use the literature effectively? How does the literature get in our way and prevent us from doing our best work?

Are there effective ways to use the literature? Of course. For one thing, scholars must say something new while connecting what they say to what's already been said, and this must be done in such a way that people will understand the point. They must say something at least minimally new. Although the empirical sciences pay lip service to the idea of replicating results, they don't pay off for it. At the same time, as you approach total originality, you interest fewer and fewer people. Everyone is interested in the topics people have studied and written about for years, both because the topics are of great and continuing general concern (why do people commit suicide?) and because they have been studied for so long that they have created the kind of scientific puzzles Kuhn (1962) identified with normal science (the literature investigating Durkheim's theory of suicide exemplifies this). The ideal scholarly contribution makes readers say: "That's interesting!" As Michael Schudson suggested to me, students must learn to connect their work to the literature in just that way, to set their results in the context of accepted theories that make it unlikely (see Davis 1971 and Polya 1954).

I remarked earlier that my use of Stinchcombe's article exemplifies what I think is a better way to use what others have done. Here's what I meant. Imagine that you are doing a woodworking project, perhaps

making a table. You have designed it and cut out some of the parts. Fortunately, you needn't make all the parts yourself. Some are standard sizes and shapes—lengths of two by four, for instance—available at any lumberyard. Some have already been designed and made by other people—drawer pulls and turned legs. All you have to do is fit them into the places you left for them, knowing that they were available. That is the best way to use the literature. You want to make an argument, instead of a table. You have created some of the argument yourself, perhaps on the basis of new data or information you have collected. But you needn't invent the whole thing. Other people have worked on your problem or problems related to it and have made some of the pieces you need. You just have to fit them in where they belong. Like the woodworker, you leave space, when you make your portion of the argument, for the other parts you know you can get. You do that, that is, if you know that they are there to use. And that's one good reason to know the literature: so that you will know what pieces are available and not waste time doing what has already been done.

Here's an example. When I was working on the theory of deviance (eventually published in *Outsiders* (1963)), I wanted to argue that when others labeled someone as a deviant, that identification often became the most important thing about the person so labeled. I could have worked out a theory about how that happened, but I didn't need to. Everett Hughes (1971, 141–50) had already developed a theory describing the way statuses develop a halo of “auxiliary status characteristics,” so that we expect, for instance, an American Catholic priest to be “Irish, athletic, and a good sort who with difficulty refrains from profanity in the presence of evil and who may punch someone in the nose if the work of the Lord demands it.” Or, to take a more serious example, although all you need in order to practice medicine is a license from the state, we commonly expect doctors to be white male Protestants of

old American stock. Hughes was especially interested in the intersection of race and professional position and, in developing his argument, made the following observation:

Membership in the Negro race, as defined in American mores or law, may be called a *master status-determining trait*. It tends to overpower, in most crucial situations, any other characteristics which might run counter to it. But professional standing is also a powerful characteristic—most so in the specific relationships of professional practice, less so in the general intercourse of people. (147, emphasis added)

The idea of a master status-determining trait, which takes precedence in identifying people socially, was no more than an aside in Hughes's article. If I were to write an article titled “The Sociological Thought of Everett C. Hughes,” I would not spend much time on it. But in working out my theory, I wanted precisely to talk about how a disreputable status characteristic like being addicted to drugs could spoil reputable statuses—genius or priest or doctor or whatever—one might think would neutralize it. Hughes wanted to talk about how the status of Negro overpowered the status of doctor. I wanted to talk about how the status of junkie overpowered the status of son or husband, so that parents or spouses locked up the family silver and jewels when their beloved dope-fiend relative came to dinner. I wanted to talk about what a character in Doris Lessing's *The Four-Gated City* meant when she said that she didn't mind being thought schizophrenic but didn't like people to think that that was *all* she was.

Hughes's language fits my case exactly. I didn't need to invent the concept; he had invented it for me. So, instead of creating yet another unnecessary new sociological term, I quoted Hughes and went on to make more use of his idea than he had in the article I took it

from. In the same way, I had no need to work out the uses of the classics. Stinchcombe had done it. I only had to quote and summarize.

Is working that way plagiarizing or being unoriginal? I don't think so, although fear of such labels pushes people to desperate attempts to think of new concepts. If I need the idea for the table I'm building, I'll take it. It's still my table, even though some parts were prefabricated.

In fact, I am so accustomed to working this way that I am always collecting such prefabricated parts for use in future arguments. Much of my reading is governed by a search for such useful modules. Sometimes I know I need a particular theoretical part and even have a good idea of where to find it (often thanks to my graduate training in theory, to say a good word for what I so often feel like maligning). When I wrote my dissertation about Chicago public school teachers, I found modules I needed in the writings of such classic sociologists as Georg Simmel and Max Weber. Discussing how teachers expected school principals to take their side of any argument with a student, whatever the facts of the case, I found a general description of the class that phenomenon belonged to in Simmel's essay on superiority and subordination: "The position of the subordinate in regard to his superordinate is favorable if the latter, in his turn, is subordinate to a still higher authority in which the former finds support" (Simmel 1950, 235). I also wanted to argue that the desire of school personnel to keep parents and the general public out of school affairs was a specific instance of a phenomenon important in organizations of all kinds. I found that module in Max Weber: "Bureaucratic administration always tends to be an administration of 'secret sessions'; in so far as it can, it hides its knowledge and action from criticism. . . . [T]he tendency toward secrecy in certain administrative fields follows their material nature: everywhere that the power inter-

ests of the dominant structure toward *the outside* are at stake . . . we find secrecy" (Gerth and Mills 1946, 233).

On the other hand, I didn't know I needed the next module until I found it; then I couldn't do without it. It did not come from one of the conventionally recognized classics, although the work it is in is elegantly excellent. Willard Waller helped me and my readers understand why schools had a discipline problem when he said: "Teacher and pupil confront each other in the school with an original conflict of desires, and however much that conflict may be reduced in amount, or however much it may be hidden, it still remains" (Waller 1932, 197).

I also collect modules I have no present use for, when my intuition tells me I will eventually find the use. Here are some ideas I have stored away recently, expecting sometime to find a place for them in my thinking and writing sometime: Raymonde Moulin's (1967) idea that, in art works, economic and aesthetic value are so closely related as to be the same thing, and Bruno Latour's (1983, 1984) idea that scientific inventions create new political forces, as Pasteur's work in microbiology did by introducing the microbe as a social actor. I may not use these ideas in their original form. I may transform them in ways their parents wouldn't recognize or approve of, and interpret them in ways students of these thinkers will find incorrect. I will probably use them in contexts quite different from those in which they were first proposed, and fail to give due weight to theoretical exegeses which strive to discover the core meanings their inventors intended. But I carry them with me, ready to apply when I make my observations or begin writing. It will be easier to use them, of course, if I have had them in mind all along. But I may also find that I had some such idea in mind, only not very clearly, and that Latour or Moulin or Waller has done the hard work of clarification for me. I am grateful, recognize that as part of the cooperative work of scholarship, and cite and quote them in the

appropriate places. My work may look like a patchwork quilt as a result. When that happens I console myself with the example of Walter Benjamin, the German-Jewish man of letters, whose methods Hannah Arendt described this way:

From the Goethe essay on, quotations are at the center of every work of Benjamin's. This very fact distinguishes his writings from scholarly works of all kinds in which it is the function of quotations to verify and document opinions, wherefore they can safely be relegated to the Notes. . . . The main work [for Benjamin] consisted in tearing fragments out of their context and arranging them afresh in such a way that they illustrated one another and were able to prove their *raison d'être* in a free-floating state, as it were. It definitely was a sort of surrealist montage. (Arendt 1969, 47)

That's the good side of the literature. The bad side is that paying too much attention to it can deform the argument you want to make. Suppose there is a real literature on your subject, the result of years of normal science or what, by extension, we could call normal scholarship. Everyone who works on the topic agrees on the kinds of questions to ask and the kinds of answers they will accept. If you want to write about the topic, or even use that subject matter as the material for a new topic, you will probably have to deal with the old way even though you think it quite foreign to your interests. If you take the old way too seriously, you can deform the argument you want to make, bend it out of shape in order to make it fit into the dominant approach.

What I mean by bending your argument out of shape is this. What you want to say has a certain logic that flows from the chain of choices you made as you did the work. If the logic of your argument is the same as the logic of the dominant approach to the topic, you have no problem. But suppose it isn't. What you want

to say starts from different premises, addresses different questions, recognizes a different kind of answer as appropriate. When you try to confront the dominant approach to this material, you start to translate your argument into its terms. Your argument will not make the kind of sense it made in its own terms; it will sound weak and disjointed and will appear ad hoc. It cannot look its best playing an opponent's game. And that phrasing puts the point badly, because what's involved is not a contest between approaches, after all, but a search for a good way to understand the world. The understanding you're trying to convey will lose its coherence if it is put in terms that grow out of a different understanding.

If, on the other hand, you translate the dominant argument into your terms, you will not give it a fair shake, for much the same reasons. When you translate from one way of analyzing a problem into another, there is a good chance that the approaches are, as Kuhn (1962) suggested, incommensurable. Insofar as they address different questions, the approaches have very little to do with one another. There is nothing to translate. They are simply not talking about the same things.

The literature has the advantage of what is sometimes called ideological hegemony over you. If its authors own the territory, their approach to it seems as natural and reasonable as your new and different approach seems strange and unreasonable. Their ideology controls how readers think about the topic. As a result, you have to explain why you haven't asked those questions and gotten those answers. Proponents of the dominant argument don't have to explain their failure to look at things your way. (Latour and Bastide 1983 discuss this problem in the sociology of science.)

My work in deviance taught me this lesson the hard way. When I began studying marijuana use in 1951, the ideologically dominant question, the only question worth looking at, was "Why do people do a weird thing

like that?" and the ideologically preferred way of answering it was to find a psychological trait or social attribute which differentiated people who did from people who didn't. The underlying premise was that "normal" people, who did not possess the distinguishing causal stigma you hoped to discover, would not do anything so bizarre. I started from a different premise: that "normal" people would do almost anything if the circumstances were right. That meant that you had to ask what situations and processes led people to change their minds about this activity and do what they formerly would not do.

The two ways of investigating marijuana use are not totally divergent. They can be made to coincide, and that's what I did when I first published the material in 1953: I made them coincide. I showed that users went through a process of redefining the drug experience that led them to regard it differently. Sociologists, psychologists, and others interested in drug use found that an interesting answer. It helped start a spate of studies of how people became this or that kind of deviant, mostly based on the premise that these were normal people who had just had some different experiences. Well, you might ask, what's wrong with that strategy?

What's wrong with it, something I did not realize until years later, is that my eagerness to show that this literature (dominated by psychiatrists and criminologists) was wrong led me to ignore what my research was really about. I had blundered onto, and then proceeded to ignore, a much larger and more interesting question: how do people learn to define their own internal experiences? That question leads to the exploration of how people define all sorts of internal states, and not just drug experiences. How do people know when they are hungry? That question has become of great interest to scientists who study obesity. How do people know when they are short of breath or have normal bowel movements or any of the other things doctors ask about

in taking a medical history? Those questions interest medical sociologists. How do people know when they are "crazy"? I think, looking back, that my study would have made a more profound contribution if I had oriented it to those questions. But the ideological hegemony of the established way of studying drugs beat me.

I don't know how people can tell when they are letting the literature deform their argument. It is the classic dilemma of being trapped in the categories of your time and place. What you can do is recognize the dominant ideology (as I did at the time with respect to drug use), look for its ideological component, and try to find a more neutral scientific stance toward the problem. You know you are on the right track when people tell you you are on the wrong track.

That goes too far, of course. Is everything that disagrees with the dominant approach therefore right? No. But a serious scholar ought routinely to inspect competing ways of talking about the same subject matter. The feeling that you can't say what you mean in the language you are using will warn you that the literature is crowding you. It may take a long time to find out that this has happened to you, if you find out at all. I only saw my mistake about the marijuana study fifteen years later (see the discussion in Becker 1967 and 1974). Use the literature, don't let it use you.