I

PROBLEMS IN DECIDING WHAT THE RESEARCH IS ABOUT

Many projects stall because it is not clear to the researcher what the research is about. "What am I studying?" "What is the point in terms of knowledge in my field?" "Why am I doing the study this way?" "What was I thinking?" "I gathered this data; now what I am supposed to do with it?" "I thought I knew what I was doing, but now that I am at this place, I know I have to change my thinking, but to what?"

Sometimes an investigator becomes aware of these issues on his or her own and becomes stalled. But sometimes it is only after receiving a rejection from a journal or a grants panel or a demanding revise-and-resubmit letter that a project stalls as the investigator now realizes that there are confusions and ambiguities to deal with concerning what the research is about. This chapter addresses how to get research moving again after it has stalled because of difficulties in deciding what the research is about.

When the Problem Is Conceptual

Research is fundamentally conceptual. So it's not surprising that many problems with stalled research are at least in part conceptual. Conceptual problems do not necessarily stand alone; other issues discussed in this book may also be part of what is stalling your project even if you can see how the project is stalled by conceptual problems. But conceptual problems, in my experience, often need to be dealt with as part of restarting a project.

Conceptualization or Theory That Is Inadequate or Unclear

Overbroad Conceptualizations or Theories. The social and behavioral sciences provide us with ideals, models, and standards for what is good conceptualization or theory, and often there are strong disciplinary "shoulds" about the crucial importance of conceptual or theoretical grounding for any piece of research. The standards and "shoulds" for conceptualization can be framed in terms of what Kuhn (1996) called a scientific paradigm. A scientific paradigm is what the researchers in a discipline generally recognize as scientific achievements, what is important to study, and the best models for how studies should be done. Paradigms in the social and behavioral sciences include broad conceptualizations or what are sometimes called theoretical frameworks, which are basic conceptual lenses for looking at and communicating about phenomena the discipline focuses on. These broad conceptualizations may be wonderfully helpful in looking at the world in a disciplinary way. But among the many conceptual issues that may stall a research project, the project may be framed with a paradigmatic conceptualization that is too broad and general to adequately organize, guide, and give meaning to the research. That is because a broad framework may not address the specific phenomena one is researching. For example, working within the broad paradigm of conflict may not be nearly as helpful as working within a more specific paradigm about conflict in couples making decisions about pregnancy. One may need a more focused conceptualization that can provide ideas specific to what one is studying. Theoretical specificity helps justify the specific study and define what is to be done. And at the write up of the study, conceptual specificity makes it easier to write about the contributions of the research to a clearly identifiable literature in the field. For example, it may be much easier to link the research to the 35 sociological writings about couple decision making regarding pregnancy rather than the 56,000 sociological writings about conflict, or, to take a different example, to the 8 works on immigration involving people from Guatemala instead of the tens of thousands of works on immigration in general.

Theoretical Terms With Unclear Meanings. Another problem that may arise when using a broad theoretical framework but that may be present even when using a narrowly focused framework is that sometimes it is not clear what one or more terms in the framework means. One tip-off that research may be stalled because of this is that various research studies in the area of one's research that cite the framework do not fit together

but seem based on idiosyncratic and incompatible derivations from the framework. If your research is stalled because a theoretical framework seems ambiguous or hard to make sense of, based on your own struggles with it or what you see in the literature, you might have to amend the framework. In what you write you might, for example, clarify a definition, create a provisional translation of the theory, or develop rules for applying the theory to the specific matter you are studying or the specific kind of data you have gathered or want to gather.

Loyalty to an Inadequate Theory. Sometimes a researcher can be stuck because of loyalty to a theory that is inadequate, unclear, or does not apply very well to the matter the researcher wants to study. If the theory is one's own, one's mentor's, or the favorite theory of the scholarly in-group to which one belongs or aspires, it may be difficult to question or modify the theory or to decide not to use it. But if the theory or one's loyalty to it in its present form is blocking one, the only alternative to amending it may be to abandon it. And if one abandons the theory, one will have to find an appropriate new theory to bring into the project. It can be controversial, risky and difficult to change theory when a project is ongoing, but people do it (see discussion later in this chapter), and it can bring life and forward motion back to a project.

Theoretical Orthodoxies That Create Problems. It is possible to become blocked by disciplinary, research area, or academic department thought concerning what constitutes proper research conceptualization. Many researchers function in an academic environment in which the paradigmatic templates and ideals in the curriculum, the standard textbooks, and much published research shape and limit ideas of what conceptualizations should look like. These limitations could create problems if they leave you confused and insufficiently guided about what your research should focus on and how it should be carried out. Conversation with experienced colleagues may help with interpretation or translation of theory into a more usable form or with identifying unwritten rules about how to get from theory X to research. You might also find models in the research literature of how to move from theory X to research. Another alternative is to find or develop a different theoretical base for your work. To accomplish that you may have to go into the literature on theory in your field and into philosophy of science to find a theoretical conceptualization or an approach to thinking about and using theory that works for you and provides scholarly guidance and legitimacy for what you will do.

Disciplinary Contradictions. Another problem that can arise with theoretical conceptualizations and lead to a project being stalled is that there may be irresolvable contradictions in how, in your discipline, the domain of your research is conceptualized. So if you are stuck it may be because you are at least somewhat aware of the contradictions in the discipline but cannot resolve what others in the discipline may not have resolved either. You may even feel it almost is an act of dishonesty to do what others in the field have done with the inconsistency (ignore it, write as though it is not there), and then you might be stuck not wanting to do what is dishonest but not having an honest way out of the dilemma. For example, the mainstream conceptualizations in the field may say that self-report is untrustworthy because people cannot and will not be honest, but those same conceptualizations and textbooks may also address people's self-concepts as though there is some kind of integrity to them and hence to research assessment of self-reported self-concepts. When faced with that kind of confusion in one's discipline, part of the trap is that one risks offending powerful others by pointing out how they have fostered or missed the contradiction. What to do? It depends on what one wants to risk. If the route out of being stalled is to challenge how powerful others have been conceptualizing, that may be what one has to do, though of course there are cautious and diplomatic ways to do it.

Issues With Informal Conceptualizations. Partly because the formal theories of a field may be less than inspiring or may not seem compelling, some researchers use informal conceptualizations either alone or in conjunction with formal theory. Informal conceptualization can give research focus, direction, and meaning. The informal conceptualization may be in the form of stories drawn from fiction, drama, or entertainment TV; accounts from the news; apt metaphors (Rosenblatt, 1994); or autobiographical anecdotes. Such a conceptualization is typically not offered in the language of formal theory but in meaningful, coherent, and evocative narrative format. It might be, for example, the story of Romeo and Juliet, a personal story about one's relationship with a sibling, a story from literature about what happens to a farm family when the crop is lost, a story of a couple whose marriage ended after a child died, or a metaphor drawn from immunology and applied to social life in a low-income community. Informal conceptualizations may motivate and guide a researcher, offering a basic idea of the phenomenon of interest; a sense of how important the phenomenon is; a sense of what to focus on; and a way to sell the research to colleagues, students, and the public. Informal conceptualizations may work beautifully; however, if your research is stalled and you are

using an informal conceptualization, perhaps it is making trouble for you. An informal conceptualization can sometimes blur what your research is about. It may be incomplete, ambiguous, or contain internal contradictions. It may have implications that are irrelevant to the situation you are focusing on (for example, the story of Romeo and Juliet has many elements that might be irrelevant to research one is doing on couples whose families might not like one another). In addition, an informal conceptualization may fit poorly in some way with formal theory you also are using. With any of these problems with informal conceptualization, you may have trouble deciding how to communicate your research plan in a proposal, how to gather and analyze your data, or how to write up the study coherently. You may not realize you have this problem until you have trouble with a dissertation committee or you receive a rejection from a journal. But once you know that there is a problem, if you are working with an informal conceptualization, you might consider abandoning it and either finding a different one that works better or relying only on a formal theory in your discipline that might provide a safer and clearer framework. On the other hand, if you love your informal conceptualization and there is a problem with it, perhaps the thing to do is to hold on to your informal conceptualization and to strengthen or clarify it, to simplify and focus it, or to use it more as an illustration and evocative source of suggestive applications than as a research guide.

Problems With the Topic Label Applied to the Research. Another conceptual challenge that can lead to a project becoming stalled will appear to be a problem with theory but is actually more a problem of how you have labeled your research topic. Your label calls for the use of certain kinds of theory, and sometimes the theory it seems to call for does not work. And no matter how you wrestle with the theory or try out related theory, things do not seem to work. In such cases I think what often helps is changing how you label your topic area. Try more abstract or less abstract labels for the topic area, or conceptualize your topic from some other angle so that a different label becomes relevant. Any of these approaches may help you to identify a new theory to use or may help you to see new ways to make use of the theories you have been trying to work with all along. Imagine, for example, that you are interested in studying how young children starting school talk about the various tasks they have to master at school, but you cannot seem quite to pin things down conceptually. You might try moving to a more abstract conceptualization—about child development, for example. You might try moving to more concrete conceptualization about children learning rules and, as part of this, learning language for rules. Or you might

get at what is going on by labeling your topic as children learning right from wrong or children learning how to listen to teachers. You might even focus on how children's use of a few specific words, such as "bad" and "wrong," changes and develops.

Too Many Research Questions. A final conceptual issue that I think is frequently present with blocked research has to with research projects defined by a relatively elaborate set of research questions. I think that often researchers with a substantial list of research questions for a project do not have a clearly focused idea of what they are doing. They may have generated complicated lists because they could not succinctly and confidently enunciate specifically what their research was about. One symptom of this is that problems arise with theories that seem inadequate or unclear, but arguably the difficulty is not with theory but with the range and complexity of what theory is being asked to deal with. From this point of view, if your research is stalled and you have more than one research question, it may help to pick one question and focus only on it. Or, if you have many research questions, at least winnow the list down to a smaller number. Alternatively, you may resume research momentum if you can drill down to whatever is fundamental in your project and that underlies all the questions. Make that most fundamental matter the subject of a single research question.

Adding Conceptualization After a Project Has Begun

If your research is stalled, it may help to add conceptual grounding to the project or to change the project's conceptual grounding. Let's say you identify an alternative, potentially helpful theoretical framework. Think through the ways that your research links to that framework. Rethink the title of your work, your research question, the literature you cite, how to justify your research methods, how you analyze the data, and how you will report your findings and make sense of them. You have now refreshed your project and quite possibly have resumed progress.

There are those who frown on adding conceptualization to a project or changing it after it has begun. They might say that if the conceptualization has not guided things all along, it is likely that what has been done so far in the project is not loyal to or congruent with that conceptualization. And if one eventually writes up the project as though the conceptualization was always present, that is misleading. On the other hand, adding or changing conceptualization after the project has begun may be the best one can do in salvaging and restarting a stalled project.

And many researchers, myself included, have done that. If one feels uncomfortable about doing it, one can be honest in one's writing, describing when and how the current conceptualization entered into the flow of the project. But however it is handled, I have seen conceptualization transfusions revitalize research projects and even make them more interesting and valuable.

If One Has Lost the Conceptual Thread

Sometimes the problem that stops research from going forward is that one has lost the conceptual thread. Perhaps the project has become so conceptually messy that it is not clear what it is about. Perhaps one cannot remember enough about how the project was conceptually grounded. Perhaps one has a set of research questions that, taken together, blur what the project is about. Or perhaps one has two or more not-quite-compatible theoretical conceptualizations in mind, and their non-fit with each other stops progress. Also, sometimes one has been away from a project for a long time, perhaps because of illness, a new baby, or a work overload, and has lost the sense of what the project was about and what held it together. Sometimes one might never have known what one was doing, there was no conceptual thread, and one's research was a fishing expedition based on a spark of curiosity or the existence of a convenient data source. Any of these issues may stall a project, but quite possibly they can be dealt with.

One may be able to force a project into conceptual clarity simply by outlining in some detail what one hopes to write or has been writing. You may not even have to outline the entire piece but can focus instead on areas of the writing where the mess seems greatest or the lack of clarity is most striking. The act of outlining may lead you to discard some ideas as tangential, may push you to be more precise and consistent in your language, and may make it clear that some ideas must be subordinated to others. In fact, outlining at any time of researcher confusion may work miracles.

However, if you did not know from the beginning what your conceptual thread was, my advice is to get in the mode one gets into when first trying to identify a topic to research. Pretend you are starting from the beginning. This is not a book about how to start new projects, but I have a few suggestions. Make your research interesting to you, even if what interests you seems too simple, too descriptive, too concrete, or not in the mainstream of your field. Research work is often hard and demanding, so best to be focused on what interests you. Although there is much

to be said for working in the mainstream of your field, I think it can be easier to make progress if one works on what interests one. Also, keeping things descriptive and concrete might be a good place to start, partly because it is easier to develop more elaborate, more theoretically informed and informing conceptualization, and a more precise plan for research if your work is grounded in concrete ideas, data, examples, and situations. For example, I think it is easier to build things up conceptually if you decide to research the problems of elderly people who were trapped in their apartments on the Lower East Side of Manhattan in the aftermath of Hurricane Sandy than if you decide to research a broad and abstract topic like stress in the elderly.

If you at one time thought you knew what you were doing, try to recover the lost conceptual thread. You could look at your research pro-posal, memos (to yourself, collaborators, or mentors) about the research, your proposal to the institutional review board (IRB) for approval to work with human research participants, outlines, and anything else you have written that might include a statement of the research question. Sometimes you may have to retrieve the material from others—for exam-ple, from colleague or IRB records. When the material has been retrieved and is in front of you, it bears careful study, particularly if, as is sometimes the case, the conceptualization is blurry so that different statements of the research goals, research question, theoretical groundings, justification, and so on, are somewhat different or seem incompatible. When you look at the materials, getting things together conceptually and feeling grounded enough to proceed may be easy. But sometimes the concep-tual material is too thin or the differences are not simple to resolve. Sometimes the conceptual foundation for the work does not persuade one that the work is worth continuing. In any of those cases, if you want to continue the project you may well have to rethink it and come up with new, possibly even quite changed, conceptual foundations. As I indicated in the previous section of this chapter, some people think that is undesir-able and that conceptualizations should remain the same throughout a project. But perhaps because I have become for the most part a qualita-tive researcher and expect conceptualizations to emerge and transform as the data are gathered and analyzed, I do not think it bad that one might come to different ideas as the study progresses. One learns new things from research participants, research data, new literature read, old litera-ture that is understood in new ways, colleagues, and life. One realizes one was mistaken or not thinking clearly about this or that. One learns enough to come to recognize the limitations of the conceptualization with which

one has been working and comes to see other ways of framing things and making sense of the data than one did at the start of the research. In fact, in my experience, researchers often change at least nuances of their conceptualizations as the research progresses.

Sometimes a lost conceptual thread can be picked up while working in isolation, but my experience is that it often helps to work on conceptual problems in dialogue with others (Rosenblatt, 1981). If one does not have collaborators on the project with whom to talk things through, an adviser, or a mentor, I recommend finding people with whom to converse or writing letters to someone explaining what one thinks the purpose of the study is. With letters I think it can sometimes help to write to someone who has no particular expertise in one's area, which forces one to keep things simple and clear. Similarly, Wolcott (2001, p. 39) suggested going to lunch with a colleague who has a good analytical mind and explaining one's research to the colleague. By having to articulate one's goals, reasoning, assumptions, and the like, to the colleague, one may come to greater clarity. In addition, the colleague may offer helpful questions, suggestions, or criticisms.

If you feel you have lost the conceptual thread, it is possible that thinking things through could lead to a decision to stop the study (see Chapter 6) because its foundation is too shaky or it seems unlikely to make an unambiguous contribution to knowledge. I am definitely not in favor of going ahead if one's thinking remains muddled and one is not sure what the study is about, but in my experience researchers often can rediscover or invent a conceptual foundation that will enable them to move forward if they can resolve their conceptualization difficulties and think things through in a refreshed way. Recovering or reinventing theoretical foundation, even if it is demanding in terms of time and how much you will have to learn, may be very rewarding because of what you gain in thinking and scholarship, because of the confidence it gives you to move forward on the project, and because of the foundation it gives for further research and writing. From some perspectives, it is why we are in the business of research, to get to new and quite possibly deeper levels of thought and scholarship. Then, too, one may decide that what one has learned through all the extra time, effort, and reading it took to get to the new and deeper place of scholarship is worthwhile writing up itself as a theoretical or conceptual paper or as part of a literature review (Wolcott, 2001, p 39, citing the first edition of Becker, 2007). Then one ends up with an additional publication, a conceptual piece one might not have originally been thinking of doing.

It May Be Acceptable to Do Descriptive, Atheoretical Research

It is legitimate in some content areas to write a research report that is descriptive and atheoretical. In some research areas, descriptive work is considered a contribution to knowledge. For example, if there was little or nothing in print about, say, Tibetan families, local-level political processes in Venezuela, or how people with Parkinson's disease experience their decline in functioning, a descriptive paper could be justifiable and valuable in some disciplines or subdisciplines even though it was minimally or not at all conceptual. Hence, if your project is stalled because you have lost (or never had) the conceptual thread in terms of underlying processes or causal dynamics, but your work is descriptive, such work is lacking in the area of your research focus, and you are willing to publish where descriptive work is appreciated, consider abandoning the search for a conceptual thread dealing with causes or underlying processes and instead make the project descriptive.

In many areas of social and behavioral science, one is expected to base one's research on theory concerning underlying processes or causes, and often a desired product of one's research is new theory, amended theory, reaffirmation of theory, new application of theory, or at least illustration of how to use a particular theory. That makes sense. Theories are tools that can be used in many situations. Moreover, in terms of keeping track of knowledge and being able to remember things, it is much easier to master a relatively small number of explanatory, insight-giving theories than to keep track of thousands of studies. Also, from a philosophy of knowledge perspective, one can argue that knowledge is conceptual and in the form of broadly applicable conceptualizations. But it also makes sense to me that what is concrete may be compelling—for example, the specifics of how people enter and leave prostitution in Atlanta may make for a very influential publication.

Imagine a researcher who has been stalled for a long time on the issue of valuing concrete description about some matter but feeling compelled to offer theoretical statements about causality or underlying processes. I think researchers frequently become stalled that way because powerful gatekeepers (a doctoral adviser, dissertation committee members, a grants panel, the editor and reviewers for the journal one most wants to publish in) insist on such theory. Issues with gatekeepers are discussed extensively in the next chapter. Here I would suggest that one possibility is to give your gatekeepers the best, most relevant causal and/or process theory you can think of, and then plan down the road to publish more of the descriptive material in an appropriate publication outlet. Another

possibility is to persuade the gatekeepers, perhaps using theory, that in your research area a relatively nontheoretical approach makes sense. Perhaps what would be easiest, if acceptable to your gatekeepers, is to work up the empirical generalizations that are likely to be part of your descriptive work into a kind of causal or process theory, or at least as an illustration of what is already in such theories. For example, if you are describing what could be taken as a system of how farm families are trapped financially in ways that push many of them out of farming, you could show that this illustrates broad systems theory phenomena, and in the process you may come to new insights about your descriptive material by looking at it from a systems theory perspective.

If You Do Not Know What You Are Doing in Your Research

I know quite a few researchers who have had the disquieting experience of realizing in the midst of a research project that they did not know what they were doing, why they were doing what they were doing, what they should or could do next, or even precisely what the project was about. This goes beyond the conceptual issues discussed in the first part of this chapter. Among many possible laments might be "Why am I gathering these data?" "How does this part of the research fit with that part?" "Why didn't I think through what analysis I would do before I gathered these data?" "What made me think this would be publishable?" Fortunately, for many researchers the uncertainty and disorientation is resolved rather quickly (though the embarrassment may linger). In only some instances does the project become stalled long term because the researcher continues to feel some version of "I don't really know what I am doing." In my experience that is more likely to happen when one is more or less a beginner at research, and that is why I want to turn next to issues with being a beginning researcher.

If You Are a Research Beginner

Research education often falls far short of educating us to do research (Hemmings, 2012). Thus, if your research is stalled because you are, relatively speaking, a beginning researcher (in general or with regard to some aspect of your research) and do not really know enough to proceed, you are where many of us were at one time and maybe still are. What to do? First of all, from my perspective there is no stigma to turning to others for help. Research can be a solitary activity, but often research that one might

assume is solo (because it is the project of a single investigator) actually involves extremely helpful interaction with colleagues, consultants, and other potential helpers. That makes sense, because even if you are an experienced researcher there are always people who have more expertise than you, and if you are a relative beginner, there are potentially large crowds of researchers who have more expertise than you. You might have to overcome a feeling that you should figure things out on your own, and heaven knows many of us who get Ph.D. degrees value our autonomy and independence and like figuring things out for ourselves, but help is available and can make a real difference.

Where do you find consultant or mentoring help? Bigger universities and bigger university departments often have people who, as part of their jobs, consult on research design, statistical analysis, data analysis, grant seeking, and academic writing. You may also find help among colleagues and graduate students at your institution. And if there is nobody nearby who seems to be available or able to help, there are scholars elsewhere who would be willing to provide advice about your research. In fact, as I suggest in many places in this book but particularly at the end of Chapter 3, it is to your advantage to create your own network of colleagues around the country and the world.

One challenge in getting help when you are a beginner is that you may not know what to ask. You may not be able to articulate where in the research process your problem is located. You may not know how much of what you "know" is too simple, wrong, or incomplete. So sometimes a consultation has to begin with a process of developing the questions that will be the focus of the consultation and to sift through what you believe to be true. That might involve your helping the consultant to understand what you think you want to do, and it might include beginning to learn the vocabulary the consultant uses and figuring out how to apply it to your project. Experienced consultants will know how to work with you to clarify matters. And if you use an experienced consultant, do not be surprised if the consultation goes somewhere other than where you thought it would go. You might, for example, have thought you needed help with data analysis, but in the end the consultation may be primarily about clarifying the research question, coming to understand what the problems are with a particular measure in your study, or deciding what in the literature is and is not relevant to your research.

Notice that I have not said that if you do not know what you are doing you should abandon your project. If you want to abandon it, go right ahead. But I think it is a mistake to abandon a project at the point where one realizes one does not know what one is doing. At the very least there

are lessons to learn about how one got into trouble and what might have been done to avoid it, and one cannot do that learning until one has gone through the work of trying to be clear about what one is doing. It is also possible that a project that is floundering because you do not know what you are doing may yet turn out to be quite interesting and productive. In fact, there may have been insight and brilliance in your initial interest in and plans for the study, and even if things became confused as the study advanced, that initial insight and brilliance may still bear fruit. There may even be great insight and brilliance in your becoming stalled on the project because you may be stalled as a result of seeing important things that others have not.

Notice also that I am not saying that if you do not know what you are doing, it is a writing problem. It may be experienced as a writing problem, but if you find your writing has stalled *and* you do not know what you are doing, do not mistake all the time necessary to achieve clarity about what you are doing as writer's block. In fact, quite to the contrary, coming to clarity about what you are doing can be a valuable part of the writing process, even if it takes days, weeks, or months to come to that greater clarity before you can write another paragraph.

Guiding Your Work by Using the Structure of Academic Writing

Academic writing in the social and behavioral sciences is often criticized as sterile and dull, and that criticism points to the limitations of the templates to writing that one ordinarily is expected to follow (for example, title page, abstract, key words, introduction, literature review, statement of the problem, methods, results, discussion, conclusion, references). That routine and generally expected organization to writing seems to some critics to stifle imagination and suppress information that could be very important and interesting. But it also helps one to recognize when one does not know what one is doing. Imagine, for example, being unable to offer readers a statement of the research problem or being able to offer only an incoherent statement of the research problem. Or imagine not being sure what literature to review. Those are warnings that one may not know what one is doing. From a related but different angle, however, the structure of academic writing offers a guide through the research process. If one does not know what one is doing, one can look at the structure of academic writing and try to think things through so one can fill in the template. What is my title? What should I say in my abstract that is accurate and meaningful? What key words would most clearly fit this paper? How should I introduce this research to readers in

a way that makes it seem a contribution to knowledge? So the standard structure of academic writing can be helpful. One can get help understanding and using those structures from published guides to research writing, such as the *Publication Manual of the American Psychological Association* (www.apastyle.org/), the *Style Guide of the American Sociological Association* (www.asanet.org/documents/teaching/pdfs/Quick_Tips_for_ASA_Style.pdf), or *The Chicago Manual of Style* (www.chicagomanualofstyle.org/home.html). Also, many college and university library websites offer rich information on style guides. Most academic journals have on their websites and in some if not all of their published issues their own style guide. In addition, when it comes to journal articles, I find it most helpful to download several recent articles from the journal I am targeting and use them as style guides and as possible templates for the structure of what I am writing. Some grant agencies feature models of successful proposals on their websites, but with other grant agencies you may have to do considerable detective work to get access to successful proposals. Of course, if you know someone who has a grant from the agency you are targeting and they are willing to give you a copy of their successful application, that can be a very useful resource in organizing your proposal.

Help With Statistical Analysis

Statistical analysis is one of the most common aspects of the research process in which people I have worked with believe that they do not know what they are doing. In my experience, many people with substantial research education think in other terms than numbers, and for them statistical analysis may be relatively meaningless or even incomprehensible. Related to this, some people have trouble bridging the gap between the abstractions and ideal cases they have been taught in their statistics courses and the realities of actual data. Also, when it seems that there is more than one way to analyze a specific quantitative data set, a researcher may not understand the options well enough to choose wisely among them.

Sometimes, however, the problem is not so much in determining what statistical analysis to do or how to do it but in understanding the quantitative data one has. In my experience, it can help a great deal to go back to the origins of the numerical data. What went on that created the numbers? What instruments, what counting procedures, and what protections against invalidity, deception, or error were used? How were the data cleaned? If the quantitative analysis is to focus on differences between categories of people or of other entities, how trustworthy and meaningful

were the processes that assigned people or other entities to those categories? Sometimes the answers to these questions can make clear to a researcher what the strengths of a data set are and how the statistical analysis might best be focused. Or, conversely, sometimes the answers to these questions raise enough concern that the next step in the research might be not statistical analysis but work at repairing, cleaning, or otherwise strengthening the accuracy, validity, and credibility of the data. But let's say you have a great deal of confidence in the data and have a good idea of what you hope to show to be true of the data, but you still are unsure about how to analyze the data statistically. What can you do?

In most universities and research communities it is not difficult to find statistical consultants. However, one tricky part with statistical consultation help is that one needs to know some things in order to make the best use of the available help. If one does not know enough, one might not be able to decide whether the help one is receiving makes sense and is the best option, may not be able to accurately apply the help to one's data, and may not be able to write about the statistical analysis with confidence and clarity. Hence, sometimes one needs consultation about how to make sense of statistical consultation. Luckily, there are statistical consultants with great expertise in getting down to the basics of making sense.

If you have been stalled on statistical issues for some time and you still do not understand what to do or why you should do what the consultant suggested doing, or if you have gone to more than one consultant and they have given seemingly contradictory advice, you probably should go back to the consultants and say you need further or more basic help. Experienced statistical consultants know that sometimes people need to come back, perhaps even quite a few times, in order to figure things out and get clarity. Alternatively, you may need to seek out other consultants. There may be someone on your campus, a faculty member or graduate student for example, with relevant expertise who is willing to help and who has the capacity to make things clear and understandable. You may also be able to get help from someone elsewhere, such as another researcher who has published in your research area using data like yours and who used statistics that passed muster of the muster of editorial reviewers.

If You Have Too Much Data to Organize or Make Sense Of

Sometimes we find ourselves with too much data to organize or make sense of. What should a researcher do if overwhelmed by the complexity or quantity of the data? What may be easiest is to organize and analyze

only part of the data. For example, if one is studying stresses in firefighter families and one has vast amounts of data from each of 500 families, one might begin by analyzing and writing up the data from 20 families, or the data only from the spouses/partners of the firefighters. That may enable one to see things with greater clarity and to then move to analysis of more of the data. Or one could analyze data from a few of the most theoretically significant measures or interview questions and either leave other data for another time or decide never to do more with them. In my experience, there are many large-scale studies in which great swathes of the data never get much, if any, attention in publications.

Stalled While Working With a Data Set Others Have Successfully Used

Sometimes there are issues with data that are not the focus of the initial publications from a project, and those issues become a problem for a graduate student, postdoctoral student, or beginning faculty member who is offered the opportunity to carry out a secondary analysis of project data and then to write up the results of that analysis for publication. I discuss secondary analysis in different ways in Chapter 5, but here I want to say that I think often what has been written up and published is what is strongest in the data and makes the most sense. What is left may be harder to work with and may yield a paper that is much harder to publish or to get the approval of a dissertation committee. Although there may be treasures in the data, one should be wary of data that come from already mined data sets. Sometimes it is easy enough to see the risks in working with such a data set. But sometimes it is difficult to discern how problematic the offered data are—for example, because the data are complex, because someone who is high status and persuasive said there is something there, or because it is not clear what processes and specific measures went into the creation of the data records available to analyze.

In general I would say that secondary data analysis should begin with careful and systematic research by you of what is in the data. For example, if you are working with quantitative data, you may gain much from asking what items underlie the specific scales and variables that you are thinking of using. How were things worded; how were scale scores created; how was the data cleaned; what was done with missing data; and, if any data were discarded, why were they discarded? The more deeply and thoroughly one digs into a data set on which one is going to do secondary analysis, the more clear it may become what one has and does not have to work with, and that clarity may help a researcher to head off or overcome being stalled.

If you have done your homework on the data you are working with but you still have problems with the data and independent experts you consult are unenthusiastic about the quality of the data you are working with, you might do well to abandon the study. On the other hand, sometimes there are treasures to be found in data that have been previously worked with if one digs deep into them.

Help With Qualitative Data Analysis

If you are stalled in carrying out qualitative data analysis because you do not really know what to do, I have some suggestions. First of all, there are many different legitimate approaches to qualitative data analysis. For some projects there are specific qualitative data analysis approaches that are preferable, but often there are a number of approaches that could be appropriate. The specific kind of analysis one chooses should fit one's statement of the research problem, one's approach to data gathering, the data one has, and what one wants to write. Assuming there are several different approaches one could legitimately use, my recommendation for people who have little or no experience at qualitative data analysis is to choose a relatively simple approach, one with some easy-to-follow resources on how to do it readily available. If you have no preference, I recommend thematic analysis (e.g., Braun & Clarke, 2006; Guest, MacQueen, & Namey, 2012; Patton, 2002, pp. 452–471). One reason I recommend thematic analysis is that it is usable across a wide range of conceptual approaches and data—for example, one can use it in phenomenological research when looking for phenomenological themes or in narrative research when looking for narrative themes.

I hesitate to recommend grounded theory analysis to someone who is a beginner at qualitative data analysis. Although many published qualitative studies claim to use grounded theory analysis, the various grounded theory approaches are demanding to master and do well (Bryant & Charmaz, 2007). In fact, there are reasons to question the extent to which many works claiming to use grounded theory analysis actually are grounded theory works by the standards of the established guides on how to do grounded theory research (LaRossa, 2005). Most established grounded theory approaches develop theory inductively in a process requiring data gathering to be carried out in interaction with evolving stages of data analysis and theory development. Perhaps the following is a gross and inaccurate generalization, but it seems to me that many qualitative projects that claim to use grounded theory analysis

are set up with prior theoretical conceptions about the matters to be studied, so they are not inductive, and they rarely are organized to carry out a series of steps in data gathering that is affected by and affects the process of developing a "grounded theory." If you have the knowledge and skills to carry out grounded theory work, that is wonderful. Authentic grounded theory research can be quite impressive. However, working as a beginner in grounded theory research, especially if one is doing it without mentoring from an experienced grounded theory researcher, is very challenging (cf. Bryant & Charmaz, 2007). Related to this, I have seen grounded theory projects swamped by the amount of detail and complexity that results from the way some researchers carry out the open-coding phase of grounded theory research. So if you are working on a stalled grounded theory project, my suggestion is to consider switching to a basic version of thematic analysis. Thematic analysis is less demanding because it focuses on the data at hand rather than demanding a process of repeated analysis, repeated rounds of data gathering, and multiple stages of theory development. Grounded theory analysis has an important place in the disciplines, and there are many very powerful, influential examples of its proper use, so I am definitely not opposed to grounded theory analysis. But I think it is easier to avoid being stalled doing qualitative research if one avoids trying to force a project into a grounded theory framework it does not fit or that demands more than one is willing or able to do.

I am not saying that all qualitative research projects that are stalled are salvageable or should go forward. Sometimes the data are too thin; too internally contradictory; or from cases that are too disparate, odd, or untrustworthy. And sometimes the chosen topic is one for which the researcher will not have adequate data. For example, even though one might have good reason to think people would have a lot to say about government-mandated high-stakes testing in schools, in fact the people asked about the topic might have had little or no knowledge or experience of it, might never have paid attention to it, might not think in those terms, or might otherwise not be people who would give reasonable data about the matter. So sometimes when a researcher has become stalled while trying to do qualitative data analysis, the problem is that there is not enough in the data. In such instances I would suggest abandoning the study (see the last chapter of this book). Trying to build a substantive contribution to the literature out of inadequate data is ordinarily a waste of time, not good for the researcher's morale, and not good for the social and behavioral sciences.

If the Analyses Are Too Complex to Grasp or to Write About Coherently

Some projects become stalled because the data analyses are too complex to grasp or to reduce to simple enough statements to write a report that others and even the researcher can track. Data with great complexity can be one of the delights of doing research. But sometimes complexity can be overwhelming and lead to a project becoming stalled. One obvious thing to try then is to simplify things so that the next steps in the research can be manageable. Sometimes simplification can be accomplished by answering the question "What is most important here?" If you have an answer to that question, then focus the analyses and writing on whatever that answer is. But if much or everything is important, another approach to simplification is to try to divide things into chunks. For example, in my book-length qualitative study of business-operating families (Rosenblatt, de Mik, Anderson, & Johnson, 1985), one chunk (of many) dealt with people in business-operating families bringing business tensions home, another with bringing tensions from home to the business, and another with the carryover of roles from business to home or home to business. If one recognizes that there are a number of chunks of data in a project, each chunk could become a separate paper or section of a paper, or a separate chapter in a book, or one might decide that one chunk is most important or interesting and write only about it. If one cannot divide things into chunks because each piece is too closely tied to others, then perhaps the thing to write about is the systemic linkage of the pieces (as opposed to going into great detail about each separate piece). The story one would write about then is that a person cannot understand A without attending to its links to B and C, understand B without attending to its links to A and C, and so on. And the most important message is that all the pieces are strongly linked.

Not Knowing What You Are Doing as a Symptom of Learning and Changing

There can be a point where a researcher is caught between her or his initial research plans and what has been learned through doing the research. The project can sometimes be stalled at that place because the demands, "shoulds," promises, and rationality of the initial research plans do not match up with what the researcher has learned, now thinks is important, and has come to believe is worth publishing. One made promises in a research proposal to study and write about certain things, and one can respect the power of existing literature and theory that

legitimates certain lines of thinking and writing. But the data one has and the learning one acquired in the course of the project may make the original plans seem no longer appropriate and the existing literature in the topic area to be irrelevant or in important ways incorrect. What to do? I do not think one is duty bound to write a research report that is disloyal to one's data and what one has learned. Officials at grant agencies want to see publications and growth in knowledge, not works that echo exactly what seemed to be promised in grant applications. The literature and theory in a discipline had best grow and change. If there are no new developments, something is very wrong. There are always defenders of the status quo in a discipline, but there are also people who question that status quo. So once one can get some clarity about what is in the data and how to write it up and stays focused on what is in the data, one's writings will have a good chance of appealing to some disciplinary colleagues and fueling needed growth and change in the field.

When the Problem Is With the Model of How to Do Research

Research in the social and behavioral sciences typically is done within a disciplinary research method model that is defined by writings about how to do research using the method model and by published examples of the method model. These methods models fit particular paradigms in psychology and the social sciences and are in crucial ways justified by these paradigms.

Perhaps the following is an exaggeration, but I think that every model of how to do research includes ideals that from some perspectives are difficult or impossible to achieve, assumptions that often cannot be shown to have been met, and an absence of guidelines on how to move from the ideal statement about the model to using the model in practice. I think any of these problems can stop a researcher from moving forward on a project. In my experience researchers often must make compromises regarding methods standards in order to move forward. However, they generally do not mention in their research reports the problems they encountered and the compromises they made in working with their research models. Embedded in the description and illustrations of the model are strong "shoulds" about what one says and does not say in one's research reports. But not mentioning problems makes it harder for other researchers, particularly beginners, to know how to work with a particular research model.

Life is filled with inconsistencies between ideals and reality—for example, how government works, how medicine is practiced, how restaurant food is made. That could mean that many people come to a research career with experience in living with deviations from ideals. Nonetheless, there are researchers who are stopped from moving forward on a project when they encounter deviations from the ideal for the research method model they are using, when assumptions for the research method model they are using cannot be met, or when they have insufficient guidance on how to apply the research method model to their research situation. I think some of us become stalled in research by problems with research method model ideals in part because we idealize research and see the scientific method (or whatever methods of discovery and learning we use) as free from ambiguity or corruption. But I think the problem is also that since the standard form of writing research reports typically obscures problems and inconsistencies, one can come to one's research having read hundreds of research reports and still not have an inkling of the problems that may be encountered. That means, among many things, that we might assume others did not have the same problem we are having so there is something wrong with us, and if we do not clear up the problem, nobody will want to publish (or perhaps even *should* publish) our research. The rare piece of research that goes into detail about the challenges of working between research method ideals and the realities of data gathering can be delightfully interesting and potentially of great help to many researchers. For example, Popie Marinou Mohring's (1985) doctoral dissertation is in part a methodological critique of what was then a standard way of assessing power in families, and even the title she chose for her dissertation reflects the ways that the people she interviewed resisted and challenged her use of the standard method of assessing family power: "Life, My Daughter, Is Not the Way You Have It in Your Books: Themes From the Confrontation of Social Science Theory and Method With Common Sense in Greek Immigrant Families."

In the remainder of this chapter, I discuss two models of research that I think can create problems for researchers that may lead to a project being stalled. The two I focus on are illustrations. For both I suggest ways one might live with or cope with the problems. Then I discuss the scientific ideal that imbues a good deal of psychology and the social sciences and explore the ways that ideal may stop some research projects from moving forward. Finally, at the end of this section of the chapter, I offer suggestions that might be helpful to researchers stalled by problems in working with any research model.

Example 1: Grounded Theory Research

Grounded theory research, which was touched on earlier in this chapter in the section titled "Help With Qualitative Data Analysis," is a qualitative approach devised by Glaser and Strauss (1967). It has morphed since their pioneering work into a diversity of related models, including, more recently, less positivist and more constructionist forms (Bryant & Charmaz, 2007; Charmaz, 2006). I think many researchers in the social and behavioral sciences attempt to do grounded theory work because they know that many published qualitative works claim to use that approach and because it seems to be the qualitative approach that best meets the scientific standards of the larger, typically more positivist, social and behavioral science research community.

In my experience with researchers who become stalled carrying out grounded theory research, there are several common, often related problems. First of all, as was said earlier in this chapter, it is difficult research to do in ways that are loyal to the standards laid out in most writings about how to do it (Bryant & Charmaz, 2007). It can be difficult to carry out an evolving, multistage process moving through successive levels of abstraction to something like theory (Charmaz, 2006). It is also difficult for beginners to do conceptual development through a comparative analysis of data (Charmaz, 2006), in part because they may not have data allowing meaningful comparison and may not be organized to gather such data. And, to take another standard that is often part of the recommended model of how to do grounded theory research, it also may be difficult or impossible to gather data through multiple steps that are coordinated with successive changes in the data analysis and theory development.

Grounded theory research is a blend of inductive and deductive empirically grounded theory-building. Useful references on grounded theory include Charmaz (2006) and the Wikipedia article on the topic. (Despite the poor reputation Wikipedia has with some people, it is peer reviewed and has many quality control elements, and I think on many topics, including grounded theory, it is thoroughly competent.) Ideally a researcher develops empirically inspired conjectures and eventually a growing, possibly more elaborate, and definitely better informed theory through multiple iterations of research effort that build on the carefully thought-through cumulative changes and explorations in a step-by-step research process. Thus, a grounded theory project ideally involves multiple data- and theory-informed revisions of and additions to data gathering as the research evolves, and quite possibly multiple changes in or additions to one's sampling of people and data and to the questions

asked of people and the data. The project thus follows a mindful path of development of theory through various stages in data gathering and analysis. But that may be a process that is so ambitious as to stall a project, either because one lacks resources to do a multistage study or because one cannot decide with enough clarity and confidence how to move the research forward beyond the stage of theory and data gathering the project has so far reached.

One way to restart a stalled grounded theory process may be to make do with less ambitious next steps. For example, one could add only a few more cases with the next stage of data gathering. Alternatively, one could use data from the work of others or in the public domain, such as journalist interviews or published autobiographies of people like those who one thinks one should next study. One can also aim for a more descriptive, more micro, more qualified form of theory, theory that one can write about as a stage in a process of studying whatever one is studying. Still another possibility, consistent with the discussion earlier in this chapter of research that has stalled because one does not know enough about qualitative data analysis, is that one might shift to a different model of qualitative data analysis, such as something involving a more generic thematic analytic approach (e.g., Guest, MacQueen, & Namey, 2012, particularly pp. 65–71; Braun & Clark, 2012; Patton, 2002, pp. 452–471). And if there are still grounded theory steps in the research using a more generic thematic analysis—for example, if early in the data analysis one uses a version of open coding—that is acceptable, even laudable. And one can say in one's write-up that one did that without claiming to have done a comprehensive grounded theory study.

Example 2: Mixed Methods (Qualitative/Quantitative) Research

Some research projects based on mixed methods (qualitative/quantitative) research become stalled because the two parts, the qualitative and the quantitative, do not fit together well. It can seem like a good idea to gather both quantitative and qualitative data concurrently—to have the precision, seeming objectivity, and relatively large numbers of cases from the quantitative part of the research and to have the depth, personal testimony, and diversity of perspective that can come from the qualitative part of the study. Also, some grant agencies push for mixed method research (see, for example, the U.S. National Institutes of Health Website, http://obssr.od.nih.gov/scientific_areas/methodology/mixed_methods_research/section2.aspx, which offers an overview, definitions, explanations, and much more about mixed methods research).

Despite the best of intentions, however, at some point the model of putting together results from the two methods may seem like a mistake. The quantitative work may not speak enough to the depth, diversity, complexity, and nuance of the qualitative work. The qualitative work may come from a frustratingly small number of cases, not allow the precision and focus of the quantitative work, and seems to contradict or somehow disqualify the quantitative work. If your project is stalled in this place, the first thing I would say is that in my experience many mixed methods projects have been stalled because of disharmony between qualitative and quantitative parts of the research. This is so because often the two kinds of data come out of different paradigms, offer different kinds of knowledge, are based on differing ideas of what good research is, and may seem to call for mutually inconsistent ways of thinking about and reporting findings.

Some researchers avoid this bind by doing one of the two kinds of research in a way that makes the study not one of concurrently gathered, coequal data. For example, it is common to first gather qualitative data as a step in developing quantitative instruments, but the quantitative data are what really count in the research and are the focus of publications (Guest, MacQueen, & Namey, 2012, pp. 198–199). I have also seen a great deal of the qualitative data in a mixed methods study ignored when the study was written up. And I have seen qualitative research in a mixed methods study that was very controlled and did not do the digging for depth, complexity, and nuance of the typical qualitative study. Thus, in this more controlled, quantitative-style qualitative part of the research, every research participant is asked exactly the same questions (and quite possibly not encouraged to say a lot). That means minimal efforts were made to tune in on the diverse understandings, experiences, and language of interviewees, a hallmark of many approaches to qualitative research.

If your research is stalled because you are having trouble connecting qualitative and quantitative work in a mixed methods project, there are three things I have seen researchers do to cope with the incommensurability of the two kinds of data and to avoid being immobilized. One is to use quotes from the qualitative study as illustrations but to make the research report structure and findings basically a report of the quantitative study. Then the qualitative data are not fully used and do not have much influence on the quantitative line of argument in the research report. Nor is the qualitative data used as a source of critique of the quantitative part of the study, even though qualitative data potentially can be a basis of criticism of what was measured and how things were

measured in the quantitative study and even of the meaningfulness of what is claimed as knowledge from the quantitative study.

A second thing I have seen is using the qualitative research as a means of validating quantitative measures. In effect, the qualitative data are used as alternative measures of key things measured quantitatively, so the qualitative data provide a check on whether the quantitative measures tap what they are supposed to tap. For example, if one is measuring postdivorce stress with a quantitative measure, one can use interviews from a subsample of all who received the quantitative measures, code the interviews on postdivorce stress, and then see whether the two measures correlate. If they correlate well enough, the quantitative measure is taken as relatively valid and is relied on more confidently in analyzing and writing up the data. But then the qualitative part of the research is never treated as a research study in itself, and quite possibly most of the information obtained from the qualitative part of the study is not used.

A third possibility is that one can analyze the quantitative and qualitative parts of the study in separate results sections of a paper and then carry out a conceptual integration in the discussion section of the paper (Guest, MacQueen, & Namey, 2012, p. 203). In that case, one may well be quite selective about what one addresses from each data set in doing the integration. With all three approaches there can be frustrations with the lost potential for synergy working with mixed methods (Andrew & Halcomb, 2009, pp. 84–112), but if it does not seem possible to achieve synergy in a mixed methods study, the alternatives I have suggested may be best for getting your project moving forward.

In my experience, another complication in resolving the dilemmas in mixed methods projects is sometimes that there are multiple players running the project and somebody is in charge of the quantitative part of the study and someone else is in charge of the qualitative part. These players also differ in power (Lunde, Heggen, & Strand, 2013). Maybe it is just a consequence of where I am located in the academic disciplines in which I publish, but in my experience the quantitative researchers have ordinarily been the ones with more power. They were more likely to be the principal investigators on the project. They were more likely to be the employers, with the power to hire or fire, of the qualitative researchers and to have more visibility and prestige in academic circles. Consequently, they had access to publication outlets that fit the quantitative paradigm, whereas the qualitative researchers, who had less visibility and prestige, had no particular access to qualitative publication outlets. So, if there was confusion about what to do with incommensurable mixed methods material, this did not cause the principal investigator or the project to become

stalled because the quantitative paradigm dominated. It was basically a quantitative project because of who had decision-making power. However, I can imagine projects in which the issues of how to blend the qualitative and quantitative parts are complicated due to the challenging relationship issues of coinvestigators who are more or less equal in power. Then the way to get the project moving forward might not be so much conceptual as it as about resolving the power and relationship issues of the investigators, perhaps with the help of a trusted mediator or consultant.

Trapped by "Science"

A dominant value in the social and behavioral sciences is that research should be scientific. Strong pressures in favor of the scientific approach also come from powerful voices in the culture and the education system (Bruner, 1983, pp. 59-60; Sarason, 1988, pp. 111–113, 232–233). There is not unanimity about what a "scientific approach" means, but in general it includes the requirement of empirical data to bolster claims, researcher objectivity, precision (which generally is seen as requiring quantification), research designs that allow for refutation of plausible alternative interpretations of the data, and a commitment only to study what can be measured objectively with reasonable reliability and validity. Scientific standards have led to enormous gains in the social and behavioral sciences, but there are those who contend that at times so much effort is put into showing others that a research study is scientific that much that really matters about human social life and human psychology may be ignored (e.g., Giorgi, 2009). Feeling that the scientific approach is too limiting, some researchers may be stopped from moving forward with a project because the enormous pressure they feel from graduate faculty, advisers, editors, senior researchers where they work, grants committees, and so on, to do "scientific" research that does not fit how they want to do research (cf. Bakan, 2009).

If your project is stalled because powerful critics have said that your work is not scientific enough, please read what I say in Chapter 3 about powerful gatekeepers. If the problem is, however, that *you* feel that you must do "scientific" work and that feeling has put you in a place where your research has stalled, I have two suggestions. One is to do the scientific work you feel pressured to do but add elements to your research and writing from outside the scientific model of research that you feel is too limited. You can, for example, find materials in fiction or drama that speak to the issue you are studying quantitatively and use those materials to inform, enliven, challenge, illuminate, illustrate, or otherwise speak to

what you are researching. Interview some people in ways that touch qualitatively on what you are studying quantitatively. Even quite informal interviewing may provide material that enlivens and inspires your work and give you vivid illustrations to include in what you write. You may also find materials in autobiographies, memoirs, personal blogs, poems, song lyrics, or elsewhere that speak to the issues you are researching and that can provide illustrations or in some other way add vitality to your research.

If importing nonscientific materials into your scientific work does not get your research moving forward because the scientific model seems too limiting, another suggestion is to find an alternative scientific line of thought and research model that you would be comfortable using. You may find the alternative in the vast literatures on the philosophy of science, the philosophy of social and behavioral science, research methods, paradigms, and epistemology—not only the literatures of broad scope but also those linked to particular disciplines. Through searching in that vast literature, you may find a model of science that fits the research you want to do and offers compelling arguments in support of that model being considered scientific. Thus, you may find a "scientific" model and its justification that frees you to move ahead with a science that pleases you and that is different from the science with which your research was stalled. Moreover, not only may what you find motivate you, it may speak persuasively to those whose demands for "scientific" work you feel have been constraining you.

Restarting a Project When the Research Model Is the Problem

As was said earlier in this chapter, I think any model of how to do psychological or social science research can create serious dilemmas for some researchers. The written and codified standards on how to use a particular method, standards that are presumably followed by all good researchers, often cannot be met. Or using the method may leave one with "knowledge" about which one has insufficient confidence. The supposedly valid approach to measurement may, for example, seem to have validity as much by pretense as by genuine scientific rigor. Or the social psychological experiment may seem more like a metaphoric illustration than a way to actually understand human social functioning. After a year of rigorous ethnographic fieldwork in an exotic cultural setting, to take another example, one might still not be sure that one knows anything important or nonobvious. Whatever the model used to get from beginning to end of the project, there is some potential for a researcher to feel stalled because of perceived limitations of that model.

One way to restart a project stalled by doubts about the research method model is to see research as being carried out in a community and culture of researchers. To get ahead, to get published, to get good advice from insiders, to find legitimacy for one's research, and to find colleagues who take one's work seriously, one must be in a community and culture of researchers who do work like one's own. That perspective suggests four possibilities for addressing one's immobility on the project:

(1) Get connected or better connected to some researchers in the community that would be the most appropriate consumers of your research and then study the key ways those people talk and seem to think. Learning how they talk and think may allay your insecurities about your research model or at least help you to see that your model is legitimate in research circles that matter to you.

(2) Your troubles going forward with your research could be a matter of personal resistance to the challenges of being in a community. With any community and culture, to fit in one must accept certain conventions, go along with much of what everyone else believes and does, not question too much, and understand that the community and culture are not without flaws but the flaws can be tolerated. From this perspective, one possibility with a person who is having a crisis of confidence in a research model is that it is related to a larger personal issue of not having reached a point of willingness to fit in and go along with communities in general. I can understand that, but I also think that it would be good to examine and work on whatever it is in you that makes it hard to make the compromises that getting along in a community requires. Doing something about the broader issue of community-belonging may make it easier to resolve the issue of the stalled research.

(3) Another possibility is that the research is stalled because you have not found a research community you are comfortable orienting to and joining. My guess, if that is the case, is that you are uncomfortable with the mainstream in-groups in the most relevant discipline. However, in many disciplines there are communities and cultures of dissenters, so if you want to write about, say, the inconsistencies, contradictions, and dilemmas in mixed methods research, there will be a community of scholars who work in that area. I imagine that community has its own inconsistencies, contradictions, and dilemmas, but those may be much more congenial to you. I hope you will be able to find a community that will valorize your doubts and legitimate your writing about and around the dilemmas that have stalled your project. Working in such a community may free you and give

you a chance to use your ability, creativity, and insight to say what you know, believe, and see.

(4) Still another way to restart a project stalled by doubts about the research method model is to learn to understand, accept, and live with the limitations of the model. That is, one can decide that even though with that model knowledge claims must be provisional, there is the potential for the next empirical study, critique, commentary, theorizing, synthesis, and so forth, that one produces to open new doors of thought that will advance the field. One does not have to let the inevitable limitations of the research model one uses prevent one from doing valuable work and advancing the field.

When the Project Seems Outdated

To add to all the other woes of a project that has been stalled for quite a while, a project can come to seem outdated. As the literature moves forward in an area, as methodological preferences and standards change, as the topics that are in fashion and most written about change, as new theories arise and old theories move out of fashion or are transformed in how they are interpreted and applied, and even perhaps as an old paradigm is dropped and a new one emerges, a project that seemed worthwhile can come to seem outdated. That can be a factor in the project being stalled. Is it worthwhile restarting and moving forward if it is outdated?

What Seems Outdated Can Be Made Current

What seems outdated is not necessarily so. Although there are topics for which timeliness is an issue, such as a forecast of an upcoming election, almost any study can be timely if packaged properly. Your research can be used to address emergent theoretical issues, or it may provide a credible challenge to what is new in the literature, to what has been said recently by policymakers, or to new ideas in the mass media. Then, too, as data age they can become the heart of a paper offering historical perspective. For example, one possibility is a history-of-ideas paper that uses the data you have to comment on the history of whatever you were researching, showing that back then people focused on X, were limited by Y, and found Z meaningful in the context of the culture of the discipline at that time. Another possibility is to write the paper as an illustration of a methodological issue, and the data gathering you did might serve as a good or bad

example to discuss in addressing that issue. From another angle, even if some measures or questions in an older data set seem dated, sometimes older studies have controls or measures that make them stronger than contemporary studies. That might make the data valuable in a paper speaking to contemporary research and theory interests.

If You and Your Field Have Left the Topic Behind

When an investigator is concerned that a stalled project seems outdated, sometimes the problem is not only that the project is outdated but also that the investigator is in a frame of mind to think of it that way. Imagine trying to finish a dissertation you started five years ago, or analyzing data that you started gathering a decade ago. Along with your field, you probably have changed in that time. For the field and for you, the topic, methods, theory, approach to data analysis, or even the point of writing up research may no longer fit what you were thinking when you started the project. What then? You could drop the project. It could be too painful and difficult to move forward on a project that no longer seems to fit the field or your interests.

However, it may be possible to reframe your project and what you write in ways that allow you to be enthusiastic and that fit the field. For example, if you framed a project in the beginning as about stress and coping and lately the field and your interests have been focused on the rights of marginalized people, consider whether it is possible to reframe your work as focusing on the rights of marginalized people. Let's say that you gathered data on stress and coping in families who had run out of unemployment benefits and benefits from Temporary Assistance for Needy Families. You might well have interesting material that speaks to issues of rights for these marginalized families. It is not always possible, but I think it is possible surprisingly often, to stretch new fashions in research and new investigator research interests to cover older data.

If what is holding you back is not so much loss of enthusiasm as pessimism about finding a place to publish your work, you might gain from reviewing how you define the publication outlets for your work. If you limit your publication targets to a small number of elite journals in a very specific field, and what they are publishing has changed and you cannot rework your research to be relevant to that change, yes, you may well have trouble publishing. But if you define your population of potential journal outlets broadly and even are willing to look at journals in related disciplines and journals that are not in the *Social Science Citation Index* (SCCI) or that have a less than stellar impact factor in that index, you

will be much more likely to find reasonable publication outlets. My own experience has been that journals in the SSCI with high-impact factors do not guarantee a high level of citations for works of mine, and journals not in the SSCI or with a low-impact factor do not mean works of mine will be cited rarely.

Stale Contact With the Literature

I have known people who were immobilized on a research project that they felt was outdated, who were unwilling to look at or work on data analysis or a manuscript, and whose sense of the literature was quite outdated. They might not even have had a citation in the reference section of their draft manuscript from the past five years. I would say if your project is stalled because it seems outdated and you have not carried out a search for relevant literature for some time, you really need to look for recent, relevant literature. With a new exploration of the literature your interest may be refreshed. You may see that your work actually is leading edge or can be made to be so. You may even find works that call for a study like yours. Or you may find studies that are related or in some ways supersede yours, but you can also see that there is a way to reframe your work or change its emphasis that makes your work a contribution to the literature.

I would not ask a research assistant or a student to do that literature review. Doing it yourself creates so much more opportunity for coming across things that you can see are relevant, whereas someone who does not know what you know and cannot think the way you do might miss these references. Furthermore, doing the literature review yourself will stimulate your thinking and interest in ways that a literature review carried out by someone else could not. In fact, I would recommend for *any* project that one recurrently do literature reviews as the project moves forward. Coming to a clearer sense of what is in your data as you have moved forward on your project may give new meanings and understandings to works in the literature that you had previously understood differently or dismissed. And anything new in the literature that is relevant to your work may help keep your thinking and writing current. In fact, on the leading edge of your field and your research area there may be ways of thinking and making sense of data that ignite your enthusiasm and interest and about which you will have valuable things to say.