

Is Deliberative Democracy a Falsifiable Theory?

Diana C. Mutz

Department of Political Science and Annenberg School, University of Pennsylvania,
Philadelphia, Pennsylvania 19104; email: mutz@sas.upenn.edu

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Abstract

To further dialogue between theory and research on deliberative democracy, I advocate abandoning tests of deliberative theory *per se* and instead developing “middle-range” theories that are each important, specifiable, and falsifiable parts of deliberative democratic theory. By replacing vaguely defined entities with more concrete, circumscribed concepts, and by requiring empirically and theoretically grounded hypotheses about specific relationships between those concepts, researchers may come to understand which elements of the deliberative experience are crucial to particular valued outcomes.

INTRODUCTION

The dialogue between political theorists interested in deliberative democracy and those who study deliberative democracy empirically has been strained at best. As Thompson (2008) describes in this volume, both groups seem to realize that they have much to gain from one another, yet frustration remains on both sides due to our inability to accept one another's assumptions and even to understand one another's terms. Indeed, for many political scientists, reading theorists' accounts of deliberative democracy can be aggravating. On the one hand, many of the assertions seem to cry out for empirical verification. On the other hand, much of the empirical work in this vein has been deemed irrelevant to the theory of deliberative democracy by political theorists.

Excellent reviews of this literature have been provided elsewhere (in addition to Thompson's article, see also Ryfe 2005, Delli Carpini et al. 2004, Mendelberg 2002). My purpose here is to delve deeper into the conversation—or lack thereof—between theory and empirical research in this important area to see what progress might be made. In contrast to Thompson, I approach this dilemma from the perspective of empirical social scientists who want to test the posited beneficial consequences of deliberative theory. The general question before us as empirical researchers is: How can we take what has been, by its origins, a normative theory and turn it into an empirically testable theory?

I begin with an overview of the problems involved in constructing deliberative democratic theory in terms that satisfy the requirements for a productive and testable social theory. A great deal of the difficulty in this conversation results from definitions of deliberative democracy that are too broad and that effectively insulate the theory from falsification. Falsifiability—the possibility of refutation—is held to be essential to the scientific method because it offers the possibility of scientific progress: Faulty theories will encounter refuting evidence and will be discarded in favor of other theories. Philosophers

of science consider falsifiability an essential requirement for a theory to be deemed scientific (see Popper 1963); some go so far as to say that unfalsifiable hypotheses are meaningless. As I describe below, unfalsifiable aspects of deliberative theory translate into concrete obstacles that prevent testing and improving the theory.

To further dialogue between theory and research on deliberative democracy, I advocate abandoning tests of deliberative theory *per se*, advocating instead developing “middle-range” theories that are each important, specifiable, and falsifiable parts of deliberative democratic theory.¹ Transforming deliberative theory into middle-range theory means replacing vaguely defined entities with more concrete, circumscribed concepts, and substituting hypotheses about specific relationships between those concepts for grander theoretical frameworks. If, as Ryfe (2005, p. 64) suggests, “The theory of deliberative democracy needlessly remains removed from its practice,” then this is a necessary step toward rectifying this problem.

This article explores the nature of the problem facing empirical researchers and then proposes how they might, nevertheless, move forward. In addition to helping bridge the normative-empirical divide in a way that moves empirical research forward, my goal is to illuminate for theorists the sources of difficulty facing empirical researchers who genuinely want to add to this conversation. While Thompson (2008) has done an excellent job illuminating the perspective of normative theorists, I seek to detail the predicament of the social science researcher interested in deliberative theory. As a social scientist, my goal is not to play armchair political theorist, but

¹ Merton (1957) advocated the “middle-range” approach as a means of bridging the gap between theory and empirical evidence and thus advancing social science. Middle-range theory suggests that “it is hopeless and quixotic to try to determine the overarching independent variable” as “grand theory” often tries to do (Boudon 1991, p. 519), but it is nonetheless important to take “theory” seriously, that is, to formulate a consolidating idea about a set of hypotheses that can be empirically tested.

rather to translate the theory from one set of terms and standards to another. If researchers do not go about testing deliberative theory in a way that speaks to political theory, then the theory is unlikely to realize its potential theoretically or empirically, and political scientists will have missed an important opportunity.

CAN EMPIRICAL THEORY SPEAK TO NORMATIVE THEORY?

Thompson (2008) characterizes empirical research on deliberative democracy as follows: “They extract from isolated passages in various theoretical writings a simplified statement about one or more benefits of deliberative democracy, compress it into a testable hypothesis, find or (more often) artificially create a site in which people talk about politics, and conclude that deliberation does not produce the benefits the theory promised and may even be counterproductive.”

Theorists clearly find empirical contributions to this conversation lacking; most of the research is said to be “not fully engaged with the normative theory” (Thompson 2008). But it is not clear if the conclusions reached by these studies are what is deemed objectionable, or the way the studies are conducted or evaluated. The latter is the more appropriate concern for empirical researchers. For a theory to be falsifiable, one should consider disconfirmations as valid as confirmations. Given the well-documented bias in published studies toward significant findings as opposed to null hypotheses, it would be surprising indeed if the outcomes of published studies were tilted in this direction (see Gerber & Malhotra 2007, 2008). Thompson’s observation suggests that, in fact, disconfirming evidence is more prevalent than confirming evidence. Other summaries have characterized findings as mixed, which comes closer to my own reading. From a normative point of view, there may be legitimate complaints about the nature of the research questions scholars ask, but there is little basis for criticizing the con-

clusions these studies reach, assuming they are done skillfully.

Normative theory requires ethical commitments on the part of researchers; that is, some kinds of attitudes and behaviors must be valued above others. However, normative theory in this case also promises empirically observable benefits. Whether at the individual level (e.g., greater tolerance, depth of understanding) or the level of the collective (e.g., a consensual decision, increased social capital), deliberation promises observable benefits. The whole reason deliberative democracy is normatively desirable is because it is thought to produce tangible benefits for democratic citizens and societies.

Of course, anyone who has read empirical research on deliberative theory will recognize that it is also value-laden, in that such studies typically begin and end with an assertion that some outcomes are more promising or disappointing than others. Empirical research findings are interesting and/or important precisely because they tell us something about some consequence that is positively or negatively valued.

Although normative theory is obviously not testable in the usual sense, to the extent that scholars agree on which criteria are beneficial for a democratic society, then we can, within the boundaries of these specific criteria, evaluate whether deliberation contributes toward those ends. This is the shared ground where normative theory and empirical social science meet. Regardless of how one defines or studies deliberation, certain kinds of outcomes are consensually valued by theorists and empiricists alike. These include, but are not limited to, more public-spirited attitudes; more informed citizens; greater understanding of the sources of, or rationales behind, public disagreements; a stronger sense of political efficacy; willingness to compromise; greater interest in political participation; and, for some theorists, a binding consensus decision. The perceived legitimacy of the decision outcome is also argued to be enhanced through deliberation, although some theorists

suggest that regardless of how it is perceived, the process is inherently legitimizing. Still, it is difficult to conceive of inherent legitimacy benefiting a democratic society without also being perceived as such by its citizens. Moreover, inherent legitimacy is not observable; empirical researchers must, of necessity, study perceptions or some other manifestation of legitimacy.

But all of this is not to say that the distinction between these two lines of research is murky. The key difference is that, in normative political theory, the activity described as deliberation is *assumed* to have certain beneficial outcomes, and in empirical research, it is *hypothesized* to have those same desirable outcomes. Hypotheses often turn out to be wrong, but assumptions, by their very nature, cannot be.

So although deliberative theorists may argue that they are not trying to make empirical predictions so much as “clarifying a normative standard” (Neblo 2005, p. 172), deliberative theory does make implicit empirical predictions, and empirical research does incorporate implicit normative commitments. The challenge for advocates of deliberative democracy is to persuade skeptics who want evidence, rather than theoretical logic, that people will, on average, be better citizens if they take part in deliberation (see Przeworski 1998, p. 142).

This general challenge is one that requires empirical research. Having established some common ground that might form the basis for bridging theoretical and empirical approaches, I next examine how well deliberative theory fits the requirements of a useful and productive theory for purposes of empirical social science.

WHAT MAKES GOOD EMPIRICAL THEORY?

The most general requirement for productive empirical theory is falsifiability, which is deemed a minimal requirement for a productive social theory. In a nutshell, for a theory

to be falsifiable, it must be the case that if a study were set up in a particular way, its results could conceivably contradict the predictions of the theory. Although no single empirical test ever completely refutes or “disproves” a theory, it must be possible that evidence could counter the theory’s assumptions, and a steady accumulation of such negative evidence would build a convincing case against it.

Falsifiability is probably the single most intransigent issue in getting normative theory and empirical research to speak to one another in the realm of deliberative theory. Several problems conspire to make deliberative theory elusive in this respect. For some theorists, deliberation is simply defined as intrinsically good. Obviously, such a claim renders empirical research irrelevant (see, e.g., Stokes 1998). But even without the assumption of intrinsic goodness, more complex problems hinder the interaction between empirical studies and political theory.

It is difficult to envision an empirical test that might produce evidence construed by theorists and empiricists alike as disconfirming the claims of deliberative theory. This is because deliberation falls short on many of the standards deemed essential to good social science theory, at least as the theory is currently construed. Beyond the general issue of falsifiability, deliberative theory falls short of meeting three requirements for productive social theory that are enumerated in virtually any textbook:

1. clearly defined concepts;
2. specification of logical relationships among concepts within the theory;
3. consistency between hypotheses and evidence accumulated to date.

It is, of course, unfair to criticize a normative theory for lacking the characteristics required of productive social science theory. But criticism is not my main purpose. Instead, I want to take seriously the admonition that the two subfields should talk to one another. To make a dialogue possible, this normative theory must be translated into the terminology

of empirical social science and must then be subjected to the standards of theory testing within the social science tradition. It is crucial to address these three problems in order to accumulate useful empirical evidence on the potential of deliberative democracy.

Social scientists generally define “theory” as a set of interrelated statements intended to explain and/or make predictions about some aspect of social life. Toward those ends, a good theory is supposed to have well-defined constructs of general theoretical interest. It is supposed to describe logical associations among these constructs (which are most often causal associations), and it should allow for connections between the theoretical constructs and observable entities. When theories cannot meet these three criteria, they are generally unproductive in advancing our understanding of the phenomenon of interest.²

What happens when empirical researchers attempt to translate deliberative theory into these terms? First, as Thompson points out, they discover a great deal of conceptual ambiguity as to what should qualify as deliberation. Moreover, the definitions offered by theorists frequently conflate causes (criteria defining deliberation) and effects (its beneficial consequences). Second, the tests of deliberative theory offered to date typically do not develop well-specified explanations for the relationships between deliberation and its many proposed benefits. Third, deliberative theory is inconsistent with much of what is already known about political discourse in group contexts. Many, though not all, of the hypotheses that flow from the deliberative framework are not well-grounded in either previous theory or empirical evidence.

Below, I discuss each of these three problems. I then turn to the more constructive business of suggesting how empiricists might more productively approach deliberative theory in the future.

CLEARLY DEFINED CONCEPTS

Deliberative theory includes many concepts that have generated tremendous research interest; this is precisely why it has become so popular. Concepts such as reason-giving and equal participation and so forth have become valuable constructs in empirical theory and research. But it would be a stretch to claim that deliberation is itself a well-defined concept. In fact, it may be fair to say that there are as many definitions of deliberation as there are theorists, although there are certainly commonalities among these conceptualizations as well.

In empirical social science, conceptual definitions are often arbitrary. They must fit logically within the broader theoretical framework of relationships proposed, but it is widely recognized as a senseless and unproductive exercise to haggle about what qualifies as true deliberation. However, to the extent that any given researcher specifies necessary and sufficient conditions, we at least have a sense of what qualified as deliberation for this particular study, and what its consequences were. The pattern of findings over time thus gradually reveals which elements in the definitions best produce which consequences.

As Thompson (2008) points out, theorists disagree on many elements defining deliberation, such as whether deliberation must culminate in a binding consensus, whether it must necessarily be public, or whether informal conversations among ordinary people qualify. Indeed, much of the normative literature is concerned with refining these definitions and debating what should or should not be required.

Although it may seem desirable to let a thousand flowers bloom in this regard, if we cannot agree on what the independent variable is, we cannot hope to systematically evaluate its impact. Interestingly, the number of conceptions of deliberation is surpassed perhaps only by the number of versions of social capital, another concept that has intrigued both theorists and empiricists. Perhaps a

²For further descriptions of these criteria for productive empirical theory, see Hoyle et al. (2002) or Babbie (2001).

certain amount of conceptual ambiguity is inherent in extremely rich concepts. Whatever the cause, the lack of agreement about what constitutes deliberation makes it extremely difficult for empirical researchers to address the claims of normative theory. How can one safely assert that deliberation has occurred when there are no necessary and sufficient conditions routinely applied to this concept? For those who study political discourse as it occurs in real-world contexts, how can one decide if the type of discourse that transpired qualifies?

For theorists, this lack of agreement and uneven stipulation of definitions is less troubling. But for those who want to know whether deliberation produces its promised benefits *before* they sink millions of dollars of foundation money into encouraging more of it, the uncertainty is problematic indeed. Thompson's (2008) review of what should and should not qualify according to normative theory illustrates a desire not to exclude, but in so doing renders deliberation a far less useful concept for empirical research than it might be. For example, Thompson suggests that ordinary political discussion should be distinguished from the decision-oriented talk that constitutes deliberation. But this argument is seemingly contradicted by the subsequent suggestion that "maintaining this distinction should not be taken to imply that other forms of discussion are somehow less worthy of a place in deliberative democracy, but we can more clearly retain the connection to the central aim of deliberative theory if we treat those other activities as part of a larger deliberative process, rather than instances of deliberation *per se*." Likewise, Thompson suggests that although like-minded discussion does not qualify as deliberation, "[T]hat is not to say that discussion among like-minded people cannot contribute to deliberative democracy."

Empirical researchers attempting to test deliberative theory can be forgiven for wanting to bang their collective heads against a wall in reaction to definitions of this kind. What does it mean to say that something is not part

of deliberation but is part of the larger deliberative process? And if one theorist's version of normative theory includes the requirement of consensus decision-making whereas another's does not, then how do social scientists design studies that address the implications of deliberative theory?

It is commonly claimed that empirical studies do not fully embrace deliberative theory, and of course this statement is entirely correct. No study could include all criteria invoked by all theorists collectively, and to do so would violate even other theorists' conceptualizations of deliberation. Thus, the conversation between theorists and empiricists is next to impossible if one aims to produce research that can be used to decide whether to pursue deliberation at all, or whether such practices need refinement in order to work beneficially. The common problem faced by empirical researchers is that when benefits are not found from a given conceptualization of deliberation in a particular study, the null findings are as easily attributed to the operationalization of deliberation as to the theory itself. Given this state of affairs, it is difficult to envision disconfirming evidence that would be widely accepted as such.

Theorists are loath to exclude many kinds of political talk from the deliberative framework; in fact, the trajectory has been toward progressively greater inclusiveness, incorporating emotional as well as rational appeals, informal speech as well as rule-bound discourse, and so forth. This very openness delays progress in understanding deliberation's consequences. If the deliberative umbrella is too broad, then it is not clear how deliberative theory can be differentiated from any of dozens of other theories. Indeed, much of the literature cited in overviews of evidence on deliberation does not purport to be about deliberation so much as about persuasion, social interaction, procedural fairness, etc. (see, e.g., Delli Carpini et al. 2004). Nor is it clear what a given confirmation or disconfirmation says about deliberative theory. A more narrowly specified independent variable might

better serve progress toward understanding how to achieve the ends sought by advocates of deliberation.

In short, my quarrel is not with how theorists have chosen to define deliberation but with the fact that the concept itself is a moving target. If every theorist's definition is somewhat different from the next, then it is impossible to study deliberation in a way that theorists collectively find relevant to their work. Upon encountering an unsupportive (or supportive) finding, it is far too easy to dismiss it as uninformative because the deliberation that took place in that particular study did not satisfy all of the prerequisites offered collectively by deliberative theory, even if it did satisfy some theorists' definitions.

The solution that theorists have generally offered is not a clear definition of this phenomenon but an evaluative distinction between "good" deliberation and "bad" deliberation. If we grade the many forms of deliberation along a continuous scale from good to bad, then we can predict that more beneficial consequences will result from good deliberation than from bad. To the extent that good deliberation actually brings about more of the beneficial consequences than bad deliberation, we can conclude that deliberation is delivering the benefits that the theory promises. The more that political discourse approaches the ideal of equal opportunities to speak, for example, the more it will bring about the proposed benefits. The more reason-giving that occurs, the more valuable should be the consequences of this activity. Fishkin (1995, p. 41) calls this continuum "incompleteness":

When some citizens are unwilling to weigh some of the arguments in the debate, the process is less deliberative because it is incomplete in the manner specified. In practical contexts, a great deal of incompleteness must be tolerated. Hence, when we talk of improving deliberation, it is a matter of improving the completeness of the debate and the public's engagement in it, not a matter of perfecting it. . .

It is unclear, however, at what point a process of this kind is so "incomplete" as to be irrelevant to the study of deliberation. Moreover, the logic behind the idea of a continuum of predictions is not as simple as it first appears. For example, should bad deliberation merely produce fewer beneficial effects than good deliberation? Or should bad deliberation produce deleterious effects, such that bad deliberation is worse than no deliberation at all? Moreover, are some evaluative standards more important than others, such that no beneficial consequences should be expected unless some minimal conditions are first met?

Because so many different criteria have been proposed for the deliberative ideal, using evaluative standards is unfortunately no easier than establishing clear conceptual criteria. In practice, good deliberation is often defined as deliberation that produces the desired consequences outlined in the theory. This circularity makes it impossible to use this approach to evaluate the claims of deliberative theory.

A related confounding of cause and effect manifests itself in two different kinds of claims involving deliberation and its consequences. The more obviously difficult situation is when the independent variable (deliberation) is defined in terms of its hypothesized effects. As Elster (1998, p. 9) notes, empiricists tend to be interested in "whether and when the empirically identifiable phenomenon of discussion has good results, rather than to define it such that it is intrinsically desirable." Theorists are more likely to treat deliberation as something to promote rather than evaluate. As Fearon (1998, p. 63) notes, to facilitate meaningful empirical claims about deliberation, "we should keep distinct (a) arguments for why more deliberation would be a good thing and (b) arguments that in effect *define* deliberation or 'deliberative democracy' so that these entail good things."

A second source of confusion in understanding the consequences of deliberation is studies that "test" deliberative theory by focusing on the extent to which political discourse meets some set of qualifications. Based

on such assessments, some scholars infer various benefits from the quality of the discussion. Just as an analysis of the content of a political advertisement tells us nothing about its effects on voters, the content of deliberation tells us nothing about whether it changes its participants in the directions theorists hope. More importantly, this confusion means that those claiming to “test” or “evaluate” deliberative theory are often testing completely different hypotheses. For example, some of the “tests” of deliberative theory identified by Thompson (2008) are examinations of whether political discussion in a particular time or place meets the standards to be considered deliberative. Does the discussion involve reason-giving, equal participation, and so forth? Other studies also reviewed as empirical tests of deliberative theory evaluate whether, once discussion does meet one or more standards for deliberation, it produces any of its theoretically claimed benefits.

These are two very different research questions, and their conclusions are logically independent of one another. A given instance of political discourse might meet all of a given set of requirements for deliberation and yet still not produce the benefits that have been assumed. Likewise, political discourse might not meet the criteria for deliberation but still produce some of the beneficial consequences claimed by deliberative theory. For example, in my social network studies (see Mutz 2002), I find that exposure to cross-cutting political discourse produces greater tolerance and greater awareness of rationales for oppositional political views. These effects result from exposure to oppositional political views even without all the trappings of deliberative interaction. In our study of political discussions in the American workplace, Jeff Mondak and I similarly find that people are influenced in the direction of political tolerance and greater awareness of the rationales for oppositional views simply by listening to their coworkers talk about their political views (Mutz & Mondak 2006). No one would call such experiences deliberation; participation in

the conversation is not even necessary. Yet understanding the kinds of benefits that derive from simply listening to others is central to understanding the benefits of the deliberative process as a whole (Mutz & Mondak 2006, Mondak & Mutz 2006).

Most theorists and empiricists agree on the general point that deliberative encounters are nearly always “incomplete” to some degree, if for totally different reasons. No one claims that the ideal speech situation exists in flesh and blood. Instead, according to theorists, deliberation is “still in large part a critical and oppositional ideal” (Bohman & Rehg 1998, p. 422). It is a useful ideal type, even if it is never realized. But, as empiricists are likely to question, is it wise to work toward the realization of a goal that we are uncertain will prove beneficial?

In my view, whether an instance of discourse meets a set of criteria to qualify as deliberative is irrelevant for theorists and empiricists alike, though for somewhat different reasons than Thompson (2008) suggests. He notes: “Although critics repeatedly brandish the findings of inequality to declare deliberative democracy fatally flawed, most deliberative theorists are neither surprised nor discouraged. . . . Research that shows specifically what conditions and changes might mitigate inequality can be useful.” Arguably, this approach puts the cart before the horse. Before we set a goal of equality within a deliberative context, or attempt to change decision-making processes to meet any other deliberative standard, we should first have evidence that the theory works as advertised and that these particular standards are crucial to its beneficial outcomes. To extend this example, until we know that moving closer to a goal of equal participation will ultimately produce more beneficial goods, setting such a goal seems premature. At this point in the development of this research, evaluations of the quality of existing instances of deliberation are irrelevant to empiricists as well, but not because they can always be made more ideal. Knowing what a given instance of deliberation is

like simply cannot tell us anything about its consequences.

The fact that the ideal conditions do not exist introduces a painful circularity into studies that attempt to test whether deliberation produces any of the benefits that are theoretically predicted. If negative evidence is produced by a study that attempts to look at the consequences of deliberation, such evidence is easily dismissed because the discussion in question did not meet all of the necessary and sufficient conditions to qualify as deliberation. Once again, deliberative theory is rendered unfalsifiable.

To summarize, studies of the quality of deliberation should not be mistaken for evidence of its beneficial effects. Likewise, deliberation cannot profitably be defined according to the effects it is anticipated to produce. Tests of whether the conditions necessary for deliberation are actually occurring must be differentiated from evaluations of whether benefits accrue when the conditions do occur. The latter is an attempt to test deliberative theory, whereas the former is purely a descriptive exercise that will not facilitate future predictions about the effectiveness of deliberative decision-making processes.

As Thompson (2008) summarizes, “The conditions under which deliberative democracy thrives may be quite rare and difficult to achieve.” But Thompson also suggests that empirical researchers focus on identifying conditions under which deliberative theory works, for purposes of figuring out how we can *make* it work by changing unfavorable conditions to favorable ones. At this juncture, the traditions of normative theory begin to befuddle the empiricist. This is because deliberative democratic theory is unabashedly a social movement as well as a theory. Its advocates promote it not only as a pet theory but also as a social cause. Whether and why it works to bring about the ends that deliberative theorists seek remains to be seen.

Rather than “making it work” or searching long and hard for conditions under which it might be made to work, I suggest a some-

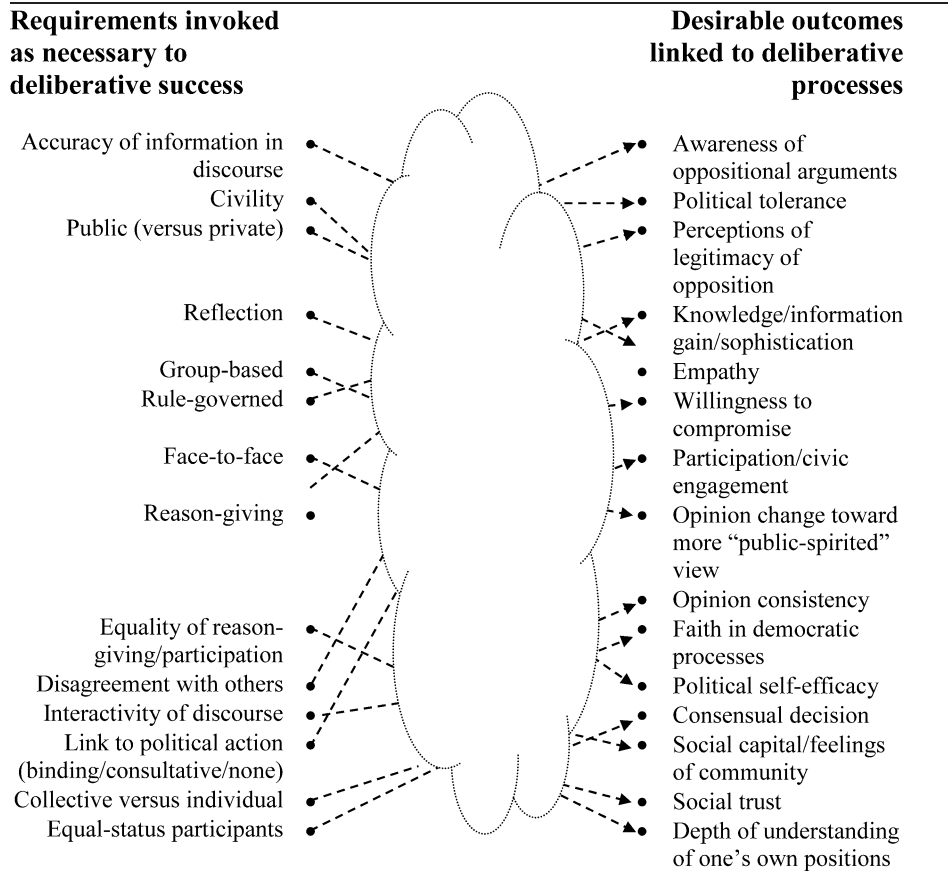
what different tack for social scientists, one that slowly but progressively accumulates evidence by testing parts of the theory while also leaving open the possibility that these ends might be better achieved another way. Although deliberative theory is full of inspiring and promising ideas, researchers should be under no obligation to “make it work” if the conditions under which it is beneficial turn out to be rare. If there is an easier, more efficient way to achieve some of these same ends, scholars should be open to these possibilities. The important question is not whether deliberative processes can be forced into a mold that will produce positive consequences, but whether this decision-making process is the best way to achieve the desired ends relative to other plausible possibilities.

SPECIFICATION OF LOGICAL RELATIONSHIPS

In addition to requiring well-defined concepts, empirical theories also require that relationships between concepts be specified in a logical manner. Assuming for the moment that we have a set of necessary and sufficient conditions that constitute deliberation, and thus lack of clarity is no longer a problem, two additional complications come into play. One problem stems from lack of attention to the internal logic of these relationships, and the second involves the nature of the assumed relationships between deliberation and its benefits in this multifaceted theory.

As reflected in Thompson’s (2008) article, theorists are realistic enough to understand that empirical tests of deliberation cannot be limited to the infrequent achievement of these ideals. Partial successes can also produce empirical evidence that speaks to the theory. As Thompson suggests, “The closer the actual deliberation comes to meeting the standards, the better it is in terms of deliberative theory.” This frequently unstated assumption has allowed at least some research to be characterized as empirically “testing” deliberative theory. We do not need to wait for status

Table 1 Example of middle-range approach to deliberative hypotheses



differences among human beings to disappear in order to test the theory, nor for all participation to be completely equal.

Unfortunately, to date, the “black box” of deliberation has been exactly that—a morass of necessary and sufficient conditions all thrown together, without specification of why each of these various components is necessary, nor theory that links each of them to a specified desirable outcome. As illustrated by the left-hand column in **Table 1**, the list of components necessary for discourse to be deemed deliberative is extensive if one simultaneously considers all that have been suggested.

To begin to understand the impact of such a long laundry list of factors, scholars should examine the impact of just one or two of these factors at a time. One study, for example,

might tell us that moderators, by enhancing equality of participation, help produce better cooperation. Another might demonstrate that greater reason-giving by participants enhances learning of oppositional perspectives. Discussions that enforce high standards of civility might also produce higher levels of willingness to compromise. The important part of this enterprise should be to identify which characteristics of deliberative practice produce which kinds of desirable outcomes.

Many of the factors listed in **Table 1** already have been studied from this perspective. For example, experimental research suggests that a face-to-face context increases people’s likelihood of cooperation (e.g., Bornstein 1992, Sally 1995), thus validating the usefulness of that component of

deliberation in producing cooperation. But Barker & Hansen (2005) find that inducing deeper, more systematic processing of information produces *less* consistent attitudes than a control group, thus suggesting that opinion consistency is not a probable outcome of increased reflection. Yet another study focusing on consensual decision making showed that exchanging information about preferences increased consensual decision making, but it did so even without any discussion (Gaertner et al. 1999). Notably, in each of these examples, the independent variable is not deliberation per se; it is some component said to partially comprise deliberation. In addition, the chosen independent variable cannot logically be expected to have an impact on all of the dependent variables in the same, beneficial direction. A given component might produce a positive impact on one outcome and a negative effect, or none at all, on another (see, e.g., Mutz 2006).

In reality, it is highly unlikely that all of the conditions on the left side of the table are necessary for each of the outcomes on the right. Moreover, to rationalize deliberative theory as a middle-range theory, scholars need to figure out precisely what elements in the left-hand column are logically related to the consequences on the right. For example, it makes sense that people must be exposed to disagreement and reason-giving in order to develop a heightened awareness of rationales for oppositional views. It does not necessarily make sense, however, that this exchange must occur face to face, or in a group rather than a dyad. Not all of these conditions are necessary for learning to occur. Likewise, it makes sense that civility of discourse should be important if the enterprise is to increase people's willingness to compromise with one another and to increase levels of social trust. On the other hand, if the outcome of interest is greater internal consistency of opinion, then it makes little sense that civility should be required.

One can conceivably treat the two columns in **Table 1** as opportunities for lines to be drawn to indicate when there is a clear theo-

retical reason to expect a relationship between an element in the left-hand column and another in the right-hand column. But to argue that everything on the left has a theoretical relationship to everything on the right stretches believability. It is this "grand theory" approach to deliberative democracy that has undermined progress toward understanding what is important for which purposes. It is plausible that multiple conditions on the left are necessary to produce a given outcome on the right, but it is implausible that each and every factor in this list must be aligned in order to produce any consequence of value.

For example, the kind of direct, face-to-face exchange that traditionally characterizes deliberation need not occur in order for people to become better informed. There are undoubtedly easier, far less expensive means of producing that end than hosting a deliberative poll, as successful information campaigns have demonstrated (see, e.g., Klingemann & Roemmele 2007). Moreover, enhancing the depth of understanding of one's own position relative to others' probably does not require a public forum; it happens commonly in private settings as well. If one wants to enhance mutual respect among those of opposing views, then civility is probably a requirement for the discourse to be effective, but requiring that the group reach a consensus seems superfluous to this particular goal. If one envisions **Table 1** as a matching game, in which everything on the right must be matched to one or more factors on the left, then we have a primitive middle-range theory generator for purposes of deliberative theory.

By opening the black box of deliberation and sorting out the importance of its contents in relation to various consequences, empirical research could greatly enhance the capacity of deliberative theory to contribute to democratic society. Few contexts demand all of the outcomes listed on the right side of **Table 1**, so advocates of deliberative encounters would simply need to decide what was most important to achieve in a given context and design accordingly. In addition, by paring

down the list of required conditions for any given outcome, advocates of deliberative democracy could more efficiently and realistically achieve those goals. But this can only happen if empirical scholars consider deliberation as one small group of requirements at a time. Holistic consideration of the concept is clearly an impediment to advancing this kind of understanding.

I suspect that, in the long run, deliberative theory may end up with a research history similar to that of intergroup contact theory. Initiated by Allport (1954), this influential social theory suggested that contact among members of different racial groups would lead to greater mutual understanding and reduce prejudice. Allport's version of the theory began with four necessary conditions for the type of contact that would prove beneficial. Unfortunately, it accumulated so many additional necessary conditions over the years that eventually it was deemed a failure because the laundry list of requirements simply became too long to be feasible. The theory's extensive requirements made it seem impractical as a means of reducing prejudice and stereotyping. Even if intergroup contact theory could work under some highly specific conditions, those conditions were thought to be so infrequent and difficult to achieve that researchers turned elsewhere for ideas on how to improve intergroup attitudes. As we now know with the benefit of hindsight, this abandonment was premature. More recently, some social scientists have begun to realize that many of the "necessary" conditions—including Allport's original four—were actually not essential for intergroup contact theory to work. Although these additional conditions facilitate more optimal outcomes, they are not necessary for contact to result in more positive intergroup attitudes. Thus the theory was brought back into the realm of practicality and became a topic of renewed research interest (see, e.g., Pettigrew 1998, Pettigrew & Tropp 2000).

Likewise, some scholars have already thrown their hands up in the face of the

long list of requirements for effective deliberation, arguing that they are impossible to achieve and/or unlikely to produce benefits even if achieved (e.g., Sanders 1997). This reaction is unfortunate because some subset of these necessary conditions (or combinations of them) are probably effective in producing some of these consequences. Empirical researchers simply have not done a good job of figuring out which are essential. It would be unfortunate to have the baby thrown out with the bathwater, but this is precisely what happened with intergroup contact theory.

As with the development of most theories of human behavior, a second step in this process of building useful empirical knowledge would involve progressively more complicated research designs. Most human behavior is sufficiently complex that mere additive models are unlikely to account for it. Instead, it is likely that many of the factors in the left column of **Table 1** interact with other factors in influencing these outcomes. Thus, it would not be logical to ask how well a given deliberative encounter stacked up on all of the factors, create a combined score of deliberative goodness, and expect more beneficial outcomes associated with higher scores. This approach would be parsimonious, but it is unlikely to work because (a) not all necessary conditions are critical for producing each consequence, and (b) many of the factors are likely to interact with one another.

Imagine, for example, political discourse that involved extensive reason-giving but also tremendous incivility. Would it be logical to anticipate that this combination would produce beneficial consequences such as greater willingness to compromise? Wouldn't one logically expect the various components to interact with one another such that some components (e.g., reason-giving) without another condition (e.g., civility) could have worse results than no deliberation at all? Interactive rather than additive relationships are the norm when modeling complex human behaviors (e.g., Kam & Franzese 2007).

If the real hypothesis of interest here concerns interaction effects—that is, when political discourse simultaneously meets some group of these conditions—then it cannot be tested without identifying deliberative situations that include all of these conditions. As recounted above, theorists may well argue that a given instance of deliberation produces legitimacy to the extent that it merely approaches its ideals, in which case perfection of this process is beside the point. But surely there is some minimal level of each of these criteria that must be met to produce beneficial consequences of some kind, a “tipping point,” before which many of these outcomes are unlikely. If this is not the case—if certain minimal conditions do not need to be met—why include these criteria as part of deliberation’s necessary and sufficient conditions to begin with?

Assuming that researchers are interested in the question of whether societies are better off investing lots of time and effort in perfecting deliberative decision-making processes, and given the agreed-on impossibility of achieving *all* of these necessary and sufficient conditions, how can we proceed? If we could safely assume that the factors identified as constituting deliberation contribute via independent, additive relationships to the desired outcomes of deliberation, then the potential for this kind of research would be far more promising. Unfortunately, this does not seem likely. Nonetheless, empirical studies of deliberation would do best by starting small, examining one or two of the necessary conditions relative to specific consequences that follow in a theoretically logical way. If all variables are thrown in at once (reason-giving talk among equals with moderators to ensure equal, rule-governed participation, and a binding consensus at the end, versus no encounter at all), then the results will not help us streamline deliberative theory down to its essential elements. If the theory retains as much baggage as it currently carries, it risks squandering its ultimate potential.

CONSISTENCY BETWEEN HYPOTHESES AND PREVIOUS EVIDENCE

Textbook descriptions of good empirical theory routinely include the requirement of consistency with previous evidence. This requirement is intended to help move theory and research forward more quickly and efficiently. Why waste time proposing a theory for which there is already a substantial body of contradictory evidence?

As an empirical theory, deliberative theory has been widely criticized for making assumptions that seem to fly in the face of what scholars already know about human behavior. By this, I do not mean that previous research suggests that humans cannot approximate deliberation, but rather that the implied consequences of such participation are unlikely based on what we know about the consequences of human communication. In other words, even if we achieved the hypothetical ideal speech situation, it would not lead to the kind of outcomes that the theory envisions. Instead, previous theories tell us that information processing is influenced by characteristics of the listener or message recipient, the speaker, the message itself, and the context in which the deliberation occurs. Notably, only one of these sources of influence (the message) should matter in a theory where “the force of the better argument” is assumed to carry the day. Although a single article cannot possibly review the large body of evidence relevant to this particular weakness of deliberative theory (but see Krupnikov et al. 2007 for an attempt), for purposes of illustration I provide a few examples of empirically well-supported theories of human behavior that appear inconsistent with deliberative theory. In these selected examples, factors other than argument quality are influential in bringing about opinion change, thus casting doubt on the idea that even well-reasoned, sincere, persuasive argumentation can bring about superior decision-making processes, more consensual decision-making,

and so forth. To reiterate, the theories and evidence that I review here do not speak to whether deliberation itself is feasible, but rather to whether, even if we do manage to coax it into existence, its consequences are likely to be as advertised. If people were all of equal status, if women spoke out as often as men, and if people were respected equally regardless of educational level, rhetorical skills, and so forth, can we be assured that deliberation would work? Very few social scientists could offer assurance in this regard. Within the social sciences, large-scale, federally funded research on persuasion was instigated around the turn of the century in response to concerns that America (and her enemies) could be persuaded by other than rational means, particularly by means of war-related propaganda (see, e.g., Sproule 1989). The outgrowth of research on persuasion in subsequent decades was not particularly reassuring; people could not always be counted on to resist irrational arguments or to ignore superfluous cues. What followed was a cataloging over many years of the sources of bias in information processing, including confirmatory bias (Lord et al. 1979), perseverance bias (Ross et al. 1975), and responsiveness to peripheral cues (Petty & Cacioppo 1981), to name just a few.

Within political science, the most commonly investigated source of bias in processing new information is the person doing the processing. Deliberative theory typically assumes that people come to the table with opinions and that they are willing to justify those views publicly in a way that brings people's views closer together rather than increases conflict. The problem with this assumption is that people with different pre-existing opinions and partisan orientations are unlikely to respond the same way to a given argument, regardless of its inherent rationality and appeal.

In a deliberative encounter, given the requirement of respectful attention, we should assume that people will not be able to selectively expose themselves to different types of information. Unfortunately, people may

still selectively interpret the implications and importance of new information, typically so that it does not threaten their initial predispositions. In the earliest empirical studies of the impact of information on mass opinion, Campbell et al. (1960, p. 133) noted, "Identification with a party raises a perceptual screen through which an individual tends to see what is favorable to his partisan orientation." Subsequent research has accentuated the importance of this original observation. The now extensive literature on selective processing of information calls into question the idea that deliberation, through the force of rational argument, will gradually bring people closer together and make mutually agreeable compromise possible (see Bartels 2002, cf. Gerber & Green 1998, 1999). When new information enters an environment, opinionated citizens tend to adjust their views in the same general direction, but they seldom converge—even when the new information seems to have obviously unidirectional implications for the issue at hand. Of course, open-mindedness is also a prerequisite in some definitions of deliberation, which might seem to eliminate the potential for this problem. But so long as people hold initial opinions on an issue, as is true of most issues worth discussing among the public, their information processing is likely to be influenced by them. People need not be closed-minded and dogmatic in order for biased processing to be problematic.

Whether social scientists like it or not, deliberative encounters are inevitably social situations. Whenever people interact with one another, they will inevitably have many motives beyond simply the desire to reach the best policy position. They also want to be perceived as likable and smart, for example. Models of political reasoning must consider that political reasoning is often motivated by goals other than accuracy (e.g., Taber et al. 2001).

Most organizers of deliberative events go to great lengths to assure us that the information provided is valid and unbiased toward any particular outcome, but faith in the deliberative enterprise rests on believing that

organizers and moderators have somehow overcome their own biases and also counteracted social psychological biases among their participants. Their efforts to ensure more deliberative group dynamics are admirable, yet many possible dynamics are unlikely to be recognized based on casual observation. And even when people are motivated purely by a desire to reach the best, most accurate conclusion with their fellow deliberators, they are still subject to conscious and unconscious biases as they process what they hear. These biases call into question whether the process of persuasive argumentation will necessarily lead to a better outcome. For example, if one person claims to have a larger number of arguments than another, he or she will be more persuasive, even when both people in fact give the same number of arguments (see Petty & Cacioppo 1981, Chaiken 1987). In addition, even if everyone in the deliberative encounter views one another as equal in status, it is likely that some will attribute their views or arguments to entities of higher status who are not present (e.g., God), thus making it impossible for the argument to stand solely on the force of its own merit (see, e.g., Petty & Cacioppo 1981).

Disagreements—a core requirement in deliberative settings—appear to trigger greater stereotyping of out-group members (see Sinclair & Zunda 2000). This phenomenon allows the in-group member to dismiss the out-group member's views, thus preventing the kind of mutual understanding that is a central deliberative benefit. So even if well-educated, older, white males were not more likely to participate in political discussion, and all contributed equally, respectfully, and so forth, there would still be a tendency for the in-group arguments to hold sway over those of out-groups.

In addition, speaking first or last in a given discussion can bias the extent to which one's arguments are memorable and influential in shaping opinions (see Haugtvedt & Wegener 1994). Those who go first are in a better position to define the terms of the debate, framing

how it is discussed substantively in a way that favors their own perspective.

Entire textbooks are devoted to the enormous list of potential sources of bias in information processing and attitude change, thus I cannot go into all of them here. Today's models of biased processing are typical of contemporary political psychology in that they share an underlying skepticism that information is the cure for all that ails the quality of political decisions. If people are not passive recipients of information, but rather active choosers, interpreters, and rationalizers, then the benefits of both information and deliberation—however closely controlled—are limited. It is a tall order to expect leaders and moderators to be able to control all of these interactions and potential sources of bias, particularly when many are not easily observed or anticipated. Moreover, any system whose usefulness depends wholly on locating disinterested, nonpartisan moderators is in trouble to begin with.

Upon reviewing even this small sample of evidence, it is clear that there are many established theories of group dynamics, communication, and persuasion that are inconsistent with deliberative theory. These are not theories merely suggesting that people do not measure up to deliberative standards when they engage in political discourse; rather, these theories call into question the likelihood of beneficial results even when people do achieve such lofty goals. Clearly, given its lack of consistency with previous evidence, deliberative theory does not meet this particular standard for productive social theory.

HOW MIGHT WE MOVE FORWARD?

It is difficult to exaggerate the current enthusiasm for deliberation. The amount of time and money invested in it by governments, foundations, and citizen groups is staggering relative to virtually any other current social science theory. Now that thousands of local and

national deliberative forums have been held, one would expect to know far more than we do about when and why it works well to produce various outcomes.

As someone who has studied elements of deliberation in limited ways over the years, I believe the theory has generated important empirical scholarship. But I remain uneasy with the progress that empiricists have made in understanding what actually transpires in such complex situations. In many ways, we are victims of our own desire to live up to the requirements of normative theorists in testing this theory. Scholars typically try to put all of a particular list of required conditions into a single deliberative encounter to study. Doing so might indeed make deliberative theorists more likely to take note of the findings, but in the end scientific progress on deliberative theory will suffer as a result. Thus, ultimately, I advocate dropping tests of “deliberative theory” *per se* in favor of testing middle-range theories. This avenue is likely to be a more productive means of evaluating the theory, and one that allows us to remain focused on essentials.

I am not suggesting that work must conform to this approach in order to contribute significantly to academic knowledge. Instead, I am suggesting that without a systematic framework along these lines, there is little hope that empirical research will usefully speak to deliberative theory, nor that theory will speak to practice.

It is informative that in his introductory section, Thompson (2008) mentions “a profusion of empirical studies, now more numerous than the normative works that prompted them,” whereas empirical scholars suggest that this area of study is in its infancy, and “not yet very rich or deep” (Ryfe 2005, p. 64). Likewise, Delli Carpini and colleagues (2004, p. 316) conclude, “Unfortunately, empirical research on deliberative democracy has lagged significantly behind theory.” Despite the seeming contradiction, both of these claims have the ring of truth. Although empirical work has indeed proliferated, it is not clear

what we have learned as a result. Research on deliberation suffers from (a) too many necessary and sufficient conditions, which are each insufficiently well-specified concepts; (b) a lack of specification of the relationships among the parts comprising the deliberative whole, and their theoretical linkage to the desired outcomes; and (c) a lack of a “control group,” that is, a baseline for comparison with other modes of decision making, or of achieving better-informed, more enlightened opinions, or of increasing willingness to compromise, or of attaining any other benefit attributed to deliberation (see Johnson 1998 and Schauer 1999 for a discussion of deliberation relative to alternatives). Deliberation is not the only means of pursuing these various ends, and advocates and detractors alike should find it useful to know how effective it is relative to other means.

It is in some ways unfortunate that deliberative theory is a cause célèbre for its advocates, as well as an important social theory. I say this not because I anticipate that it will necessarily have negative effects on democracy when implemented, but rather because once a phenomenon acquires such a head of steam as the deliberative democracy movement has, it seldom slows down for purposes of advancing scientific understanding. Instead, there is a rush to implement deliberative encounters willy-nilly, because advocates genuinely believe that its consequences must, of necessity, be beneficial. Just as drug companies cannot be counted on to publicize the negative side effects of their drugs, advocates—whether individuals or large organizations—who have invested huge amounts of time, energy, and money into organizing and promoting deliberation are not likely to be the first to perceive, let alone publicize, any shortcomings. Thus, whether the consequences of deliberation are, in fact, consistently beneficial or not, without careful, methodical study, we will not know why in either case.

Attention has now turned to large-scale, institutional implementation of deliberative practices. These projects are not oriented

around the best possible research designs for purposes of understanding what deliberation can and cannot deliver so much as they are designed to spread an already accepted practice as widely as possible. I think this kind of action is premature.

DISCLOSURE STATEMENT

The author is not aware of any biases that might be perceived as affecting the objectivity of this review.

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