
 How to Find What to Solve

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Another super-clever, super-knowledgeable book from Andrew Abbott. This one is a guide for how to find something to write about when doing a research project or paper, that is, the part that gets left out of methods courses. This book purports to be aimed at undergraduates, and maybe it will reach some of them, but probably by way of the professor becoming enamored with the book. Much of the conceptual scheme is too clever and the examples from existing research will make most sense to professionals. But it's all for a good cause, and this certainly ought to lighten up the lives of a happy medium of graduate students.

My favorite chapter is the first, where Abbott argues that research methods cannot be classified into any simple or consistent scheme, since they differ on at least three dimensions (types of data gathering, data analysis, and number of cases), yielding an unwieldy grid of 36 combinations. He picks out five main types: ethnography, historical narration, formal modeling, small-N comparison, and standard causal analysis (i.e. multi-variable number crunching). He goes on to show where these fit in a three-dimensional space taken from Charles Morris's modes of language: pragmatic, syntactic, and semantic. I think the hypothetical undergraduate is going to be reeling at this point, but the professor is going to be catching his or her breath in admiration.

It gets easier. The simplest way to come up with something new to write about is what Abbott calls "the routine heuristics of normal science," i.e. adding one more variable to what has previously been studied, or moving the theoretical question to another place. Abbott excoriates this as timid and boring. He includes here politically-motivated versions, such as the "add-women-and-stir" formula (p. 245) for making a feminist study out of any topic X.

More adventurous heuristics involve making analogies to patterns in other fields, or transplanting their methods, problematizing what seems obvious, reversing the usual

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order of argument; changing the context or level of analysis. Sometimes one goes against the grain by putting a static analysis into historical time, sometimes by freezing history by making what is contingent analytical or vice versa. These chapters are loaded with brief examples of research that fit the particular heuristic. Sometimes the method seems obvious, at other times I seriously wonder whether the authors originated their ideas in that way, and instead Abbott is just playing a game of fill-in-the-conceptual-box from his wide-ranging reading. Abbott tells us that his inspiration for this book was G. Polya's *How to Solve It*, a book for mathematicians and some of Abbott's heuristics seem arbitrarily borrowed from math, where reversing terms and working backwards are useful devices. Transposed to sociology, some of these devices seem artificial.

Yet there is a deeper theme. Abbott argues in the early part of the book that no method can solve all its problems and defend itself against all critiques; hence, there is a tendency for methods to recycle, with each methodological camp subdividing into factions which recapitulate internally their lines of division with opposing methods. This, of course, is what Abbott has shown in his previous book, *Chaos of Disciplines* (2001). Here Abbott argues that these fractal cycles can be turned to advantage, by deliberately making such moves as heuristics for generating new research ideas.

What one sees as fractal heuristics, though, depends on what dimensions of deep-seated conceptual division one thinks exist in the field. Abbott lists nine of them, including positivism vs. interpretivism, behaviorism vs. culturalism, and individualism vs. emergentism. Taken as heuristic moves on this terrain, Abbott again is semi-

convincing, crossing back against the grain might be the way some of the authors he cites made their innovative moves, but it might also be Abbott's own retrospective imputation of strategies as he fills in the boxes he has created. Some of the dimensions he construes are weird; for example, he holds that on the conflict vs. consensus dimension, "consensus thinkers hold that people are disorderly and greedy" and hence they spend all their efforts explaining how they are constrained, whereas "conflict thinkers hold that people are by nature orderly" and conflict must be explained by social institutions (p. 201). Abbott gets trapped in his overly clever formulation. Durkheim and Parsons, consensus thinkers par excellence, certainly did not think that it is human nature to be disorderly and greedy (Abbott has Hobbes in mind here). And apart from a utopian strain in Marx, there are not many conflict thinkers who hold that human nature is harmonious, or indeed that there is any such thing as human nature at all (or that there is any need to invoke it in explaining either social peace or conflict).

The breezy tone that Abbott takes through this brief book has its costs—but on the whole, it serves him well. For 200 pages I was wondering if any kind of new idea, no matter how artificial, is okay with Abbott; what makes some of these ideas successful while others fail? In the final chapter, Abbott makes this a matter of practical advice. Avoiding epistemological questions of whether an idea is true, he tells the apprentice sociologist to try one's ideas out on data, and even more importantly, on other people. This leads to some clever (and realistic) advice on the pitfalls of being too easily understood, and how to deal with being misunderstood. One also needs to locate oneself in a wide range of scholarly literature and academic encounters, and to develop one's taste for what is good and bad research. Here

again Abbott comes down hard on banal, formulaic sociology. He really is a thinker of the fractal gaps; good intellectual taste for him is crossing over against the usual divides of intellectual camps, making combinations and counter-moves that are surprising and elegant. He also holds that these are the best moves, the most fruitful in some sense which linger in the background of his argument, not fully explicated.

A clue is in his closing pages, where he notes that sociologists often get their problems from moral and political commitments, or from personal experiences or identity problems. He rejects the former kind of work as formulaic, the latter as making researchers one-dimensional, making their statements and then burning out in middle age. In contrast, "faculty who are deeply puzzled about the social world without having a personal or social agenda are often the hardest to come to know. Their passionately disinterested curiosity seems strange to the majority of us, who have come to social science from personal and social concerns. But they are always among the most creative" (pp. 247–8). This is admirable. Is it then just a rare personality trait? No, but what it is cannot fully emerge in a book on heuristics. My clue: these creative thinkers are deep inside the network of intellectuals, rather than "heteronomous" (in Bourdieu's terms), inwardly oriented towards the past and future of its networks of struggles over intellectual attention space. Being in this kind of network position involves not only a command of heuristics, but is also a resonance with the intellectual community by means of emotions. Conveying the emotion along with the heuristic is how this network propagates itself and getting deeply into the network is the ultimate heuristic of all.

Reference

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An Eye-Opener!

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This book, a pint-sized paperback about five by eight inches, has only 248 pages of text, but it packs a wallop. At only \$17.40 at Amazon, it's a steal. For all the social and behavioral sciences, it offers an exciting boost to our "craft or sullen art." If you are in the business of research, don't hesitate: read it immediately. No matter your persuasion, you won't be sorry. The otherwise helpful Glossary offers only an abstract definition of the key word *heuristic*: A discipline that aims to facilitate invention and discovery of new facts and ideas in the sciences. But Abbott actually uses the word in its dictionary meaning: "Involving or serving as an *aid* to learning, discovery, or problem-solving by experimental and especially *trial-and-error* methods" (Merriam-Webster online, emphasis added).

This definition demotes heuristic from a highfalutin discipline to the lowly level of aid, as if one were to use a mere shovel or screwdriver to build a house—so much for sacrosanct loyalty to pure disciplines, sub-disciplines and "schools of thought." Quantitative, qualitative, sociological, psychological, historical, experiments, surveys, ethnography, conversation analysis, feminism, Marxism, and all other methods, theories, and approaches are only aids against the sluggishness of thought. They are to be ignored whenever they don't help with the particular problem or data at hand. Hang loose, dudes and dudesses!

To illustrate the use of heuristics, Abbott draws upon brief profiles of particular studies in the social sciences. Examples from history, economics, political science and sociology hammer home the practical details of heuristics as aids to discovery, even when the authors didn't think of their work in this way. Abbott's meta-analysis makes new even the most familiar of studies, like any good heuristic, often to the point of eclipsing the point of view of the original authors. Perhaps this is the reason that in his review of Abbott's 2001 book, Charles Tilly noted that it varied "in tone from Don Quixote, to Cassandra, to Vlad the Impaler . . ."

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One serious limitation of Abbott's use of examples is the absence of studies drawn from psychology, psychiatry and psychoanalysis. For reasons not explained, there is no empirical work by Solomon Asch, Freud, Colin Parkes, and many, many others who have made substantial contributions. Another book should be written about heuristics in these fields because they are at least as important as those chosen from the social sciences.

Some of the aids that Abbott proposes are so simple as to sound like tricks or gimmicks. Running out gas on a research proposal? Try reversing a well-known hypothesis or re-labeling the concepts. Come to think of it, in my own work I have begun to "emotionalize" language that hides emotions under cognitive and behavioral disguises. For example, "terror management" studies of "mortality salience" can be translated into the simpler and more universal language of fear. Likewise the innumerable studies of shyness, social anxiety, lack of self-confidence, self-esteem can be outed from the embarrassment/shame closet (Scheff and Fearon 2004).

Many of the heuristics that Abbott proposes are fancier, however. What he calls lumping, splitting, and analysis of counterfactuals seems to be at a middle level. Perhaps the most sophisticated approach is his celebration of fractals: ". . . the property of recurring at finer and finer levels, always in the same form" (p. 250). Abbott has also published an earlier work (2001), mentioned above, that promotes fractals.

Fractal models might help explore a central issue in social science: how can we build links between the most microscopic levels of human conduct and experience up to the highest levels of culture, social institutions and society? In his 2001 essay, Abbott applied

this idea to the reproduction of conflict at various levels between and within disciplines and sub-disciplines in the academy, and in the world of intellectuals in general. Perhaps the enigma that is Goffman's *Frame Analysis* (1974) might yield up its treasures to a fractal analysis of Goffman's otherwise dizzying arrays of boxes within boxes within boxes (Scheff 2005).

The fractal idea gives rise to interesting questions in social science. The elegance of fractal geometry in the physical world arises because of the EXACT duplication of forms at different levels, with no difference at all except in size. Snowflakes provide an example. Goethe, in his botanic studies, noted that in plants such as palms, the whorls of the trunk can be found repeated down to the smallest living subunits.

Yet arguments over Marxism at various levels in the history of the socialist movement are similar in some ways and different in others. Certainly Proudhon's rebuff to Marx was never repeated, at least in so eloquent a form. The shape of snowflakes is elementary compared to the complexity of human discourse. Both sides of the conflict between the Leninist and Trotskyite lines probably varied with each argument, depending upon context, emphasis, choice of words, overt and/or covert emotional content, etc. To use the fractal heuristic effectively one would require conceptual and operational definitions of each "line," so that the extent of variation could be noted. The same reasoning applies to deciphering Goffman's frame analysis, because neither he nor any of his commentators provided a clear definition of frames. Heuristic analysis puts interesting demands on the researcher.

I promise that this book will help any researcher who reads it, but it's not easy. Because it is meta-analysis, the logical path is relentless and without digression or humor. There are no images of actual human beings or their voices. Although it has a hands-on feeling about conducting research, it provides only a bird's-eye view of the human condition. This reader found it necessary to take a break and a breath after each page as a heuristic for digestion.

At the risk of being impaled, a final complaint. It seemed to me that a faint wisp of complacency about social science hung over the text. I could be mistaken about this, but

it appears that in choosing the best of social science studies as examples, there was a tendency to equate best of a bad lot with good in some absolute sense.

One example is Blau and Duncan's 1967 study of occupational status attainment between generations. This study is mentioned several times, and given high marks each time. Innovative at the time, it provided the model for many subsequent quantitative studies. It is often seen as magnificent social science. The amount of variance accounted for is higher than most social and behavioral science studies: compared to studies based on self-esteem scales, it is gigantic (Scheff and Fearon 2004). But even so, it is still quite small, nowhere near half the variance, indicating to me that the major causes of status attainment are still undocumented.

As far as I know, replications, like snowflakes, continue to produce copies of the original study without gaining ground on identifying major causes. What about luck, pluck, obsession, greed, and criminal/unethical proclivities?

Durkheim's study of suicide has been replicated many times, because it was innovative at the time it was conducted. Like Blau and Duncan's, Durkheim's study accounts for only a tiny proportion of the variance. Even the best of the subsequent studies didn't seek the major causes of suicide, content to be snowflakes. Innovative studies should be honored, not worshipped. After enough replications, they should become heuristics, not shrines.

Still, I find Abbott's book the most exciting contribution to the craft of research for many a year. Don't argue, just buy, borrow, or steal it without further delay.

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