The dictatorship of the problem: choosing research methods

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ABSTRACT. It is relatively easy to investigate how to employ a particular research method in the social sciences. It is considerably more difficult to decide which to use. Which method to use is arguably a more important question than how to use that method. "Which method?" is, at least, the necessarily prior question. One cannot look up how to do something until one has decided what that something is. Methodological innovation depends directly on methodological choice. Researchers continuing a tradition, or working within a paradigm can often avoid making difficult methodological choices. Researchers seeking to innovate cannot. The question "which method?" is particularly important for selecting research designs, because design choice importantly shapes most of the other choices researchers make. Designs are most effective and have the greatest potential for innovation when they are dictated by the nature of the research problem.

KEYWORDS: *Methodological innovation, Multimethod research, Research design, Research method, Research problem*

Introduction

The field of research methods in the social sciences is richly endowed with excellent texts and reference works. Thus it is relatively easy to investigate *how* to employ a particular research method - survey research or interviews or regression analysis or grounded theory, and so on. We are less richly endowed with works that help us decide whether to use surveys, interviews, regression, grounded theory, multi-level models, participant observation, etc. - and which of them to use in what circumstances. *Whether and which* methods to use would seem more important questions than *how to* use a method. "Which method?" is, at minimum, a necessarily prior question. One cannot look up how to do something until one has decided what that something is.

Consciously selecting research methods is also crucial for researchers who would conduct innovative research. One can work within a "paradigm" or follow a tradition without making deliberate methodological choices, but it is hard to see how one could be innovative without making choices, consciously seeking the most

effective methods rather than simply proceeding along traditional lines or following an algorithm.
The question "which method?" is particularly important for selecting research designs, because design choice importantly shapes most of the other choices researchers make, including choices of methods for sampling, coding, and analysis. This is why the research design should be selected first, before methods of sampling, of coding and measurement, or of analysis.
It is common to recommend that one's design choice should be dictated by the nature of one's research problem. But this seems to be easier said than done. Like the dictatorship of the proletariat, the dictatorship of the problem may appear to be inevitable. While it is customary to recommend the dictatorship of the problem to one's students, there is much to suggest that this criterion of methodological choice is more honoured in the breach than the observance.

The priority of design

Design, sampling, measurement, and analysis are closely related. It is not really possible to discuss one of them adequately without discussing the others. But we cannot think about everything simultaneously. To organize our thinking, it is necessary to pick an order. Any ordering of the consideration of these connected things will be somewhat arbitrary. Still, there is, I think, a natural sequence, as illustrated in Figure 1:

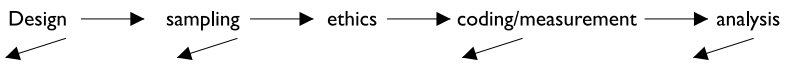

Figure 1.
Natural sequence of methods choices

By putting sampling, ethics, measurement, and analysis after design, I am suggesting that it is generally more effective to choose a design before making decisions about other issues. But the feedback arrows in the figure acknowledge that choices at each step (beginning with sampling) can have an influence in both directions: ahead to the next link and back to the previous ones (Vogt, 2008 *forthcoming*; 2009 *forthcoming*).
It is easy to agree in principle that the nature of the research questions, and the specific problems that arise as a consequence of trying to answer them, are primary in directing good design work.

But researchers' attention is often diverted by secondary questions. Today, the most prevalent of these secondary questions is whether the data the researcher gathers and analyzes should be qualitative or quantitative or both. The "qual-quant" distinction is important for data coding, or measurement[1] and for analysis, but it is not a design distinction. We know that coding and analysis are not matters of design from the simple fact that all research designs can be, and have been, used to generate either quantitative or qualitative data - or both. This means that designs are not well categorized by the kinds of data they can yield - because the categories are not mutually exclusive.

The point is most easily made by example. Survey research is usually categorized as a quantitative method, but this is misleading in two ways. First, there is nothing quantitative about the way most survey questions are written. Finding the right phrasing for survey questions, even questions that one plans to code numerically, is a delicate and nuanced art that requires a great deal of thought about what words mean to people. For example, studies have shown that respondents often answer survey questions about a proposed law differently depending on which of two seemingly equivalent phrases is used to describe it: Should X "be forbidden" or should X "not be allowed"?

Second, surveys quite often contain open-ended questions that enable respondents to express their beliefs and attitudes in their own words, sometimes at length. These expressions of opinion are qualitative data and are usually analyzed using qualitative techniques. So, surveys are qualitative because of the way questions are composed, and they may be qualitative because respondents may give qualitative answers to survey questions. But surveys are also quantitative. Many of the questions included in a typical survey are written to generate answers that are best analyzed using quantitative techniques. In sum, although the analysis of survey data is frequently quantitative, it is not exclusively so. Surveys, like all research designs, can be used to collect either qualitative or quantitative evidence - or both.

The same is true of so-called qualitative designs. For instance, researchers conducting interviews or engaged in naturalistic observations usually collect and analyze qualitative evidence. However, they may, sometimes do, collect quantitative data as well. Interviewers might count the number of positive adjectives used in

1. I use coding and measurement more-or-less interchangeably to describe the process of assigning names or categories or numbers to phenomena

interviewees' descriptions. And they routinely collect quantitative data about interviewees, such as their ages or years of experience. Likewise, naturalistic observers, while making observations that use themselves as the measuring "instrument", not infrequently also use lickers and stopwatches to count the frequency and to time length of specific kinds of social interactions. As these examples from surveys, interviews, and naturalistic observation illustrate, all designs can be employed to collect qualitative or quantitative data or both. Similar examples could be found for other designs. To claim otherwise, as many textbooks do, that surveys *are* quantitative or that interviews and naturalistic observation *are* qualitative is to engage in stereotyping. The stereotypes may contain a "kernel of truth", to use the old phrase from the study of racism, but they are stereotypes nonetheless (Vogt, 2008).

Selecting among design options and then crafting the details of a research design requires great deliberation. One reason to think hard about research design and to consider all the options before making a final choice is that design shapes everything else. Another reason is that, once a project is under way, design is very difficult to change. By comparison, coding and analysis corrections and adjustments may be comparatively easy to fix even toward the end of a research project. You can often change your mind about coding and analysis, and still preserve the basic project. If you change your mind about design, you have to start over. Oddly enough, books written to help researchers improve their work often pass quickly over design choices that are determinative and linger over analytic choices that would be fairly simple to alter and that need not be set definitively until quite late in a project. For example, say that after you have collected all your data and entered it into a spreadsheet you are persuaded by a colleague that you should use regression analysis rather than ANCOVA to aid you in the interpretation of your data. Such changes rarely involve more than entering a few commands into a program.

Dictatorship of the problem

How does one get from the problem or the research question to the design? To do so, I have found it very helpful to ask my students to address six questions about their research questions. And when I begin a new project, I ask the same questions of myself.

These questions about the question are meant to orientate design choices. Here is what I ask myself: *To answer my research question, should I or do I need to…?*

- Make causal inferences?
- Generalize from the cases studied (sample) to a broader group (population)?
- Study change over time?
- Interact with subjects/participants?
- Generate my own data sources and data?
- Use more than one design?

After answering these design-orientating questions and selecting the basic design, then it is easier to move, first, to the important issues of sampling or selecting cases, and the ethical implications of the intersection of the design and the cases; second, to the issues of coding and analyzing evidence - including whether that evidence should be qualitative or quantitative or graphic, or all three. It is more fruitful to ask whether to use narratives or numbers or pictures (or all three) *after* having settled the above design questions about causal inferences, generalizations, studying the topic over time, and so on. Of course, decisions about how to gather evidence, and from whom, will shape coding and analysis choices. But decisions about how to code and analyze evidence should not determine the answers to the big questions listed above. That would be exactly backwards; it would be to confuse the goal of answering a research question with the types of evidence analyzed to answer it. Of course, many people *prefer* collecting and analyzing quantitative data; others prefer qualitative evidence. But, as Light and Pillemer put it, "the pursuit of good science should transcend personal preferences for numbers or narrative" (Light, Pillemer, 1984, p. 143). To say that design is logically prior in planning research is not to underestimate the importance of coding and measurement. Coding, and the quant-qual distinction, is an important branch in the decision tree. But it should rarely, if ever, be the first branch.

What is a research design? What are the main categories of design? One simple definition is: a "research design is a plan for collecting evidence that can be used to answer a research question" (Vogt, 2006, p. 8). Different authors have come up with different lists of the basic types of research design. Brewer

and Hunter list four: fieldwork, surveys, experiments, studies of naturally occurring data (Brewer, Hunter, 2006). A fifth type combines two or more of these into multimethod research. I find it more useful to discuss seven types of design, each of which may be combined with others in various ways. The main types of research design are: document analysis; secondary analysis of data, such as census data; naturalistic observation; surveys; interviews including focus groups; experiments and quasi-experiments; and participant observation. An eighth type is any combination of two or more of the first seven types (the seven designs are listed according to the degree that they require researchers to interfere in the lives of the persons being studied. Document analysis and secondary analysis of data require the least encroachment; experiments and participant observation entail the most). A good research *design* is justifiable in terms of the research question. At minimum, the researcher should offer an argument that the design chosen is an effective one for the job. This is another piece of good advice with which it is easy to agree but which is less often followed than one might expect. Frequently, even major projects, such as doctoral dissertations, offer little more than a few sentences by way of justification of what is arguably the most consequential of all the choices made in planning a research project. Answering the six questions about the problem/question helps generate some of the conceptual capital needed to explain and justify design choices. To repeat, the questions are: to answer your research question do you need to (1) make causal inferences, (2) generalize to a broader group, (3) study your topic over time, (4) interact with human subjects, (5) generate your own data, and (6) use more than one design? Briefly addressing each of the design-orienting questions shows how they can help facilitate design choice.

First: is your research question such that, in order to answer it, you need to make causal inferences? Or is your research question one that leads you mainly to describe cases rather than relate variables causally? It is actually pretty rare to encounter researchers in the social sciences who forswear all attempts to make any causal inferences. Any effort to relate one thing to another is ultimately causal. But there are many ways to make causal inferences. Indeed, the range of ways to make causal inferences is wide, and so is the degree of confidence the researcher can have in the inferences. Furthermore, it is also quite rare for researchers attempting to

come to causal conclusions to exclude all non-causal, descriptive elements in their research. I would venture to say that, strictly speaking, neither *purely* causal nor purely descriptive research is possible. But there are major differences of emphasis. Deciding how much emphasis to put on causal inference in order to answer your research question will importantly shape your research design. Because justified causal inferences arise more from design than from measurement or analysis, it is crucial to incorporate causal considerations in the early design phase of planning a research project.

The next decision is whether, to address your research problem/question, you need to generalize from the cases studied (sample) to a broader group (population)? Or, are you interested only in the cases studied? You might be interested only in the cases studied because they constitute the entire population of interest to you (such as all the visitors to a clinic). Or you could focus on your cases and not generalize because the cases are interesting in their own right quite apart from whether they are representative of others. Almost all researchers generalize to one degree or another. It is hard to speak or write without doing so. All thinking and writing requires some degree of generalization. As is the case with causation, it is a matter of degree. For some research, generalization is the main goal. In that case the researcher has to pay very careful attention to choosing a sample from which it is legitimate to generalize. By contrast, in case studies, generalizations may be a tool, not the goal. One often has to resort to general principles to describe and explain a specific case (Ringer, 1997, on Weber's explanations of singular events). In any event, the role of generalization in answering the research question should be decided up front. How you make that decision will incline you to select some designs over others.

An important but often overlooked question is: does your question require you to deal with the passage of time? Do you need to study change in individuals, cases, or variables over time? Or is a one-time ("cross-sectional") approach more appropriate? Are you interested in long-term effects or immediate consequences? Are you aiming to take a photograph or a moving picture? If, in an ideal world, you would like to have a movie, but can only afford a photo, this is a limitation and needs to be made explicit. Time is an important consideration in many designs. Sometimes, as in experiments, the time from treatment to measurement is a confounding variable,

one that gives rise to so called "history effects". In other cases, as in prospective studies or in retrospective historical studies, time is the whole point of the research.

The first three questions - on causal inference, generalization, and change over time - deal with questions about the nature of the *answers* one seeks to address the research problem. By contrast, the next two questions deal with the *sources* of the data or evidence relevant to the research problem.

The fourth question is: to study your problem, do you need to interact with the subjects of your research? Although it is most common to think here of human interaction and the rights of human subjects, interaction is a broader issue. Animals are one example, but interaction need not be limited to animate subjects of research. The issue can come up even in document analysis. Do you need to handle the original documents directly (e.g., diaries), or can you make do with copies, transcriptions, or translations? Regardless of the topic, the question arises: is direct interaction of the researcher likely to provide insights available in no other way? Or is such interaction likely to bias the observations? Or both? The answers to these questions can importantly shape design choices. To use a medical analogy: a general practitioner would see a patient; a pathologist would see a piece of a patient (sample); but a radiologist would only see a picture of a patient. It would be silly to claim that one of these is the 'right' approach. Different approaches work better for different problems or aspects of problems. In both medical and social research, the most effective approach is often pluralistic; and combining perspectives from different data sources can be an important source of new insights and methodological innovations. Like the question about researchers' interaction with the subjects of the research, the next design-orientating question addresses sources of the data. In order to answer your research question, do you need to generate your own data? Or, can you answer your question better by using records or publically available data archives? If you do not need to generate your own data, or if there is no way you could possibly generate data as good as that which is available in data archives (e.g., census data), you *select* among data generated by others. While, this is in itself can be considered a kind of data generation, it is certainly very different from conducting dozens of interviews or surveying hundreds of respondents.

There are many kinds of research that are "archival", broadly

speaking. Historical research is the most closely tied to archival sources. Meta-analysis is another fundamentally archival method based on secondary sources. Because literature reviews and meta-analyses are fundamental to good research, even when one's purpose is best served by generating one's own data, most studies have an archival aspect when they review previous research. Probably the most common archival research in the social sciences in the United States involves employing huge public-use data bases generated by the Bureau of the Census, the National Centre for Educational Statistics, the Centres for Disease Control, survey organizations such as the General Social Survey, and so on. Still, as amazing as these archives are, and beginning researchers often underestimate how useful they are, sometimes there is no alternative but to generate one's own evidence. Obviously, this decision about data sources is a key determinant of research design.

The final design-orientating question is: to address the research problem, do you need to use more than one design? Or is your question one that can be best answered or concisely answered - or, at least, answered sufficiently well - by one approach to data gathering? Here the question is whether the clear costs of using more than one design are compensated for by the considerable benefits. Multiple designs can combine the strengths and avoid the weaknesses of single designs. This advantage of multimethod research is powerfully advocated by Brewer and Hunter (2006) as well as most proponents of mixed-method research (Creswell, Plano-Clark, 2007). Perhaps the most important feature of the answers to the design-orientating questions is that they are not independent of one another. They interact. The number of combinations and permutations in answer to these questions is huge. Assume, for the sake of illustration, that each question can be answered with a simple yes or no. If you answered "Yes" to all six you might:

1. Conduct an experiment to draw causal conclusions.

2. In order to generalize the results, you try to make the control and experimental groups sufficiently representative.

3. Since you are interested in the long-term effects of an intervention, you need to follow up regularly over a period of two years.

4. In your experiment you generate your own data.

5. Generating your own data involves interacting directly with the people who have agreed to participate.

6. You use multiple designs because you need not only measure the effects of the intervention, but also to understand how the people at the receiving end have reacted to that intervention. This leads you to conduct interviews with the people who have been studied in the experiment. So you conduct an experimental manipulation to establish a causal link and then conduct interviews to understand its nature.

Or you could answer "No" to all six questions.

1. You wish to make no attributions of cause but seek, rather, to describe your cases thoroughly.

2. Because you are studying an entire population - such as, all the students enrolled in a university - the question of generalization is moot.

3. You are not going back in time; you are interested in the present.

4. You don't need to interact with the subjects of your research because you have rich records.

5. You don't need to generate your own data because what you are interested in is best obtained from those archival records.

6. And, because those records suffice for your purposes, there is little to be gained from using multiple designs.

Obviously, the number of possible combinations of answers to the six questions is very large. You could say yes to the first question and no to the others. Or yes to the first two and no to all the others. Or you could answer the first two in the negative and say yes to all the others. Hundreds of combinations are possible - 720 possible

combinations (6 factorial) to be precise. Some combinations are less likely - roads less travelled - than others but all are theoretically possible. And fruitful innovation could possibly arise through considering and implementing unusual permutations.

More complicated still, the answers to some of the questions, especially the first three, are a matter of degree, and cannot easily be answered with a simple yes or no. Although you might start out and conclude with a yes/no summary, many of your answers will be matters of degree. How strictly experimental must the design be? How representative should the sample be? How far over time should change be measured?

Struggling with the six questions helps clarify one's design choices. After answering the questions about the research question, selections among the broad categories of design (experiment, participant observation, survey, interview, and so on), should be easier to make and easier to justify. In short, designing a research project involves a lengthy journey over somewhat unknown terrain and requires making many choices. And one needs to make those decisions at an early stage. Paradoxically, this means that the most important choices that shape the rest of the work need to be made at a point in the project when you know the least about the subject.

Sampling, coding, and analysis

Although, as Shadish, Cook and Campbell put it, "Design Rules!" one cannot ignore the other components of planning a research project (Shadish et al., 2002). In addition to a design, which is a blueprint for gathering evidence, a researcher also needs a sampling strategy, which is a plan for determining which data to gather or cases to study, a coding or measurement plan, which specifies the means of handling evidence, and an analysis plan, which is an outline of how the evidence will be interpreted. The four elements are necessary to make a well-functioning whole. It is usually best to *plan* them in the logical order of: designing, sampling, coding, analyzing. First collect the evidence, then sort it, then interpret it. Yet, in the final product, all elements have to work together. They are like the bones, muscles, and nerves of a functioning hand. If any one is damaged or missing, the others will work poorly if at all. Sampling, coding, and analysis are briefly discussed below.

Sampling or selecting cases to study and research ethics

A key decision facing researchers is *which* cases to study; also important is *how many* of them to study. The cases may be individuals, organizations, events, or any other distinct units of analysis. Thus, while a good design is crucial, so too is using that design to study relevant types and numbers of cases. Which cases and how many are particularly important questions if the researcher wishes to generalize from the cases studied to a broader population. After deciding which cases to study, then comes the task of gaining access to those cases. Sampling, in the broad sense of the term also includes recruiting informants for interviews, selecting sites for participant observation, and assigning subjects to control and experimental groups.

Questions of ethics become most salient at the intersection of design and sampling. Once the researcher has considered the basic methods of gathering the data and decided from whom they will be gathered, then ethical issues are concrete enough that they can be addressed. Ethical concerns are best discussed in terms of specific designs and sampling plans. One can hardly consider, for example, the rights of subjects until one knows who those subjects will be and what will be asked of them.

Coding and measurement

Coding and measurement decisions are facilitated by answering a key question about your research problem: is it more fruitful, in terms of answering your research question, to think of your evidence in terms of categories or continua? This is where the quant-qual choice becomes most salient.

The choice between categories and continua should mainly be a *substantive* choice driven by the nature of the phenomena being studied. It has major measurement implications, but is not simply a measurement question. And it is much more complicated than it might at first seem. For example, in his pioneering work in genetics, Mendel studied several traits of garden peas. One of those traits was height. Height would seem to be an obvious case of a continuous variable. But the way genetic inheritance worked in Mendel's peas, height was a categorical variable. Peas were either tall or short. Using the categories tall and short would be an erroneously crude way of coding the heights of many things, but it was highly effective for discussing the inheritance of a trait in garden peas. Coding and

measurement decisions require much conceptual work concerning the nature of the phenomena being studied, and such work may facilitate innovative methods of coding and measurement.

Analysis plan

A researcher also needs an analysis plan. A question to guide decision making here is: what analysis techniques fit best with the data-gathering design and the sampling, coding, and measurement plans? If approached in the order suggested here, the answer to the question about the analysis plan will be quite constrained by the answers to the previous questions. Although analysis planning may come late in the process, one needs to think about it *before* gathering data. Otherwise you may be in for an unpleasant surprise if you expect to be able to use a particular technique of analysis, but you have not collected enough of the right kinds of data from a sufficient number of cases or in enough depth to apply that technique. Powerful analytic techniques - such as grounded theory, confirmatory factor analysis, and multilevel modelling - require more evidence gathered in more detail than many novice researchers realize.

Even though true decision trees can be brought into play when choosing analytic techniques, controversy is not eliminated. I have been surprised by sharply negative reactions to what I considered recommendations made largely on technical criteria. For example, when I have claimed that there really is no longer much point in discriminant analysis, now that we have logit regression, some people have become very annoyed. I have seen similar reactions when suggesting something similar about ANCOVA. I once somewhat flippantly asked: "What's the point of ANCOVA? Isn't that just sneaking regression in through the back door and tacking it on to an ANCOVA? Why not simply use regression, which is a more powerful and versatile technique?" That question produced sputtering rage in some listeners. And the battles between principal components analysis and principal axis factoring - or orthogonal and oblique rotation - can also be quite nasty. The same antagonism can be found among those who favour and those who oppose software for the use of qualitative data, or different approaches grounded theory (Strauss vs Glaser), or different approaches to case study research (Stake vs Lin).

The intensity of some debates about methodological choice suggests

that to understand them fully it may be necessary to apply concepts from social psychology and cultural anthropology. Methodological groups are also always socio-cultural groups. These social groups can be characterized by ethnocentrism concerning other cultures and the kind of antipathy concerning outgroups that can be found in any social group. The late Philippe Besnard often claimed that we need to interpret sociological and methodological schools as "clans". Cultures and clans are conservative and resist innovation. Even groups of innovative researchers may quickly "morph" from work teams into clans that are hostile to outsiders and confuse loyalty with competence.

Evidence and illustrations drawn from interviews with researchers

Choices in research planning inevitably require trade-offs, or opportunity costs. I favour taking a pluralistic and pragmatic approach to these choices. An approach is *pluralistic* when saying that "good" answers to methodological questions vary with research problems. There is no best approach. An approach is also *pragmatic* when it claims that the appropriate way to make methodological choices is to pick the methods that work best. As Edison reportedly said, when asked about the methodological rules in his laboratory: "Hell, there are no rules here - we're trying to accomplish something". If you are trying to accomplish something, any rules of method will be tentative and will vary with the problems investigated.
On the basis of interviews with two dozen researchers in 2001-2, researchers whose work I considered exemplary, and who were highly regarded in their fields, I concluded that many of them were often not pluralistic and pragmatic in making their methodological choices. Pragmatic pluralism is actually quite a tough or unusual standard. In my interviews with researchers, I asked them a double-barrelled question:

a. Would you tell me about an important methodological choice you have made?
b. What were the criteria you used to make it?

What were the main methods of selecting methods adduced by these researchers? I found four themes in their responses. The criteria

for making methodological choice fell into four main categories.

1. *Tradition.* In one way or another, respondents indicated that "this is how researchers in my field have always approached this kind of problem".

2. *Lack of appreciation of alternatives or even knowledge that there are any*. Researchers working in a defined area and who keep up with the literature in that area may not pay much attention to methodological developments occurring after they left graduate school (when I went to graduate school structural equation modelling and multilevel modelling - and LISREL and HLM software - did not exist). The lack of interest in new methods is often described as a healthy resistance to chasing after "fads".

3. *Commitment.* There really are a lot of folks around who believe that there is one best method or one best brand of software. Researchers can be so committed to a favoured approach (Straussian grounded theory maybe) or to a particular software (M plus perhaps), that they find it difficult to conceive using another. Clans form around intellectual leaders. Methodological sects have acolytes. Allegiance can replace choice, which, in turn, chokes off innovation.

4. *Opportunism.* The forth theme in the criteria used to make methodological choices is a kind of pragmatism. A nifty data set becomes available or a new grant opportunity suggests an unusual line of research, and the researcher seizes the opportunity. Such opportunism can be a source of innovation.

My evidence for this four-part categorization is hardly overwhelming. It comes from interviews with around two dozen researchers for an unfinished book on the practice of research based on evidence about researchers' reflections about how they made their methodological decisions. But if the above criteria for making methods choices are as widespread as I think they are, it is no wonder that innovation is comparatively rare. If tradition, lack of knowledge, and ideological commitment to a method are

major criteria for making choices, researchers may be left with little defence against those who claim that science is not based upon reason - e.g., proponents of methodological anarchism or nihilism or post-modernism. And, if opportunism - i.e., seizing an opportunity not of the researcher's making - is the primary motivation for trying something different, this will make it difficult for researchers to innovate, except by happenstance. These ideas about typical research practice are not new. They have sources in "classic" theory. In Kuhn's terms, most researchers engage in "normal science" most of the time, which means that many choices are already made. Or as Herbert Simon described the choice problem, when faced with intractably complex decisions, the rational approach is to "satisfice" rather than try to "optimize" and engage in a vain quest for the best method. An obvious way to satisfice is to stick with the tried and true. In more recent interviews, in 2006-7, I built upon my earlier work and talked with 13 more colleagues on the same themes. I also probed their ideas about multiple or mixed methods. These new interviews suggested two new themes. First, combining quantitative and qualitative methods - usually known as mixed methods - has become the all but exclusive way of thinking about multiple methods or methodological pluralism. Second, although mixed methods are popular, some researchers strongly resist. The most common reason for objecting to mixed methods is that such methods cannot be fruitfully combined because they are based on different paradigms or philosophical presuppositions or theoretical perspectives. Colleagues making this point spoke of more or less epistemological positions, or paradigms, with a kind of reverence I'm not at all sure they deserve. This is especially true, I think, of folks who call themselves qualitative researchers. They see mixed methods as a way of sneaking positivism back in, just when they thought they had marginalized it.

Of course, you probably cannot mix everything. What can you mix? I'd say that there is no question that you can mix designs - such as surveys and interviews or fieldwork and experiments. And different kinds of coding, measurement, and analysis are frequently employed in the same research study. But can you mix philosophical paradigms? How could you possibly mix nominalism and realism? Perhaps pragmatism allows this. But does it allow mixing purposes, problems,

or constructs? One further limit to mixing or pluralism and rational choice making is the sheer number of choices using multiple methods opens to researchers (Simon et al., 1986).

Multi- and mixed-methods approaches

One of the themes brought up by my informants in 2001-2 and one that became more important in the later 2006-7 interviews is the interest in multi-method, especially *mixed*-method, research. I think mixed-method research is progressive in that it is multi-method research. Its main limitation in my mind is that people who advocate mixed-methods restrict the mixing to data coded as words and numbers, to research that crosses the word-number divide. I think the mixed-methods movement can lead to innovation, but it could be innovative if it were not focused on coding differences. Again, I use the word "progressive" in the pragmatist sense of opening possibilities to further growth. Methodological pluralism, or multi-method research, which makes no specifications as to coding, strikes me as more progressive.

Some researchers seem to see the mixed- or multi-method approaches as a way to moderate or to finesse the choice problem in research design. Why choose between two methods? Why not do both? But I think multi- and mixed-methods approaches exacerbate the choice problem, or at least complicate it. The complications have both negative and positive aspects: on the negative side combining methods makes complicated choices even more complicated; on the positive, combinations often open opportunities for innovation.

An extended example of the nature of the complications arising from combining methods will help emphasize the point. Say your research problem involves learning about some population's social and political attitudes and beliefs, including why respondents hold certain attitudes and beliefs, the strength with which they are held, and their relations other attitudes and beliefs as well as to the subjects' social backgrounds. To learn about your topic you are thinking about some combination of interviews, focus groups, and surveys. Consider the possible combinations and permutations of those three methods: 3 = 6. There are six possibilities from which to choose. There really are six because permutations as well as combinations need to be considered.

The *order* in which you combine can be as important as what you combine. But the number of possibilities is actually much greater than six. It is useful, I think, to belabour the point about combinations and permutations of multiple methods. The three we are considering are:

I *Interview*
F *Focus Groups*
S *Survey*

Here are the six original possibilities:

F-I-S
F-S-I
S-I-F
S-F-I
I-S-F
I-F-S

But what if you only pick two of the three? That gives you another six possibilities:

F-I
F-S
S-I
S-F
I-F
I-S

Furthermore, you can use the same combination in different ways. Take survey-interview (S-I): you can survey respondents to find good informants to interview. Or you survey, and then you interview, so as to gain clarification about why respondents answered as they did. But let's not count that kind of difference for now.
What if you *pick two* and allow for one repeat, that is, you use two of the methods, but repeat one of them?

I-S-I
I-F-I
S-I-S

S-F-S
F-I-F
F-S-F

This is not fanciful. For example, you interview people to help you write your survey questions, survey a group of respondents, and then interview some of the respondents to learn why they answered questions as they did.

One further possibility will help bring this extended example to a close. You use all three and allow one repeat. Perhaps you conduct a few focus groups to get a sense of the range of attitudes of potential respondents. Then you draft a set of survey questions. Then you do what is called cognitive interviewing to refine the questions. Then you draw your sample and administer the survey. After the survey you interview a sample of the respondents to gain a better understanding of some patterns of responses. That would be: F-I-S-I. At any rate, pick three with one repeat = 4 and 4 = 24. Add that 24 to the 18 we have already examined, and we see 42 possible designs combining the three methods in various ways.

And to push it a bit further: John Creswell has argued and shown empirically that one distinction in mixed-methods research is between multiple methods being used *sequentially* (as in my examples in which each result informs what you do next) or *concurrently* (where the methods are applied more or less simultaneously) (Creswell, Plano-Clark, 2007). So if we look at our 42 and add this final classification, we double the number to 84 possibilities.

Without too much of a stretch, combining only three methods, we have spun out an almost absurd number of combinations. Of course, there are many more than three methods. Whatever moderately large number you pick as the number of methods, the number of combinations and permutations is, if not quite infinite, at least breathtakingly numerous. Say, for example, there are 10 methods. Well, 10 = 3.6 million. Obviously, we've long passed the point of diminishing returns from further taxonomy work. The returns are not just getting smaller, they are becoming negative. More taxonomizing of choices makes things worse. Taxonomy of methodological choices can reach a dead end by collapsing under its own weight. It contains the seeds of its own destruction (to refer once more to the ghost of Marx). We all need some means of rapidly narrowing down the conceivable choices. No wonder tradition,

commitment, and studied ignorance of alternatives are so common. The choice problem is too complicated to be solved with a few algorithms. *Rules* of choice are not helpful. Perhaps even *guidelines* prescribe too much. If there are rules, they have to be awfully vague. And perhaps this is not so bad. I have sometimes thought that in fact there are only three invariant rules of method. And that all the rest is footnotes: Be smart. Be honest. Work hard. These rules can be put negatively: don't be stupid, lazy, or a cheat. The rest is details that are worked out on a problem-by-problem basis.

Checklist of methodological choices

Saying "don't be stupid, lazy, or a cheat" is cute, perhaps, but it's probably not all that helpful. What can we do on a day-to-day basis to help us be smart? I have found it helpful in my own work, and in guiding the work of students, to consider checklists of reminders. My advice is to make extensive use of any checklists, orientating questions, algorithms, and decision trees keeping in mind one point of departure: the requirements of the research problem. Of course, while such lists are helpful aids to thinking about the methods consequent upon a research problem, they cannot do your thinking for you. Still, when faced with complicated work, checklists are helpful simply to make sure that you've not forgotten something important (think of airline pilots who, no matter how experienced, rely on checklists).
My checklists for thinking about methods choice include:

- the six orientating questions with which I began this paper;
- the expanded lists of "threats to validity" in Shadish, Cook, and Campbell (2002);
- the checklist of questions in the taxonomy in Table 1.

The last of these grew out of my interviews with researchers and my reflections on what they told me (note that the Table is structured into four categories that closely parallel the order of choices in Figure 1).

Four categories of answer capture the different kinds of choices researchers referred to in these interviews:

1. *orientations* to research problems,

2. *designs such as quasi-experiments*, survey research, or participant observation,

3. *sampling and measurement* or methods of handling evidence such as quantitative versus qualitative approaches,

4. *analysis* techniques such as discourse analysis, ANCOVA, or regression.

What is missing from this list is any very direct discussion of paradigms or epistemologies. The first column, orientations, is not as philosophical and ideological as many methods writers say it should be. And I had to extract lots of these answers from my respondents. They weren't offered spontaneously. When I interviewed my 22 researchers, the word "paradigm" never came up, nor did critical theory, or constructivism, or positivism. And epistemology was mentioned only twice. Of course you could say these researchers were influenced by these presuppositions even if they weren't aware of it - much like Freudians claim that sexuality influences everything, even when we repress this fact.

Perhaps some of the reason that philosophical presuppositions were not stressed by my informants is that these were the researchers I picked as folks who had done good work on substantive topics. They were researchers first and methodologists only second if at all. Perhaps a researcher engaged in what I think of as epistemolophilia was not likely to make it onto my list. Obviously I have biases and presuppositions too. Epistemology was a more common concern in my second group, which was chosen largely to probe attitudes about multi-method research. Multi-method work tends strongly to promote choice, and choice probably promotes reflection about choice. That is good but it can sometimes lead to too much reflecting and not enough doing. Researchers are often reminded that it is important to state their presuppositions and possible biases. I am obviously letting mine "hang out" here. I tend to get impatient with "interpretivist" researchers who seem to have less

interest in doing research and more interest in explaining why others cannot successfully do so either. Our social backgrounds and presuppositions hobble us all.

I would not deny that interpretation is always necessary and that the facts do not speak for themselves. There is no such thing as an *immaculate perception*. Or, if there were ever to be one, it would truly be a miracle. So although context and presuppositions always have an influence, we shouldn't let ourselves be immobilized by that fact. Again, the philosophical foundation of my position is pragmatism. My definition of pragmatism in methodology is a relatively simple Deweyian one. A method is good if it is *progressive* if it opens the way to further growth in the ways we learn to solve problems. If it tends to constrict our opportunities it is not a progressive method. I think a lot of the more ideological approaches are not *methodologically* progressive. They tend to constrain the growth of ways to learn. I believe this to be so even in the case of the egalitarian ideologies to which I am quite committed. There can be a difference between being socially and methodologically progressive.

Concluding thoughts

If, as someone once said, logic is the morality of the intellect, I suppose that methodology aspires to be the morality of research. As in most discussions of morals, so too in methodology, assertion tends to outweigh evidence, and arrogant assertions are quite frequent. I have been as guilty of these flaws as most methodological writers. Although some of the later parts of the paper contained assertions based on evidence gathered from interviews with researchers, the paper is more prescriptive than descriptive.

I have also been guilty of a somewhat naïve idealism. For example, this paper does not address the question of resources. Often the practical question a researcher faces is not: what is the best set of methods that could be used to tackle this problem? But rather the researcher's question is: *given my resources* (time, funds for travel, money to pay assistants and subjects, and so on), what is the best I can do? Many important research projects have been conducted by teams of researchers, funded by substantial grants. A solo researcher will have considerably fewer options than such a team. Often innovations, especially those involving multiple methods, are only possible by combining the talents of several researchers, each

of whom tackles an aspect of the problem (see for examples Pearce, 2002, and Newman et al., 2001).

However, a lone researcher may have different opportunities for producing creative or innovative work. The research agendas of powerful and influential members of one's subdiscipline privilege some research questions and methods and ignore others. These agendas provide good guidelines to successful work, and there is considerable risk to working outside of those guidelines. But, there may be potential rewards as well. Be that as it may, one's resources and the disciplinary consensus in one's field cannot really be disregarded in one's choice of research methods. My hope is that these constraints are not always determinative. To the extent possible, the nature of the problem should dictate one's choice of methods; it should be the earliest and strongest influence on methodological choices.

An earlier version of this table was presented in Vogt (2002).

Table 1. A taxonomy of methodological choices

1. ORIENTATIONS to research problems
2. DESIGN (Instead of what you had planned, why don't you ...?)
3. SAMPLING AND CODING
4. MEASUREMENT AND ANALYSIS

1.1.	Will the research stress evidence or interpretation?
2.1.	Why not conduct an experiment?
3.1.	Data collected by independent observation or asking subjects?
4.1.	How will reliability, or trustworthiness, be assessed?
1.2.	Will the approach be primarily deductive or inductive?
2.2.	Why not use a quasi-experiment design?
3.2.	Will the emphasis be on cases or on variables?
4.2.	Could the investigation's results be confirmed by replication?
1.3.	Is the main goal testing theory, constructing theory, or neither?
2.3.	Why not investigate a natural experiment?
3.3.	How important is researcher inference in gathering evidence?
4.3.	What is the most appropriate level of analysis or aggregation?
1.4.	What is the likelihood of discovering causal relations?
2.4.	Why not use a time-series design?
3.4.	Can variables be operationally defined?
4.4.	What will be done about participant attrition?
1.5.	Is the main goal, description, explanation, understanding, or

prediction?
2.5. Why not undertake an ethnography?
3.5. How will the participants or subjects be selected?
1.6. How broadly can the conclusions be generalized?
2.6. Why not engage in naturalistic observation?
3.6. Will participants' attributes be controlled or studied?
4.5. Will evidence be used mostly to test significance or measure association?
1.7. How big a problem is researcher bias?
2.7. Why not do a participant observation?
3.7. Could the way the evidence is used threaten subjects' privacy?
4.5.1 Will parametric or non-parametric tests be used?
1.8. Will the research assume that the subjects are rational actors?
2.8. Why not conduct survey research?
3.8. Will evidence about change over time be gathered?
4.5.1.1. Should data be transformed or left in the original metric?
1.9. Will the research be based on a particular disciplinary perspective (e.g. psychology or sociology)?
2.9. Why not conduct interviews and focus groups?
3.9. (How) will contextual effects be addressed?
4.5.1.2. ANCOVA or regression?
1.10. Is the topic better studied in the laboratory or in the field?
2.10. Why not do a secondary analysis of data such as census data or archives?
3.10. Will the evidence be quantitative or qualitative--or both?
4.5.1.3. OLS or logistic regression?
2.11. Why not conduct a meta-analysis?
3.11. What criteria are used to decide which evidence to gather?
4.5.1.4. Discriminant analysis or logistic regression?
2.12. Why not combine 2+ or more of the above into a multi-method design?
3.12. How will documents/texts be analyzed?
4.5.1.5. Planned or post-hoc comparisons?

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Sintesi

Nell'intraprendere una ricerca scientifica, la scelta della metodologia migliore per raggiungere un determinato obiettivo è un passaggio cruciale. Nella pratica reale, molto spesso questo sforzo iniziale è trascurato e tale omissione compromette il grado di innovazione della ricerca stessa. Per sviluppare il potenziale innovativo della ricerca, non solo sociologica, occorre, infatti, dedicare tempo e cura agli aspetti metodologici in un'ottica pragmatica. L'adozione di un approccio pragmatico relativamente alla metodologia da assumere spiega anche la centralità del problema. La domanda fondamentale per un ricercatore quindi diventa: quale metodo scegliere per affrontare questo tema? Farsi guidare dal problema determina, d'altra parte, una sequenza di scelte che parte dalla definizione del Design, prosegue con quella del target e dei casi da studiare (Sampling), a cui si connettono eventuali riflessioni etiche (Ethics), quindi la classificazione e misurazione dei dati (Coding/Measurement) e infine l'Analisi (Analysis).

Alcuni elementi di questa sequenza entrano in gioco solamente in una fase avanzata del lavoro, ma il ricercatore dovrà operare delle scelte metodologiche fin dall'inizio, con la consapevolezza che queste condizioneranno il lavoro nel suo complesso.

La scelta più impegnativa è quella del Design, poiché il piano progettuale condizionerà tutti i passaggi successivi e non potrà essere cambiato in corso d'opera senza provocare una revisione strutturale del progetto, con grande dispendio di tempo e risorse.

Per Research Design si intende infatti: "un piano (plan) per raccogliere dati (evidence) che possono essere usati per rispondere ad una domanda di ricerca".

In vista di una corretta definizione del Design, è utile ricorrere ad alcune domande chiave che possono guidare nella scelta, in base alla natura dei dati da ricercare (inferenze causali? dati generalizzabili? serie diacroniche?); alla fonte dei dati (dati rilevati direttamente? dati di seconda mano?) o alla necessità di applicare Design multipli. Per quanto riguarda l'ultimo punto, occorre, tuttavia, tenere presente che il maggiore sforzo legato alla moltiplicazione dei Design deve essere sempre compensato da benefici consistenti in termini di risultati della ricerca.

Queste riflessioni sul metodo e sul processo di ricerca nascono da una lunga pratica

e da una serie di interviste a ricercatori, raccolte in due fasi, nel 2001-2 e nel 2006-7. Dall'analisi delle interviste si deduce che la scelta del metodo è meno considerata dai ricercatori di quanto ci si aspetterebbe. Al contrario, la tendenza a conservare il metodo di ricerca tradizionale nella propria disciplina, la personale propensione verso un certo metodo e la poca conoscenza delle possibilità tra cui scegliere contribuiscono a far sì che la scelta del metodo sia spesso scontata, con la conseguenza che si riducono lo spazio e le possibilità di innovazione.

Scelto il Design, la ricerca procede con la raccolta dati, classificazione e interpretazione, fasi che richiedono nuove scelte metodologiche e la definizione di un piano per determinare la raccolta dati e lo studio di casi, con la specifica dell'utilizzo e la relativa descrizione. Tutte le componenti sono solo apparentemente momenti separati della ricerca, in realtà funzionano come un tutto interconnesso in cui ogni parte condiziona e determina le altre.

Negli ultimi anni è cresciuto l'interesse per i cosiddetti mixed methods che combinano metodi quantitativi e qualitativi. Una certa resistenza, tuttavia, rimane abbastanza frequente tra i ricercatori soprattutto nei confronti dell'uso di metodi quantitativi. Eppure la ricerca multi-modale con mixed methods risulta spesso più innovativa e non dovrebbe essere limitata alla sola fase di misurazione dei dati. Piuttosto, nei modelli multi-modali può costituire un problema l'eccessiva, e a volte ingestibile, quantità di scelte preventive richieste, che finiscono per contrarre il tempo dedicato alla ricerca vera e propria.

In conclusione, le scelte metodologiche necessarie dovrebbero essere sempre condotte in modo pragmatico e pluralista, considerando tutte le variabili e preferendo il metodo in grado di produrre un effettivo accrescimento di conoscenza o un'apertura di nuove prospettive per la ricerca.