Getting Started on Political Research

Benjamin A. Most, *The University of Iowa*

Foreword

Steven C. Poe
University of North Texas

Benjamin A. Most, or "Ben" as he was affectionately known to his friends, colleagues, and students, was an associate professor at the University of Iowa when he died suddenly in November of 1986. In the discipline of political science he will be remembered most for his provocative and cogently argued "think pieces," written with Harvey Starr, that force researchers in the international relations field to reflect on the logical and theoretical bases of their inquiry.

While his published contributions to the field will live on for some time, in a few of us who were lucky enough to be his students he leaves another legacy. From the opening day of his introductory graduate I.R. class it became clear that Ben had an infectious enthusiasm, even excitement for political research. As the semester progressed, students—especially those who got close to him—often "caught" Ben's love of research, which I believe remains alive in us still, someday perhaps to be passed on to yet another generation of students.

Ben's enthusiasm, as well as the thoughtfulness with which he approached his discipline, are communicated in the following piece, in which he presents many of his ideas on the research enterprise. It is hoped that its publication will help students of political science currently struggling with the perennial questions of, "What should I do?" and "How should I do it?" to finally get started.[1]

What They Ask and How They Think

Students—at least those with whom I have worked over the years—find it extraordinarily difficult to focus in on research questions. Failing all else—and confronted by the approaching end of the semester which raises the specter of (yet another) incomplete—they typically visit my office to discuss "what they should do." All too commonly they have no idea how to begin; if they do have an idea, it is not always a terribly useful one. Getting started appears to be very difficult for them. If I understand what they are saying to me, the experience is also more than a little traumatic.

There are at least two obvious explanations for this situation. One suggests that the difficulty is with the students: They are "not very good," as some would argue; hence, "it's not terribly surprising that they don't do very good research." That explanation is facile from the standpoint of a faculty member, but it strikes me as a bit too convenient.

The other obvious explanation is that students have problems in getting started because we—perhaps I should say, "I"—am not very good at *teaching them how to get started.* Perhaps when all is said and done, students do no worse—and lamentably, not any better—than what I train them to do. If so, then it seems incumbent on me to examine the styles of some successful researchers, and also to reflect on how I do my own research.

Intuitive Leaps and Creativity

An important assumption should be made explicit at this point: There are different styles or ways of beginning to do research and there is probably no one way that is best for all analysts studying all problems. Nonetheless, *how* one begins does appear to affect the quality of the work. As even a cursory consideration makes clear, our greatest thinkers not only perform differently; they also have unique or at least distinctive research styles. If they go further than most of us, it may only partially be that they are brighter; it may also be that they succeed, produce, and discover because they have learned to reason and research in ways that are particularly efficient.

John R. Platt, professor of physics and biophysics at the University of Chicago, puts these points quite nicely when he observes that:

> . . . anyone who looks at the matter closely will agree that some fields of science are moving forward very much faster than others, perhaps by an order of magnitude, . . . Why should there be such rapid advances in some fields and not in others? . . . I have begun to believe that the primary factor in scientific advance is an intellectual one. These rapidly moving fields are fields where a particular method of doing scientific research is systematically used and taught, an accumulative method of inductive inference. . . . (Platt 1964, pp. 347-353)

That said, it should be added that many scientific breakthroughs and much success in research may be, as Platt puts it, "outside any rule or method" (ibid., 351). As the following observations suggest, great strides sometimes come from researchers' intuitive leaps and creative insights:

> Does anyone thing that [a law such as Kepler's] is found by taking enough readings and then squaring and cubing everything in sight? If he does then, as a scientist, he is doomed to a wasted life; he has as little prospect of making a scientific discovery as an electronic brain has.
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> It was not this way that Copernicus and Kepler thought, or that scientists think today. . . . [Copernicus'] first step was a leap of imagination—to lift himself from the earth, and to put himself wildly, speculatively into the sun.
>
> . . . All science is the search for unity

> in hidden likenesses. . . . The scientist looks for order in appearances of nature by exploring such likenesses. For order does not display itself of itself; if it can be said to be there at all, it is not there for the mere looking. There is no way of pointing a finger or a camera at it; order must be discovered and, in a deep sense, it must be created. What we see, as we see it, is mere disorder. (Bronowski 1965)

Alternatively, consider the following comments by William N. Lipscomb, 1976 Nobel Prize Winner in chemistry for his work on boranes:

> . . . A scientist proceeds in making discoveries in very much the same way that an artist goes about working. You have to master a large discipline, and your discoveries are not necessarily made by planning them. They arise intuitively. You suddenly perceive brand-new connections that you were unaware of before. Material somehow reorganizes itself in your mind, and that leads to the spawning of a new group of ideas.
>
> One of the major problems in this process is not the lack of information but rather the abundance of contradictory material. You somehow have to see through the contradictions and fit the material together in a new way. Then, all of a sudden, things click. You begin to think, 'Well, maybe there is something to it.' The scientific method comes into play only after you make your intuitive jump. You use it to test your ideas—but not in the generation of new ideas. (interview with Lipscomb, *U.S. News and World Report,* April 20, 1981, p. 85)

If creativity, intuitive leaps, and inspired insights are indeed part and parcel of major scientific breakthroughs as these observations suggest, then it may be difficult to know what one can do so that "things click." Perhaps there is no way, no thing that can be done, to insure success.

It is also possible, however, to turn the problem on its head. Even if no method or style of research can guarantee a creative flash, there may be approaches that increase the probability that leaps will occur or that they will be recognized and exploited when they do.

Amplifications and Corrections

The Wingspread Conference on International Education, described by Michael G. Schechter in *PS,* September 1990, pp. 461-63, was co-sponsored by the American Political Science Association, the American Historical Association, and the Association of American Colleges, along with the Atlantic Council.

Ellis Sandoz details National Endowment for the Humanities support for political science in *PS,* September 1990, pp. 455-60. Clarification is warranted regarding the description of the genesis of Project '87 as given on page 459:

Project '87, the joint effort of the APSA with the AHA devoted to research and education on the U.S. Constitution, was established in 1977 by James MacGregor Burns and the late Richard B. Morris. Project '87 received considerable support from the National Endowment for the Humanities, including grants for planning, instructional television, classroom lessons for secondary school students, and the publication of *this Constitution: A Bicentennial Chronicle.* The grant awarded by NEH to Robert A. Goldwin for a ten-year study of the Constitution supported conferences and monographs of the American Enterprise Institute, an effort independent of Project '87.

Successful Thinkers and Disciplined Creativity

Even if it is the case that creativity and intuitive leaps are important components of successful research, it seems clear that some analysts and teams of researchers have a sort of creative knack. They get the job done.

One illustration of this is a highly successful "mentor chain" which is described by Robert Kanigel ("The Mentor Chain," *F&M Today* 10:5, 1981). The chain consists of a string of pharmacologists running from Bernard Brodie, to Julius Axelrod, to Solomon H. Snyder, to Candace Pert and Gavril Pasternak, and most recently to Terry Moody. Each senior member of the chain, according to Kanigel, has earned great distinction; taken together, the group's performance has been outstanding.

As Kanigel describes this chain, each traditional master or senior analyst had apprentice Ph.D. candidates and "post docs." Each of the older, more seasoned researchers proceeded in his or her turn to pass along knowledge and technique to their apprentices. As Kanigel puts it, each one also passed along "an approach, a style, a taste in the mouth or feel in the gut for just what makes 'good science.' " It is this style, what the members of the chain call the "Brodie Legacy," that appears to be linked in important ways to the group's success (see Appendix). Put briefly, the scientific legacy that seems to have come down all of the way from Brodie to Moody might be summarized in the following way:

> Don't bother with the routine problems; leave them to others. Don't bother, either, with big, fundamental problems that are simply not approachable with available techniques and knowledge; why beat your head against the wall? Half the battle is asking the right question at the right time—when it's neither premature to tackle it, nor invites too obvious an answer; when the right methodology is available; when enthusiasm is at its peak. . . .
>
> And then just *do it.* Don't spend all year in the library getting ready to do it. Don't wait until you've gotten all the boring little preparatory experiments out of the way. Don't worry about scientific controls, at least for now. Just go with your hunch, your scientific intuition, and isolate that simple, elegant, pointed experiment that will tell you in a flash whether you're on the right track. (Kanigel 1981, p. 8)

An alternative, but nonetheless useful, way for developing disciplined speculation is presented of course by Lave and March:

1. Observe some facts.
2. Look at the facts as though they were the end result of some unknown process (model). Then speculate about processes that might have produced such a result.
3. Then deduce other results (implications/consequences/predictions) from the model.
4. Then ask yourself whether these other implications are true and produce new models if necessary. (Lave and March, 1975, 19-20)

Platt outlines a similar, but obviously more complex, approach that he labels the method of "strong inference." As he describes it, the procedure is designed to encourage the researcher to work rapidly through a "conditional inductive tree" or "logical tree" in a way that exclude alternative hypotheses.[2] Although it may sound cold and rather dreary, the following observations attest it may be anything but:

> On any given morning at the Laboratory of Molecular Biology in Cambridge, England, the blackboards of Francis Crick or Sidney Brenner will commonly be found covered with logical trees. On the top line will be the hot new result just up from the laboratory or just in by letter or rumor. On the next line will be two or three alternative explanations, or a little list of 'What he did wrong.' Underneath will be a series of suggested experiments that can reduce the number of possibilities. And so on. The tree grows during the day as one man or another comes in and argues about why one of the experiments wouldn't work, or how it should be changed. (Platt 1964, 348)

The research strategy of another noteworthy researcher, Thomas Alva Edison, might also be noted. While his style seems to have been distinct from that just outlined, the point to note is that Edison had a way of proceeding which appears in retrospect to have been a major contributor to his success. A recent report reveals new insights into Edison's style:

> The new portrait of Edison is marked by his powerful ability—never fully recognized until now—to reason through analogy. It was perhaps this trait more than any flashes of brilliance or cries of 'Eureka!' that accounted for his great inventiveness. It is now thought that this hidden ability is what transformed one successful invention into another, eventually producing the phonograph, the incandescent light bulb, systems of electric power generation and motion pictures. (Broad 1985)

Even though Edison's phonograph and his kinetoscope or motion picture machine looked completely different, for example, it is now clear from Edison's notes that the inspiration and original ideas for the kinetoscope came from the already successful phonograph. The new device did not spring to life in a flash of insight; rather, it evolved as Edison worked by analogy from one invention to another. Edison's papers reveal that he sought solutions to new problems by returning to solutions that had worked before.

The discussion here could go on, of course, but the point should be clear. Even though creative insight has been critical in the work of some scientists and such leaps may be beyond method, some of our most noted researchers appear to have had a disciplined creativity that increased their odds of getting lucky.

My Own Ways of Beginning . . .

It would be presumptuous to suggest that my own ways of beginnings are comparable to those just outlined. Nonetheless, if reviewers' comments are to be believed, I do have ways of proceeding which are at least different, if not necessarily good.

To begin, it seems to me that I only very rarely begin with the standard question: "How do I (how could one) 'explain' y?" Much more commonly, my interest is attracted by situations in which it appears initially that y and $-y$ are, in some sense, both true.

I'm not the only one who is intrigued by such situations, of course, but others are inclined to sort out the $y/-y$ problem by conducting some sort of crucial or critical test to see which is "right." My own response is different. If theory, empirical research, crude interpretations of reality or any combination of those considerations suggest that y and $-y$ are both true, I simply accept that and move on to focus—not on *which* is right—but rather on *how both* could be right.

An example may clarify the point. In the IR literature we have two hypotheses that purport to link dyadic power relationships and the onset of war. One argues that parity between two nations leads to conflict; sharp imbalance serves to insure peace. The other suggests that imbalance leads to conflict while parity leads to peace. The two hypotheses—parity/war and imbalance/war—appear to be mutually exclusive; at least that's how they're treated in the IR literature. Analysts are forever arguing for or against one or the other; various studies are published, each using different research designs, cases, power indices, war measures, analytical techniques, and so on. Not surprisingly, the results don't add up. Support is found for—and also against—both hypotheses. Also not surprisingly, reviews of this state of affairs typically yield calls for more or better analyses that will eventually reveal which is right. For the moment, *neither* hypothesis is ruled out definitively. Both postulates remain live hypotheses; the field seems unable to take the next step.

As I reconstruct a recent consideration of this state of affairs, it seems to me that I did at least two things differently. First, I tried to think through what the world would look like if one or another of the hypotheses held. I tried to do this quickly, spending only a couple of hours on the task. Given those crudely impressionistic predictions or derivations, I then did a "quick and dirty," intentionally nonsystematic consideration of what I call "stylized facts," some simple observations which provide insight on the research question, and a simple hypothetical case.[3] Again I tried to move quickly.

I did not want to spend 6-12 months designing a project, developing measures, collecting data, and conducting the analysis. What I did want was some sort of simple shortcut that would either bypass that work altogether or give me a good reason to believe that spending the 6-12 months would prove productive.

I imagined a simple system of three nations, one of which (the ith) was powerful while the other two (j and k) were weak. I imagined the system to be at peace. I recognized immediately that *both* power imbalance (between i and j and i and k) *and* power parity (between j and k) were consistent with the occurrence of peace. The inverses of the two, *apparently contradictory* hypotheses *must both be true* whenever any mixed system is totally at peace. More correctly, both hypotheses can be rejected as they stand; the simple hypothetical shows that *neither* parity nor preponderance can be *sufficient* for conflict initiation.

Stylized facts (e.g., a consideration of the peaceful U.S./Canadian/Mexican triad; the Bolivian/Argentine/Brazilian subsystem; the shifting power relationship between the U.S. and the USSR during the post-World War II era, and so on) led me to identical conclusions.

Thus informed, I hazarded that the explanation of the ambiguous results might not rest in the ways in which analysts had done their work. A better hypothesis, it seemed to me, was that the results were contradictory because that's the way the world really is: *Both* hypotheses must be true *sometimes.*

Given that presumptive conclusion from the quick and dirty, impressionistic, and logical considerations, I moved on—not to a focus on which is right—but rather to an effort to figure out what the additional—prior or initial conditions—might be that influence which outcome will pertain. If one postulate is written, "if parity, then conflict initiation," and the other is phrased, "if parity, then no conflict initiation," I search for the "X" in the following:

> Given x: If parity, then conflict initiation.
> Absent x: If parity, then no conflict initiation.

(Note "x" here could be a single variable or a complex set of relationships. The key is only that "x" defines the domains in which the parity/initiation relationship does and does not hold. Note also that the connector "if" which links x with the argument is meant to be merely illustrative. Other linkages were in fact specified.)

A second example may clarify the point. A colleague recently presented a paper in which he hypothesized that being in a war would lead a nation (its people and/or its leaders) to avoid subsequent conflicts; a war experience at some t leads to a decreased probability of war at $t+1$.

Letting y_1 denote nations' "first war" experiences and y_2 signify their second engagements, the author discovered that y_1s covaried only weakly with y_2s and various derivations thereof. Nations with y_1s did not necessarily have fewer y_2s; nations with lots of y_1s did not have longer —or shorter—waits until their next war experience. In short, after a fairly lengthy study, the analyst concluded that occurrences of y_1 have no systematic relationship on $P(y_2)$.

When the presenter had concluded, the discussant on the panel offered what I thought were two interesting observations: First, he pointed out that one could imagine—indeed, one could point to actual—cases in which, having been involved in a war, nations were either satiated or they shied away in revulsion from subsequent engagements; for them, y_1 decreased the $P(y_2)$. One could also, however, point to other cases in which nations became targets of their neighbors or they set out to exact vengeance and reestablish the status quo ante; for those nations, y_1 increased $P(y_2)$.

The discussant's second point was this: Given that one could imagine (or actually come to know on the basis of already available, quick and dirty, evidence) that y_1 sometimes leads to increases in $P(y_2)$ but leads to decreases in $P(y_2$ in other instances, it really isn't worthwhile exploring the y_1/y_2 relationship, *per se.* One knows—before the research is even undertaken—that the y_1/y_2 relationship will probably wash out or be ambiguous because *one knows* that the y_1/y_2 argument is incomplete! Something(s) operate to mediate or alter the effect that y_1 has on y_2 and there is little sense in going on until that something is specified!

Confronted by situations such as these in which I know—or have prima facie evidence which leads me to believe—that different outcomes sometimes hold, I try simply to create a story describing a process or processes that could produce the different results. In doing this, I find two analogies helpful. The first is a simple computer program flowchart. A process is begun and runs along for awhile. Eventually, a branch point is reached; processes go different ways—into different subroutines—and come out differently. The second analogy is spun from Starr's concepts of "opportunity" and "willingness." They probably do not need to be explained here, except to note that I use them as guides to avoid what I think of as excessively complex (and hopefully unnecessary) arguments.

Once the story is created, I am not interested in waiting around to test it in the normal fashion. At a minimum, I expect it to account for the stylized facts and hypotheticals with which I began; beyond that, I expect that it should deal with some additional known situations and illustrations. My point, however, is that I find it helpful to move quickly to make some crude tests. I look for more stylized facts. I try again to think about what the world would look like if. . . . I try to figure out what we know that bears or might bear on the problem. I'm not much interested in spending months and months writing equations, developing measures, collecting data, and estimating parameters unless I have reason to believe that I'm on to something. Time is too valuable, life too short, to waste my time on dead ends. If the story fails to fit in these initial tests, then it's back to the drawing board! If it works—but *only if* it works—then I will be interested in investing the time and energy to subject it to more rigorous specification and tests.

Conclusion

A professor of mine used to say that, "People never do research the way they tell others how to do research." I suspect that the observation is largely true. I'm often loathe to prescribe too much to my students because I am not at all certain that I really do my own work the way I think I do.

That said, it may nevertheless be useful to think from time to time about how we work. While there's probably not too much that any one of us can do to make ourselves more intelligent, we certainly can learn to reason and research more efficiently and effectively.

Appendix: The Mentor Chain

Julius Axelrod, 1970 Nobel Prize Winner in Physiology and Medicine, on his mentor, Bernard Brodie, Goldwater Memorial Hospital, New York:

> He made every experiment seem earth-shattering and encouraged the kind of 'quick and dirty' experiment that might suggest whether an approach was worth pursuing more deliberately. . . . Somehow, by taking a chance and driving ahead, it was as if you were wrestling with the gods themselves. Instead of thinking of all the reasons why you should hold off, Brodie's dictum was: 'Oh let's take a flier on it.'
>
> . . . Do an apparently simple experiment that gives you an important bit of information. . . . Ask the important question at the right time. If you ask it later, then it's obvious.

Solomon H. Snyder, professor of pharmacology at Johns Hopkins Medical Center, on the style he learned from his mentor, Julius Axelrod:

> . . . science is as creative as any of the arts. He'd talk of theories that were beautiful . . . symmetrical . . . the kind of things you get excited about, lose sleep over.
>
> . . . A student will say, 'But it's good science, isn't it?' and I'll say, 'Yes, but it's boring. I think we can do something more exciting.

Candace Pert, National Institutes of Health, on her mentor, Solomon H. Snyder:

> He had a pragmatic, handyman approach to science. He was always sidestepping the grey muck of experimental tedium, always reaching for the heady scientific heights—the more fundamental, more exciting problems that sneered at routine. He went right after what he wanted: Need a new technique just appearing in the scientific literature? Don't spend days in the library poring over journals; just call up its originator and get the details directly. Spy a striking new tack to take with a problem? Don't worry about scientific controls for now: 'Just get hysterical and do it.'

Terry Moody, assistant professor of biochemistry at the George Washington Medical Center, on his mentor, Candace Pert:

> She's always willing to take the long-shot.

Source: Developed from Robert Kanigel, "The Mentor Chain." *F&M Today* 10:5, 1981, 1-8.

Notes

1. The first draft of this essay was prepared for members of the 3-I (Illinois, Indiana, Iowa) seminar on complex systems. This is an edited version of a later draft, presented at the meetings of the Midwest Political Science Association in Chicago, Illinois, in April of 1986. It should be noted that Professor Most had planned to make some revisions before submitting this piece for publication, and that I have edited it in spots for that purpose. Therefore responsibility for any errors is mine, and not his.

2. See Platt (1964, pp. 347-353) for a more detailed discussion of what is involved in the method of strong inference.

3. See Most and Starr (1989, chap. 7) for a further discussion of the utility of stylized facts. See Chapter 6 of this book, and Most and Starr (1987) for the results of the research project described here.

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Helpful Hints for Writing Dissertations in Comparative Politics

Peter A. Hall, *Harvard University*

Perhaps fortunately, one is rarely given the opportunity to read fifteen doctoral dissertations in comparative politics within a brief period of time. Having recently served on a committee which presented the opportunity, I can only say that it tends to inspire uncontrollable bouts of reverie about such matters as the state of the discipline, the long-forgotten experience of writing one's own dissertation, the nature of causal arguments, and the inexplicable moments of human frailty that lead one to agree to serve on such a committee in the first place.

One of the subjects to which the mind wanders, however, is more useful than the rest. That is the issue of what makes for a good doctoral dissertation and what pitfalls might be avoided when the final draft is constructed. As I read these dissertations, I was reminded of those newspaper columns about good housekeeping or home repair, with titles like "Hints from Heloise" or "Help Around the House." What advice might Heloise give to the aspiring doctoral student about to put pen to paper? Are there any generic hints about what to aim for and what to avoid in the presentation of the research that might be useful to all who write such a dissertation?

What follows is a list of *'do's'* and *'don't's'* that occurred to me in the