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## 6

# Is Political Psychology Sufficiently Psychological? Distinguishing Political Psychology from Psychological Political Science

JON A. KROSNICK

During the last thirty years or so, political psychologists have turned out a great deal of empirical research and theory of which we can be quite proud. In the midst of this productive enterprise, we have occasionally taken time out to lobby other scholars outside our circles to make use of the theories and methods that we find most useful (e.g., Kinder and Palfrey 1993). Less often, we have taken a step back from our empirical work to don a self-critical hat and ask whether we are going about our enterprise in as constructive a fashion as we might (see, e.g., Kuklinski, Luskin, and Bolland 1991).

One purpose of this book is to do so quite deliberately. Other chapters address the questions of whether political psychology is sufficiently theoretical and whether it is sufficiently political, two matters on which we have been criticized by political scientists who take different approaches. In this chapter, I will address a different question, asking whether political psychology is sufficiently psychological.

My goal is to be controversial in raising philosophical issues about political psychology as an enterprise that may deserve more explicit consideration than they currently receive. In short, I will suggest that two very different sorts of political psychology are being carried on, sometimes within the same research project or even within the same paper. I will clarify the distinction between the two, illustrate that one seems to be far more common than the other, and argue that it would be useful to reduce this imbalance by doing a more psychological version of political psychology more often.

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## POLITICAL PSYCHOLOGY VERSUS PSYCHOLOGICAL POLITICAL SCIENCE

Is political psychology sufficiently psychological? "A preposterous question to ask!" you might say. "Of *course* it is sufficiently psychological! Everything we do uses psychological concepts!" But merely using psychological concepts is not what I mean by psychological.

To clarify what I do mean, consider a linguistic analysis of the term "political psychology." This reigning label for our joint enterprise, given its ordering of the two constituent disciplines within it, makes politics a modifier or qualifier of the type of psychology being done. That is, the label can be viewed as suggesting that we are doing *psychology*, but in a particular context, the political one. Thus, the name we have settled on for our enterprise might seem to place the priority on the goals of *psychology* and suggest that political psychology is a subtype of the larger discipline of psychology generally. So to understand what the principal aim of such an enterprise would be, we must establish what psychology is.

According to Zimbardo (1988), psychology seeks to answer one fundamental question: "What is the nature of human nature?" (5). Stated more concretely, psychology is "the scientific study of behavioral and mental processes . . . [with an interest] in discovering . . . general laws" (5). Because psychology seeks to identify *generalizations* about the mind, a person doing political psychology true to its name would not be interested in identifying patterns that hold *only* in the political context. Rather, he or she would study the political context in a search for more general principles of thought and action that are pan-contextual. This would be political psychology true to its name, and it would be comparable to "social psychology" or "cognitive psychology" or "consumer psychology" or "health psychology," all terms routinely used to describe currently active subfields seeking to identify generalizations by detailed studies of particular contexts.

To anyone knowledgeable about the history of political psychology, it is immediately apparent that this conception of our field falls quite short of effectively describing most of what we do. If one were to gather up all the political psychology studies published during the last thirty years, "psychological political science" would most certainly seem to be the more apt descriptor in most cases. That is, we usually have a primary interest in serving the core goals of political science.

What is political science? According to Easton (1953), it is the study of "activity that influences significantly the kind of authoritative policy adopted for a society and the way it is put into practice" (128). Put more succinctly, politics may be said to be activities "of or concerned with

government" (*Webster's New World Dictionary*, 1988). Stated in more detail, according to Lane (1963), the central questions of political science include:

> How shall government power be organized so as to achieve a "just" distribution of benefits? How can government be made both efficient and responsible, and what are the relationships between these two goals? What is the relationship between majority rule and minority rights? How do nations relate to one another, in what terms, through what channels, with what results? What is the nature of law, and how does law develop, how is it interpreted? (583–4).

Thus, political science attempts to understand how and why the processes of politics unfold as they do, with no special interest in understanding whether these principles apply to other domains of behavior. Indeed, political science concepts are often defined in ways that inhibit easy and direct applications to domains such as health or consumer behavior.

Of course, what distinguishes political psychology from other sorts of political science is that we explain political phenomena by taking a *psychological* perspective. We could instead, for example, adopt an economic perspective, attributing significant political events to economic forces, which are typically more easily observable via purchase behavior, interest rates, unemployment rates, and the like (e.g., Alt and Chrystal 1983). But instead, we place our emphasis on unobservable psychological processes unfolding in the minds of political actors. Thus, the modifier structure of "psychological political science," though more linguistically cumbersome, might more appropriately emphasize that we are doing a particular type of political science, to be distinguished from other types of political science.

In fact, two somewhat different forms of psychological political science seem to be practiced. The first ignores the psychological literature and focuses its explanations of political phenomena on psychological concepts. The second begins with a particular theory or finding in psychology and attempts to apply it to an analysis of political events. These two sorts of work have rarely yielded *new* psychological theories. Instead, they tend to apply existing, more general theories of the mind in the specific context of politics, or they develop a new set of psychologically tinged ideas so tailored to the political context that exporting them is difficult to envision.

To illustrate the first of these forms, consider Kinder's (1981) idea of sociotropic politics. The starting point for this work was the finding that presidential popularity shifts with the state of the national economy: as economic conditions improve and decline, so does presidential approval (Kernell 1978). The most popular interpretation of this result was the notion of "pocketbook voting" (e.g., Neustadt 1960). According to this perspective, citizens who are doing well financially support the president (to whom they partly attribute their own successes), and citizens suffering financially partly blame the president and consequently withhold their support. The more people are in the latter category, the lower a president's level of popular support supposedly falls. Thus, this view portrays the American citizen as focused on his or her own personal welfare, selfish rather than collectivist in orientation.

Kinder (1981) offered a very different interpretation, one that provided a more appealing portrait of ordinary Americans. According to his view, people distinguish between their personal economic circumstances and the economic state of the nation as a whole. And Kinder's data demonstrated that people base their presidential approval primarily on judgments of the national economy, not of their own personal circumstances. Thus, it seems, people want what is best for the country as a whole, not necessarily what is best for them personally.

This conclusion cast an optimistic light on an empirical relation that might otherwise have raised troubling questions about the motives of democratic citizens. And to rescue the promise of democracy in this small way, Kinder offered a psychological analysis. Yet he did so without citing a single theory or research finding from psychology. The distinction he drew between individually focused and collectively focused economic judgments was one developed specifically for this purpose. The finding that collective judgments are more consequential than self-focused judgments has not been cited especially often by mainstream psychologists since then, but it was very valuable indeed to political science.

The second sort of psychological political science I mentioned is illustrated by research by Lodge, McGraw, and Stroh (1989) on processes of candidate evaluation. Some years before Lodge and colleagues embarked on their enterprise, Hastie and Park (1986), two social psychologists, had published a paper in *Psychological Review* drawing the distinction between memory-based and on-line decision-making. Memory-based evaluation occurs when a person is prompted to make a new evaluation of an object, such as when a survey interviewer asks a respondent a question about which he or she has not previously formed an opinion. In this case, the respondent presumably digs into his or her memory to retrieve whatever relevant information would be useful to make this judgment and constructs it on the spot. In contrast, when on-line evaluation occurs, a person forms an overall evaluation of an object and updates it as each new piece of relevant information about the object is acquired in the course of daily life. So, when he or she is asked about this overall evaluation, it is simply retrieved from memory and reported. The distinction between memory-based and on-line evaluation was very helpful for a number of reasons, by making sense of apparently contradictory results regarding the relation of judgments to recollections.[1]

Lodge et al. (1989) built a bridge from Hastie and Park's general theoretical assertions to the world of politics. Evaluation processes apparent in psychologists' decision-making studies in the laboratory should also be apparent in the thinking of voters operating in the real world, said Lodge et al. (1989). Their study was a contrived experiment as well, rather than an investigation of a naturally occurring election. But it did involve uniquely political stimuli, uniquely political judgments, and a cross section of adults (rather than the typical college student sample).

Lodge et al.'s (1989) study challenged the reigning presumption in political science that voters evaluate candidates in a memory-based fashion (Kelley 1983) and offered compelling evidence that voters are at least sometimes on-line evaluators instead. Their work also motivated subsequent research by others that has made further use of this conceptual distinction and has offered additional evidence in support of the on-line view of voters (e.g., Rahn, Krosnick, and Breuning 1994). And Lodge et al.'s (1989) article illustrates a very common form of psychological political science: taking an existing psychological theory or idea and applying it to understand a political phenomenon.

Many other examples of this sort of psychological political science have appeared over the years. For example, Converse's (1964) landmark work on belief systems grew directly out of reigning psychological theories of cognitive consistency (Festinger 1957; Heider 1958). Zaller's (1993) work on the diffusion of political attitudes was inspired by McGuire's (1968) two-factor notion of exposure and acceptance as regulators of attitude change. And numerous studies of voters' perceptions of political candidates' stands on issues have brought to bear psychological theories of cognitive consistency (Kinder 1978), social judgment (Granberg and Brent 1974), schemas (Conover and Feldman 1986), expression structures (Ottati, Fishbein, and Middlestadt 1988), and more. In all of this latter work, the goal has been to understand the striking diversity among citizens in their perceptions of a single object (i.e., a candidate). And to do so, preexisting, well-developed psychological theories were used to derive predictions that could then be tested in the political context, with the primary goal of understanding *that* context more fully.

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[1] The seeds of this distinction were quite apparent in a number of earlier psychological works. For example, Lingle and Ostrom (1979) had developed the idea of forming a judgment that is retained in memory while the ingredients are lost, which is the very notion of on-line evaluation. Nonetheless, Hastie and Park's (1986) article was a cogent and compelling statement of the distinction.

Table 6.1. *Enrollment in the Ohio State University Summer Institute in Political Psychology by Discipline*

| Discipline | Year |  |  |  |  |  |  |
|---|---|---|---|---|---|---|---|
|  | 1991 | 1992 | 1993 | 1994 | 1995 | 1996 | 1997 |
| Political science | 38 | 36 | 33 | 33 | 45 | 29 | 33 |
| Psychology | 14 | 11 | 18 | 11 | 18 | 11 | 4 |
| Other | 11 | 2 | 9 | 1 | 1 | 6 | 2 |

## ASYMMETRIC INTEREST IN THE TWO APPROACHES

During the last couple of decades, these forms of psychological political science have dominated political psychology true to its name. That is, political science and political scientists have placed much more value on political psychology than have psychologists. This is revealed by a variety of different indicators, one of which is enrollment rates in the Ohio State University Summer Institute in Political Psychology, a one-month intensive training program we have run for the past seven years. Each summer, we host about fifty participants, mostly graduate students from as many as fifteen different countries, all interested in learning about political psychology. And in each of the first seven years, enrollment has been strikingly out of balance (see Table 6.1). We hosted more political scientists (presumably interested in doing psychological political science) than psychologists (interested in doing political psychology) by a wide margin.

Greater interest in psychological political science is also apparent in hiring practices at major research universities. Political science departments have often hired faculty members with Ph.D.s in psychology, including Donald Kinder (initially at Yale University and now at the University of Michigan), Margaret Hermann and Tom Nelson (at Ohio State University), Rick Lau (initially at Carnegie-Mellon University and now at Rutgers), Victor Ottati and Leoni Huddie (formerly and currently at the State University of New York at Stony Brook, respectively), and Kathleen McGraw (initially at the State University of New York at Stoney Brook and now at Ohio State). But it is much more difficult to think of individuals with primary appointments on psychology faculties whose Ph.D.s are in political science. So the asymmetry apparent in our Summer Institute participants is apparent as well in their faculty mentors' departmental affiliations. Thus, one could view these affiliations as reinforcing the notion that psychology has much to offer political science, but political science has little if anything to offer psychology.

This asymmetry is reflected in journal publication patterns as well. It is not uncommon at all for psychology Ph.D.s to publish articles in political science journals. And indeed, the creative application of psychological notions in the study of politics often gets significant and prominent attention, as evidenced by prominent publications in the *American Political Science Review* in recent years. In contrast, it is much harder to come up with prominent publications in major psychology journals written solely by political science Ph.D.s. One notable exception is John Zaller, who has published work on attitude change in the *Journal of Personality and Social Psychology* and in *Social Cognition*. But generally, journal publication patterns also suggest that psychology seems not especially interested in political science, and certainly much less so than political science is interested in psychology.

This portrait is further reinforced when one examines the book publishing area. A growing number of publishers are producing books on the psychology of politics, some explicitly titled as such and others clearly addressing relevant subject matter without the label. Although there are a few examples of such work being handled by psychology editors at the presses and marketed primarily to psychologists (e.g., Milburn 1991), it is far more common for them to be handled by political science editors and marketed accordingly (e.g., Cambridge University Press, the University of Chicago Press, the University of Michigan Press). Thus, it seems that political psychology again falls under the disciplinary umbrella of political science much more often than under psychology.

If anything, this perspective is likely to be reinforced still further by a smattering of recent publications in social psychology on the value of basic psychological research for understanding "applied," real-world phenomena. In the face of shrinking federal budget allocations for basic psychological research and rising skepticism about the value of such research (in contrast with basic research in chemistry and physics), social psychologists seem to feel the need to demonstrate that their work is valuable for helping to solve significant social problems.

For example, a book edited by Ruble, Costanzo, and Oliveri (1992) offers numerous chapters illustrating how basic social psychological theories (regarding, e.g., social cognition and attitudes) can be useful for understanding mental health. Fazio (1990) edited a special issue of *Personality and Social Psychology Bulletin* entitled "Illustrating the Value of Basic Research." It contained articles addressing aggression among children, depression, jury decision making, eyewitness memory, desegregation, energy conservation, acquired immune deficiency syndrome (AIDS), and other phenomena. More recently, Mark Snyder's (1993) presidential address to the Society for Personality and Social

Psychology illustrated how basic personality and motivational theories can be useful for understanding when and why people volunteer to help others. As psychologists make the case that their research has valuable potential for application in general, this campaign will undoubtedly reinforce the notion that political science in particular stands to benefit from careful attention to the literature of psychology. Yet the notion that psychology can benefit from exporting its insights and reading the literatures of other fields is rarely expressed.

## REASONS TO DO POLITICAL PSYCHOLOGY TRUE TO ITS NAME

I want to challenge this presumption: to suggest that complementing psychological political science should be a genuine political psychology that is true to its name. But why, one might ask, should a discipline focus intensely on a single context (i.e., the political one) if the goal is to describe cross-context consistencies? As counterintuitive as this may seem, there are at least three reasons why explicitly context-focused research has the potential for great payoffs for psychology. The first reason grows from the observation that there is in fact no such thing as context-free psychological research.

Although psychologists might hope to set up novel and unfamiliar information environments in their laboratories to study "basic" psychological processes as they occur *across* social contexts, it is impossible to prevent people from bringing to these situations the experiences they have had in the outside world. And in doing so, experimental subjects diagnose the sort of situation they face in the laboratory and draw on their previous experiences with similar situations in order to decide how to think and act. Therefore, every laboratory finding seems likely to be conditional, specific to the nature of the context involved.

A few years ago, my colleagues and I produced one illustration of this notion (Krosnick, Li, and Lehman 1990). Our focus in this investigation was Kahneman and Tversky's (1973) famous demonstration that people underutilize base-rate information and overweight individuating, vivid, case-based information when making social judgments. In particular, these scholars showed that when laboratory subjects were asked how likely a randomly selected man was to be a lawyer or an engineer, people overweighted the implications of a brief personality sketch of him and underweighted the proportions of lawyers and engineers in the sample from which he was selected.

We demonstrated that this occurred because the base rate was presented to subjects before the personality sketch. Based on the norms of conventional conversation in everyday life, subjects presumed that the information presented last by the experimenter was the information he or she considered most informative. Although Kahneman and Tversky (1973) had presumed that subjects would simply take in the information and make a judgment with it, regardless of presentation order, subjects interpreted the incoming flow of information in light of their previous real-world experiences and yielded a result that was misinterpreted as being a general tendency in human social judgment.

If the objects and interactions being explored in a psychological study are inherently political in nature (as in the numerous attitude change studies that have focused on political objects, such as politicians and controversial social policies), it would be narrow-minded for a researcher not to consider the role played by subjects' previous real-world experiences with such phenomena. To ignore those experiences would be to attempt to understand what happened in the laboratory without attributing proper responsibility to these prior experiences. Therefore, understanding politics (by consulting the political science literature when it touches on relevant topics) will allow one to interpret and perhaps even to design psychological laboratory experiments more effectively and to avoid attributing outcomes to the wrong causes.

But even more importantly, careful attention to the political context can help psychologists, because many of them study explicitly political phenomena, not merely psychological processes in a political context. For example, consider the following core topics in social psychology: aggression, altruism, intergroup relations, stereotyping, prejudice, cooperation and competition, and group decision making. These topics are all clearly, inherently, and unavoidably political and have been studied extensively by political scientists. In fact, their political nature is most obvious when considered at the level of international relations: nearly all the interesting questions about how nations interact with one another involve each of these processes: aggression, altruism, intergroup relations, stereotyping, prejudice, cooperation and competition, and group decision making. And to discuss these processes at an intergroup or an interindividual level is to discuss politics.

Yet remarkably, when one reads any of the numerous introductory social psychology textbooks on the market these days, citations of research done by political scientists on these topics are essentially invisible. Although a great deal of work has been done examining just these "psychological" processes and phenomena by political scientists, psychologists seem either unaware of or uninterested in it. I believe that psychologists can do our business better by becoming acquainted with studies that have explored precisely the same topics that are of interest to us in other disciplines.

Most important of all, however, there is an even greater potential value of political psychology true to its name for psychology as a whole, even for psychologists who are not necessarily studying inherently political phenomena: careful attention to the political context will help to inspire new directions for theory development. The idea here is that thinking about real political events will push psychologists to develop new constructs, to focus on new variables, and especially to identify new interactions: conditions under which effects occur, and classes of people among which effects are most likely to occur.

To illustrate this notion, consider the basic work in psychology on priming. Early studies of this phenomenon demonstrated that if people were exposed to words signifying a particular attribute (e.g., hostility), they were subsequently likely to interpret ambiguous behavior by a target person as being consistent with the attribute. So, for example, exposure to words like "nasty" and "rude" increased the likelihood that subsequently observed actions would be interpreted as hostile. The presumption here was that the initial exposure made the concept of hostility more accessible in memory and thereby more likely to be used in subsequent thinking.

Once a psychologist has identified a process like this, where does he or she go from there in theory development? Some factions of psychology tend to presume that the phenomenon demonstrated holds in all situations for all people until proven otherwise. Therefore, part of the value of a new psychological theory or insight is its generality – the fact that it presumably occurs everywhere for everyone. For example, Bargh, Chaiken, Govender, and Pratto (1992) asserted that automatic activation processes like priming are quite general. Given the appropriate stimulus, they said, a priming effect will appear in essentially everyone, because these effects are automatic results of the structure of knowledge in memory (see also Bargh and Pietromonaco 1982). But most psychological effects are unlikely to occur identically for all people in all situations.

Why would this implicit view of generality be so prevalent among psychologists? Undoubtedly, some of the basis for this presumption is psychologists' reliance on college sophomores as research subjects and laboratory settings. To acknowledge that different sorts of people may be more or less susceptible to a certain effect is to acknowledge that a very homogeneous, limited subject population may be inadequate for studying it. So psychologists may be drawn to the view that their effects are quite general as a way to feel comfortable with their anything-but-representative subject populations. Similarly, it is quite conceivable that

effects observed in contrived laboratory settings might not occur in the more rich and complex real-world settings in which significant social behaviors occur. To acknowledge the possibility that situational factors may turn on or off the effects that are observed in the laboratory may also threaten the apparent value of the work.

These two hallmarks of psychological research (i.e., collecting data from college sophomores in laboratory settings) are not fatal flaws in the approach. Every research method has its potential weak spots, whether it is a survey study or a content analysis or a participant observation. Laboratory studies, of course, have the unique virtue of being able to demonstrate causality compellingly. Thus, when conducted in a contrived setting with college sophomore subjects, such a study can demonstrate that a causal effect *can* happen under those particular circumstances and among those particular people. Then, subsequent studies could presumably attempt to pursue those conditions and demonstrate the generalizability of the result.

Furthermore, science will almost certainly progress more quickly and efficiently if we place the burden of proof on the critic of laboratory studies rather than on the investigator in one particular sense. It is very easy for critics to charge that all laboratory studies are uninformative because of limited subject populations and artificial circumstances. Similarly, it is easy for qualitative researchers to reject all traditional survey research out of hand because of its tendency to place respondents in artificially structured categories and prevent people from expressing the richness and fullness of their views in their own words, including essential qualifications and elaborations. And yet because every research method is subject to such criticisms, the fields of psychology and political science alike will be left with no believed evidence at all if we accept this blanket rejecting approach.

Instead, we should embrace all research methods as potentially informative, and each set of results should be subjected to careful scrutiny, in an attempt to identify plausible alternative processes that may be responsible for the observed results. And if such processes can be identified, one should be appropriately skeptical of the observed results. But in the absence of such plausible alternative mechanisms for observed results, we are best off accepting the results from laboratory and survey studies alike as useful elements in the progress of science. But because many psychologists have elevated the laboratory experiment to the level of "most desirable method," they are likely to be invested in the belief that their findings are quite generalizable.

There is another, more interesting and significant reason for psychologists' view regarding generalizability as well: it is hard to identify interacting variables within a context-free framework. Consider, for example, the priming case: What variables should interact with priming stimuli? In what contexts should priming be most effective? Among which sorts of people should it be most powerful? These are not easy questions to answer in the abstract. So it is no surprise that most of the initial psychological research on priming presumed that it was quite a general phenomenon across people and situations.

To identify regulating variables, it is useful to refer to specific real-world social contexts, and this is where the literature of political science and the political context more generally have something to offer psychologists. It is much easier to begin by selecting such a context (e.g., the case of news media coverage of issues and its impact on presidential evaluations) and then speculate about when and among whom the effect should be most pronounced. With the context in hand, one can phrase one's hypotheses about interactions in ways that are pan-contextual. But the inspiration for the idea comes from the inherent parameters of the context itself, as I will illustrate with a review of politically inspired research on priming. As it will illustrate, a benefit of doing political psychology true to its name is that it may help psychologists to expand their theories by identifying more, and more interesting, interactions.

Paying close attention to political contexts helps one to spot new potential causal processes as well. Certainly, if we begin an investigation focused on a theoretically derived effect of an independent variable on a dependent variable (as psychologists tend to do), our attention is likely to turn first to variables likely to interact with the independent variable. But as we continue to think about a rich social context, we are likely to recognize other independent variables that are also likely to influence the dependent variable of interest. Thus, the potential value of political psychology is not limited to interactions, though it may initially begin that way for any given investigation.

These benefits of political psychology cannot accrue if one remains at the surface of political science. In order for truly new ideas to emerge in psychological research as the result of attention to the political context, a theorist must become immersed in that context. An afternoon's reading of a few key political science references (or even a few days' reading) is at best likely to function as a Rorschach inkblot, providing an opportunity for the psychologist to see what he or she already believes and is comfortable identifying and understanding. Only when one gets deeply into the literature of political science can one truly adopt a new perspective thoroughly enough to push theory in truly new directions. In this sense, I am suggesting that the enterprise of psychology should become more politically sophisticated in order to permit more effective political psychology true to its name.

Is the political context a particularly valuable one for psychological research? Is there some reason why doing political psychology will be more useful to psychology than doing consumer psychology or health psychology? I suspect not. Each domain of behavior is likely to be rich and interesting, and different domains may be useful for investigation of psychological processes in different ways. But I see no reason why the political arena is better or richer or more interesting than any other context in which social cognition and action unfold. I am suggesting the value of context-specific investigation in general, and the political context is clearly no less interesting or potentially fruitful than these others.

## ILLUSTRATIONS OF POLITICAL PSYCHOLOGY TRUE TO ITS NAME

In an effort to complement the strong interest in psychology among political scientists, I want to encourage political scientists and psychologists alike to see that psychology has much to gain from careful attention to political science. And to do so, I will point to a few examples illustrating how research in political psychology has yielded useful payoffs for psychology in the pursuit of its core goals. These examples are from my own work, because they are the ones I know best and can discuss most easily. But the work of others could easily be substituted. My intention here is simply to illustrate the types of benefits that can be accrued from political psychology true to its name.

Interestingly, the three examples of work I will discuss are cases in which we set out to do psychological political science – to apply empirically validated psychological theories to understand a political phenomenon. And in each case, we presumed that our hypotheses would be confirmed, an expectation that was met in some cases and not in others. But in each instance, a useful yield for psychology as a discipline was produced.

### *Branching and Labeling in Political Attitude Measurement*

The first and simplest example involves attitude measurement (Krosnick and Berent 1993). Our investigation was inspired by the predominant view in the political behavior literature, offered especially clearly in *The American Voter* (Campbell, Converse, Miller, and Stokes 1960), that American citizens' identifications with political parties are highly crystallized and are powerful determinants of people's political decisions, whereas their attitudes on matters of government policy are highly flexible and relatively inconsequential. This view has been supported in numerous investigations that documented the superiority of party identification measures over policy attitude measures in terms of over-time consistency and in terms of correlations with criterion judgments such as candidate preferences in elections.

Although all this seemed quite reasonable to us, we had also been aware of a methodological confound inherent in nearly all prior investigations of these matters. Most past studies had relied on the National Election Study (NES) surveys, and many of the other studies in this literature had collected original data using questions taken from the NES. And in the NES, the two sorts of variables of interest had been measured in notably different ways.

Party identification was measured by branching respondents through a sequence of questions involving fully verbally labeled response options. All respondents were asked initially whether they considered themselves a Republican, a Democrat, an independent, and so on. Then, people who reported identifying with a party were asked whether they did so strongly or weakly. And self-proclaimed independents were asked whether they leaned toward one party or the other. In contrast, policy attitudes were measured in one step by asking respondents to place themselves on a 7-point scale, with verbal labels on only the endpoints.

It seemed to us that two differences between these measurement approaches might have contributed to the relatively low apparent stability and consequentiality of policy attitudes. First, decomposing the attitude-reporting task into two component parts, one reporting direction and the other reporting extremity, might reduce the cognitive demands of the task and might improve precision, especially among respondents for whom the cognitive demands of the task were most burdensome. Second, it seemed that simply numbering some attitude-rating scale points leaves their meaning relatively ambiguous and requires that respondents infer the meanings of those points. Providing verbal labels to all respondents for all scale points seemed to us again to reduce the cognitive burden of the task and thereby possibly to improve the precision of attitude reports.

Some previous studies in psychology suggested that verbal labeling enhances measurement reliability (Peters and McCormick 1966), though other such studies failed to document this relation (Finn 1972). No previous psychological studies had examined whether decomposition could improve the reliability of attitude reports. However, a variety of studies had illustrated how decomposition could help people to answer quiz questions such as "How many families were living in the United States in 1970?" more accurately (see, e.g., Armstrong, Denniston, and Gordon 1975; Einhorn 1972).

To test these ideas with regard to the measurement of political attitudes, we conducted eight experiments, two in the laboratory and six in typical survey settings. Some of our studies involved face-to-face interviewing, some involved telephone interviews, and some involved self-administered questionnaires. Some studies involved college student subjects, and others involved regional or national general population samples. Across all these instances, we found support for our two hypotheses. Furthermore, our general population studies indicated that branching and labeling are the greatest help to respondents with the least amount of formal education, whose cognitive ability at abstract manipulation of verbal concepts was presumably relatively low on average.

This finding seems clearly useful to political science. A widely accepted truism and a central pillar of the American politics literature was discredited, at least a bit, by this evidence. Our findings did not completely refute this central idea, because the gap between party identification and policy preferences in terms of stability and consequentiality remained even after we equated the measurement approaches. But what had initially appeared to be a very large gap was significantly reduced by this action.

Thus, our work on this topic was clearly "psychological political science," because it was a psychologically informed investigation of a political question that yielded a useful new insight for political science. Furthermore, this work set an example by illustrating the need for careful attention to measurement approaches (for another example, see Green 1988). Perhaps other findings in political science will be similarly reinterpreted when measurement problems are solved, and thus the field will progress forward.

At the same time, our study of branching and labeling is valuable for psychology as well. Our demonstration regarding decomposition helps to establish the validity of this notion by documenting it in a new domain: attitude measurement. And we demonstrated that decomposition is especially important among a subset of people: those for whom effortful cognitive exercises are the most difficult. Furthermore, our evidence documents a general principle for attitude measurement that is useful for researchers across the social sciences: that data quality can be enhanced via sequences of fully verbally labeled branching questions measuring attitude direction and extremity separately. Thus, our work appears to have been political psychology at the same time that it was psychological political science.

## *News Media Priming*

The next example again began explicitly as psychological political science and ended up yielding surprising insights for psychology. When

Shanto Iyengar, Don Kinder, and I began our work on news media priming, we borrowed the term and the notion of "priming" from psychology. Our inspiration came from research done in psychological laboratories demonstrating that activating a body of knowledge in a person's memory led that knowledge to have enhanced impact on subsequent relevant judgments (Higgins and King 1981; Wyer and Hartwick 1980). This work, as well as other related laboratory studies in psychology, contributed to the rising popularity of an associative network model of memory processes (Anderson 1983; Collins and Loftus 1975). Within this conceptual framework, activating any given node in memory yields spreading activation to other linked nodes, which enhances the likelihood that they will have an impact on relevant judgments as well.

It seemed to us relatively straightforward to apply these notions in an analysis of news media effects on political cognition. By repeatedly addressing a certain issue day after day, the news media seemed capable of activating the body of relevant knowledge in viewers' memories, thereby making that knowledge more accessible and causing it to have greater impact on a variety of relevant political judgments, such as evaluations of a president's job performance. This hypothesis offered the promise of a new understanding of the types of effects the news media could have on the conduct of politics: by focusing on some issues and not others, we suspected, the news media might shape the public's approval of the president and thereby determine in part his ability to shape policy making in Washington.

We conducted a series of studies testing the viability of this idea, initially in the laboratory and later in real-world settings via surveys (for a review, see Miller and Krosnick 1996). Our first studies exposed laboratory subjects (general population samples in some cases and college students in others) to different sets of television news stories, varying across individuals the extent to which particular issues received attention. And as we expected, greater coverage of an issue led subjects to base their overall evaluations of the U.S. president's overall job performance more on his handling of that issue (see Iyengar, Kinder, Peters, and Krosnick 1984).

Later survey studies showed similarly that when news coverage of a particular issue increased, the weight citizens attached to that issue in forming presidential evaluations also increased. Specifically, we showed first that news coverage of the Iran/Contra affair increased the weight attached to President Reagan's handling of relations with Central America (Krosnick and Kinder 1990). And subsequently, we demonstrated that news coverage of the 1991 Gulf War increased the weight attached to President Bush's handling of it (Krosnick and Brannon 1993).

These findings, in and of themselves, were not groundbreaking for psychology, despite the fact that the initial work was published in the *Journal of Personality and Social Psychology*. Psychologists could legitimately have responded to the work by saying: "Of course! Why *wouldn't* our priming effects replicate?" Yet it was nonetheless useful to demonstrate first that the effects could be observed in the laboratory with more real-world-like stimuli (i.e., television news programs) and judgments (of presidential performance) that were likely to be based significantly on information obtained previously, outside the laboratory. And second, it was useful to demonstrate that real-world shifts in news media coverage were associated with shifts in the bases of citizens' consequential political judgments. Of course, this latter evidence alone does not definitively document the causal process(es) responsible for the observed associations. But coupled with the laboratory studies that did demonstrate the causal impact of news media coverage, the survey studies seem informative about media influence as it occurs in the course of ordinary daily life. And the survey studies demonstrated the social significance of an effect that could conceivably have been limited to the laboratory setting, with its relatively impoverished judgment conditions and the experimental demand characteristics that could conceivably have lurked within.

The more useful contribution to psychology, however, came as we explored interactions in this work. We began by speculating that resistance to priming effects might be a function of political expertise. This suspicion was not driven by any explicit research on expertise and priming within psychology. In fact, the psychological literature in the mid-1980s did not point to any interacting variables that might regulate priming effects. It seemed taken for granted that notions such as "spreading activation" were relatively universal processes that were inevitable results of the architecture of knowledge structures in memory.

We suspected instead that political experts might be especially resistant to priming effects for a variety of reasons. Principally, individuals with little knowledge have only a minimal ability to see flaws or distortions in new information, and they have few other bases from which to derive political judgments. And just as we anticipated, our laboratory studies revealed that priming effects were strongest among political novices, a result that was replicated in our first survey study, of the Iran/Contra affair (Krosnick and Kinder 1990).

But when we set out to do the Gulf War study, it seemed that simply demonstrating the priming effect and the expertise interaction again would not be especially valuable. We therefore decided to pursue an idea that had been lurking in the backs of our minds for some time, inspired by the writings of McGuire (1968), Converse (1962), and Zaller (1987, 1989). The idea was to differentiate factors that regulate the dosage of media coverage a person receives from factors that regulate his or her resistance to the media's influence.

In our previous studies, we had thought of expertise as a basis for resistance, and we had identified political experts by their performance on political knowledge quizzes (Iyengar et al. 1984; Krosnick and Kinder 1990). When asked a series of open-ended or multiple-choice questions about factual matters, some individuals provided much more accurate information than others, and we labeled these two groups "political experts" and "novices," respectively. But at the same time, we had always been aware that in the world outside the laboratory, political knowledge is positively correlated with two other relevant factors: interest in politics and exposure to political information through the news media (e.g., Krosnick and Milburn 1990).

In designing our Gulf War study, it seemed useful to think of these two factors as related to dosage: the more a person was exposed to political news, and the more he or she was interested in it, the stronger the priming manipulation and the more pronounced the effect should be. Because interest and exposure are positively correlated with knowledge and yet were expected to have oppositely signed effects on the magnitude of priming, we suspected that a multivariate analysis looking at the effects of all three factors simultaneously might eliminate suppression and thereby reveal an even stronger effect of knowledge than we had thus far observed.

In fact, our results were quite the opposite (see Krosnick and Brannon 1993). We found plenty of effects of knowledge, interest, and exposure, but they each regulated priming in ways precisely opposite to what we had anticipated. Rather than high knowledge being associated with resistance, high knowledge *enhanced* priming effects when all other variables were controlled. And rather than high levels of exposure and interest strengthening priming, it was actually weakened by them, controlling for all other variables. Interestingly, when we returned to the Iran/Contra data and repeated the same multivariate analyses, we found the same pattern of relations, thus suggesting that it was not specific to the Gulf War context.

These findings were quite startling to us, yet they make sense in light of the entire literature on cognitive social psychology (see Krosnick and Brannon 1993). First, consider knowledge. Although we originally thought of knowledge as a basis for counterarguing or scrutinizing incoming information, our results are consistent with two alternative possibilities instead. First, more knowledge could be associated with more elaborate storage structures in memory, which would allow

priming stimuli to be remembered better and thereby to have effects over longer periods of time. Alternatively, more structured knowledge in memory means that more links exist among knowledge bits, which would therefore facilitate more spreading activation. Among people with few political knowledge structures, there is little stored in memory that news coverage of an issue could activate, so there would be no cognitive mechanism by which any effect could occur.

With regard to interest and exposure, our results made sense in light of notions involving cognitive overload and interference. Even people with low levels of exposure and interest probably can't help but receive the big message of the news media, which was the Iran/Contra affair in late 1985 and the Gulf War in early 1991. But people with higher levels of exposure and interest undoubtedly get those big messages plus many other little messages, which may have diluted the priming effect regarding the big issue. Thus, the cacophony of messages that comes to any highly attentive television news viewer or newspaper reader over a period of days may actually reduce the impact of coverage of any single issue.

Another possible explanation for the interest effect we observed is suggested by recent psychological studies of priming. Priming manipulations in the laboratory have the greatest impact when they occur without people paying much attention to them (Lombardi, Higgins, and Bargh 1987; Strack, Schwarz, Bless, Kubler, and Wanke 1993). When people attend closely to and are aware of the potential impact of context on their judgments, they correct for it. In our case, highly interested readers and viewers are likely to have noted the content of the news they received, and might therefore have adjusted the weights they attached to issues in order to correct for any news media impact. In contrast, less interested readers and viewers probably absorbed news media content without much awareness, so they were unlikely to make the correction. This would then have yielded larger priming effects among these individuals, just as we observed.

The hypotheses that knowledge structures might facilitate priming and that multiple priming manipulations might dilute any given priming effect are not especially radical ones for psychology. They can easily be accommodated within the reigning conceptual perspectives used to understand priming effects generally. But they are hypotheses that have not yet been carefully addressed in that literature to date. Perhaps the biggest payoff of this research will be as psychological political science, in terms of an understanding of which citizens are most influenced by the news media and why. But there is also a potential payoff as political psychology true to its name, in terms of the evolution of basic priming theory.

## Attitude Importance and Policy Issues

The last example I will describe also began with the widely accepted notion that policy attitudes are relatively peripheral to American citizens' thinking about politics. In the view of political theorists, this idea challenged the viability of democratic governments because it called into question the motives and/or capabilities of democratic electorates (Dahl 1956; Pennock 1979). Yet stashed at the end of Phil Converse's (1964) monumental chapter on nonattitudes was a compelling hypothesis that, at its core, stood to rescue the apparent competence of democratic citizens. Rather than attending to the entire array of policy issues facing the nation, and forming crystallized and consequential attitudes on each one, asserted Converse, the ordinary citizen focuses on just the handful of issues that touch his or her life most directly. And in doing so, his or her attitudes on those issues become firmly anchored within cognitive structures and have significant consequences for political thought and action. In this sense, each citizen presumably falls into one or more "issue publics," groups of people passionately concerned about those particular issues.

As compelling as these ideas were conceptually, they had rarely been subjected to empirical scrutiny as of the early 1980s. No doubt, an important part of the challenge here was that Converse provided no clear guidelines on how to identify members of any given issue public. He alluded vaguely to having used open-ended survey questions to identify the issues about which survey respondents were most concerned. Yet Converse (1964) did not report his findings in detail, and later studies that used the same approach failed to produce evidence that issue public members (as gauged via open-ended questions) did indeed have more crystallized and consequential attitudes (e.g., Maggiotto and Piereson 1978; Natchez and Bupp 1968).

When I began to address these questions, I chose to rely on a device of convenience: closed-ended rating questions asking NES survey respondents how important various issues were to them personally. My first moves were to assess whether issue public members (as identified in this way) did indeed have more crystallized attitudes and whether their attitudes did in fact have more impact on cognition and action. All this turned out to be true: highly important ratings were associated with greater attitude stability, greater ideological constraint of attitudes across issues, and greater impact of attitudes on candidate preferences and voting (see Boninger, Krosnick, Berent, and Fabrigar 1995; Krosnick 1990).

These initial findings had payoffs both for political science and for psychology. From the political science perspective, this evidence demonstrated the utility and validity of the issue public notion, and it suggested that policy issues were having more impact on election outcomes than individual regression coefficients in full-sample analyses of survey data sets had heretofore suggested. Thus, American voters appeared to be more "responsible," to borrow Key's (1966) term, and "rational," to borrow Goldberg's (1969), than had appeared to be the case.

For psychology, my initial work contributed to the emerging literature on attitude strength. As Raden (1985) illustrated in his extensive review article, various attributes of attitudes had been shown to be related to their strength (as gauged by stability and consequentiality). These attributes include intensity of feeling, the amount of knowledge one had about the object, the certainty with which the attitude was held, and many more. My findings on attitude importance added to this list and in some instances expanded the correlates of strength dimensions that had thus far been documented. But the more valuable contributions to psychology have come in our subsequent work. In it, we have explored the mechanisms for the basic effects of importance we initially demonstrated.

For example, consider one of our initial findings: that people who consider an issue more important perceive larger differences between competing candidates in terms of their stands on the issue (Krosnick 1988). People who aren't very concerned about an issue tend to see competitors as taking relatively similar, moderate stands. In contrast, people who attach more importance typically see the candidates as more extreme on opposite sides and therefore see them as quite distinct.

In thinking initially about the possible mechanisms of this effect, I speculated that it might be due to increased perceptual accuracy among issue public members. And consistent with this idea, subsequent survey analyses revealed that these individuals were more likely to know on which side of an issue a candidate stood (Krosnick 1990). We therefore set out to explore the cognitive mechanisms of this apparent accuracy.

In particular, we suspected that two possible processes might be at work (see Berent and Krosnick 1993a, 1993b). The first is "selective exposure." In using this term, we do not mean selective exposure to attitude-supportive information, as the dissonance theory–inspired literature on the concept initially did (Festinger 1957). Rather, we mean that people may selectively expose themselves to information on issues they care deeply about, thus giving them the raw information base with which to build accurate perceptions. Second, we suspected that attitude importance might inspire "selective elaboration" of attitude-relevant information. That is, people who care deeply about an issue may be especially likely to think deeply about information relevant to that issue, thereby leaving a strong, integrated trace of that information in memory that should be easy to retrieve later.

To test these ideas, we began by conducting a survey study in which we telephoned a regional sample of adults before and after a nationally televised debate during the 1988 U.S. presidential campaign. In the post-election interviews, we administered free recall and recognition measures gauging respondents' memory for specific statements made by the candidates the night before. In addition, we measured the personal importance that respondents attached to the various issues addressed. As expected, higher importance was associated with better memory.

This result could have been due to selective elaboration in part, but it could also have been due solely to selective exposure, whereby viewers might completely stop paying attention to the debate when it turned to issues they did not care about. We therefore attempted to replicate the same finding in our laboratory using videotaped presentations of debate excerpts. Doing so allowed us to hold exposure levels constant and to see whether a memory effect remained that could be attributable to selective elaboration. Subjects viewed a thirty-minute videotape containing excerpts of various presidential debates and addressing a range of issues, and they returned to the laboratory one day later to complete free recall and recognition memory measures and to report the importance of the issues involved. As expected, higher importance was associated with better memory.

We then conducted three studies designed to document more definitively the mechanisms of the effect. In these studies, all of our laboratory subjects read one-sentence statements supposedly made by a set of political candidates on an array of policy issues. However, subgroups of the subjects differed from one another in terms of the conditions under which they were exposed to the initial candidate statements. One day later, all subjects returned to the laboratory to complete memory and importance measures.

Among subjects who initially read the statements at whatever pace they preferred, the expected relation between importance and memory accuracy appeared. But among subjects who were forced to read the statements very quickly and who therefore had no time to elaborate on the information, the relation disappeared. Furthermore, giving these latter subjects extra time to think about a set of statements (after they could no longer read them) reinstated the importance–memory relation. And finally, labeling the statements to indicate the issue each one addressed reinstated the importance–memory relation among rushed subjects, presumably because they could selectively expose themselves to only those statements on issues about which they cared deeply and thereby elaborate upon their implications within the limited time span

allowed. In some supplementary studies, we have used more traditional laboratory paradigms for gauging selective exposure and have documented the expected relation between importance and this tendency.

Taken together, these recent findings do not have especially important implications for political science. From that discipline's perspective, simply knowing that higher importance is associated with relatively accurate candidate perceptions is enough to suggest that issue public members are equipped to be responsible and rational voters. Knowing precisely which cognitive mechanisms are responsible for this relation does not add a great deal of new understanding that would be of use in an analysis of political events.

But from the psychological viewpoint, our findings regarding mechanisms are indeed useful for the advancement of theory. We have shown for the first time how a dimension of attitude strength motivates information exposure and depth of processing in ways that shape memory for that information. In this sense, we have built a bridge between one dimension of strength (i.e., importance) and another (i.e., knowledge; see Wood, Rhodes, and Biek 1995), documenting how the former causes the latter. We have also helped to build the bridge between the literature on attitudes, in this case on attitude strength in particular, and the social cognition literature on memory.

In other work on attitude importance, we have investigated the relation of importance to various other psychological phenomena, and these studies yielded useful new insights for psychology as well. For example, in one study, we examined the "false consensus effect," whereby people tend to overestimate the prevalence of their own attitudes among others (Ross, Greene, and House 1977). The prevailing theoretical explanations of this effect presume that it occurs because people's attitudes shape their perceptions of others' attitudes (see, e.g., Marks and Miller 1987). If this is so, such effects would presumably be more powerful when attitudes are more personally important to individuals. Yet we found this not to be the case, thereby calling into question the prevailing views regarding the effect's mechanism (Fabrigar and Krosnick 1995) and lending support to some alternative possible explanations.

Another issue we have examined is the impact that small changes in question wording, format, or ordering have on people's reports of their attitudes. Many observers have taken for granted that these effects are concentrated primarily among people whose attitudes are weak and uncrystallized (Cantril 1944; Converse 1974; Gallup 1941:261). But in an investigation involving twenty-seven experiments conducted in the context of national surveys, we found this generally not to be the case (Krosnick and Schuman 1988). Again, this surprising result may push theory-building in the area of attitude measurement in new directions.

Our work on attitude importance began as psychological political science. The starting point was the evidence of apparently low stability of citizens' policy preferences and the apparently minimal impact of those preferences on their vote choices. In demonstrating the role of attitude importance in regulating these two processes, our contribution was initially to shed light on how contemporary American democracy operates. But in our more recent work, we have again shifted the focus to political psychology true to its name, with a primary interest in understanding how attitudes and information processing operate, regardless of implications for democratic theory.

This shift seemed natural when we made it, because our initial findings raised questions about the psychological mechanisms involved that prevailing theories could not readily answer. Now that we have generated plausible explanations, we are inclined to turn back to the agenda of psychological political science in doing research on attitude importance. That is, we are now planning studies that will attempt to demonstrate the political consequences of issue-public–driven decision making in elections.

It is useful to note that our work on attitude importance dovetails with our work on branching and labeling not only in terms of origins but in terms of implications as well. Like the branching/labeling work, our study of issue publics demonstrated that citizens' policy preferences have more impact on vote choices than previous studies had recognized. In this sense, incorporating measures of attitude importance into vote choice equations narrowed the gap between policy preferences and party identification in terms of predictive power, just as branching and labeling did. As more and more such steps are taken in future research, it is conceivable that the gap may disappear altogether. And that result would be a significant one indeed for our understanding of how democracies operate.

## CONCLUSION

Perhaps making a distinction between psychological political science and political psychology will lead researchers to be a bit more self-conscious about whether their work at any given moment can be considered either or both. Although nearly all the research being published with the label "political psychology" appears to be primarily psychological political science, some of this work may also be of great use for basic psychological theory-building. If we all make efforts to identify those components or aspects of our work and portray them explicitly as such whenever possible, our research can eventually be seen as contributing

both to political science and to psychology, and perhaps ultimately in even measure. Such a balancing would undoubtedly be highly desirable, because the support for our enterprise would then run wide and deep on both sides of the disciplinary fence.

Such a balancing would probably have a number of tangible effects, one of which is more cross-disciplinary publishing. At present, political psychologists within political science seem to publish in psychology journals only as a means of last resort to get their work in print. I have heard more than one story of a political scientist submitting an article to political science journals, only to have it rejected on the grounds that it is too psychological. Although there was plenty of psychology apparent in the work, the study seemed not to have made a useful and novel contribution to understanding politics. So the author's next inclination was to submit the article to a psychology journal on the assumption that if there is no contribution to political science, then perhaps the work contributes to psychology. Yet these papers are most often rejected by psychology journals as well, because the work was not initially designed to speak to the goals of psychology. To do so requires consciously and effortfully understanding the state of knowledge in that field and the directions in which future work could valuably move.

To do political psychology true to its name, and to contribute to both political science and psychology, are certainly not easy tasks. Trying to reframe an investigation after the fact to get it published somewhere is much more difficult than bearing in mind the goals of both disciplines from the start of a project or shifting one's goals as the data come in. Doing so, however, requires a mastery of both disciplines in ways that pose significant challenges.

One effective way to meet this challenge is for graduate students to take a good number of courses in the other discipline or to participate in unusual training opportunities such as the OSU Summer Institute in Political Psychology. Another, complementary approach is to form teams of political scientists and psychologists who work together and simultaneously shape and frame the work from the perspectives and value systems of both disciplines. The challenges to setting up cooperation and communication within such teams are no doubt significant, but the disciplinary payoffs are well worthwhile.

Whether an individual investigator strives to address both disciplines singlehandedly, or a multidisciplinary team works together to do so, it is important to recognize that the primary benefits of one's efforts cannot necessarily be mapped out in advance. Our work has moved through phases of contributing primarily to political science and primarily to psychology, largely in response to the findings of initial investigations.

Which direction is more fruitful at any given moment may not always be fully clear. And it will certainly not be clear long in advance. For this reason, interdisciplinary teams or individuals with joint departmental affiliations are probably best equipped to reap the biggest payoffs, because they will not shy away when the benefits of one's work begin to shift toward one discipline or the other.

With all this in mind, let's toast to a long and prosperous life for both political psychology and psychological political science. May the benefits of our enterprise serve the disciplinary goals of all!

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# 7

## *Political Psychology, Political Behavior, and Politics: Questions of Aggregation, Causal Distance, and Taste*

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The political beliefs, attitudes, and behaviors of ordinary citizens absorb a goodly share of the attention of modern political science. At a guess, they form the subject of as much as a quarter of the articles in the leading journals. Ours is a prosperous subdiscipline. Yet partly perhaps for that reason, partly because every subdiscipline occasions some ennui in other quarters, and partly for reasons we shall explore, it also has critics. Ignoring the egoistic ("it's boring") and the antiscientific (it deals in generalizations, worse yet in numbers), the most common complaint is that it is insufficiently political.

This is true even – especially? – when the objects of study are such familiar and psychologically macro variables as political sophistication, partisanship, and split-ticket voting – the domain of the field perhaps most commonly known as *political behavior*. It is also true when they are such less familiar and psychologically more micro variables as reliance on on-line versus memory-based processing, public responses to politicians' blame management strategies, and the use of likability heuristics to locate candidates on issues – the domain of current, cognitively influenced *political psychology*.

Needless to say, these fields overlap, as the oeuvres of this volume's contributors attest. As a matter of social rather than intellectual structure, political psychology is largely a subfield of political behavior: most political psychologists study mass politics. Perhaps intellectually it should be the other way around, with political behavior (and much of the rest of political science) regarded as applied psychology. But, for purposes of this chapter, I shall treat "political psychology," with some admitted injustice, as referring to the study of mass politics.

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