Introduction: Political Psychology and the Study of Politics

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Fields of scientific inquiry follow a common pattern. At the outset, excitement and enthusiasm prevail as a small group of founders offers a new conceptual framework and, usually, a new, related methodology. Sometimes the specific topics of inquiry are also new, at other times only the ways to think about them. Other, often young, scholars adopt the new perspective, and before long it becomes an active, visible part of the discipline. Typically, this very growth in prominence portends the beginning of a leveling off, if not decline, in research activity. Continuing scholarship takes the form of adding small increments of knowledge to the key central questions that the founders had posed much earlier.

Often missing from this sequence is a self-evaluation by the practitioners themselves. Concerned, as they should be, with substantive questions, the researchers don't stop to scrutinize what they do and how it fits into the larger discipline of which they are part. The criticisms usually come from elsewhere and consequently tend to undercut rather than strengthen the field.

In this volume, political psychologists take a hard look at political psychology. They pose, and then address, the kinds of tough questions that those outside of the field would be inclined to ask and those inside should satisfactorily be able to answer. Not everyone will agree with the answers the authors provide, and, in some cases, the best an author can do is offer well-grounded speculations. Nonetheless, the chapters raise questions that, if taken seriously, will lead to an improved political psychology.

But, one might protest, the idea of political psychologists evaluating political psychology is equivalent to the idea of police officers monitoring their own department. In both cases, the conclusions are foreordained, such that the scientific field in one case and the department in the other will be evaluated more positively than it should be. It is indeed true that most of the chapters that follow find an important role for political psychology in the larger discipline. It is also true that these same chapters set forth hard-hitting criticisms and formidable challenges for future research. Moreover, the expectation is that this volume will generate further commentary, not stifle it.

The individual chapters are organized around four themes. The remainder of this introduction delineates these themes and briefly summarizes the individual contributions.

DEFINING POLITICAL PSYCHOLOGY

Fields of scientific inquiry should be definable. Sullivan, Rahn, and Rudolph offer a tour de force of political psychology, and in the process show how diverse and ill-defined the field is. Political psychologists, unlike, say, students of rational choice, do not share a single set of assumptions or even a general perspective. As Sullivan, Rahn, and Rudolph state in their opening paragraph, the field "includes – has always included – a wide diversity of theories, approaches, quantitative and qualitative research methods, and verdicts." They identify three distinct eras – the first dominated by studies of personality, the second by attitude theory and change, and the third by human cognition and information processing – and note that today all three perspectives are a part of what is normally called political psychology.

The diversity in fact is even wider and deeper. Even within the currently dominant information-processing perspective, which is the focus of this volume, scholars examine a variety of cognitive processes ranging from attribution to cognitive heuristics to on-line processing. Researchers have also begun to ascertain how affect and emotions interact with cognition to shape political judgments.

So what, exactly, is political psychology? A simple answer is also a pretty good one: the study of mental processes that underlie political judgments and decision making. Because there are many mental processes, and to date no general framework that integrates them, a political psychologist can – must? – focus on those that seem most applicable to the political task people are facing. This freedom to pick and choose is both a plus and a minus. On the one hand, the wide range of perspectives on political decision making can be compared and contrasted within the academic marketplace. In time, presumably, the strongest and most beneficial will prevail. On the other hand, the accumulation of agreed-upon evidence is slow and, at worst, could not occur at all.

Sullivan, Rahn, and Rudolph also note that political psychologists have overwhelmingly used psychoanalysis to study elites and information processing to study citizens. In principle, this dichotomy need not and should not exist. That it does reflects the extreme difficulty of gaining access to public officials for the purpose of conducting the kinds of surveys and experiments that are at the core of the information processing perspective. Unfortunately, the dichotomy precludes systematic comparisons of the two groups. One outstanding question, for example, is whether elected officials and other political activists make the same kinds of errors – anchoring, overconfidence, and so on – that ordinary citizens make. Or do the institutional settings in which they function reduce such errors? Lacking equivalent experiments across the two groups, we really cannot say. To assume that the latter make better political judgments is understandable, but it might not be right.

Similarly, political psychologists have not been inclined toward cross-national analysis. This is an opportunity lost. Differences in political structures – presidential versus parliamentary systems, two-party versus multiparty systems, and so on – are natural manipulations that facilitate examining how structures affect and interact with individual decision making.

THEORY AND CONTEXT

A typical psychological study entails formulating a hypothesis and then testing it experimentally (political psychologists often substitute experimental surveys for the laboratory). Both Lupia and Conover and Searing find problems with this venerable approach to research, although for very different reasons.

Lupia argues for a closer relationship between formal theory and political psychology. In his words, "interactions between political psychologists and rational choice theorists can generate substantial *gains from trade*" (italics in the original). Political psychologists tend to use experiments to test hypotheses and make inferences. By design, experiments simplify the world by holding constant everything except those factors that interest the researcher. This very strength, the isolation of a cause-and-effect relationship, is also the experiment's primary vulnerability. It is a big inferential leap from an experiment to a much more complex political environment. Whether the experimental results hold in the real world is always open to question.

How, then, can researchers maintain the strength of experiments while at the same time increase confidence that the experimental results are externally valid? One answer might be to uncover similar relationships in the real world. As Lupia argues, however, the complexity of politics renders this a difficult if not impossible task; indeed, it is this complexity that motivates experimental work in the first place. Lupia proposes, instead, that experimentally oriented political psychologists look

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to formal models. Based on a deductive logic, these models begin with a set of assumptions from which the researcher then can derive precise and testable implications. However, models also simplify – they must be analytically tractable – and thus they are not especially good analogues of actual human behavior, either.

What Lupia recommends, therefore, is the joint use of axiomatic theory and experiments. On the one hand, rigorous theory leads to precise predictions about human behavior that not only shape the design of the experiment but also serve as the test criteria. On the other, experimental research directs theorists to a realistic set of assumptions about political decision makers and also provides the vehicle by which to test their models empirically. In Lupia's words, "an explanation that combines political psychology and rational choice theory trumps explanations that ignore either or both approaches." As he cautions, however, a formal model is no panacea for a badly designed experiment, nor is a poorly formulated model a panacea for experimental research. Lupia (also see Lupia and McCubbins 1998) then presents an illustrative study, of political persuasion, that includes a formal model of the relationship between speakers (public officials) and listeners (citizens) and an experiment embedded in a national survey designed to test its implications.

Lupia's study builds on the idea that the typically uninformed citizen must use available cues that more informed others provide. Reviewing the major psychological studies of persuasion of the past fifty years, Lupia argues that past work has not satisfactorily explained what differentiates a persuasive from a nonpersuasive cue. Specifically, none of the extant models identifies the necessary or sufficient conditions for cue persuasion.

The formal theoretical framework that Lupia offers begins with the theorem that "perceived common interests and perceived speaker knowledge are each necessary for persuasion." Satisfaction of both necessary conditions, plus the condition that the listener be sufficiently uncertain about two (or more) alternatives so as to be open to influence, comprise the sufficient condition. But listeners often cannot directly observe a speaker's knowledge and common interest, and thus they will look to speaker attributes that help them decide whether or not the speaker is knowledgeable and shares their interests. As a matter of empirical research, the task for the researcher is to identify such attributes, for only they can have a nonspurious correlation with cue persuasiveness. Speaker attributes that people ignore might be statistically related to persuasiveness, but they cannot be a cause of it.

Elaboration of the basic model entails introducing a third actor, an observer. This leads to the revised theorem that if the observer believes his interests conflict with both the speaker's and the listener's, and if the speaker has an incentive to be truthful, then the speaker can persuade the observer. An Illinois liberal who hears Jesse Helms criticize a pending bill before his most devoted North Carolina constituents should be persuaded to favor it.

The call for increased formalization in political psychology warrants serious consideration. Rational choice theorists have demonstrated its value, and there is no obvious reason why political psychologists should not benefit from it. This said, two comments are in order.

First, it would be wrong to leave the impression that deduction belongs solely to the realm of rational choice and experimental testing of hypotheses to political psychology. Zaller's *The Nature and Origins of Mass Opinion* (1992) is a notable example of deductive reasoning in political psychology. Borrowing from extant psychological research on persuasion, Zaller systematically deduces a set of predictions that he then tests. Although most other research programs in political psychology admittedly are less axiomatic, they too build on assumptions about individual thought processes. The on-line processing model (Lodge, McGraw, and Stroh 1989), for example, takes as its point of departure the importance of affect and the limitations of long-term memory. Given these features of human thinking, it follows that people will forget the specific events that occur during a political campaign but will incorporate their reactions toward the events via a "running tally" of affect toward the candidates.

The second observation is the more crucial. It is one thing to propose that political psychologists use formal theory, quite another to propose that they use rational choice theory. Lupia understandably recommends bringing political psychology into a rational choice framework. To be sure, he rejects the traditional assumption of omniscience found in rational choice models and adopts the psychology-sounding idea "that people do the best they can with the knowledge and skills they have." Nonetheless, he employs a signaling model that has its roots fully in economics and that assumes the ordinary citizen to be a rational, strategic actor. Whether political psychologists will readily adopt a rational choice framework that they themselves might modify further remains to be seen. Some of the most influential psychological research on which political scientists draw portrays people as incapable of even approximating the canons of rationality in their decision making. For example, they consistently and unknowingly use rules of thumb that lead to biased errors in judgment (Kahneman, Slovic, and Tversky 1982; Kahneman and Tversky 1979). Emotions are not an element of rational choice models, and yet they apparently precede and are a necessary condition for rational thinking (Damasio 1994; LeDoux 1996). More generally, the diversity of political psychology, noted earlier, would seem to preclude an easy incorporation of the one field into the other.

Conover and Searing use their study of citizenship in the United States and Great Britain to offer a fundamentally different set of recommendations for the field of political psychology. Not only do these recommendations eschew formalization, they also challenge the very foundations of the kinds of empirical research that political psychologists conduct. The authors' recommendations draw heavily on interpretivist ideas.

The authors explicitly state their central presumption as follows: "there are no context-free thinking processes . . . and . . . political thinking . . . is therefore best studied in the cultural and political contexts of meaning in which it occurs." By implication, this means, first, that people cannot be studied in laboratories or via traditional surveys and, second, that the pursuit of universal laws is ill-directed ("there are . . . only particular citizens thinking and behaving in particular times and places, thinking particular thoughts and applying particular decision rules"). Conover and Searing readily acknowledge the radical nature of their premise, which departs markedly from the assumptions underlying their own past work.

The second implication, that social scientists should not strive for universal generalizations, is perhaps the more profound, for it runs counter to a widely accepted principle that motivates nearly all empirical research in political psychology and the study of public opinion more generally.

What is it that Conover and Searing believe political psychologists should do? The single word that best captures their answer is "discover." Typically, researchers set forth hypotheses that they then verify (or not, although the latter occurs infrequently unless it is someone else's hypothesis that is being rejected). More importantly, in Conover and Searing's eyes, the researchers also select the concepts and language that motivate the hypotheses. Very much in the interpretivist tradition, the authors urge researchers to discover the categories that people use in their everyday lives rather than impose them. This entails, in turn, identifying the political culture within which people function and that shapes how they interpret and give meaning to the world around them. In short, political thinking is contextual, and the most directly relevant context is the political culture.

As an empirical matter, focus groups and in-depth interviews replace experiments and surveys. Conover and Searing discuss how they themselves are employing focus groups to identify the descriptive categories that people use when thinking about citizenship and their roles as citizens. Some of their focus groups consist of students and parents, others of eight adults plus a moderator who asks the participants a set of questions that are accompanied by specific probes. Other than that, the subjects simply talk with one another, using their own concepts, themes, and language. All of the sessions are recorded, and the researchers then use the transcripts to create a coding scheme. Thus the final coding scheme emerges from the participants' own words and the meanings they share.

Interestingly, the focus groups lend themselves to experimental manipulation that could define more precisely what the boundaries of a culture are. Suppose, for example, that one were to undertake some of the focus groups with uneducated rural residents and others with educated urban residents. Suppose, furthermore, that the two demographic groups use distinctly different concepts and language. To ascertain whether the different cultures are due to education or place of residence (or both), one would then conduct parallel focus groups in which one or the other demographic is held constant.

The authors are aware of the possibilities for experimental manipulation, and in fact undertake one that is fundamentally important: they compare citizens' concepts and language across countries. If national cultures exist, their focus groups presumably will reveal them. In one of their early discoveries, Conover and Searing find that the citizens of the United States and Great Britain define their roles differently.

In prescribing comparative political psychology, the authors are not content to remain at the micro level of analysis. Rather, "once the study of political psychology moves into this comparative world, the case for adding qualitative and historical analysis to its strategies of inquiry becomes compelling, since qualitative case-oriented studies are, for good reason, the dominant tradition in comparative politics." In other words, comparative political psychology lends itself to connecting the psychology of individual thought to the institutions – family, school, media, voluntary associations – of a particular culture.

Adopting the prescription for this case-oriented approach requires accepting the twin ideas that universal laws are not attainable and (thus) that social scientists should be engaged in discovery. Many political psychologists will reject this idea out of hand, pointing to its single case orientation as the very thing that is wrong with comparative politics. Why transport this weakness into one's own field? Nonetheless, the Conover and Searing prescriptions warrant a second thought. One of the principal criticisms of political psychology is that in reducing everything to mental processes it loses sight of politics and political institutions. The authors offer one way to help ensure that this does not happen. Moreover, theirs is simultaneously a call for comparative political psychology, which currently is notable for its absence.

One can be sure that many political psychologists will resist the argument that research findings are necessarily limited to a particular time and place. Nonetheless, Conover and Searing's chapter is intended to provoke debate about a goal of social science – generalization – that most social scientists have long taken for granted. Such a discussion is particularly important to political psychology, where laboratory-based research has already raised questions about ecological validity. If laboratory findings cannot be generalized anyway – this is a point of debate, not a conclusion – then the call for a case-oriented political psychology at least warrants a hearing.

Krosnick raises some fundamental issues even for those who conduct more traditional modes of research, such as experiments and analysis of survey data. To make his case, Krosnick focuses on attitude perception research, to which he has made substantial contributions. What he has to say, however, applies equally well to all topical areas within political psychology.

The first lesson to be learned, he argues, is that political psychologists rely far too heavily on cross-sectional data, even when those data are not appropriate for the task at hand. As he pointedly states, "nearly every causal hypothesis of significance in political psychology is tested initially using cross-sectional data." It is easy to understand why: cross-sectional data, either survey or experimental, are the most readily available.

Using the projection hypothesis as his example, Krosnick thoroughly documents how researchers tried but failed to use cross-sectional data as a legitimate test of the hypothesis. Some, for example, employed models that extracted correlated measurement error. Others used instrumental variables to account for possible mutual causation between variables. Despite the creative and highly sophisticated statistical maneuvers, however, no one fully succeeded in either eliminating alternative hypotheses or ascertaining causal direction. Krosnick's point is that no one could, since cross-sectional data simply are not appropriate for the kinds of tests the projection hypothesis calls for.

The second lesson goes hand in hand with the first: not only do political psychologists typically begin with cross-sectional data, they also unthinkingly use linear measures of association. This characterization, of course, applies to just about all areas of political science. Krosnick demonstrates, in a highly detailed and careful manner, precisely why linear measures do not always work in tests of the projection hypothesis. Specifically, he convincingly argues that "although measures of linear association are well suited to estimating the magnitude of positive projection onto liked candidates, assessing negative projection with a measure of linear association is wholly inappropriate."

More generally, Krosnick shows how not using the right data or not using the right statistical analysis leads to contradictory and often wrong conclusions. His most important message is that researchers can avoid the problems of wasted time and wasted effort by carefully considering what it will take to test their hypothesis correctly. Linear statistical analysis of cross-sectional data might be the easiest route, but more often than not it is also the wrong one. Although Krosnick's discussion applies specifically to a single hypothesis, his discussion is required reading for everyone who conducts survey or experimental research. It sets a new standard for thinking through the relationship between theory and data analysis *before* the research begins.

THE PSYCHOLOGY–POLITICS NEXUS

The single most consistent criticism of political psychology (and political behavior more generally) is its neglect of politics or, at best, its reduction of politics to a psychological phenomenon. For the most part, political psychologists have dismissed the criticism out of hand and pursued their research unabated. The three chapters in Part III address the issue directly, albeit very differently.

Rahn, Sullivan, and Rudolph begin by documenting the dramatic growth in published research on political psychology during the last decade or so. Although publications on rational choice grew more rapidly, the increased influence of psychologically oriented research is undeniable, which renders questions about its value to the study of politics even more crucial.

Rahn, Sullivan, and Rudolph begin at the beginning: when critics deem political psychology to be insufficiently political, what do they mean by "insufficiently political?" They offer three plausible constructions of what the "naysayers" (their term) might have in mind and refute all three. The first possibility is that political psychologists do not pay enough attention to the role of elites in mass political behavior. This criticism, they argue, fails on the evidence. Numerous works have considered the connection between mass decision making, on the one hand, and elite discourse and behavior, on the other. To be sure, the authors continue, these studies typically incorporate elites as informational sources, not strictly as political actors, but the provision of political cues and messages is an integral part of the political process.

Second, perhaps the critics deem political psychology to be insufficiently political because it concentrates too heavily on the individual as the unit of analysis. After all, it is aggregate opinion, not individual choices and judgments, that elected officials see, hear, and pay attention to. True, admit Rahn, Sullivan, and Rudolph, but even students of macro politics draw on psychological models developed at the individual level, and "the question is not whether we will rely on psychological models to explain both individual and aggregate phenomena, but whether we will rely explicitly or implicitly, naively or with expertise, on such models." An even stronger assertion, which Rahn, Sullivan, and Rudolph do not make, is that the nature of collective opinion will never be fully understood without understanding the psychological processes underlying the individual decisions that get aggregated in democratic societies.

Finally, Rahn, Sullivan, and Rudolph speculate that what critics really have in mind is a specific approach, best exemplified by the Stony Brook School. This approach relies heavily on experimentation, some of it involving students, and delves deeply into psychic processes. Best known is the on-line processing model, which is described with nonpolitical-sounding terms like "updating" and "judgment operators" (Lodge et al. 1989). Reductionism, Rahn, Sullivan, and Rudolph state, is an inherent feature of political psychology for which researchers need not apologize. Moreover, the study of the most extremely micro psychological processes might well have big payoffs for the more traditional concerns in political science. It just takes time and hard work to get there.

Moving from the defensive to the offensive, Rahn, Sullivan, and Rudolph posit that just about everyone would identify the three most basic elements of politics as power, conflict, and governing. If political psychology is to be sufficiently political, therefore, it should have something to say about each. The authors then review literatures that, in toto, speak to all three elements of politics. In fact, they conclude, it is impossible to imagine how political scientists would study power, conflict, and governing without psychological concepts.

Rahn, Sullivan, and Rudolph conclude with a typology of extant research. The first category consists of research that directly applies psychological concepts and theories to political phenomena. As they note, most of the burgeoning literature on mass political cognition falls into this category, which begs the question "How worthwhile to political science are these straightforward applications?" The authors offer a strong argument that these direct applications have enriched our understanding of politics.

The second category of research also applies psychological concepts and theories, but less directly than the first. In this case, the researchers reformulate the psychological ideas so that they are specific to politics. Although Brady and Sniderman (1985) borrow the idea of a decision-making heuristic from psychology, for example, they do not merely apply an already identified heuristic to political decision making. Instead, they develop the idea of a "likability heuristic," which citizens can use effectively to predict political groups' policy positions. Finally, Rahn, Sullivan, and Rudolph observe that some political psychologists have actually helped to reformulate psychological theory by demonstrating that psychological processes deemed to be universal in fact are domain-