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4

Psychological Political Science Versus Political Psychology True to Its Name: A Plea for Balance

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"What is political psychology?" the editor asks. There are as many different answers to this question as there are chapters in the volume. Everyone with a nodding scholarly acquaintance with "political psychology" understands that it is the intellectual and scientific activity that takes place at the intersection of political science and psychology. Some prominent commentators on the "state of political psychology," perhaps wisely, avoid more precise definitions beyond this Venn diagrammatic designation (e.g., Iyengar & McGuire, 1993; Sears, 1987).

In our view, answering the question "What is political psychology?" requires acknowledging that there are really two very different sorts of political psychology. These two types are defined not by substantive, conceptual, or methodological cleavages that exist in the discipline, but rather by the fundamental priorities of the research enterprise. Our goals in this chapter are threefold: (a) to clarify the distinction between the two types of political psychology; (b) to assess the prevalence of each type; and (c) to evaluate the current state of political psychology and its future potential within the framework.

POLITICAL PSYCHOLOGY AND PSYCHOLOGICAL POLITICAL SCIENCE[1]

A brief linguistic analysis of the label *political psychology*, certainly the dominant label for our interdisciplinary exercise, is quite illuminating. The two constituent disciplines are represented, but *political* is a modifier or qualifier of the noun *psychology*. Absent any understanding of the actual practice of political psychology, this ordering suggests that the goals of psychology are central to the enterprise. From this perspective, political psychology could be viewed as a subtype of the larger discipline of psychology, comparable to the core subdisciplines of cognitive, social, developmental, and clinical psychology. The discipline of psychology has enormous range, and its territory borders on the biological sciences on the one end and social sciences such as anthropology and sociology on the other. According to Zimbardo (1988), the fundamental concern of psychology writ large is "the scientific study of behavioral and mental processes . . . [with an interest] in discovering general laws" (p. 5). Because the goal of psychology is *generalizations* about human nature, scholars engaged in political psychology "true to its name" (Krosnick, in press) would *not* be primarily interested in identifying and explaining relationships that hold *only* in the political context, but rather would make use of the political context to generate more general principles that are pancontextual.

Why should scholars interested in psychological questions engage in political psychology true to its name? That is, why should one stray from the usual approaches to psychological research in order to explicitly pay attention to the political context? At least one answer to this question begins with the recognition that research by cognitive and social psychologists during the last century has often been done with a relatively restricted subpopulation of people (i.e., college students) and in laboratory settings that are intentionally constructed to be barren, simple, and streamlined, focusing participants' attention on the stimuli on which they are intended to focus, minimizing their attention to other stimuli, and minimizing the baggage they bring to the situation from prior experiences. All this makes perfect sense if we wish to understand how people respond to such unusual situations. And all this makes sense if we want to minimize between-person variance in participants' thinking and behavior in order to maximize the statistical power of the study to detect the effects of manipulations that are implemented.

However, there is a cost paid when taking this approach, in terms of theoretical richness. When researchers design laboratory experimental paradigms to test psychological theories, they typically choose stimuli relatively arbitrarily, because their theories should, in principle, apply to all stimuli. So a study of attitude change could be done equally well regardless of whether a persuasive message is about vegetables or about vacationing in Alaska or about tooth brushing. In building a laboratory experiment, researchers typically want to strip away the unique complexities that may come along with any particular attitude object. This allows for a clean test of the hypothesis the researcher brings, based on abstract theory, to the testing situation.

Yet careful attention to the idiosyncrasies of a particular target and situation can have wonderful payoffs for the development of basic, pancontextual theory. In particular, this can lead to new insights about moderators and mediators of effects (Baron & Kenny, 1986), as well as new effects that researchers might not previously have considered. Imagine, for example, a researcher who is interested in the organization of social information in memory, and in particular the question of how individuals represent the various (human) alternatives in a choice task (e.g., which secretary to hire, which suitor to marry). This is, interestingly enough, an issue that the social cognition literature has largely overlooked, a central question being the extent to which such choices are represented as person-centered or attribute-centered structures. A researcher approaching this question from the perspective of political psychology true to its name might turn to the political domain for insights, because this representational problem is intrinsic to voting decisions. From his or her understanding of the political context, the researcher might design studies aimed at specifying the moderators, or "conditions under which," different representational outcomes occur. For example, he or she might determine the impact of different information presentation formats (e.g., "holistic" candidate-centered formats such as campaign brochures versus attribute-centered formats such as point-by-point newspaper summaries of candidates' differences), or investigate how participant characteristics, such as membership in or identification with relevant social groups, or the subjective importance of the decision, influence the representation of the alternatives. The researcher might also specify the critical psychological mediating processes, such as affective responses to the alternatives taken individually and relative to each other, that shape the content and structure of the representation. Moreover, she or he might then examine how the representation of the alternatives influences the decision-making process. Eventually, the researcher might turn to other social contexts (e.g., organizations, interpersonal relations) to verify and extend these principles.

The key point of this hypothetical example[2] is that the goal of the research process is to develop psychological theory that is intended to be applicable across contexts. More generally, in doing political psychology true to its name, the conclusions are relevant not only to politics but rather are more general and pancontextual. The research may be done in the context of politics, but describing political events and the processes underlying them is not the central goal.

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[1] This section is drawn heavily from Krosnick (in press).

[2] In fact, the example is not fully hypothetical; see Rahn (1995; Rahn, Aldrich, & Borgida, 1994) for evidence relevant to the choice representation problem.

Of course, in practice, the enterprise of political psychology rarely looks like this. To more accurately capture what most political psychologists actually do, the modifier and noun should be reversed in the label, yielding *psychological political science* as a more apt descriptor of most scholarship in the field. That is, political psychologists usually have a primary interest in serving the core goals of political science. In general, the scientific study of politics is regarded as the attempt to understand how and why the processes of politics unfold as they do, with no interest in generalizing beyond the political context to other domains of human behavior. From this perspective, political psychology is a subtype of political science, and its distinguishing feature is theoretical explanations of political phenomena that are rooted in psychological theory and concepts (as opposed to, for example, economics or sociology). Definitions of political psychology that emphasize the application of psychological theory to understanding political processes have this perspective in mind. In contrast to *political psychology true to its name,* the label *psychological political science* emphasizes that the enterprise is a subtype of political science, not psychology.

One especially prominent example of *psychological political science* is Lodge, McGraw, and Stroh's (1989) research on processes of candidate evaluation (see Rahn & Sullivan, in press, for a similar analysis of Lodge et al., 1989). In 1986, Hastie and Park, social psychologists, published a paper in the prestigious *Psychological Review,* drawing a distinction between memory-based and on-line decision making. Memory-based judgment occurs when a person is prompted to express an opinion or choice, and at the time the judgment is expressed, the person canvasses his or her memory to retrieve whatever information is available and deemed relevant. That information is then integrated to form an opinion, so that the judgment is constructed on the spot. In contrast, on-line processing occurs when the person is motivated to form an opinion about some entity (person, policy, group) on his or her own and updates a "running tally" of that opinion as new relevant information about the object is encountered in the course of daily life. When asked to express an opinion about the object, the person simply retrieves the existing on-line tally from memory, not the specific pieces of information about the object that are available in memory.

Lodge et al. (1989) devised and conducted an experiment that extended Hastie and Park's general assertions about human decision making to the political domain of candidate evaluations. The study provided compelling evidence that at least under some conditions, citizens evaluate political candidates in an on-line fashion. Those familiar with the social psychological literature at this time may have very likely reacted to this political demonstration by saying, "How could it be otherwise?" As such, the Lodge et al. (1989) study represents a very common form of psychological political science: taking an existing political theory and applying it to understand a political phenomenon.

This study could have been designated as "merely applied," accompanied by a sneer, implying it was a second-rate undertaking, because it did not involve the development and testing of new and original theoretical ideas. Applied work is often regarded as mechanical application of others' theoretical ingenuity. The Lodge et al. (1989) study was not accorded this second-rate status, and it seems to us there are two main reasons for its acceptance and influence. The first is that the study challenged reigning presumptions in political science that citizens evaluate candidates in a memory-based fashion (Kelley, 1983; Kelley & Mirer, 1974) and so forced a reconsideration of the processes by which candidate evaluations and vote choices emerge. The second is that it motivated subsequent work, extending and modifying the on-line view of voters (e.g., Lodge & Steenbergen, 1995; Lodge & Taber, 2000; McGraw, Lodge, & Stroh, 1990; Rahn, Aldrich, & Borgida, 1994; Rahn, Krosnick, & Breuning, 1994) as well as ongoing controversies about the on-line model's validity (e.g., Redlawsk, 2001; Zaller, 1992), and so generated a continuing stream of productive intellectual discourse.[3]

THE DOMINANCE OF PSYCHOLOGICAL POLITICAL SCIENCE

A number of indicators underscore our suspicion that *psychological political science* dominates *political psychology true to its name.* One set of indicators appears to reveal the value placed on the enterprise by the disciplines of psychology and political science. If the enterprise of political psychology is often "true to its name," then we would expect to see more value placed on it by psychologists, because the goals of psychology are fundamental. On the other hand, if the enterprise of political psychology usually has the characteristics of psychological political science, it is likely to be valued more by political scientists, because the concerns of political science are central. And in fact, these indicators suggest that psychological political science dominates its cousin.

For example, one prominent indicator of disciplinary value is hiring practices at major research universities. Political science departments have hired faculty members with Ph.D.s in psychology (in particular, social psychology; e.g., Leonie Huddy, Don Kinder, Rick Lau, and Tom Nelson), but to our knowledge, there are no scholars with Ph.D.s in political science who hold primary appointments in psychology departments at major research universities.

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[3] For an extended discussion of other research that might be classified as psychological political science, see Krosnick (in press) and Rahn and Sullivan (in press).

Publication trends in scholarly journals reflect the same asymmetry. Psychology Ph.D.s on occasion publish articles in major political science journals (even when their primary affiliation is in psychology; e.g., David Sears, Jim Sidanius, and Philip Tetlock). In contrast, it is more difficult to identify publications in major psychology journals and book series written by political science Ph.D.s (John Zaller, Shanto Iyengar, John Sullivan, and Richard Herrmann are notable recent exceptions).

A third indicator of the asymmetry is revealed by the enrollment rates at the Summer Institute of Political Psychology, run at Ohio State University for the past 10 years. The Summer Institute is a 1-month intensive training program in political psychology. Each year, an imbalance between psychologists and political scientists has been evident, with the proportion of participants from psychology generally hovering around 25%; most of the remainder have been from political science. Taken together, these indicators demonstrate that the practice of political psychology has been more valued by the discipline of political science than by the discipline of psychology.

A PLEA FOR MORE POLITICAL PSYCHOLOGY TRUE TO ITS NAME

We make a plea for a more balanced political psychology, one in which a genuine *political psychology true to its name* complements *psychological political science.* This requires a very real shift in the practice of political psychology, through a self-conscious attempt to contribute to psychological theory by paying careful attention to the political context. Our recommendation here is not for research that borrows from political science to inform psychological theory (applied political science) because we find it difficult to imagine what such an enterprise would look like. Rather, the more modest version of *political psychology true to its name* that we have in mind is a research enterprise that contributes regularly (if not equally) to both psychology and political science, and it is a research enterprise in which we hope more psychologists will become engaged.

The primary means by which this balance can be accomplished is by recognizing that careful attention to the key parameters of the *political context* often inspires modifications, elaborations, and extensions of psychological theory developed in context-free work. The presumption underlying much psychological research (particularly laboratory research in social psychology) is that psychological processes that are simple, basic, and generalizable across social contexts can be identified and explained, but this is rarely a tenable assumption. In fact, we believe that one of the challenges in studying the psychology of political thought and action lies in grappling with the parameters of the political context and the nature of the differences between types of actors that often complicate the straightforward application of psychological theory.

Next, we illustrate what we mean with examples from our own separate research programs. We want to apologize in advance for confining this discussion to our own work—this is not meant to suggest that our work is unique or even especially good for illustrating the principles of interest here. Instead, this is simply a choice of convenience, one driven by the fact that we know our own work and its genesis best. So we settle for a few of the many possible examples that could be used for this purpose, ones we hope can help motivate further consideration of political psychology true to its name.

Each research program we describe next began with an interest in understanding a political phenomenon and looked first for relevant insights in the psychology literature. Thus, we set out doing psychological political science. Then, after thinking about the real-world history, context, and implications of the political phenomena of interest, we developed and tested hypotheses that ultimately led to insights that went beyond the psychological theories we initially consulted. In some cases, we found effects of variables that the psychological theories had not anticipated. In other cases, we found that psychological theories we thought would explain a phenomenon did not, in fact, do so and that instead a different psychological account was required. Thus, we were able to identify limiting conditions of the applicability of the original theory. Stated generally, the examples we review next illustrate instances in which careful attention to politics uncovered mediators and moderators of effects that had not yet been incorporated in psychological theory.

To be clear, these are not examples of researchers setting out to do political psychology true to its name, and we do not want to suggest that researchers must begin their work with the goal of evolving psychological theory in mind in order to achieve that goal. Quite the contrary, in fact. We set out first and foremost to understand a political phenomenon, and the payoffs for psychological theory were unintended and incidental. What is not unintended and incidental, though, are our efforts to *highlight* the payoffs for psychological theory after the fact. So looking again at much of the psychological political science already completed may lead people to see findings that can help to advance psychological theory. We hope to encourage researchers to take such looks at the work they have done and that they do in the future and to make efforts to spotlight those payoffs in what they write.

Illustrations of Political Psychology "True to its Name"

News Media Priming. The notion of *priming* in psychology refers to the impact of recently activated cognitive constructs on subsequent judgments. Iyengar, Kinder, Peters, and Krosnick reasoned that this psychological process offered the promise of identifying a new type of effect the news media might have on public opinion: By focusing on some issues and not others, the news media might shape the "ingredients" contributing to the public's approval and disapproval of the president. Laboratory experiments and survey data all lent support to this notion (Iyengar & Kinder, 1987; Iyengar, Kinder, Peters, & Krosnick, 1984; Krosnick & Brannon, 1993; Krosnick & Kinder, 1990), and these studies are elements of the reinvigorated literature on media effects. These studies were clearly psychological political science because they applied the notion of accessibility to media effects, and the findings seemed to lend support to the psychological theories employed.

Contributions to psychological theory occurred as a byproduct of this work when Krosnick and his associates explored contingency effects, focusing in particular on the impact of political expertise on priming effects. The research began with a relatively straightforward demonstration that priming effects are strongest among people who knew relatively little about politics (Krosnick & Kinder, 1990). This suggested that perhaps accessibility effects are minimal when a person is expert in a domain, a possibility not yet recognized in the psychology literature. These investigators realized, however, that knowledge about politics is positively correlated with two other variables that might have different moderating impacts on priming: interest in political news and exposure to political news. Indeed, when controlling for these latter two variables, it appeared that political knowledge facilitated priming rather than impeding it (Krosnick & Brannon, 1993). This is consistent with the notion that "those who have, get," in the sense that knowledge breeds the accumulation of more knowledge. This raised for the first time the idea that news media priming might not be mediated by cognitive priming (i.e., increases in the accessibility of constructs in memory) at all but rather might be the result of learning.

Miller and Krosnick (2000) investigated this latter issue directly, by exploring whether news media priming is, in fact, mediated by changes in accessibility and found that it is not. This surprise further encouraged consideration of the possibility that news media priming is the result of learning. Another finding of Miller and Krosnick's (2000) supported that notion even more: Media priming was only apparent among people who knew a lot about politics *and* were highly trusting of the media to provide accurate and unbiased information. Furthermore, Miller and Krosnick (2000) found that among these people, priming was mediated by judgments of the national importance of issues: Media coverage of an issue led these people to infer that it was a more important issue for the country than they had thought, which led them to weigh that problem more heavily in evaluating the president's job performance.

Thus, the search for moderators of an effect led these investigators to explore the mediators of the effect, which in turn yielded a challenge to the most significant and fundamental assumption underlying the literature on this and other effects of the news media: that these effects result from changes in construct accessibility. In doing so, this work yielded an important lesson for psychology: Despite the highly plausible applicability of a psychological theory to a real-world phenomenon, this leap must be made cautiously. What appeared to be cognitive priming turned out not to be.

Indeed, this finding raises an even more intriguing possibility. Many psychological studies thought to have demonstrated cognitive priming did not, in fact, measure accessibility and demonstrate through mediational analyses that accessibility shifts were, in fact, responsible for the effects documented. Therefore, it is conceivable that these effects occurred as the result of rather different processes, a possibility definitely worth testing. Doing such testing may lead to further refinement of psychological theory. More generally, this line of thinking reminds all researchers that we should always try to measure presumed mediators and document their roles statistically, rather than taking those roles for granted.

Attitude Importance and Policy Issues. Throughout the 20th century, psychologists regularly acknowledged the idea that some attitudes are stronger than others, presumably meaning that some attitudes are more crystallized and consequential. However, that literature was foggy in two particularly important regards: what exactly *is* attitude strength and where does it come from.

Krosnick's work on attitude importance had its impetus in Converse's (1964) seminal chapter on belief systems in mass publics. In the conclusion of that piece, Converse proposed that rather than attending to the entire array of policy issues facing the nation and forming crystallized and consequential attitudes on each one, the ordinary citizen focuses on just a handful of issues. Attitudes on those issues, he thought, become anchored within cognitive structures and have powerful consequences for political thought and action. However, Converse himself provided very little by way of data to test this proposition.

Krosnick (1988, 1990) set out to explore the viability of this idea and quickly confronted the problem of how to identify the few issues people focus on. Drawing on work from psychology exploring attitude strength, he chose to use an empirical handle available in a number of national survey data sets at the time: people's reports of the personal importance of issues to them. Krosnick (1990) demonstrated that attitudes on policy issues that are more subjectively important exhibit greater stability, greater ideological constraint, and have greater impact on candidate preferences and voting. All this reinforced Converse's notion of issue publics and helped to clarify how issue preferences are formed and influence political behavior.

Because studying the world of politics always requires consideration of normative implications, Krosnick and his colleagues were pushed to think about whether the existence of issue publics was good or bad for a democratic society. To fully understand the normative implications of his evidence on issue publics, Krosnick and his colleagues set out to understand *why* personally important attitudes are more resistant to change and more impactful, and what the causes of personal importance are. The result of their work was an elaborate causal model of the causes and consequences of attitude importance (Boninger, Krosnick, Berent, & Fabrigar, 1995) that is still the subject of empirical research today, highlighting the origins and consequences of attitude strength. The work was valuable to psychology in two principal ways.

With regards to the origins of attitude importance, Krosnick and colleagues' work has pointed to three principal causes: self-interest, social identification with individual and reference groups, and core values. This finding helped to build a bridge between the long-established literature on attitude functions (which was quite cognizant of the roles these three classes of factors play in justifying the existence of attitudes but not specific at all about any other consequences they might have) and the quite separate literature on the cognitive and behavioral effects of attitudes.

Krosnick and colleagues' thinking about *how* importance has its effects led them to develop the notion that importance is first and foremost a choice by an individual to attach significance to an attitude, and this choice has an array of controlled consequences (e.g., gathering relevant information, thinking carefully about it, placing weight on an issue when evaluating a political candidate). However, Krosnick et al. also realized that these processes will have automatic effects eventually, in terms of increased attitude accessibility, the building of links between nodes in memory, and more. All this represented an advance in basic understanding of attitude strength processes.

This led to serious consideration of the relations between importance and various other attitude features thought to be related to their strength, including accessibility, knowledge, certainty, extremity, and more (see Petty & Krosnick, 1995). Some observers have asserted that multiple features actually reflect single underlying constructs (Bassili, 1996; Pomerantz, Chaiken, & Tordesillas, 1995). Others have argued that the dimensions are actually all independent constructs and deserve theoretical status as such (e.g., Krosnick, Boninger, Chuang, Berent, & Carnot, 1993; Lavine, Huff, Wagner, & Sweeney, 1998). This debate is currently quite active in psychology and far from resolved, and its payoffs are likely to be quite valuable for the study of attitudes.

Managing Blame. McGraw's program of research on political blame management was rooted in her social psychological research in the area of attribution theory. She became interested in applying that perspective toward understanding attributions of responsibility for political predicaments, and so set about designing a study that was a rather straightforward adaptation of key principles from social psychological attribution theory (e.g., manipulating outcome severity and target similarity). The resulting design was frustratingly inadequate because it failed to capture what anyone familiar with the political context would take to be self-evident: namely, that when politicians find themselves in some predicament, they provide an explanation or account to contain the political damage. Recognizing that public officials are not passive bystanders but rather take an active role in trying to shape citizens' reactions to political events led McGraw to refocus the research project on the impact of accounts on attributions of responsibility.

There is a psychological and sociological literature on accounts, but it did not provide much in the way of theoretical guidelines as to the psychological process mechanisms and conditions under which accounts might have an impact on public opinion, nor was there a link between the psychological literatures on attribution of responsibility and accounts. To remedy these omissions, McGraw created a 2 × 2 typology of four different types of accounts (excuses, justifications, concessions, and denials), and developed a theoretical framework that specified the unique impact of the different types of accounts on specific political judgments, such as attributions of responsibility, trait inferences, and opinions about controversial policies (McGraw, 1991, in press). The key lesson here is that different types of accounts have an impact on subsequent global evaluations through different mediational routes, such as changing attributions of responsibility, or by shaping perceptions of character traits, or by changing opinions about the severity of the problem. Later work considered the impact of critical political individual difference variables (in particular, sophistication and trust in government) and situational parameters that moderate the impact of political accounts (McGraw, Best, & Timpone, 1995; McGraw & Hubbard, 1996), and extended the model to consider the impact of political accounts on opinions about the political institutions and organizations where wrongdoing occurs (McGraw, 1999).

Specifying the impact of political explanations on public opinion is critical to political scientists' understanding of the delicate and negotiated relationship between citizens and their elected representatives, but this research also enriches, in several ways, more general psychological theory about attributions of responsibility and accountability. First and foremost, it suggests that the static and impoverished laboratory paradigm used to study attributions of responsibility overlooks a critical aspect of most real-world wrongdoing, namely that alleged transgressors make excuses, offer justifications, and in general are quite adept at trying to manipulate the perceiver's reactions to the event. Second, the theoretical model developed by McGraw and her colleagues, and the subsequent empirical validations, identified a set of mediating and moderating principles that spell out why and when accounts have an impact on opinions about others in our social world in greater detail than previous social psychological theorizing had considered.

Suspicion. McGraw and her colleagues (McGraw, Lodge, & Jones, 2000) also investigated the antecedents and consequences of political suspicion for public opinion. There is an emerging literature on suspicion in social psychology (e.g., Hilton, Fein, & Miller, 1993), and in thinking about the implications of suspicion for understanding public opinion, McGraw et al. were struck by a paradox. The dramatic decline in political trust that has been documented over the past several decades has arguably made contemporary politicians particularly vulnerable to suspicion about the sincerity of their stated policy positions. However, the psychological literature indicates that suspicion produces an active state of cognitive appraisal and scrutiny. Although it may well be that citizens are predisposed to be suspicious of the positions taken by public officials, the heightened state of cognitive activity that is the defining characteristic of suspicion is anathema to most citizens (Kinder, 1998). In other words, although a generalized cynicism toward politics is widespread, suspicion directed toward specific political actors seems unlikely because of the amount of cognitive work that is required in sustaining suspicion.

Thinking about this paradox led McGraw and her colleagues to think carefully about the strategies politicians use when setting forth their policy stands, and in particular about how strategically rational politicians are skillful at crafting political communications that are congruent with their audience's preferences (*pandering* is the pejorative term for this practice). This in turn led to a consideration of the evaluative reactions that citizens can have to potentially suspect communications, and the theoretical prediction (supported empirically) that citizens' reactions to political policy statements regulate the experience of suspicion: policy agreement derails suspicion and its attendant negative consequences, whereas policy disagreement magnifies the experience of suspicion.

The systematic social psychological study of suspicion is in its infancy, and the McGraw et al. (2000) study points to important considerations that might shape future developments. Whereas current formulations emphasize the cognitive processing mechanisms involved in suspicion (Hilton, Fein, and Miller, 1993), the work of McGraw and her colleagues points to the critical role that evaluative reactions play in regulating the experience of suspicion: simply, perceivers are less likely to be suspicious of communications that they agree with or that make them happy. In addition, as with the McGraw work on accounts and blame management, consideration of the political context underscores the importance of understanding the influence of strategic interactions among social actors in the dynamics of suspicion: Skilled communicators have tools at their disposal to minimize the arousal of suspicion.

Implications

All four of these examples are cases where we set out to do psychological political science—to apply established psychological theories to understand a political phenomenon. However, the political context forced us to think more deeply and carefully about the limits of those existing theories and to generate new extensions and modifications that could better capture the complexity of political reality; the results are findings and insights that psychologists may find of use. But recognizing these uses is the result of a constant interplay between psychological and political theory when facing real-world phenomena. So beyond pursuing the goal of understanding political phenomena, we have found ourselves contributing to psychological theories of human thought and behavior.

The proposition that theories developed in a context-specific environment can illuminate cross-contextual generalizations may seem counterintuitive, but it is an insight harkening back to the explanatory framework proposed by Lewin in 1936, when he stated, "Every psychological event depends upon the state of the person and at the same time the state of the environment, although their relative importance is different in different cases" (p. 216). More precise is Lewin's classic formulation:

$$\text{Behavior} = f(\text{Person, Environment}).$$

From this perspective, theory develops as new "conditions under which" principles are identified and empirically supported. These conditions are both the situations in which certain outcomes are more or less likely to occur and the classes of people among which outcomes are more or less likely to be observed. It seems to us to be no coincidence that some of the most prominent elaborations of Lewin's notion of the interplay between personal and situational factors have been proposed by scholars working at the intersection of political science and psychology: Smith's (1968) "intellectual strategy" for the study of political personality; McGuire's (1969) matrix of persuasibility and his (1983) "contextualist theory of knowledge;" and Sniderman and his colleagues' contributions to our understanding of the dynamics of public opinion (1993; Sniderman, Brody, & Tetlock, 1991).

In sum, we are convinced that taking the political context seriously results in scholarship that contributes to both political science and psychology (see Rahn and Sullivan, in press, for additional examples). In our experience, practicing political psychology true to its name yields lots of intellectual satisfaction, just as does the successful application of psychological theory required by "psychological political science." If political scientists and psychologists alike focus their efforts, at least on occasion, to highlight the payoffs of their work for psychological theory, this may enhance the apparent value of political psychology in the eyes of all psychologists. The most important payoffs of such perceptions may be a greater flow of students into the field of political psychology within psychology, more faculty hiring in the area in psychology departments, and greater financial support for our efforts by psychological funding agencies, all of which would contribute to the vitality of the field.

CONCLUSION

Our hope in raising the distinction between *psychological political science* and *political psychology true to its name* is that scholars will be more self-conscious in thinking about the findings their research generates, always on the lookout for ways that their evidence can help to advance basic psychological theory. We recognize that contributing to both political science and psychology is not an easy task, but we believe the payoffs—for both individuals and the disciplines—can be substantial.

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