Can Deliberative Minipublics Influence Public Opinion? Theory and Experimental Evidence

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Sean Ingham[1] and Ines Levin[2]

## Abstract

Deliberative minipublics are small groups of citizens who deliberate together about a policy issue and convey their conclusions to decision makers. Theorists have argued that deliberative minipublics can give observers evidence about counterfactual, "enlightened" public opinion—what the people would think about an issue if they had the opportunity to deliberate with their fellow citizens. If the conclusions of a deliberative minipublic are received in this spirit and members of the public revise their opinions upon learning them, then deliberative minipublics could be a means of bringing *actual* public opinion into closer conformity with *counterfactual*, enlightened public opinion. We formalize a model of this theory and report the results of a survey experiment designed to test its predictions. The experiment produced evidence that learning the conclusions of a deliberative minipublic influenced respondents' policy opinions, bringing them into closer conformity with the opinions of the participants in the deliberative minipublic.

The ideal of a deliberative democracy, in which public opinion reflects the exchange of reasons and deliberation among free and equal citizens, is widely embraced, but its practical implementation raises hard questions. One solution involves "deliberative minipublics": small groups of ordinary citizens who deliberate together about a policy issue before reaching a binding decision or conveying their opinions to decision makers. Deliberative minipublics are not mere thought experiments; in the past two decades, dozens have been convened for various purposes, from the review of ballot initiatives to the design of constitutional reforms. The most studied examples include James Fishkin's Deliberative Polls, the 2004 British Columbia Citizens' Assembly (BCCA), and the Citizens' Initiative Review (CIR) in Oregon. These and other deliberative minipublics continue to generate debate among scholars (Chambers 2003; Dahl 1989; Fishkin 2009; Fung 2003; Gastil 2000; Goodin and Dryzek, 2006; Lafont 2015; Niemeyer 2011; Warren and Gastil 2015; Warren and Pearse 2008).

How might deliberative minipublics advance the values deliberative democrats embrace? By definition, not all citizens participate in deliberative minipublics. Nonetheless, they could in theory have effects on public opinion similar to more inclusive deliberative processes. This is because deliberative minipublics purport to reveal the counterfactual "enlightened" public opinion—what the public would

think about an issue if they had deliberated and informed themselves about the issue. Their incorporation into the democratic process could have a salutary effect on *actual* public opinion because their policy conclusions could have recommending force for outside observers, serving as imperfect, but informative *signals* of what observers themselves might conclude about a policy if they had the same opportunity to engage in meaningful deliberation as the members of the deliberative minipublic. Outside observers who treat their conclusions as such signals would adjust their opinions accordingly.

In many cases, the ability of deliberative minipublics to influence the opinions of outside observers—either the public or policymakers—is the only mechanism by which they could possibly affect policy, as they are rarely vested with formal decision-making power. Thus, whether they have this ability is crucial to any assessment of their potential role in the democratic process. Despite its importance, only a few studies have addressed the question.

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This paper makes two contributions. The first is theoretical. Political theorists have given informal explanations for why deliberative minipublics might influence the opinions of outside observers. Building on this literature, we formalize a model of a signaling mechanism for a deliberative minipublic's influence. The model clarifies the reasons for expecting deliberative minipublics to influence public opinion and shows that these reasons are more widely applicable and robust than previous informal discussions of the theory would lead one to believe. The second contribution of the paper is empirical. Hardly any studies have tested the expectation among normative theorists that deliberative minipublics could influence the general public. We designed a survey experiment to test for this ability. The survey experiment tested whether learning about a real-world deliberative minipublic and its policy conclusions influenced respondents' policy opinions, as our signaling theory of deliberative minipublics predicts. The results of the experiment provide evidence that deliberative minipublics *do* have the ability to influence public opinion. This conclusion comes with a caveat, however. Among observers who are also made aware of where the major parties stand on the policy issue, the influence of deliberative minipublics may be limited to independents.[1]

This caveat notwithstanding, on the whole, the evidence from the survey experiment is consistent with the signaling theory of deliberative minipublics. As with almost any experiment, questions remain about the causal mechanism at work, and whether the particular mechanism hypothesized in the signaling model was responsible for the observed effect. But experimental evidence of the ability of deliberative minipublics to influence public opinion is important—and scant—even if further research is needed to determine the causal mechanism at work.

## Deliberative Minipublics in Theory and Practice

The idea of a deliberative minipublic has generated interest among democratic theorists over the past two decades in part because of the many real-world implementations of the idea.

The best-studied example is James Fishkin's Deliberative Poll (Fishkin 1995, 2009; Merkle 1996; Sturgis et al. 2005). A random sample of a target population is invited to participate in the event, which takes place over a long weekend. Participants are paid and reimbursed for the costs of travel. They deliberate over the weekend about one or more policy issues. Their opinions before and after deliberation are recorded, and their postdeliberation judgments are conveyed to decision makers or the public. Dozens of Deliberative Polls have been conducted in multiple countries.

Two other prominent examples of deliberative minipublics are the 2004 BCCA and the CIR in Oregon. In the BCCA, 161 residents of British Columbia were recruited, almost all of them by stratified random sampling,[2] to deliberate over the course of ten months about reforms to the province's electoral system. They were authorized to submit a proposal for an alternative system to voters in a binding referendum. A majority of referendum voters approved their proposal for a version of single transferable vote, but it failed to clear the supermajority threshold the legislature had stipulated in advance (see Warren and Pearse 2008).

The CIR in Oregon, begun in 2010, uses near-random sampling to recruit, from Oregon's list of registered voters, two dozen panelists who deliberate together for five days on upcoming ballot initiatives. After deliberating, they write up a one-page statement that is included in voters' pamphlets. The statement summarizes the panel's factual findings and indicates how many members support or oppose an initiative as well as their stated reasons for doing so (see Gastil, Knobloch, and Richards 2015).

In all three of these examples, participants engage in structured deliberation, receive informational briefing materials, and hear testimony from expert witnesses or policy advocates. The BCCA had formal agenda-setting power, but in most other cases, deliberative minipublics do not have any policy-making powers. The only mechanism by which they can hope to influence policy is by influencing the opinions of decision makers or the public. Why might one expect them to have such influence?

Theorists have argued that "citizens may come to support the substantive policy findings of a minipublic because that position is the product of reasoned discussion and open participation" (Fung 2003, 352). Knowing what a group of citizens concluded about a policy after weighing expert testimony and deliberating on its merits may convey information to observers about what the observers would conclude if they were better informed. The findings of a deliberative minipublic can serve as informational shortcuts for citizens who are relatively uninformed about the policies under debate (Ferejohn 2008; Gastil 2000; Gastil 2014; MacKenzie and Warren 2012; Warren and Gastil 2015). They may serve as an alternative to partisan cues and endorsements from special interest groups. Fishkin et al. (2015) propose creating deliberative minipublics with the authority to review and publicize endorsements of ballot initiatives. The argument, in part, is that an endorsement from a deliberative minipublic could be a useful cue for voters, telling them that "a proposal got on the ballot partly because a random sample of the people thought it was a good idea after they really thought about it in depth" (Fishkin et al. 2015, 1039).

Under one interpretation of these informal claims, the core idea is that a deliberative minipublic should be able

to influence public opinion because observers will treat its conclusions as reliable *signals* of the policy-relevant information acquired by the participants in the minipublic. That a group of citizens came to support a policy after reasoned deliberation and hearing the evidence should be an informative signal—albeit a noisy and imperfect signal—that the benefits of the policy outweigh its costs. A rational observer, receiving the signal, should adjust her beliefs about its benefits and costs, and the marginal observers—those who were previously almost indifferent—should revise their preferences over the measure. We refer to this as the *signaling theory* of deliberative minipublics.

These claims may seem intuitive enough, but we believe it is useful to model them more formally. The model we present is neither especially sophisticated nor innovative by the standards of formal modeling in political science, but, all the same, it facilitates original and subtle insights into the reasons for expecting deliberative minipublics to influence observers' opinions. In particular, it helps us see that the reasons for predicting this effect are more plausible and widely applicable than informal discussions in previous literature would suggest. For example, informal theoretical discussions in previous literature leave the impression that the reason for the predicted effect is that the members of the deliberative minipublic are "like" the population of voters, in that they are an approximately random sample from the population. This statistical representativeness is offered as part of the explanation for why voters can reasonably trust the deliberative minipublic's conclusions. The model helps us think more clearly and subtly about the sense in which this thought is correct and the sense in which it is misleading. In particular, the model shows that even if observers believe they likely differ from the minipublic's members along dimensions that affect policy preferences, they might still reasonably trust the deliberative minipublic as a reliable source of information.

The model also helps us think more clearly about the situation in which observers receive signals from political parties. It clarifies the reasons a deliberative minipublic's influence should persist even in the presence of partisan cues, the reasons its influence among independents might be greater than its influence among partisans, and the reasons the partisan cues could either dampen or amplify its influence. It would be difficult to express these claims in informal, "plain" language, and they are important for assessing the results of the survey experiment.

## A Model of the Signaling Mechanism

Here is a simple story about deliberative minipublics that conveys the core intuition behind our expectation that they can influence public opinion. We focus on the same policy choice that is considered in the survey experiment described below: a ballot initiative that would lower the constitutionally mandated supermajority threshold needed for raising new taxes. Voters are unsure about how the proposed ballot initiative would affect a variable of broad, common interest, like economic growth, but they know it would lead to new taxes. Everyone prefers higher values of the variable of common interest, but voters bear different costs from the increased tax burden associated with the initiative. When the members of the deliberative minipublic support the initiative after learning about its effects, their support is evidence, for an outside observer, that the initiative's effects are better than previously expected. For it is evidence that its benefits are at least large enough to outweigh its costs in the eyes of the minipublic's members. Upon seeing the minipublic's support for the initiative, a larger fraction of observers should conclude its expected benefits outweigh its costs.

This informal story conveys the basic intuition for expecting the signal from the deliberative minipublic to affect observers' preferences, but, stated informally, it invites questions that are hard to answer without formalizing its assumptions. What if an observer believes that the members of the minipublic are more, or less, averse to increased taxes than she is, or what if she is unsure about this? What if the observer believes that the minipublic members acquire evidence of the initiative's effects, but do not learn them with certainty? And, most importantly for the survey experiment, what if the observer also knows that the Democrats support the initiative while the Republicans oppose it? Should we be less confident in the minipublic's potential to influence public opinion in this case?

To answer these questions, let us retell the story with less ambiguity. We present one particular version of the story, involving particular numbers and probability distributions; in the online supplementary materials, we give a more general, abstract rendition. Assume each voter $i$ gets a payoff of $\theta - c_i$ if the initiative is approved and a payoff of zero if it is rejected, where $c_i > 0$ is the cost to voter $i$ of the increased tax burden and $\theta$ is the initiative's unknown effect on the outcome of interest.[3] A voter supports the initiative, we assume, if she believes its expected benefits outweigh its costs, that is, if the expected value of $\theta$, conditional on the voter's information, exceeds $c_i$. Suppose $\theta$ is uniformly distributed on the interval $[0,1]$, so that an observer $i$ believes $E(\theta) = 1/2$ in the absence of further information. From our point of view, lacking knowledge of how the voter perceives the cost $c_i$ of the initiative, the probability that the observer supports the initiative is $\Pr(c_i \leq 1/2)$. What if the observer learns that the deliberative minipublic supports the initiative? For simplicity's sake, assume the

minipublic is one person drawn from the electorate, and after learning the true value of $\theta$, this person declares support for the initiative if and only if $\theta \geq 2/5$, where $2/5$ is this person's perceived cost of the increased tax burden. The observer who learns that the deliberative minipublic supports the initiative knows that $\theta \geq 2/5$, and so, given the uniform prior distribution of $\theta$, concludes that $E(\theta \mid \text{minipublic's signal}) = 7/10$. The probability that this observer supports the initiative is $Pr(c_i \leq 7/10)$. Thus, the effect of learning the minipublic's support is to increase the probability of support from $Pr(c_i \leq 1/2)$ to $Pr(c_i \leq 7/10)$. In this example, all observers for whom $1/2 < c_i < 7/10$ are "marginal" observers, in the sense that they would support the initiative if, but only if, they learned that the minipublic supported it.

The conclusion about the positive causal effect depends on neither the particular numbers used in the example nor the assumption of a uniform prior distribution. (A more abstract and general version of the story can be found in the online supplementary materials.) If the minipublic supports the initiative if and only if its benefits are sufficiently great, then an observer should make a more favorable assessment of the initiative's benefits conditional upon observing its support. Marginal observers, who previously thought that the initiative's benefits almost, but not quite outweighed its costs, should be induced to support it. We can also dispense with the assumption that the deliberative minipublic is (1) one person, who (2) becomes perfectly informed about $\theta$, whose (3) perceived cost (i.e., the threshold at which she supports the initiative) is known to outside observers, and who (4) automatically reveals her sincere preferences over the initiative to outside observers. Each part of this assumption can be relaxed without affecting the model's conclusion, as we explain in the online supplementary materials.

One value of formalizing our story is that the model identifies a reason the deliberative minipublic could have influence over public opinion even if observers believe the minipublic's participants differ from themselves. It was not assumed that the minipublic's threshold for supporting the initiative was similar to the threshold at which the median or average observer would support it. In the example, we could imagine observers are more "conservative" than the minipublic, perceiving a higher cost to the increased tax burden associated with the initiative: specifically, suppose $c_i = 3/5$ for every observer $i$. Then none will support the initiative in the absence of further information, and all will support it after observing the minipublic's support and updating their expectations to $E(\theta \mid \text{minipublic's signal}) = 7/10$. This is true even though they know the minipublic would support the initiative in circumstances in which they would oppose it, namely, when $2/5 \leq \theta < 3/5$. The claim that the minipublic might influence even observers who doubt its

members are a representative sample runs counter to the intuitions that normally guide informal theorizing about deliberative minipublics (MacKenzie and Warren 2012). But that discrepancy—between previous expectations and the expectations that the model underwrites—just underscores the value of formalizing our theoretical story. The formal model shows that the extent of similarity between minipublic participants and observers does not on its own imply anything about the minipublic's influence.

We offer this observation, not as a prediction we plan to test, but rather as an explanation for why the prediction that the deliberative minipublic will influence observers' opinions—the prediction we do test—is plausible even if we have doubts about the extent of similarity between observers and minipublic participants. (We might harbor such doubts even if organizers recruit the minipublic's participants from the population of observers using random sampling techniques, because we may worry that unobserved traits predict both willingness to participate and relevant political attitudes.) An important caveat to this claim is that the model assumes a certain kind of similarity between the minipublic's preferences and those of all observers: everyone prefers for the initiative to pass if its expected benefits are sufficiently large, and they differ only in their determinations of how large these expected benefits must be to justify the costs associated with the initiative. The deliberative minipublic never has preferences diametrically opposed to the observers'. This commonality between the minipublic and observers is of course part of the explanation for its influence. But, subject to that proviso, its influence is consistent with the minipublic's participants being prepared to support the initiative under conditions quite different from the conditions under which observers would support it.

Now consider the situation in which observers also know that the Democratic Party supports the initiative and the Republican Party opposes it. Suppose that the Democratic and Republican parties, like the deliberative minipublic, learn the value of $\theta$. Imagine Democrats support the initiative if and only if $\theta \geq 1/4$, while the Republicans support it if and only if $\theta \geq 1/2$. An observer who learns only the party positions thus infers that $1/4 \leq \theta < 1/2$. Given the uniform prior, such an observer concludes $E(\theta \mid \text{partisan signal}) = 3/8$, and the probability that such an observer supports the initiative is $Pr(c_i \leq 3/8)$. By contrast, someone who observes both the partisan signal and the deliberative minipublic's support for the initiative infers that $2/5 \leq \theta \leq 1/2$ (recall that the minipublic supports the initiative if and only if $\theta \geq 2/5$). Given the uniform prior, she concludes that $E(\theta \mid \text{both signals}) = 9/20$. The probability such an observer supports the initiative is $Pr(c_i \leq 9/20)$. Once again, the signal from the deliberative minipublic, now in the presence of the partisan signal, increases the probability
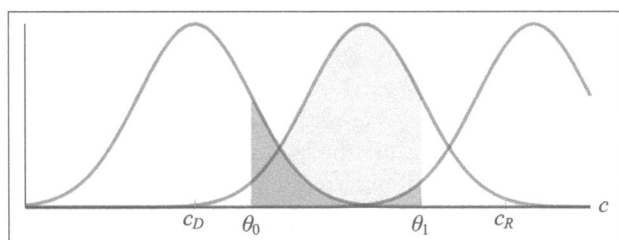
**Figure 1.** Hypothetical example of heterogeneous effects. The figure depicts a hypothetical scenario in which the distributions of the private costs of the initiative's increased tax burden differ across Democrats, Independents, and Republicans. The density functions for the three distributions are depicted. On the horizontal axis, $c_D$ and $c_R$ refer to the thresholds at which the Democratic and Republican parties support the initiative, respectively, while $\theta_0$ and $\theta_1$ refer to $E(\theta \mid \text{partisan signal})$ and $E(\theta \mid \text{both signals})$, respectively. For each density $f$, the shaded area under the curve between $\theta_0$ and $\theta_1$ is the average causal effect of the signal from the deliberative minipublic in the presence of the conflicting signals from the parties.

of support among observers, compared with the counterfactual in which observers only learn the parties' positions. As before, this conclusion depends neither on the particular numbers we used for the example nor on the assumption of a uniform distribution for $\theta$. We only need to assume that the minipublic's threshold for support lies between the two parties' thresholds, so that its support is not redundant information. (Alternatively, we can relax the assumption that the observer knows the minipublic's threshold with certainty and make minimal assumptions about its probability distribution; see the online supplementary materials.)

One might think the effect of the signal from the deliberative minipublic, on its own, should be greater than its effect in the presence of the partisan cue. But this need not be so. The observer may be more surprised to learn of the minipublic's support for the initiative if she already knows the Republican Party opposes it than if she knows nothing about the parties' positions (see the online supplementary materials for a more detailed explanation). Even if the minipublic's support is less surprising for observers who know the parties' positions, the partisan signal could still amplify the impact of the minipublic's signal if, after receiving the partisan signal, an observer is more likely to be the "marginal" type, the type who is on the proverbial fence and whose preferences will be sensitive to marginal changes in beliefs about the initiative. In the terms of the numerical examples, our point here is that there is no reason to suppose that $\Pr(c_i \leq 7/10) - \Pr(c_i \leq 1/2)$ is less than $\Pr(c_i \leq 9/20) - \Pr(c_i \leq 3/8)$, absent further assumptions about the distribution of the cost, or "threshold," variable $c_i$.

Is there any reason to expect the effect of the signal from the deliberative minipublic, in the presence of the partisan cues, to be greater among independents than

Democratic and Republican observers? The intuitive reason for thinking so is that in a setting where everyone knows the conflicting positions of the two political parties, one might expect a larger proportion of "marginal" voters, whose preferences are sensitive to marginal changes in their beliefs about the initiative, among independents than Democrats or Republicans; one might expect that many partisan observers will have already made up their minds on the basis of the partisan signal. The model can help us clarify intuitions about the reasons why this prediction might and might not hold. Figure 1 depicts a hypothetical scenario in which the prediction holds. The Democratic Party is assumed to support the initiative if and only if $\theta \geq c_D$, while the Republican Party supports it if and only if $\theta \geq c_R$. Thus, conditional on observing the Democrats' support and Republicans' opposition, everyone forms an expectation about $\theta$ conditional on the knowledge that $\theta \in [c_D, c_R]$. Let $\theta_0 = E(\theta \mid \text{partisan signal})$ and $\theta_1 = E(\theta \mid \text{both signals})$. The three distributions depicted are the distributions of the perceived cost, $c_i$, of the initiative among Democrats, independents, and Republicans. As the figure is drawn, independents are more likely to fall in the interval $[\theta_0, \theta_1]$ than either Democrats or Republicans; they are more likely to be the "marginal" voters. The probability of falling within this interval is the causal effect of the signal from the deliberative minipublic on the probability of support for the initiative. Thus, the effect is greatest among independents. But with a little imagination, we can see that this prediction could also fail consistent with the model's core assumptions. For example, hold everything fixed but modify assumptions about the underlying prior distribution of $\theta$ and the minipublic's threshold, such that $\theta_1$ moves closer to $\theta_0$, and then Democrats will be more likely to be the marginal voters. For this reason, the prediction that in a partisan context the deliberative minipublic will have greater influence among independents should be considered more speculative than the previous predictions, seeing as it depends on more speculative assumptions.

### Empirical Literature

Most of the empirical literature on deliberative minipublics examines how deliberation affects participants, not observers (Karpowitz and Mendelberg 2011, 268). For example, in their study of the 2011 "What's Next California?" Deliberative Poll, Fishkin et al. (2015) examine the effects of deliberation on participants. In line with previous studies (Fishkin 2009), participants' opinions changed over the course of deliberation. But the authors do not address the question that motivated our survey experiment, namely, whether learning the results of a deliberative minipublic would affect observers' policy opinions.

The results of some nonexperimental research are suggestive of the possible effects. In surveys, a majority of Oregon voters claim to have used the CIR statement when deciding how to vote (Gastil, Richards, and Knobloch 2014). The finding is important, but without additional evidence one might wonder about the accuracy of voters' self-reports. Cutler et al. (2008) report the results of a survey administered before the referendum on the BCCA's proposal. Respondents' knowledge about the BCCA predicted support for its proposal. But their research design does not credibly identify a causal effect unless knowledge of the BCCA was as good as randomly assigned, conditional on the observable covariates measured in their survey. Given how many unobserved factors must affect both what citizens know about deliberative minipublics like the BCCA and their political opinions, we believe an experimental research design is needed to identify causal effects.

In the case of the Oregon CIR, some experimental evidence is available. An online survey experiment, administered to a sample of Oregon voters before the 2010 election, produced evidence that reading the CIR statement influenced respondents' voting intentions (Gastil and Knobloch 2010). The CIR statement includes lists of the reasons for and against the initiative, prepared by the deliberative citizens' panel, in addition to a description of how many of the panelists support the initiative. Thus, the effect found in Gastil and Knobloch's (2010) study could be the effect of learning the position taken by the majority of participants in a deliberative minipublic, the effect of learning the participants' reasons for favoring or opposing the initiative, or—most plausibly—the joint effect of learning both types of information. Our study builds on theirs by testing just one of these hypothesized effects, namely, the effect of merely learning the majority opinion of the deliberative minipublic's participants. Gastil et al. (forthcoming) use a survey experiment to evaluate the effect of reading the Citizen's Statement from Oregon's 2010 CIR, which was included in the Voter's Pamphlet ahead of the 2010 general election. They found that the CIR statement increased voters' opposition toward a ballot initiative that set a minimum sentence for certain felonies.

We got mixed results from a previous survey experiment in which some respondents received descriptions of a deliberative minipublic's conclusions about changes to the Social Security program (Ingham and Levin, forthcoming). In that study, survey respondents in the treatment conditions received only a brief description of the deliberative minipublic. The present survey experiment provides a more powerful test of the theory, in so far as respondents learned considerably more about the deliberative minipublic whose conclusions were then shared with them.

The relative dearth of empirical studies addressing this question does not owe to lack of interest. Theoretical debates about minipublics often invoke conjectures about whether observers would revise their policy opinions upon learning the conclusions of a deliberative minipublic (Chambers 2003; Lafont 2015). Discussing the fate of California's Proposition 31, elements of which were discussed in the "What's Next California?" Deliberative Poll, Fishkin et al. (2015) speculate that the proposition might have fared differently had voters known about the results of the Deliberative Poll.

> It is worth noting that . . . the connection to *What's Next California* was not part of the campaign. . . . If this origin had been part of the campaign, it might have provided an alternative heuristic to assist with the credibility of the proposal. (Fishkin et al. 2015, 1038)

This is precisely the conjecture that our survey experiment tests in the context of one of the other policy proposals considered by the "What's Next California?" Deliberative Poll. Among the proposals they considered was a proposal to lower the supermajority threshold needed in the legislature to pass new taxes from 67 percent to 55 percent. Our survey experiment, described in the next section, asked respondents about this proposal after exposing some of them to information about the Deliberative Poll and how its participants judged the proposal.

## The Survey Experiment

### Design

Our survey experiment was embedded in a public opinion survey of 1,750 California residents, eighteen years and older. The survey was conducted between July 18, 2014, and July 30, 2014, and was administered online using Qualtrics survey software.[4] The polling firm Qualtrics recruited participants through their partner company e-Rewards. Subjects were offered compensation in the form of e-Rewards Currency for completing the survey, which they could later redeem for Rewards available on an e-Rewards Rewards Chart. Compared with the California population, our sample overrepresented non-Hispanic whites as well as individuals with high levels of education. To adjust for these imbalances, we calculated poststratification weights so that our adjusted sample resembled California's adult population along key sociodemographic attributes (see section B of the online supplementary materials). These weights were used for adjusting descriptive statistics reported in the text.

Respondents were randomly assigned to one of four treatment conditions, differing along two dimensions: (1) whether they learned about the "What's Next California"

**Table 1.** Experimental Design.

|  | No partisan cue | Partisan cue | Total |
| --- | --- | --- | --- |
| No deliberative cue | Condition I<br>n = 449 (25.6%) | Condition III<br>n = 446 (25.5%) | n = 895 (51.1%) |
| Deliberative cue | Condition II<br>n = 421 (24.1%) | Condition IV<br>n = 434 (24.8%) | n = 855 (48.9%) |
| Total | n = 870 (49.7%) | n = 880 (50.3%) | n = 1,750 (100%) |

Deliberative Poll and its stance on a proposed amendment to the state's constitution, and (2) whether they learned the positions of the Democratic and Republican parties on the proposed amendment. For brevity's sake, we sometimes refer to the information about the Deliberative Poll as a "deliberative cue" and the information about the parties' positions as a "partisan cue."

The deliberative cue consisted of a message and two-minute video about the "What's Next California" Deliberative Poll.[5] The message read,

> In June 2011, a group of California registered voters were brought together to share their views about the state's government and policy issues affecting the state.
>
> Before proceeding with the rest of the survey, we will ask you to watch a brief video describing the deliberative process these citizens participated in.

Of the 855 respondents who were shown this message, 94 percent reported having seen the video in a follow-up question. To validate these self-reports, we examined the amount of time respondents spent on the web site containing the video. Our calculations largely confirmed the self-reports, as 89 percent of those respondents who were invited to see the video spent at least two minutes on the page where the video was embedded.

Subsequently, all respondents were asked a question about their support for reducing the size of the majority required for raising taxes in the California State Legislature. The question was preceded by content that varied with the treatment condition:

> Since the passage of Proposition 26 in November 2, 2010, California's constitution requires a supermajority consisting of at least 67% of the State Legislature in order to raise taxes.
>
> Some people have proposed an amendment so that the State Legislature could pass new taxes with a majority of 55%.
>
> [*Conditions III and IV only*] Democrats tend to approve of this proposal, while Republicans tend to disapprove of it.
>
> [*Conditions II and IV only*] The participants in the deliberative poll, whom you saw in the video, considered this question,

and after deliberating together, most of them approved of lowering the majority requirement to 55%.

How do you feel about this proposal?

Table 1 shows the number of respondents assigned to each treatment condition.

Note that in the primary treatment condition, respondents learn that "most" participants approved of lowering the majority requirement, but they do not learn whether a large or small majority approved of the measure. Some theorists have claimed that citizens have reason to trust deliberative minipublics only when minipublic participants are broadly united, while a sharp division of opinion within the minipublic might reasonably undermine citizens' trust (MacKenzie and Warren 2012). The indeterminacy of the majority's size would be problematic from that theoretical standpoint. But the signaling theory we formalized above identifies reasons to expect the information in the treatment condition to be influential even though respondents do not learn the size of the majority in deliberative poll. See, in particular, remark 1, part (a), in section A of the online supplementary materials.

The primary outcome variable of interest was respondents' reported levels of approval of the proposal to reduce the legislative supermajority threshold.[6] As a secondary measure of respondents' attitudes, we included questions about their preferences over several proposals for reducing legislative gridlock, including the constitutional amendment referenced in the treatment question. To conserve space, we have placed the results concerning these outcomes along with details about question wording in the online supplementary materials.

## Hypotheses

The signaling theory of deliberative minipublics suggests several testable hypotheses about the effects of learning about the "What's Next California?" Deliberative Poll and its participants' postdeliberation opinions.

The survey asked respondents about a proposed amendment to the state constitution that would lower the supermajority threshold needed in California's state legislature for passing new taxes from 67 percent to 55 percent.
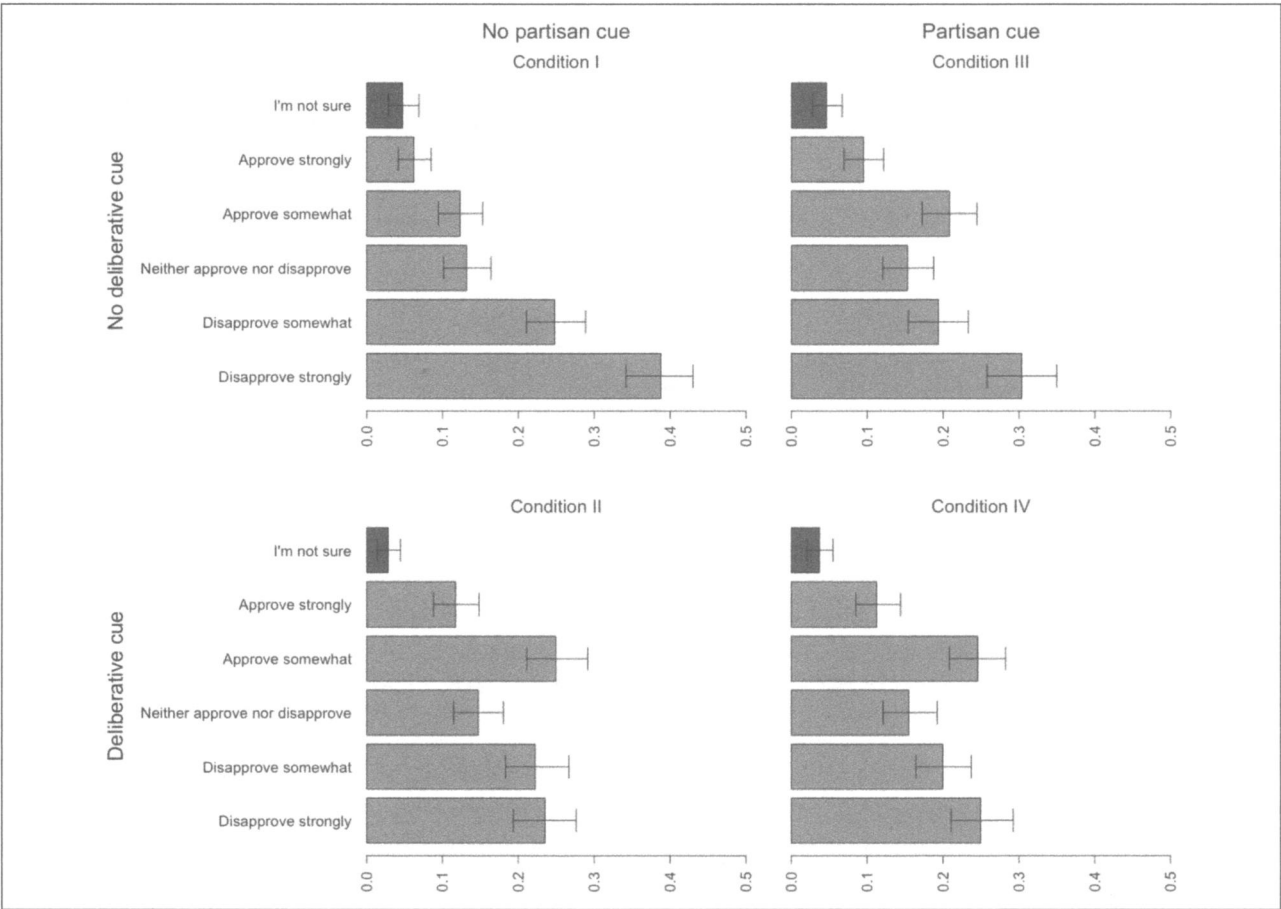
**Figure 2.** Approval of proposal by treatment condition.
Bars represent proportions of respondents. Segments indicate 95% bootstrapped confidence intervals for the proportions. Calculations were made using poststratification weights. For more details see Table 2.

Respondents in treatment condition II learned that a majority of participants in the Deliberative Poll had declared their support for the proposed amendment. For the reasons explained above, we predict that respondents in condition II should be more likely to support the proposed amendment than respondents assigned to condition I.

Past literature provides ample evidence of the effects of partisan cues on public opinion (Bullock 2011; Lupia 1994; Lupia and McCubbins 1998; Zaller 1992). Given this literature and the theoretical reasons for expecting such effects, we hypothesize that respondents who identify as Democrats (alternatively, Republicans) and receive the conflicting partisan signals (condition III) should be more (less) likely to support the proposed amendment than their counterparts in the control condition.

What about respondents who receive both types of signal? For reasons set out above, learning the position reached by a majority of deliberative minipublic participants should have positive average treatment effects on observers' opinions even if the observers have also received partisan cues. The survey experiment, which exposed some respondents to the partisan cues and some respondents to the partisan cues as well as the signal from the deliberative minipublic, allows us to investigate this possibility as well. We predict that survey respondents who received both types of signal (condition IV) should be more likely to approve of the amendment than respondents who received only the partisan cue (condition III). Moreover, these effects should hold even conditional on partisan identification. In a more speculative vein, we might expect them to be more pronounced among independents, as explained in the discussion of Figure 1.

## Results: Effects on Policy Attitudes

### The Effects of Deliberative Cues

Figure 2 shows the distribution of support for the proposed amendment to lower the supermajority threshold needed for passing new taxes, among respondents assigned to each treatment condition. In addition, in Table 2, we present the differences in levels of support for respondents

**Table 2.** Approval of Proposal in Partisan and Nonpartisan Contexts.

| | No partisan cue | | Difference (II – I) | | Partisan cue | | Difference (IV – III) | |
|---|---|---|---|---|---|---|---|---|
| | Condition I | Condition II | M | SE | Condition III | Condition IV | M | SE |
| Approve strongly | 6.2 | 11.7 | 5.5 | 1.9 | 9.5 | 11.3 | 1.7 | 2.1 |
| Approve somewhat | 12.3 | 25.0 | 12.7 | 2.6 | 20.8 | 24.6 | 3.8 | 2.7 |
| Neither approve nor disapprove | 13.2 | 14.7 | 1.6 | 2.4 | 15.3 | 15.5 | 0.2 | 2.5 |
| Disapprove somewhat | 24.8 | 22.2 | −2.5 | 2.9 | 19.4 | 20.0 | 0.6 | 2.7 |
| Disapprove strongly | 38.8 | 23.5 | −15.3 | 3.1 | 30.4 | 24.9 | −5.4 | 3.2 |
| I'm not sure | 4.7 | 2.8 | −1.9 | 1.3 | 4.6 | 3.7 | −0.9 | 1.3 |

The table provides the distribution of approval of the proposed reform among respondents assigned to each treatment condition (first and second columns), as well as differences in levels of approval between respondents who were and were not exposed to the deliberative cue (third column). The fourth column provides bootstrapped standard errors for the differences in levels of approval.

assigned to conditions I and II (nonpartisan context) and for respondents assigned to conditions III and IV (partisan context).[7]

We began by testing our hypothesis that learning the deliberative minipublic's position would increase respondents' support for lowering the required legislative majority to pass taxes. We did so by comparing support for the proposal among respondents assigned to condition I, who were not exposed to the deliberative cue, with support for the proposal among respondents assigned to condition II, who watched the video and learned that most participants in the Deliberative Poll approved of lowering the required threshold. Results presented in Table 2 indicate that exposure to the deliberative cue reduced disagreement by 17.8 percentage points, with most of the change driven by a 15.3 point reduction in strong disagreement. The counterpart to this change was an increase in approval of 18.2 percentage points, driven mainly by a 12.7 increase in the proportion of respondents who somewhat agreed with the proposal. The deliberative cue did not significantly affect the proportion of respondents expressing no opinion on the issue—about 18 percent, who were unsure about their views on the issue (less than 5%) or neither agreed nor disagreed (around 14%).

To put these results in perspective, it is useful to compare the effect of being exposed to the deliberative cue only (condition II vs. condition I) with the effect of being made aware of partisan divisions on the issue in the absence of deliberative cues (condition III vs. condition I). Our results (depicted in Figure 2 and presented in more detail in Table C2 in the online supplementary materials) suggest that, overall, respondents who were only exposed to the partisan cue were 13.8 percentage points less likely to disapprove than respondents in the no-cues control group, with most of the change consisting of an 8.4 percentage point reduction in strong disagreement. The counterpart was an increase in approval of 11.8 percentage points, with most of the change driven by an 8.5

percentage point increase in mild approval. Thus, the estimate of the partisan cue's effects is considerably smaller than the estimate of the deliberative cue's effects.

We hypothesized that Democrats would become more supportive of the proposal, and Republicans less so, after reading that Democrats tend to support it and Republicans tend to oppose it. Our results are broadly consistent with these expectations (see Table C3 in the online supplementary materials). Although the partisan cue significantly decreased disagreement (either strong or mild) among Democrats by 24.6 percentage points, the reduction in disagreement among independents and Republicans was smaller in magnitude (6.0 and 1.2 percentage points, respectively) and lacked statistical significance.

We next tested our hypothesis that the deliberative cue would increase support for lowering the required legislative majority for tax increases even when respondents were aware of where the two main political parties stand on the issue. We did so by comparing respondents assigned to condition III, who received this partisan cue but not the deliberative cue, with respondents assigned to condition IV, who received both cues. Results presented in the lower panel of Table 2 contradict our hypothesis: the deliberative cue did not influence overall support for the amendment among respondents who were also told where the two parties stand. There was a 4.8 overall reduction in disagreement, explained mostly by a decrease in strong disagreement, but the change is not statistically significant.

Figure 3 shows the effects of the deliberative cue in the two settings. The effects among respondents who were not told the parties' positions are displayed in the left-hand panel, and the effects among respondents who were told their positions are displayed in the right-hand panel. As the figure makes vivid, the deliberative cue had significant effects only when it was unaccompanied by information about where the parties stand on the issue.
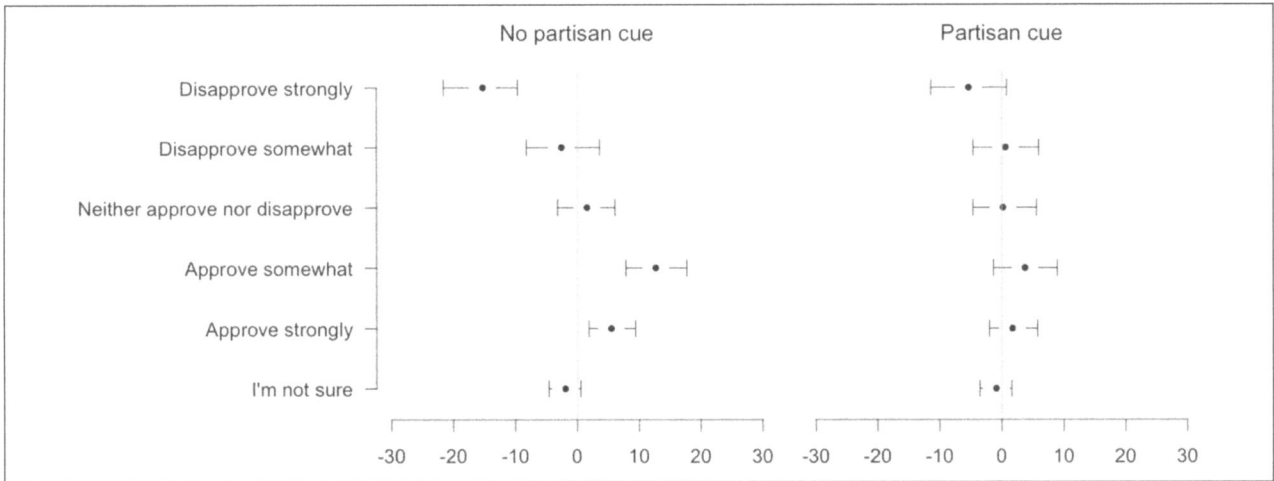
**Figure 3.** Effects of deliberative cues in partisan and nonpartisan contexts.
Segments represent 95% confidence intervals for the change in the level of approval resulting from exposure to the deliberative cue. The plot on the left ("No partisan cue") presents differences in approval between respondents assigned to conditions II and I. The plot on the right ("Partisan cue") presents differences in approval between respondents assigned to conditions IV and III. Calculations were made using poststratification weights. OLS = ordinary least squares.

Regression analyses produced similar results (see Table C4 in the online supplementary materials). We first estimated an ordinary least squares (OLS) regression (first column in Table C4) of support for the proposed reform, measured along a five-point scale, with controls for a number of individual attributes and allowing the effect of the deliberative cue to vary depending on whether it was delivered jointly with the partisan cue. Results suggest that the provision of the deliberative cue increased support for the proposed reform by 0.52 points along the five-point scale. The increase in support was 0.22 points lower in magnitude when respondents were made aware of where the parties stand on the issue, although this decline does not reach our threshold of statistical significance. An ordered logit regression (second column in Table C4) produced results consistent with those from the OLS estimation.

### Effects by Party Identification

We next looked at whether the effects of deliberative cues varied across partisan subgroups.[8] We first created a binary indicator of exposure to the deliberative cue (DC), a binary indicator of exposure to the partisan cue (PC), binary indicators of respondents' partisan identification (D and R),[9] and an ordered variable measuring support for lowering the required legislative majority on a five-point scale. Subsequently, we analyzed the determinants of this variable using OLS and ordered logit regressions, with the following specification for the linear predictor:

$$
\begin{aligned}
y_i = & \gamma_1 DC_i + \gamma_2 \left( DC_i \times R_i \right) + \gamma_3 \left( DC_i \times D_i \right) + \gamma_4 R_i + \\
& \gamma_5 D_i + \gamma_6 PC_i + \gamma_7 \left( DC_i \times PC_i \right) + \\
& \gamma_8 \left( PC_i \times R_i \right) + \gamma_9 \left( PC_i \times D_i \right) + \\
& \gamma_{10} \left( R_i \times DC_i \times PC_i \right) + \gamma_{11} \left( D_i \times DC_i \times PC_i \right) + \mathbf{X}_i' B
\end{aligned}
$$

where $\mathbf{X}_i$ denotes a vector of individual attributes included as covariates. Regression estimates (see Table 3) suggest that although all respondents became more supportive after exposure to the deliberative cue, the change in support for the proposal varied considerably by party identification. The results of the OLS estimation (first column in Table 3) indicate that while the increase in support reached 0.73 points (along the five-point scale) for independents in the absence of partisan cues, the increment was 0.30 and 0.27 points lower for Republicans and Democrats, respectively, although partisan differences were not statistically significant.

The change in the probability of strong approval varied markedly among individuals with different party identifications, as baseline levels of support were considerably higher among Democrats than independents and Republicans. To better illustrate this finding, we used the results of the ordered logit estimation (second column in Table 3) to simulate the effect of deliberative cues on the probability of strong approval for Democrats, independents, and Republicans (see Figure 4). The results suggest that, when respondents did not receive the partisan cue, the increase in strong approval was particularly intense among independents and Democrats (about 6.3 percentage points within each group). For Republicans,

**Table 3.** Regression Analysis of Approval of the Proposed Reform.

|  | OLS | Ordered logit |
|---|---|---|
| Deliberative cue | 0.73 | 1.10 |
|  | (0.18) | (0.26) |
| Deliberative Cue × Republican | −0.27 | −0.38 |
|  | (0.26) | (0.38) |
| Deliberative Cue × Democrat | −0.30 | −0.54 |
|  | (0.23) | (0.32) |
| Republican | −0.18 | −0.30 |
|  | (0.18) | (0.28) |
| Democrat | 0.81 | 1.21 |
|  | (0.16) | (0.24) |
| Party cue | 0.38 | 0.58 |
|  | (0.17) | (0.26) |
| Party Cue × Deliberative cue | −0.18 | −0.30 |
|  | (0.24) | (0.35) |
| Party Cue × Republican | −0.33 | −0.61 |
|  | (0.25) | (0.39) |
| Party Cue × Democrat | 0.12 | 0.06 |
|  | (0.22) | (0.32) |
| Party Cue × Deliberative Cue × Republican | −0.06 | 0.01 |
|  | (0.36) | (0.53) |
| Party Cue × Deliberative Cue × Democrat | −0.08 | 0.00 |
|  | (0.31) | (0.44) |
| Includes controls |  | Yes |
| N |  | 1,537 |

The table provides point estimates and standard errors (between parentheses) for the coefficients of OLS and ordered logit models of approval of the proposed reform. Both models also included intercepts and the following control variables: gender, age, Hispanic ethnicity, income, and education. OLS = ordinary least squares.

the effects were smaller but still sizable (3.1 percentage points). All in all, we find that the willingness to trust a deliberative minipublic as a source of information appears to cut across partisan lines. In contrast with the partisan cue, the deliberative cue did not contribute to widening partisan polarization on the issue.

Does the crosscutting influence of deliberative cues persist even in a partisan context? Regression results suggest perhaps not. The results of the OLS estimation (first column in Table 3) indicate that the effect of the deliberative cue was lower among all respondents in a partisan context, although not significantly so. Among Democrats and Republicans, the effects of the deliberative cue were 0.24 and 0.26 points smaller (along the five-point scale) when exposed to information about the partisan divide on the issue, but the difference with respect to the nonpartisan context was not statistically significant. Among independents, the effect was 0.18 points smaller along the five-point scale, compared with the nonpartisan context, and also lacked statistical significance.

In a partisan context, we again found that the change in the probability of strong approval varied depending on party identification. The effect of the deliberative cue on the probability of strong approval, assessed using the results of the ordered logit estimation, remained statistically significant in the partisan context only among independents (see right panel in Figure 4).[10] For independents, being exposed to both partisan and deliberative cues, instead of partisan cues only, increased strong approval by 6.7 percentage points. Among these respondents, then, the effect of the deliberative cue on the probability of strong approval approximated in magnitude the effect found in the nonpartisan context. For Democrats and Republicans, however, changes in their reported levels of approval were smaller in magnitude (3.9 and 1.5 percentage points, respectively) and did not reach our threshold of statistical significance. Among partisan respondents, then, the partisan cue appeared to dampen to undetectable levels the effects of the deliberative cue found in the nonpartisan context.

As noted above, the survey experiment produced an additional measure of respondents' policy attitudes, using question items that asked respondents about their preferences over the constitutional amendment—the reduction to the legislative threshold—and two other constitutional reforms aimed at reducing legislative gridlock. The pattern of treatment effects on this measure of respondents' attitudes was largely consistent with the pattern of effects on the primary measure. See the online supplementary materials for details.

## Discussion

The results of the survey experiment confirmed the main hypothesis derived from the signaling theory of deliberative minipublics. Learning that a majority of participants in the Deliberative Poll supported the proposed amendment to the state constitution had, on average, a positive effect on respondents' propensity to support the amendment. The signal from the deliberative minipublic had, however, no statistically significant effect on support for the proposed amendment when survey respondents also received partisan cues. This finding contradicted our expectations, based on the formal model of the signaling mechanism. What might explain this finding?

One possible explanation is that the signal from the deliberative minipublic was less surprising for observers who already knew where the two parties stood on the issue. For example, observers might initially expect the benefits of the initiative to be modest, such that learning that the deliberative minipublic supports the initiative causes a significant revision to their expectations; but when they already know that the Democratic Party supports the initiative, it is less surprising to learn in addition
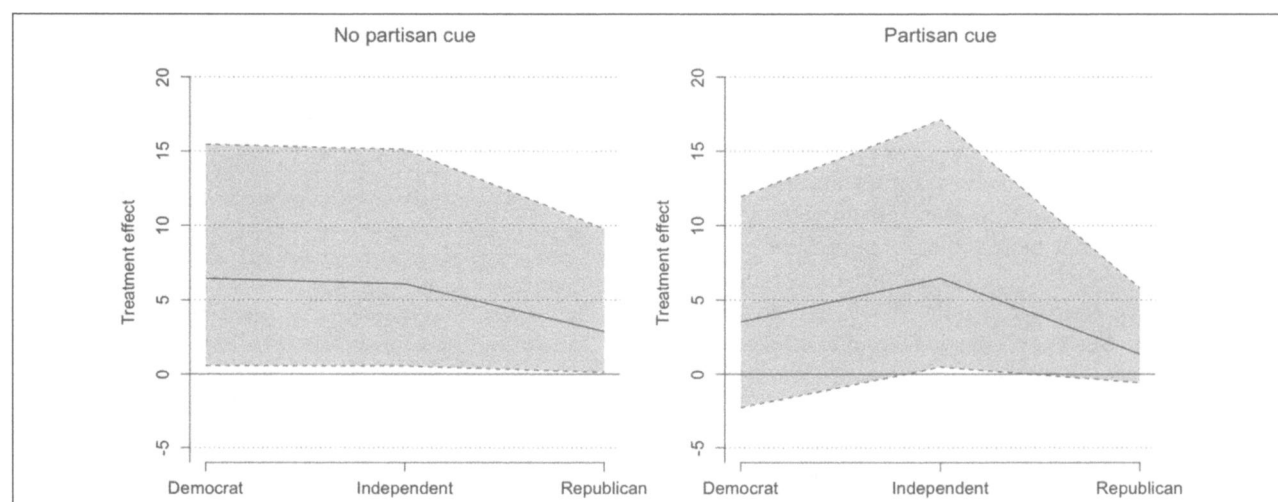
**Figure 4.** Change in probability of strong approval by party identification.
Gray areas represent 95% confidence intervals for the change in the level of approval.

that the minipublic supports it. In passing, note a simple but practically significant corollary of this explanation: if partisan cues dampen the effect of the signal from the deliberative minipublic, then, trivially, the signal from the deliberative minipublic also dampens the effect of the partisan cue. (That is, if the average difference in support between treatment conditions III and IV is less than the average difference in support between conditions I and II, then the average difference in support between conditions II and IV must be less than the average difference in support between I and III.) If signals from deliberative minipublics dampen the effects of partisan cues, some people might value their inclusion in the democratic process even if their direct effects on public opinion are weak.

A distinct and—we think—more plausible explanation for the result is suggested by the scenario hypothesized in Figure 1. Knowing that the Democratic Party supports the measure while the Republican Party opposes it may have been enough information for most Democratic observers to conclude that the initiative's benefits were sufficient to outweigh its costs, and for most Republican observers to conclude that its costs outweighed its benefits, such that few partisans who had this information remained "marginal" observers, whose preferences could be influenced by the marginal revisions to beliefs induced by learning the position of the deliberative minipublic. If so, then the average effects of the signal from the minipublic, in the presence of the partisan cue, might have been too small to detect within these groups. A virtue of this explanation is that it is also consistent with the finding that the signal from the deliberative minipublic continued to have significant effects among independents who knew the positions of the two parties.

This interpretation also suggests why deliberative minipublics' potential to influence public opinion, in partisan contexts where observers know the partisan significance of a measure, could remain practically significant even if the conclusions of the minipublic have, on average, statistically insignificant effects on the population as a whole. In many elections, a deliberative minipublic could affect the outcome if it could affect the opinions of independents, even if its effects among Democrats and Republicans are negligible. The number of independents exceeds the margin of victory in many competitive elections.

Future research should try to determine whether the particular causal mechanism posited by the signaling theory is responsible for the observed effects, or whether an alternative theory, positing a different mechanism, can explain the effects just as well. The mechanism posited by the signaling theory is that observers believe members of the minipublic learned about the benefits of the measure, and that the most likely explanation for a minipublic's declared support for a measure is that most of its members discovered the net benefits were greater than expected. A rational observer, observing the minipublic's support, should adjust her beliefs about its relative benefits accordingly. Observing the deliberative minipublic's support should cause marginal observers, who previously believed its costs marginally outweighed its expected benefits, to support it. Future research should attempt a more direct test of the core claim about the mechanism at work, namely that observers believe members of the deliberative minipublic learn relevant information about the policies they deliberate over. In particular, it would be worth testing the assumption that observers attribute the learning to the specifically *deliberative* aspects of the minipublic. We might, for example, compare the effects

of learning the conclusions of a deliberative minipublic with the effects of learning the results of a simple public opinion survey or, instead, with the conclusions of citizens who are arguably informed but not in virtue of a specifically deliberative process. It would also be interesting to know whether conveying the results of the minipublic's deliberations has an even greater effect when respondents learn, not merely that a majority of participants supported a measure, but rather that a supermajority of a certain size supported it; the theory we presented above would lead us to expect even stronger effects here.

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## Declaration of Conflicting Interests

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## Notes

1. Replication materials will be made available at https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/HL8HCA following publication.
2. Of the 161 participants, 158 were chosen by stratified random sampling. The chair then concluded that indigenous peoples were underrepresented, so two more members of First Nations were added. The last member was a chair appointed by a government official.
3. The term $c_i$ need not be interpreted as the monetary cost to voter $i$ of the increased tax burden; it could also represent, say, the strength of her ideological opposition to taxation. The crucial assumption is just that voters vary in their perceptions of the costs of the initiative, not that they identify the costs with the initiative's impact on their own pocketbooks.
4. At the beginning of the survey, respondents were asked a series of screener questions on basic demographics. Individuals that failed to meet gender, age, and education quotas were terminated from the survey. Quotas were removed on July 29, 2014, to ensure the timely completion of the data collection process; by then, more than 1,600 complete responses had been collected. The survey also included three trap questions used to identify and terminate inattentive respondents. We focus our analysis on 1,750 responses from individuals who completed the entire questionnaire. Excluded from our analysis are 972 partial responses from individuals that failed a trap question.
5. The video these respondents were assigned to see can be viewed at https://www.youtube.com/embed/k5cpY0MuMDU?start=94&end=216&version=3&autohide=1&controls=0&showinfo=0&rel=0 (last accessed December 20, 2017).
6. Response alternatives included the following five options: *approve strongly, approve somewhat, neither approve nor disapprove, disapprove somewhat, disapprove strongly,* and *I'm not sure.*
7. In Table C1, in the online supplementary materials, we present measures of the effect of exposure to treatment conditions on support for the proposed reform calculated without using poststratification weights. Unweighted measures of treatment effects are somewhat different in magnitude but qualitatively similar to those shown in Table 2.
8. Party identification is an observational characteristic that was not randomly assigned to respondents. Therefore, differences in the way partisan subgroups react to the deliberative cue may not be a result of party identification per se, but of differences in other individual characteristics that correlate with party identification.
9. We coded party identification based on the question "Generally speaking, do you usually think of yourself as a . . .," with response alternatives: *Republican, Democrat, Independent, Another party,* and *No preference.* The order of the first three response alternatives was randomized. Respondents reporting Democratic Party identification were coded as Democrats, those reporting Republican Party identification were coded as Republicans, and those selecting any of the remaining two options were coded as Independents.
10. The effects of the deliberative cue on the probability of strong approval were calculated for hypothetical respondents with the following characteristics: female, approximately forty-seven years old, non-Hispanic, income level of about eleven on a nineteen-point scale, and educational attainment of about four on a six-point scale. Other characteristics (party identification and exposure to experimental cues) varied depending on the scenario under consideration (absence vs. presence of deliberative cues, and partisan vs. nonpartisan context).

## Supplemental Material

Supplemental materials for this article are available with the manuscript on the *Political Research Quarterly* (PRQ) website.

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