ARTICLES

WILL AND WILE: THE WAY OF THE RESEARCHER*

Discussions of the research process, even when they focus on histories of research projects rather than on formal methods, do not sufficiently emphasize the personal qualities, skills, and associations that make for excellence in research. In this essay, which is intended for advanced undergraduates and beginning graduate students, I use examples of well-known researchers to discuss the qualities of passionate commitment, persistence, ingenuity, and self-discipline that are essential to a researcher. I also discuss the significance of files, multiple data sources, and personal associations to success in research.

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We have some excellent pieces in sociology about researchers and the research process (see, e.g., the chapters in Berger 1990; Hammond 1964; and Laslett and Thorne 1997), but I am not certain that the characteristic life conduct of great researchers has been described accurately or vividly enough. Even C. Wright Mills, who wrote perhaps the most penetrating piece of all on the craft of research, does not help us very much when it comes to conveying the will and wile of the sociological researcher. Oddly, given Mills’s reputation for contrariness, his famous appendix on intellectual craftsmanship in The Sociological Imagination (1959) expresses the temperate, mannerly tone of the men of letters he admired, rather than the plain talk for which he would soon become famous. Mills writes, for example: “It is best to begin, I think, by reminding you, the beginning student, that the most admirable thinkers within the scholarly community you have chosen to join do not split their work from their lives. They seem to take both too seriously to allow such dissociation, and they want to use each for the enrichment of the other” (Mills 1959:195).

But the classical balance Mills commends gives way at times. For the researcher who is fully enthralled with her project, work also absorbs life. Certainly, it colors life. And, often, it ransacks life for materials that it can put to use.

In this essay, I would like to emphasize that sociological work at its best is usually not conducted by the finest rhetoricians (or the finest technicians, for that matter), but by people obsessed with finding out the truth. And these people are not just obsessed, but able through the force of will and wile to realize their aims. They are also capable of great feats of self-denying discipline in the service of their chase. This essay, then, is about the marriage of passion, practical craft, and discipline that goes into the work of those who truly have the calling for sociological research.

Affinities certainly exist, as introductory textbooks often suggest, between the work of detectives and the work of researchers. But I think dogcatching may be a better metaphor. First, there is something that I find inappropriate about the image of detective work as a metaphor for research, at least as it is conveyed in the “hard-boiled” genre of the mystery novel. It is depicted as a grim occupation, practiced by disillusioned characters who serve justice, but in a rather jaded or cynical way. My sense of research

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is not as grim as that; it is closer in feeling to a romance. Second, and more important, I prefer a metaphor which brings to mind the world of childhood. Anyone can catch a dog—it is a down-to-earth activity—and it is not encumbered by the need to demonstrate that the world is, after all, governed by a moral order. But you cannot catch a dog without a romp through the tall brush. This is a lot like doing research. I also like the metaphor because dogcatching is an activity that, if one is to be successful at it, requires devotion and energy, and also more than a little wile.

Although I have chosen a whimsical metaphor to describe the research process, I do not wish to present a frivolous image of research. Research is serious business of course, with papers to be presented and published, reputations to be staked, and careers to be made. The research process can be lonely and frustrating at times, or interrupted too often by other obligations. One runs into dead ends and false leads on a regular basis. But for those who have the feel for it, it also has a quality of play; there is the thrill of racing after an idea, the spirited chase, the stealthy circling, and the final determined and satisfied wrestling of a once-elusive object to the ground.

This essay is based on a talk that I have been giving for the last several years to first year graduate students in my capacity as graduate advisor in my department. This piece has therefore been addressed to students who are just beginning to stick their toes into the research pool (though it could also be addressed to advanced undergraduates). One reason I give this talk is that I want students to believe that they can succeed as researchers and also to know that they cannot succeed simply by absorbing the ideas and methods they are taught in class. Graduate committees look at grade point averages and standardized test scores together with letters of recommendation and writing samples to determine whether applicants should be admitted to their programs. However, test scores are not highly correlated with performance in graduate school. Indeed, the correlation approaches zero in many programs—and I have heard of a few cases of negative correlation. Our predictions would be so much more accurate if we could look into applicants’ hearts to see if they have a passion (even a potential passion that could be ignited) for following the trail of an idea with energy, ingenuity, and persistence. There is another reason that I think the themes of this essay are important for incoming graduate students to hear. These people are thrown into a tough situation; they often struggle to keep up with all the reading and work in their courses, and many of them ask themselves: “Can this really be worth it?” I want to remind students—at a time when many need it most—of the romance of research.

I would like to begin by noting the importance of research for every prospective sociologist. Some students think of research as either beyond or beneath them. Those who think that research is beyond them usually only need a little experience (and the help of a mentor) to know that their fears are ungrounded. Those who consider it beneath them can be a tougher audience. I must confess that when I entered sociology I thought of myself as a theorist, and had a little of the arrogant attitude about research that so often comes with that territory. One of the important lessons I learned in graduate school was: It’s virtually impossible to be a competent theorist without also being an accomplished researcher. Another vital lesson-stayed to fill out the short evaluation all found the talk “definitely” valuable, and all but one of the 10 said that it had changed their approach to research at least a little. Following the presentation, the Sociology Graduate Students Association sent a note of thanks for the “delightful…and inspiring” talk.
son was: *Research does not simply translate as competence in the latest statistical estimation procedures.* Analyzing dozens of data sets using cutting-edge methods does not necessarily make a person a good researcher. Excellence in research requires also—and I think more importantly—the search for relevant evidence wherever it is to be found, and the determination to make sense of that evidence with the best fitting analytical ideas possible.

I learned these lessons primarily from an early mentor, Jerome Karabel. As a graduate student, I worked with Karabel on a sponsored research project on politics and inequality in higher education. When I first came on the project, I was assigned to write long memos analyzing the literature on various topics in which we were interested, such as the major groups influencing the development of junior colleges, the effects of college quality on later life success, and the relative standing of different professional occupations and the structure of stratification within them. I wrote several long memos on topics like these. Every time (it seemed) that I reached to make a generalization from the literature, Karabel would scrawl a marginal comment: *Evidence? Data?? Alternative Explanation??* I could infer his level of skepticism by the number of question marks after these marginal notations. He was at that time acquiring a reputation as a theorist, but he was absolutely relentless about collecting and understanding all of the relevant data on the topic on which he was working. “Okay,” he would tell me: “you have looked at six different kinds of prestige rankings of universities. Is there a seventh that you have missed?”

This might sound like the worst sort of scholarly drudgery. But I gradually began to understand that, as odd as it may sound, attention to detail is part of the romance of scholarship—it is part of the journey of understanding and the sense of satisfaction in having done your work well. Karabel was interested not only in the amount and quality of the data I had found to support my conclusions, but whether alternative explana-

tions might explain the patterns I had observed in a more satisfactory way. In Karabel’s comments, I recognized a demand for rigor and discipline, but also for the play of mind. The marriage between play and discipline is necessary because research is about exploring problems thoroughly and finding out the most satisfactory answers. It is about catching the whole dog, not just a leg or a tail.

**VIRTUOSOS**

Over the years, I have discovered that many outstanding sociologists are not just people who have good ideas, but are also *virtuosos of the discovery, collection, and interrogation of data.* They are, first of all, passionate about finding out answers, and they are willing to look for answers in places that most people would not think to look. Some sociologists may have heard stories of Erving Goffman, who was so interested in what goes on in “backstage” talk that he kept notes on what his colleagues at conventions were saying to one another when they visited the men’s room (Collins 1981). These virtuosos of data collection are like the ideal dogcatcher. They are determined to catch that dog; they will chase it anywhere it goes, and they will use all the means at their disposal to chase it down.

Here is a picture of the virtuoso: She is alive with a full range of half and fully-baked ideas. She buttonholes anyone within arm’s reach to talk about her latest thinking, all the while imploring her companion to tell her everything she knows about the topic and wondering aloud whether an idea that has just occurred to her sounds promising. She reaches out with what seems like eight arms to scoop up evidence and reactions to her ideas. Yet, she is also calm and careful at the workbench. She designs her studies with the care of a master architect, and she inspects every facet of the resulting data with the practiced and skeptical eye of a diamond cutter. Methods courses in graduate school will provide much help with the architect and diamond cutter sides of the
model researcher. But few will show you the dynamo with eight eyes and eight arms who runs the operation.

Let me illustrate by providing snapshots from my album of virtuosos, focusing on the working style of four sociologists: Seymour Martin Lipset, Stanley Lieberson, Randall Collins, and Diane Vaughan. In each case, a passion for understanding sociological problems has translated into an enduring body of work.

Seymour Martin Lipset. Lipset, the great political sociologist, has always had an enormous appetite for data. Between the years 1955 and 1985, I do not think there could have been a person with a higher level of energy for tracking down and making sense of data. Even today, in his 80s, Lipset’s capacity for digging hard and assessing evidence clearly remains formidable. At one point, I was interested in how the politics of professors had changed over time—and I wanted to go as far back as possible. Lipset told me that I might try to get hold of a poll the Yale student newspaper did in 1936 on the political views of the Yale faculty. He observed that even if the survey represented only one small group of professors, at least it would be a beginning. How could he have known about this obscure student newspaper poll? I can tell you: He was omnivorous when it came to data. He talked to many people. He remembered (or wrote down) everything he read and heard. And he had great instincts for how to determine whether an idea was true. You would see, if you read a paper of Lipset’s, how these qualities take material form in the hands of a master craftsman. If the question concerns the conditions favoring democracy over other political systems (Lipset 1979), you will see studies cited from virtually every corner of the world, data analyzed by himself and his graduate students, explanations of apparently deviating cases, and the spoken words of the most important theorists and political leaders on the subject—always with the idea in mind of reaching the smallest possible set of key explanatory ideas. Similarly, in his work on the sources of working-class politics (Lipset 1983), you will find a simple and elegant thesis: Where the working class was deprived of both economic and political rights, those who favored social change were necessarily revolutionary. You will also find an extraordinary range of evidence to support this thesis—everything from the observations of Lenin to a trend analysis of the socialist vote in Finland from 1905 to 1916, to an analysis of how regional differences in German political structure created different levels of support for revisionist and revolutionary doctrines among workers in Prussia and Southern Germany.

Stanley Lieberson. Lieberson has the same thirst for information—he once told me proudly that as part of his work on metropolitan development, he had memorized the names of the counties in which every major city in the country was located—and perhaps an even greater curiosity to pursue leads down back alleys and up hidden staircases where others would be unlikely to look. I briefly occupied an office across from Lieberson in the late 1980s, when he was working on a project that concerned personal names as indicators of cultural change and social divisions. He wanted to know why certain names were more popular in some parts of the country than others and among some groups rather than others; why the popularity of names changed; and how names played into processes of group differentiation. He had tracked down data sets on popular names from publishers of baby-naming books, city registries, genealogists, and others. He was on the trail all over the country corresponding with people who could provide data from Terre Haute to New York City. I had lunch with Lieberson occasionally during this period, and I can tell you that he was not a man to let a bowl of soup and a sandwich interrupt his work. As we walked down the street, he would tell me: “I’ve got to stop at this bookstore. They have a book on baby names that I don’t have yet.” During one lunch a month after my daughter was born, we spent 30 minutes talking about why my wife and I had named our daughter as we
had. "What gave you the idea for that name? That's an unusual name. Did you know any Julianas? Were there any in the family? Are you sure? Can you think of anyone named Juliana? What associations does that name have for you?" Never once did he ask the usual questions: whether the baby was healthy or ill, round or long, or whether dad and mom were getting any sleep. I did not mind Lieberson's indifference to the ceremonies of interaction with a new parent; this was an unusual opportunity to observe a great sociologist chewing on his current obsession.2

Randall Collins. Because Collins is universally known as a theorist, it may sound peculiar to describe him as a passionate researcher—but it is an accurate description. When I was a graduate student and much inspired by Collins's Conflict Sociology (1975), I decided to make a pilgrimage to visit Collins. He seemed happy enough to have a young disciple visit. He was working at the time on several projects, ranging from the most micro to the most macro level. As part of his work developing the idea of interaction ritual chains, he was videotaping face-to-face interaction for clues to the transfer of emotional energy. One of the macro-projects was connected to his work on the rise and fall of empires. He showed me the computer simulations he was running on changes in state boundaries based on the unfolding of a small number of basic principles of geopolitics. It was a revelation to see the maps of the world change as Collins varied the formulas for interaction among states. Collins was no theorist aloof from the "real world," holding forth from his own special cloud of abstractions. He was firmly rooted in the world and in the history of the world's civilizations, and he was busy reaching out to understand and to model every level of social life.

Diane Vaughan. The distinction between quantitative and qualitative research—and the many inter-tribal conflicts that arise from this distinction—masks the commonalities in will and wile shared by outstanding researchers in both tribes. If one looks at the work style of Vaughan, an outstanding ethnographer of relationship and organizational failures, many of the same dispositions come into view. Vaughan has often described to me her need to pursue research systematically while reaching out to test intuitions along the way. When she began her book on the Challenger disaster, The Challenger Launch Decision (1996), she told me that she had started with the same working thesis as everyone else—that the people who were responsible for launching that ill-fated rocket were "amoral calculators" taking chances because of production pressures from above even in the face of evidence of risk. As she went along, she discovered that certain pieces of evidence seemed to contradict this image and led her to consider alternative explanations. Acts that seemed to be rule violations were actually not rule violations. When she began to look at the incident prospectively, as a process unfolding, rather than retrospectively as even the Presidential Commission had done, she began to see that the tragedy was more the result of the accumulation of small misjudgments in the interpretation of risk, combined with particular blocks in communication between parts of the many organizations involved in NASA missions. "Once you find an anomaly," Vaughan told me: "you have to redo your starting theory until you explain everything you find." Instead of stopping with the first volume of the report of the Presidential Commission, as nearly every commentator had done, Vaughan read every page of the four other volumes as well. These included 200,000 pages and interviews with 160 people involved. As she became more doubtful of her original expla-
nation, she threw out hundreds of handwritten pages of manuscript. And when she had begun to develop the outlines of a new explanation about why the Challenger was launched, she sought out the principals involved for a new round of interviews. Vaughan’s are the conclusions that nearly every sociologist who has the calling for research reaches: “You can never really go deeply enough.” And: “You have to force yourself to develop an explanation that accounts for anomalies in your data. If the explanation is a good one, all of the evidence—not just some of it—makes sense.” If it is a really good explanation, it can be formulated at a level of generality that can be applied to other cases.

I am not suggesting that every student of sociology will have the instincts, the energy, or the analytical gifts of these exceptional people; however, I do think that all of us can share the charismatic spirit of inquiry they represent in an ideal form. All of us can capture something of the excitement of tracking down information, sometimes in out-of-the-way places, to explore our ideas—that thrill of romping through the tall grass to sight, to circle, and, finally, to tackle a pet research question.

When it comes to the knack for research, it is simply not the case that you either “have it or you don’t.” Instead, it is a matter of developing the “habitus” of a researcher. As used by Pierre Bourdieu (1979), the rather mysterious term “habitus,” which can be loosely translated as the habits of a way of life, involves a complex mix of dispositions developed through life experiences, practical “tricks of the trade,” and relationships that provide aids, models, and reinforcers for the conduct of life. I believe Bourdieu’s concept can help to illuminate the distinctive features of the researcher’s way of life. Therefore, let me focus now, in a more analytical way, on the elements of the habitus of sociological researchers: disposition, craft practices, and associations.

DISPOSITION

I have suggested that the disposition of the researcher involves, at a minimum: (1) an obsessive concern with finding out the answer to a question; (2) a willingness to exercise unusual levels of self-discipline in the pursuit of those answers; (3) a dissatisfaction with incomplete or superficial work—including a propensity to do much more than is required in finding, analyzing, and interrogating evidence; and (4) a capacity to play with different approaches to understanding a problem and different ways of looking at a problem.

The difficult part for many novice researchers is not the time spent in the library or in other solitary research activities. Most have proved themselves capable of this sort of discipline or they would not be contemplating a scientific career. For this reason, I have emphasized the way that virtuoso researchers are constantly on the lookout for new data, constantly exploring ways to test and refine their ideas, and continuously playing with ideas that make sense of what they have observed and learned through previous work. It is a mistake, I believe, to think of master researchers as simply very disciplined (and, of course, very bright) people. Most are, in fact, unusually undisciplined in wondering about possible ways of making sense of the world—promiscuous even in their willingness to embrace, weigh, and discard possible explanations. But, they are also disciplined. This discipline comes from reading intensively, learning the techniques one needs to do the work, patiently collecting and analyzing data, checking and rechecking results, and writing up those results with convincing clarity. What is required, then, is nothing less than a marriage between seeming opposites—intellectual playfulness and rigorous discipline.

The disposition for research also has a social side. The ability to talk enthusiastically about one’s work and to bounce back from vigorous opposition are vital qualities in a successful research project. Teaching can be a very helpful way of developing these important capacities because it too involves both salesmanship and persistence. But another way is to start research activities
with the following premise in mind: For every colleague or funding officer who wants to help you, you are certain to run into one or two who will not. Persuasiveness (and competence) may raise the proportion of help to resistance, but neither one will alter the iron law of intermittent resistance to your work.

Here is an example of what I mean—and one that at first glance would not seem to describe a terribly difficult task. I recently organized a scholarly conference. In organizing the conference I spent hours and hours in interaction with funding agencies, potential panelists, and staff assigned by the university to help me. I was able to obtain sponsorship from several funding agencies, but I approached twice as many that declined. For every scholar who agreed to participate, at least one declined. The conference was a success, but without persistence in the face of many disappointments, it would never have occurred. I believed in it. I worked to present it in an enthusiastic and persuasive way. I followed up repeatedly, and I made sure that all the organizational details received attention and were double-checked. The point is this: Everything in the world that is done well requires a tremendous amount of energy and an equally large capacity to roll with the punches. In this regard, a conference is relatively easy; research can be significantly more demanding.

How does one develop the disposition to care passionately about research and do it well? Anselm Strauss (1959) and others have talked about how forming an identity as a researcher is similar to a conversion experience. Both involve committing oneself to a new identity, collecting the props appropriate to the new identity (files, bookshelves, briefcases, journal subscriptions and the like), emulating the style and habits of a guide to the new identity, meeting the challenge of a difficult performance (for example, delivering a paper in a professional meeting), and often changing one’s primary associates. The latter is important because one needs friends who will confirm a new identity, rather than friends who will question it. Strauss used the metaphors of masks and mirrors: One has to try on new “social masks” and put oneself into the position of being reflected in a new set of “social mirrors.” I would add that one has to learn the practical habits of successful researchers. None of this is easy, and it only rarely unfolds in a simple, linear way.

CRAFT PRACTICES

Once students are started on the research process, the will and wile to see their research to a happy conclusion remain all-important qualities. These dispositional qualities can be greatly aided by a few tricks of the research trade. These “tricks of the trade” are simply ways of maximizing involvement and efficiency with potential resources. I will discuss three of them: (1) cultivating files, (2) scanning the horizon for data resources, and (3) looking for alternative paths when one’s original path is blocked.

Cultivating project files: Shortly after it appears as an idea, a research project takes material form as a set of files. These files include relevant articles, available data resources, statistical tables, analytical ideas, and references, among other things. As the importance of a topic becomes evident, the file for that topic will very often become too large and will have to be subdivided in order to be useful. The amount of information collected on different topics determines the types of files developed for the project.

For a project on the development of professional education in the United States, I have developed separate files of statistical information on training and employment in the 12 professions I am using as case studies and single files for “standard curricula” and “licensing/registration” requirements. That is because, at the moment, I have a lot of statistical information on the number of people in each profession at different points in time, but only small amounts of material on the curriculum and licensing requirements connected to each field. If this latter information expands, so will the number of
files I keep. (Conversely, if I were working on a paper on professional licensing, you can be sure that each profession would have its own file related to licensing requirements. The fact that I have even one file on licensing suggests that in my thinking on the subject, I have reason to believe at this point in the project that educational programs are influenced by licensing requirements, and that variations in licensing requirements are therefore analytically interesting.)

Files provide the nutrients out of which research grows, but they have to be considered dynamic rather than static elements in the research project. They grow and develop with a project. Students need to make sure that their files are in good working order and that they check and rearrange them periodically. They should get into the habit of using tabbed dividers to separate types of files, and they should not be afraid to change the headings on those tabbed dividers or to rearrange the content of their files. Sometimes they will want to photocopy pages to place in more than one file. Because they have to be written down rather than copied, quotes and references should generally be kept in a separate filing system—either in computer files or in old-fashioned index card boxes.

**Scanning the horizon for sources of data:** Data sources can be a very useful set of files. Students can find sources of data by looking at government documents, by exploring the holdings of data archives, by scouring Web sites and, in particular, by talking to faculty and fellow students. They will find that researchers have collected a tremendous amount of data, and that people are very often willing to share data, even when it is not already in the public realm. (This is not invariably true, unfortunately; some researchers want to keep proprietary control of their data. In interaction with other researchers about the use of proprietary data, salesmanship and persistence once again come into play.)

Researchers have to be creative about using other people’s data. Secondary analysis can be an exercise in frustration. Other people's questions are rarely exactly the same as one's own, so it is important to be able to improvise, drawing on questions from many data sets, or adding new variables to existing sets. Often, no one has studied exactly what you are interested in investigating. In these cases, there is no choice but to collect one’s own data.

Here it is usually necessary to make the most of limited resources. Few researchers have unlimited resources to study their problems. In my view (a view not popular among methodological purists), some data are usually better than none at all. For example, recently I have been working on how universities have changed over the last 30 years. It is my thought that the arts and sciences have not suffered as much as might be expected given the enormous growth in occupational and professional training programs in universities (Brint, forthcoming). It occurred to me that one reason might be the propensity of professional programs to encourage an orientation to clients in the outside world, while professors in liberal arts disciplines consider the university their real home. Professors who are involved more closely in the everyday operations of their universities might be able to protect their disciplines, even in the face of student preferences for more practical training. I did not have the resources to examine this idea for all universities or even a random sample of universities, so I asked for data on the chairs of Academic Senate committees at eight campuses of the University of California and compared the proportion coming from arts and sciences disciplines to those coming from professional disciplines. I compared these proportions to a count of all faculty on the eight campuses, again divided between liberal arts and professional programs. I found that professors in the liberal arts on most of these campuses were, in fact, more involved in Academic Senate committees. This is only one indicator, and it is certainly not the only one possible. However, it allowed me to begin to explore a possible connection between political participation and centrality within the university.
Most "normal science" does not require a large amount of effort in scanning the horizon for sources of data. One can become the master of a particular data set and, if it is a rich enough set, write one substantial paper after another from this single source of data. It is also possible to concentrate as an ethnographer on the lives of a group in a naturalistic setting.

Those people interested in "big questions," however, have to learn to work with a number of different sources of data. Furthermore, to explore these questions it is usually necessary to work with quite a bit more data than is used. Thus, for the project I mentioned above on the rise of occupational and professional training in colleges and universities, I worked with more than 10 types of data: (1) data on trends in degrees awarded over time; (2) data on the geographic location of professional programs from the 1920s on; (3) data on the change in professional school curricula for a sample of 12 very different professional occupations; (4) data on changes in college and university curricula grouped by clusters of similar institutions; (5) data on the prestige of different academic and professional programs based on their academic selectivity and the expected mid-career incomes of graduates; (6) data on attitude differences between students and faculty in professional schools and those in the liberal arts; (7) data on the disciplines and schools involved in industry-university research centers; (8) data on the disciplines involved in community volunteer and other service work; (9) data on the career lines of doctoral graduates in liberal arts and professional fields; (10) data on the academic backgrounds of college and university presidents; and (11) data on the disciplinary backgrounds of faculty senate leaders. Not all of the data I used ended up in the final paper, but even the data I did not use helped me to see more clearly the themes I needed to develop. In most cases, it is important to do much more than is required.

Finding alternative paths when initial paths are blocked. Many people throw up their hands much too quickly in the face of difficulties. Every good researcher has at least the virtue of determination. I recently had an experience that brought home the importance of not stopping at the first sign of trouble. I have been working on a project concerning continuity and change in American higher education. As part of this project, we are surveying presidents, provosts, and directors of institutional research at some 365 American colleges and universities. We discovered that highly selective private colleges and universities were less inclined than others to participate in the project. I received counsel from some colleagues to simply make the best of it and be happy that all other sectors of higher education were well represented. Once I realized that a problem was brewing, my approach was to think through the possible solutions, not to abandon ship. Highly selective private colleges and universities are an important part of American higher education. It was inconceivable to me that we would simply accept a sample in which they were significantly underrepresented.

Rather than give in and "make the best of it," as I was counseled, I developed plans to overcome the problem. First, we would obtain feedback from the nonparticipating institutions about the reasons for their nonparticipation. On the basis of this feedback, we might reconsider some of our data collection strategies. Second, we would consult with people familiar with the thinking of top administrators at these institutions about what approaches are most likely to succeed. Third, we would work to develop relationships with executive assistants to the presidents and directors of institutional research at these institutions. Fourth, we would apply for additional funding to form an advisory committee of people respected in the community of selective private institutions and hope that they would help us to gain access, perhaps through writing a new letter of invitation to participate. As part of the new proposal, we would also request funds for site visits to collect some of the data for these institutions, on the assumption that
some directors of institutional research at these highly selective institutions would be too busy to cooperate. Fifth, we would alter the questionnaires (without sacrificing any questions of great importance) to make them easier to complete.

The moral of this story is: When you are at a loss, it is a good time to start talking to people about the problems you have encountered, and to start working toward new approaches. Persistence in pursuing an idea and willingness to talk about your ideas with others are at least as important as having good ideas in the first place. Often this means developing a plan to overcome initial resistance. At times, however, it may mean finding new data that may be relevant to your project. For example, if you are blocked in your efforts to compare private and nonprofit providers of the same service because private providers do not want to provide access, perhaps there are two nonprofits that are sufficiently different to allow for a useful comparison. If you feel that the problems are too great, that you are locked in a jail without chance of escape, you will almost certainly give up. But most of these jails are easy to escape. Instead of giving up, it is usually time to start planning the jailbreak.

ASSOCIATIONS

This brings me to the final theme that I wish to develop in this essay—the importance of other people to one's own research. Another problem with the detective novel as a metaphor for research is that the main characters are almost always solo practitioners, or at most accompanied by a relatively dim sidekick. These people rely primarily on their own analytical abilities, nerve, and instincts to solve cases. This image is not appropriate to research work. Even when researchers are not part of a team, they understand that research is not an activity for lone rangers. It should be thought of as a social activity through and through. Understanding research as a social activity means cultivating a disposition to interact in the right spirit and developing practical habits of association that can improve one’s work.

Researchers also have to think strategically about how to maximize social input that will contribute to their research. Here are three ideas: (1) stretch your networks, (2) build partnerships with people whose strengths complement your own, and (3) present work widely.

Stretching one’s networks. Most people have someone or a small circle of people whom they talk to regularly about their work: a spouse or partner, a friend or two from the graduate program. It is enormously important to have a basic support structure, particularly if this structure includes people who will tell you frankly if they think you are heading in the wrong direction. But small circles can also be limiting. As Georg Simmel (1971 [1908]) and Mark Granovetter (1973) both recognized, new ideas often enter small circles from outsiders, rather than insiders. In research, it is important to stretch your networks for many reasons. Here are only a few of the most important: to learn about important research and researchers; to hear about new sources of data; to test ideas on different sets of ears; and to get to know people who might prove even more helpful to your work in the future. By stretching one’s networks, I mean taking the risk to discuss research and exchange comments on papers with peers whose work overlaps with your own (and, even better, with more senior scholars in the same category).

Of course, it takes courage to stretch one’s networks. An overture can always be rebuffed or refused. Some senior scholars will injure a young researcher’s sense of dignity by treating him as someone not worthy of the time of day. But it is important to stay in the play and not to give up easily. A thick skin is a necessary accompaniment to serious dogcatching. (Think of all the brambles that you will likely encounter on such a chase.) Students will learn; if they do not already know it, that the research world is full of all types. Some are egocentric and dismissive, but others are kindly and help-
ful. It is surprising how often well-known sociologists are flattered by being called to discuss a finding from their research. I have had this experience many times, and so have those students who have had the courage to reach out. One of my students was amazed to get an immediate and lengthy response from Peter Blau about a question she had concerning a book he wrote in the 1960s. There is no reason to think that well-known researchers are less likely to be responsive than less well-known scholars. It is true that those who are still active are usually very busy, but they are usually also among the people most interested in research, and therefore most likely to respond to intelligent questions. It is important, however, to prepare well for encounters with people more experienced than oneself. Senior scholars may not have time to provide introductory-level explanations.

**Develop working partnerships with people who have complementary strengths.** Social science research is increasingly taking on the characteristics of research in the natural sciences. Most papers by natural scientists are based on research by a team of people, sometimes numbering as many as 100 or more. Most students’ first research experiences will be based on working in a team, usually for a faculty person or team of faculty persons. This is invaluable experience in the craft of research, and I highly recommend that every graduate student experience an apprenticeship of this type. It is impossible to understand what goes on in a research project from reading about it in a book; it has to be experienced. In addition, there are any number of unintended benefits in such an experience. When I worked with Karabel, I learned how to think like a researcher. But I also learned about the life of a professional social scientist. I learned something, for example, about the review process when work is submitted for publication. I learned how much time people put into book reviews and letters of recommendation. I learned something about the different and sometimes conflicting interests of writers and publishers. And I learned that research skills are translatable into many spheres of life. (One of our friends came down with a serious disease while I was working with Karabel, and I was intensely interested in Karabel’s decision to immerse himself in the medical literature on our friend’s disease to assess the validity of the studies on which doctors were basing their recommendations for treatment.)

Once students have completed their apprenticeship in research, it is a good idea for them to take a hard look at their strengths and weaknesses as researchers. Most people are not excellent at every aspect of research; partnerships can help to compensate for weaknesses. If a student is strong in statistical understanding, but not as strong in developing interesting hypotheses, it will prove useful for them to find teammates who compensate for their weaknesses. I am not a first-rate statistician; yet I work at times on studies that use relatively sophisticated statistical techniques. I have found it to be very much in my interest to find strong statisticians to join me on studies like these. Often, they enjoy testing dozens of competing statistical models. I would much rather be working on virtually any other part of research. Out of such complementarities, great research collaborations can be born.

Of course, not all research partnerships work out well. Disagreements about the proper distribution of credit have, in all likelihood, ruined more professional friendships than any other cause. Other issues can also ruin research partnerships: sometimes both partners will feel strongly that they are right about issues in the analysis and cannot come to a satisfactory resolution. Researchers may become irritated at their partner’s sloppiness, forgetfulness, dilatory habits, or other negative characteristics. You may, in turn, have traits that grate on your partner. A good research partnership is like a good marriage in the sense that partners need to be explicit about their expectations, sincerely concerned with one another’s lives, willing to overlook small irritations, and fair about giving credit for contributions to the success of the partnership. Fortu-
nately, the children do not suffer if a re-
search partnership falls apart.

**Circulate and present work widely.** I re-
cently had a student come to my office
requesting funds to present his work at a
professional meeting. I told him that we had
exhausted our funds to support travel. He
was disappointed and asked: “What’s my in-
tentive to go to the conference then?” The
advantages of presenting work are so obvi-
ous to me that I must have looked at him
with a faintly incredulous expression.

Work can begin to be known only if it is
circulated and presented. This is usually the
first step in the process of publication. By
circulating their work, researchers make
others in their field aware that they are
working on a particular topic. Some col-
leagues might respond with comments or
criticisms of the work. These responses can
help push projects forward. At conferences,
students will get practice presenting work to
a professional audience. They can meet
people who are interested in the same areas,
discuss their research with people who are
sincerely interested in it, and receive feed-
back from commentators and audiences.
Presenting a paper at a conference is conse-
quently the single best thing a student can do
for herself during graduate school—that is,
besides publishing.

Of course, it is risky to circulate and
present work. Not everyone will like the
student’s work, and they may even be criti-
cized in public. It is important to believe in
one’s work before presenting it and to make
certain to defend it if it is attacked. The truth
comes out (when it does come out) through
conflict more often than through accla-
amation. Things that are said at conferences will
help students rethink parts of their work that
may need rethinking. Sometimes the expe-
rience of presenting work orally is suffi-
ciently different from the experience of writing that
an idea will spring up at the podium for the
first time in the process of responding to
questions. The worst thing that can happen is that a presentation will not go well.
Fortunately, few people remember papers
they heard at conferences, so students can
learn from their experience without suffer-
ing any serious consequence.

Responding to other people’s work is part
of being a member of the sociological com-
munity, and there are benefits to it. Those
who respond to other people’s work begin a
process that may lead to the formation of an
intellectual friendship. Asking a question at
a panel may get an answer to an issue that
interests you while helping the presenter
clarify her thinking. By reading and re-
sponding to other people’s work, re-
searchers keep up with their field and earn
the gratitude of those whose field and earn
the gratitude of those whose field and earn

**CONCLUSION**

In this essay, I have discussed qualities of
outstanding researchers that, while rarely
discussed in methods courses, are, I believe,
largely responsible for the enduring quality
of their work. I have focused in particular
on the disposition, practical craft skills, and
associations that make up the life conduct of
outstanding researchers. I have rejected the
view that these qualities are innate, and I
have provided instead advice concerning
how these qualities can be learned.

I would emphasize in conclusion that one
of the great pleasures of research is that it
allows for—even demands—a wide range of
human qualities: self-denying discipline and
self-indulgent playfulness, cautious search-
ing and driving intensity, advice-seeking and
salesmanship, grinding persistence, and cre-
ative improvisation. Students may find a
certain liberation in embracing these oppo-
sitions. In any event, they must learn to do it,
for important achievements have rarely been
attained by those who cannot combine seem-
ingly opposite qualities of mind and prac-
tice. Anyone who wishes to catch the dog
will need to carry along both hiking boots
and sprinter’s cleats.

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