The Democratic Dilemma among Elites: Do Legislators Make More Reasoned Choices than Voters?

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Abstract

Critics of representative democracy have often focused on the perceived shortcomings of voters' decision-making while leaving aside elected representatives — those who actually draft and vote on laws. This paper studies decision-making through a paired experiment with legislators and voters. Our design elicits knowledge about one consequence of public policy, its fiscal cost; provides expert research concerning fiscal impact on randomly-selected bills; and observes policy support via the survey and, for legislators, their roll call voting decisions. We find that legislators' decision-making looks a lot like voters': policy knowledge is inaccurate and biased in systematic ways, but both groups learn about policy in response to expert research. Neither group bases their policy positions on the fiscal consequences of bills unless provided expert guidance. Legislators distinguish themselves, however, in one respect: by the time they are asked to vote on policy, all legislators — not just those in the high information condition — are able to align their positions with fiscal impact. In the survey experiment, legislators' knowledge, learning, and decision-making resemble voters', but the legislative process provides them the information they need to make reasoned choices by the time bills reach a vote.

Word count: XXXX

...democracy requires citizens to make reasoned choices. Reasoned choice, in turns, requires that people know the consequences of their actions. Can voters, legislators, and jurors make reasoned choices? (Lupia et al. 1998, p. 1)

In respect of width of view, of capacity for penetrating thought on political problems, representatives are scarcely above the class from which they came, that of second-rate lawyers or farmers, less often merchants or manufacturers. (Bryce 1931 [1888], p. 150)

Contemporary critics of democracy have questioned the apparent factual ignorance, inattentiveness, and irrational decision-making of voters, though the strength of these claims has been vigorously questioned (Lippmann 1922; Campbell et al. 1960; Achen and Bartels 2016; Delli Carpini and Keeter 1996; Bartels 1996; Healy, Malhotra, and Mo 2010; Page and Shapiro 1992; Popkin 1994; Key 1966; Fowler and Hall 2018; Ashworth and De Mesquita 2014). In contrast, there has been relatively less attention to, and certainly less criticism of, the reasoning skills of legislators. The performance of representative democracy depends just as much on the competence of legislators as voters. Are legislators informed about the choices they have to make, and do they understand the consequences of their actions? Are legislators more informed and capable of "penetrating thought" than voters?

Historically, political observers were unimpressed with the quality of elected representatives (Rocca 1921). John Stuart Mill criticized the members of Parliament as "inexperience sitting in judgment on experience, ignorance on knowledge" (Mill 1865, p. 37). In a backhanded compliment, Lord Bryce described the "keen though limited, intelligence" (Bryce 1931 [1888], p. 149) of American legislators as being a surprise to anyone who listened to how regular citizens describe their politicians. Alabama Governor Emmet O'Neal summed up the prevailing opinion when he stated that "a session of the Legislature is looked upon as something in the nature of an unavoidable public calamity" (O'Neal 1914, p. 685). The key problem was that legislators did not know enough about policy to draft, and enact, good legislation, because policy-making is a demanding task for which few are prepared and even

fewer have the time for serious study (Bagehot 2015 [1915]; Luce 1935).

Quantitative studies of what legislators know, how they learn, and how positions respond to new information lead to a somewhat mixed picture regarding legislators' reasoning skills. Following the groundbreaking study of Miller and Stokes (1963), several surveys have shown that legislators, and their offices, hold inaccurate, and often biased, views of what their constituents want or where experts stand on a particular issue (Broockman and Skovron 2018; Pereira 2021b; Hertel-Fernandez, Mildenberger, and Stokes 2019; Lee et al. 2021; Lee 2021). Further, when given new information, legislators often update beliefs in asymmetric and irrational ways, and they are subject to numerous decision-making biases (Baekgaard et al. 2017; Butler and Dynes 2016; Vivalt and Coville 2020; Linde and Vis 2017; Arceneaux, Dunaway, and Soroka 2018; Fatas, Neugebauer, and Tamborero 2007). Legislators' inaccurate perceptions of the world and biased learning suggest they are little better at decision-making than voters.

Experimental studies of position-taking, in contrast, are generally more sanguine about legislators' skills. That legislators' policy positions are responsive to information about the preferences of the public, advice of experts, analyses of legislative research bureaus, or appeals by bill sponsors is taken by many scholars as evidence of their intellectual reasoning (Butler and Nickerson 2011; Lee 2021; Pereira 2021a; Zelizer 2018, 2021; Hjort et al. 2021; Bergan 2009; Bergan and Cole 2015). Hjort et al. (2021, p. 1477), for example, conclude that "Mayors (and other local policymakers in our sample) appear to be fairly sophisticated consumers of accessible research." While legislators are ex ante uninformed and imperfect learners, they appear able to incorporate information to make better decisions when expert information is provided to them.

This paper brings together the survey literature on legislators' knowledge and the field experimental evidence of their responsiveness to information. To understand whether legislators make "reasoned choices" that take into account the consequences of legislation, we need to know whether they understand the consequences of bills *and* align their actions with

those consequences. Thus we need both surveys to measure their beliefs and a field experiment that examines whether policy positions respond to those beliefs, either before or after being provided supplemental information. Otherwise, it might be the case that legislators' knowledge is imperfect but irrelevant, or that treatments are influential but not informative. Even more useful would be a study that combined these elements and examined the decision-making processes of legislators and voters on comparable tasks.

This paper explores legislators' and voters' reasoning and position-taking using a paired survey and field experiment. Our study examines whether legislators and voters know about, and take positions based on, one consequence of legislation: its fiscal impact. Our experimental design elicits beliefs about the fiscal impact of legislation through a survey; randomizes the provision of policy research about fiscal costs from legislatures' own research offices; measures respondents' positions via a survey and, for legislators, via roll call voting. With measures of pre-treatment beliefs, post-treatment beliefs, and policy support via survey responses and real-world behavior, we illuminate each stage of the decision-making process to see whether legislators and voters make reasonable decisions: do they hold reasonable prior beliefs about bills? Incorporate new information into posterior beliefs? Align policy positions with beliefs, either ex ante or ex post? And for legislators, acquire the experimental information outside the scope of our experiment?

Our findings suggest that legislators' decision-making processes looks a lot like voters'. Both groups hold biased beliefs about the fiscal consequences of bills; even more striking, biases are correlated with the same individual characteristics across groups. When provided policy research, both groups learn, though legislators moreso than voters. Posterior beliefs converge to the truth. Third, neither group of respondents bases their policy positions on the fiscal consequences of legislation in the control group, but both groups do when they are provided expertise. Finally, legislators acquire relevant information about bills after our experiment but before they are asked to vote on bills. Roll call votes reflect the cost of legislation regardless of whether legislators were assigned to treatment or control for that

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bill.

Within the confines of our experiments, legislators appear to be rationally considering the policy information they are provided, but in ways similar to voters. Both groups want to make decisions consistent with policies' impacts but are limited by their inaccurate beliefs. Crucially, legislators are able to learn about policy impact before they decide how to vote. Legislators' strong performance is thus less likely a result of any inherent advantage in knowledge or reasoning skills than in their support from, and immersion in, the day-to-day policy-making process of the legislature.

Reasoned Decision-Making in Legislatures

To examine the relative decision-making competence of legislators and voters, we first need to determine the criteria on which to compare them. While there are numerous definitions, and often fierce debate, about the meaning of rationality in studies of decision-making, there are a set of common considerations we can point towards to determine if legislators make good decisions. Rational choice theories of legislatures make regular assumptions about rationality with respect to three stages of decision-making: the beliefs that a legislator has about legislation; how a legislator learns in response to new information; and how decisions relate to the consequences of legislation (Austen-Smith and Riker 1987; Gilligan and Krehbiel 1987; Callander 2008; Battaglini 2002; Schnakenberg 2017; Kamenica and Gentzkow 2011). In practice, though, scholars rank individual rationality somewhere between a behavioral convenience that facilitates comparisons across formal models of institutions and a highly problematic and misleading description of how decision-making actually works (Diermeier and Krehbiel 2003; Sheffer et al. 2018). What is the evidence concerning the competence of legislators' decision-making behavior?

Legislators' expectations about the consequences of legislation should be right, on average. This rational expectations assumption lets legislators off easy in one respect: no one should be expected to be right about the consequences of every bill, all the time. Sometimes

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an infrastructure project will cost more than expected, a military operation will occasion more casualties, or a political platform will be less popular than expected. We cannot expect legislators to always accurately perceive the positions of constituents, advice of experts, or outcomes of legislation.

The more difficult aspect of the rational expectations assumption, for legislators, is that they should not be persistently wrong, in the same direction, over time. Otherwise legislators are in essence refusing to learn from mistakes. There is some evidence that legislators' beliefs are systematically biased. Broockman and Skovron (2018) show that members of Congress overstate public support for two policies in their survey and understate support for five, but in all cases they err to the benefit of conservatives. Two studies show that legislators' inaccurately predict the positions of experts or voters, respectively, but neither analyzes whether legislators are systematically wrong in the same direction (Lee et al. 2021; Pereira 2021b). With the replication data from Pereira (2021b), I examine the directional bias of local Swiss officials' predictions of their constituencies' support for two referenda, one on the de-horning of cows and the other on Switzerland's obligations to international organizations.

Figure 1 shows whether officials overstated or understated constituents' support for the referenda. Across over 2,750 local officials, Swiss politicians underestimated the public's support on the horned cow referendum by 4.2 percentage points on average ($p < 0.001$) but overestimated support on the self-determination referendum by 6.2 percentage points ($p < 0.001$). While the policymakers were systematically wrong on each issue, they were wrong in opposite directions such that, over a large number of bills, it is not clear whether they are systematically biased or not.

A second criteria for reasoned decision-making is that legislators should learn in response to trustworthy information. In most formal models of legislative decision-making, learning is characterized by Bayes' rule, which satisfies this condition (Crawford and Sobel 1982; Gilligan and Krehbiel 1987; Callander 2008; Schnakenberg 2017). Legislators' posterior beliefs about the consequences of a policy should converge to that of a trusted source, like an expert with
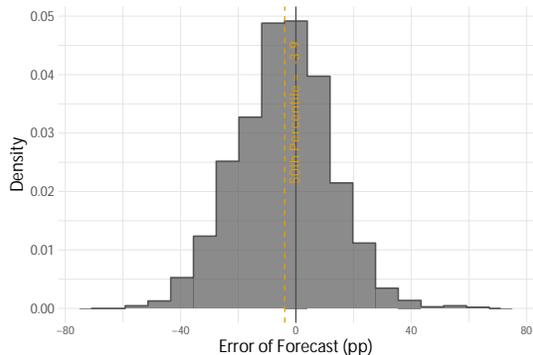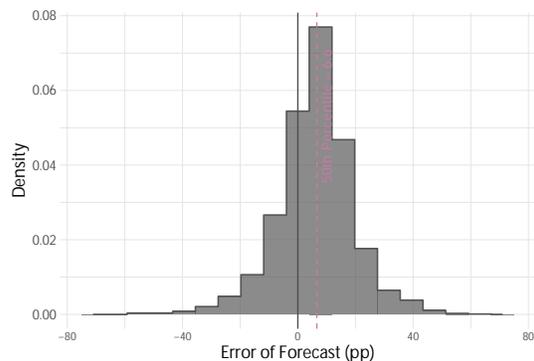
(a) Horned cow referendum.   *(b) Self-determination referendum*

Figure 1: Accuracy and bias of Swiss legislators' beliefs about constituency support for two referenda, via Pereira (2021b)

the legislature's research services or an outside academic. Motivated reasoning, in which an individual decides whether to update based on whether the message agrees or conflicts with their priors, does not guarantee convergence.

Evidence about legislator learning is decidedly mixed. Lee (2021) finds that legislators update towards the opinion of experts and do not exhibited biases like motivated reasoning (see also Hjort et al. 2021). Several studies, however, find that legislators learn irrationally by ignoring useful information that conflicts with their prior beliefs or by updating more to good news than bad news Baekgaard et al. (2017); Vivalt and Coville (2020); Butler et al. (2017)One benefit of Lee (2021) is that legislators were asked about real policy issues, including needle exchanges, GMO bans, and rent-control ordinances, rather than artificial choice problems like a public goods or ultimatum game or discounting choice task for which the costs of biased learning or behavior or . None of the studies ask policymakers about issues that are currently pending before them and require As a result, these studies show how legislators learn when presented information within the confines of a survey experiment, but not how much legislators know, or learn, through their routine activities in government.

A third criteria for reasoned decision-making rests on the relationship between legislators' actions and their beliefs. Since Miller and Stokes (1963), scholars have demonstrated the correlation between legislators' policy positions and salient inputs like the preferences of

constituents (Lax and Phillips 2009; Lax et al. 2019). Butler and Nickerson (2011) showed a causal relationship between inputs and positions when they randomly provided information about constituents' preferences to legislators and found that treated legislators took votes substantially more aligned with constituents' positions. The underlying logic of these studies is that a reasonable legislator should base their decisions on important criteria, like the positions of constituents.

Studies of decision-making show that legislators fall short of this ideal in at least two ways. First, legislators do not always accurately perceive these inputs. Their decisions reflect their inaccurate or biased perceptions of reality such that legislators are making decisions divorced from reality or facts. Second, legislator' decisions also appear to motivated by irrational considerations like emotional reactions, cognitive biases, or logical fallacies (Linde and Vis 2017; Arceneaux, Dunaway, and Soroka 2018; Fatas, Neugebauer, and Tamborero 2007; Sheffer et al. 2018). Just like voters, legislators are subject to framing effects, the sunk cost fallacy, and status quo bias. Their decisions may not be based on their beliefs or knowledge as much as irrelevant factors like which policy outcome is deemed the status quo.

Taken together, the existing evidence about the competence and quality of legislators' decision-making is limited in several respects. First, there is simply not much evidence regarding legislators' beliefs or responsiveness to new information. Surveys and experiments with elites are difficult to conduct, and only recently have scholars started collecting the data behind elites' decision-making.

A second limitation concerns the timeliness of data collection. Because it is so difficult to survey elites, most measures of legislators' knowledge ask about broad policy questions that may not be relevant to a given lawmaker. Finding an issue that is relevant and timely for all respondents is difficult, particularly in cooperative surveys that recruit elected officials from all over the country to respond to a single, extensive survey. As a result, lawmakers may exhibit limited knowledge simply because the issue is not salient for them, and they are spending their scarce time learning about other issues.

A third limitation is that most investigations of elite behavior lack relevant comparison groups. Surveys or experiments with elites would be even more informative about their limitations or strengths if paired with a comparable survey of voters. There are substantial obstacles to conducting truly equal surveys of elites and the public, and any investigation would need to be cautious about asymmetries in the information available to, or survey procedures used with, each group (Hill and Huber 2019). Nevertheless, paired studies would tell us how elites perform relative to the voters whom they represent.

To address these limitations, we conducted a paired survey and experiment of legislators and voters concerning ten bills pending before the Missouri legislature.

Experimental Design: Legislature

We conducted an experiment in the Missouri legislature. Missouri ranks 16th out of the states on Squire's measure of legislative professionalization (Squire 2017). It features unified Republican control of state government and eight year legislative term limits.[1]

We approached legislators' staffers in the Missouri capitol to schedule meetings with the legislator; 93 meetings were scheduled. We began the face-to-face meetings with the legislators by obtaining legislators' consent to participate in our study; 80 of the 93 legislators consented after being informed of its purpose and procedures. Legislators chose whether to participate prior to being informed of treatment assignment or being informed of the bills included in the survey. The survey procedure was described to legislators as follows:

> "If you agree to participate, I'd like to give you quick summaries of 6 bills. After I give you that research I'd like to ask you some questions about the legislation, namely how much you think the bills will cost and whether you think you could support them knowing what you do (either based on my research or whatever you may have heard elsewhere)."

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[1]Legislators are allowed eight years of lifetime service in each chamber, for a total of sixteen years of service in the legislature.

Legislators' offices were approached non-systematically, and their decisions to schedule meetings and then participate in the survey were obviously not random. Table 1 describes the characteristics of legislators who participated in the survey compared to those who did not. It also compares legislators who participated to those who explicitly declined to participate after hearing our description of the study.

Table 1: **Characteristics of Participants: Legislators**

	Participants	Declined	Non-Participants
N	80	13	83
First-term	48%	15%*	30%**
Male	83%	77%	67%*
Republican	74%	85%	69%
Committee Chair	21%	31%	27%
ACU (Dems)	21	22	23
ACU (Reps)	83	81	82

Two-tailed p-values indicated at $p < .05$ (*) and $p < .01$ (**).
Comparison category for both Declined and Non-Participants categories is Participants.

Legislators who participated in the survey were newer to the legislature (48% serving their first term vs. 30% of non-participants), disproportionately male (83% vs. 67%), more likely to be Republicans (74% vs. 69%), and less likely to be serving as committee chairs (21% vs. 27%). Importantly, however, the members of the legislature who participated in the project largely represent the ideological diversity of the legislature. Participants were no more ideologically moderate than non-participants: the average American Conservative Union (ACU) score among participants was 21 for Democrats (and 23 for non-participants) and 83 for Republicans (82). While our sample may have differed from the broader population, it appears representative in its ideological polarization.

Surveys were conducted from February 20th to April 17th 2019. The timing of surveys was not randomized. Surveys were conducted with legislators based on their availability. None of the bills were enacted into law before the last survey was conducted.

Bills

Our experiment focused on one quantifiable dimension of legislation: its cost. Both our measures of legislators' beliefs and our treatment concerned the estimated fiscal impact of legislation. Our main outcome measure is legislators' position-taking on bills via roll call voting.

We selected ten bills on the legislative agenda for the experiment. Bills were selected based on two criteria: 1) they were among the first bills provided a fiscal estimate by the Missouri Committee for Legislative Research (CLR), and 2) the fiscal impacts were estimated to be no greater than plus or minus $10 million dollars. We selected bills based on the timing of their cost estimate because we needed time to draft the survey script and hold meetings with legislators before the bills were resolved; asking legislators their positions on bills they had already voted on, or would never vote on, would by default render our information treatment ineffective. All of our surveys were completed before bills' were resolved. We picked bills with roughly similar fiscal impacts to avoid outliers.

While the bills were not randomly selected, we believe they are largely representative of the type of bills whose fiscal impacts matter to legislators. As a result of budget constraints, Missouri's Fiscal Review and Government Reform committee hears any non-appropriations legislation that impacts the state budget by $100,000 or more. Bills with predicted impacts over this threshold must pass both the committee with substantive jurisdiction and this additional fiscal review committee.[2] Figure 2 displays the projected fiscal cost or benefit for the 61 house bills heard by this committee in 2019, broken out separately for bills that would eventually be enacted into law.[3]

Of the 61 bills, 90% featured a fiscal impact between a $10 million cost and a $10 million savings. Only two bills were projected to cost more than $20 million per year, whereas 40 were projected to cost less than $1 million per year. Despite the seemingly small cost,

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[2]Appropriations-only bills are heard by the Appropriations Committees.

[3]The figure excludes 22 house bills with no known or unknown fiscal impact. The Speaker can also refer legislation without a fiscal impact, or with an unknown impact, to the Fiscal Review committee.
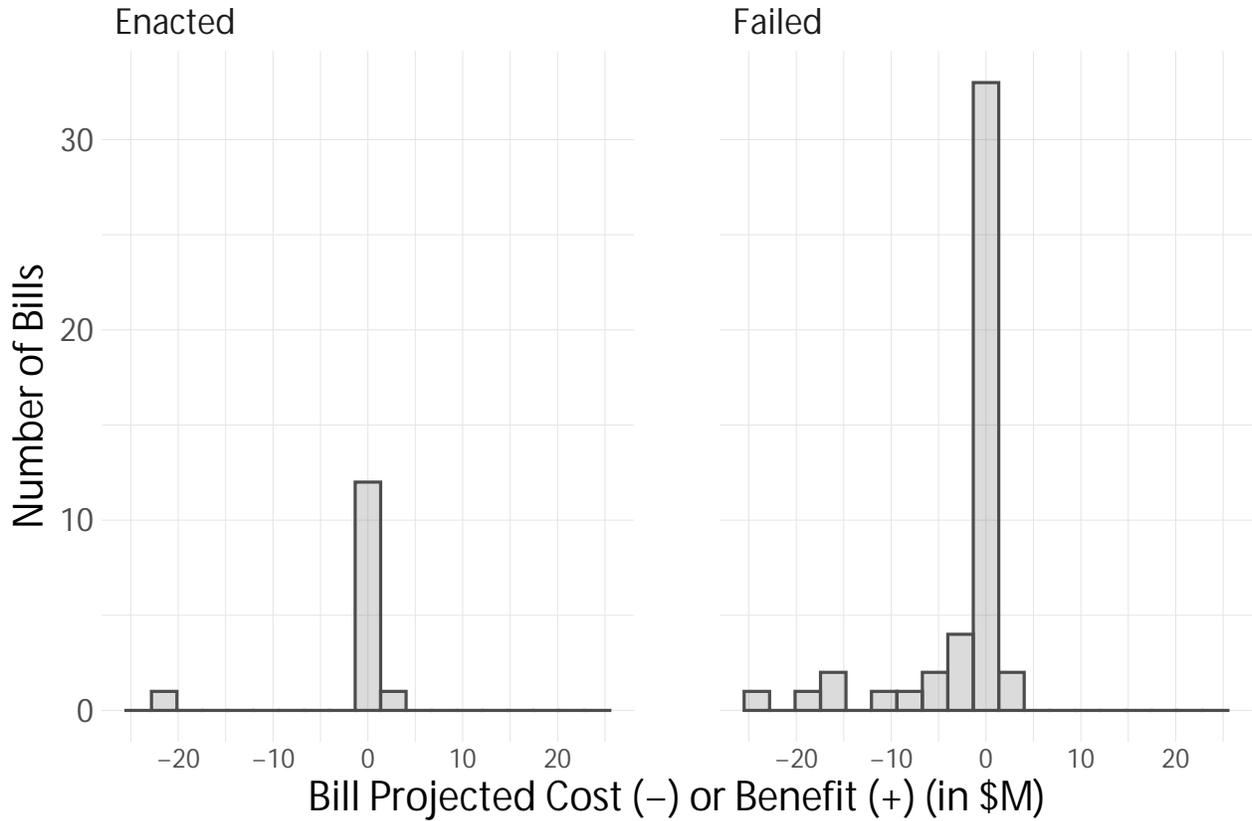
Figure 2: Histogram of cost for bills heard by Fiscal Review Committee in 2019.

77% of bills heard by the Fiscal Review Committee — bills that had already passed their substantive House committee — were not enacted. These 47 failed bills would have added, in aggregate, $112 million to the budget. While the bills may not seem overly costly on their own, bills of this size are central to the fight for fiscal responsibility in state legislatures.

Treatment

The unit of treatment-assignment is the legislator-bill dyad. For each of the 80 legislators, the 10 bills in the study were assigned to one of three conditions: 1) four bills to *pure control* - these bills were omitted from the survey for this legislator; 2) three bills to *survey control*; and 3) three bills to *survey treatment*. We utilized complete random assignment across legislator, so different bills were assigned to each condition for each legislator.

Treatment information about the bills included in the surveys is displayed in Table 2.

For the three survey control bills per legislator, legislators were read a short bill description that summarized what the bill does; this information is akin to the bill caption that appears in legislative calendars, or on legislative websites, that tells legislators what bill is being discussed. This bill description included the sponsor of the bill.

For the three bills in the survey-treatment condition, legislators were given the sponsor and bill description plus key supplemental information related to the bill's fiscal impact: the salient bill details on which the CLR based its fiscal forecast. We did not, however, provide the CLR's forecast itself. The four pure control bills were not included in the survey for that legislator.[4]

The survey treatments are, in a way, math problems meant to guide legislators to the bill's anticipated cost (or savings) to the budget. They include the number of new state employees necessary to implement the policy; the number of individuals eligible for the program; or maximum appropriations for the program. If legislators believed the estimates and had time and interest to work through them, they should arrive at the CLR's estimate.

CLR forecasts are publicly available information, but there are several reasons why legislators may have been unaware of them at the time we conducted the surveys. Over 1,250 bills were filed in the 2019 session in Missouri, and 102 passed the legislature. Legislators simply do not have time to analyze each bill in depth. They may rationally ignore bills until they are required to consider them in committee or on the floor. Even then legislators may make decisions based on cues rather than in-depth study of legislation. Even if legislators did seek out a CLR report, fiscal analyses are dense, lengthy documents. They may include forecasts from several executive agencies, over multiple years, stretching to many pages. We assume that, absent treatment, most legislators would be unlikely to read the CLR reports for most bills in our study. If they had done so, the information would be less novel and, as a result, less influential.

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[4]We included a larger number of bills than we were able to survey both to gain greater diversity in the profile of bills studied and because the pure control bills are themselves useful. Although we do not collect legislators' beliefs about their cost or positions on them, we can collect legislators' roll call votes on pure control bills.

The treatments may seem stacked in favor of finding large learning effects because the information is so clear. We do not find that conclusion overly problematic. The information comes from actual legislative research reports, so it is a naturalistic treatment. Research from budget offices is meant to inform legislators, so we should expect it to be effective. Further, the treatments do not include arguably the strongest, most straightforward piece of information in the reports: the projected cost. To the extent legislators only care about the cost of legislation, the treatments will lead them to only partially update their beliefs.

Rather than testing the null hypothesis that information does not change beliefs, we are more interested in how much legislators update beliefs, whether learning varies across legislators, and whether learning about policies influences bill support. To answer these questions, large learning effects are useful. As we will see, even with these blunt information treatments, learning is not as widespread as we might have expected.

Outcome Measures

We collected measures of legislators' fiscal beliefs and policy positions. The survey asked legislators for their estimate of each bill's cost and whether they supported it. We also collected two behavioral measures of bill support, cosponsorship and roll call voting.

Six of the ten bills in our sample reached a roll call vote. As a result, roll call data exhibits attrition. [5] We believe it is unlikely that our treatments influenced whether bills reached a vote. We surveyed only half of the legislators in the lower chamber, and each legislator was only surveyed on six of the ten bills, chosen at random for each legislator, to keep the survey at a reasonable length. On average, then, only about 25% of the chamber was briefed on each bill. Even substantial effects on bill support among those legislators in the survey treatment condition would not translate to large aggregate shifts in the legislature as a whole. For the analysis of roll call voting, then, we assume that attrition was independent of treatment

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[5] Attrition results primarily from bills not reaching a vote. Another possible form of attrition is legislators failing to vote on a bill that did reach a vote. We conduct analysis both excluding such votes — and implicitly assuming attrition at random for such observations — and by coding votes as opposition. Since bills need a constitutional majority to pass, failing to vote, in practice, counts as a vote against a bill.

Table 2: **Description of Bills: Legislators**

Bill	Sponsor	Description	Supplemental Information	Cost
HB26	Stacy	Changes the law regarding primary elections. Two components of fiscal cost: 1) software to allow voters to indicate their partisan preferences at polling places; and 2) additional poll workers.	There are over 2,700 polling places, each of which needs a $500 tablet containing registration software and one additional poll worker, who receive stipends averaging almost $150.	$1.7 million
HB37	Walsh	This proposal extends the expiration date to collect surcharges that are deposited into the DNA Profiling Analysis Fund.	These surcharges are used to process over 20,000 offender DNA samples at a cost of approximately $50 per sample tested.	-$1.0[1]
HB43	Burnett	This bill increases the penalty for animal abuse from a class A misdemeanor to a class E felony.	Adds two assistant public defenders and increases incarcerated population by about 25 individuals per year at a cost of about $6,000 per.	$0.3
HB299	Hansen	Establishes a tax deduction for 100% of un-reimbursed educator expenses incurred by an eligible educator, not to exceed $500.	Nearly 70,000 individuals took a similar federal deduction, and the estimated tax rate is approximately 5% on such deducted income.	$1.8
HB312	Walker	This bill allows a taxpayer to claim a tax credit up to $500 for the cost of contraception incurred during the year.	The tax credits allowed under this section shall not exceed $5 million in any fiscal year.	$5.1
HB333	Shaul	Allows interest on deposits held at a Federal Reserve bank to be subtracted from a taxpayer's adjusted gross income.	Corporate tax rates of 4% applied to $430,000 of such funds held by taxpayers in Missouri.	$0.1
HB404	Messenger	This proposal establishes the Retirees Experiencing a Better Living Initiative Fund to advertise Missouri to retirees in other states.	In fiscal years in which net general revenue collections grow by $100 million or more, the newly created Retirees Experiencing a Better Living Initiative Fund shall receive 5% of the net difference.	$9.6
HB547	Griffith	Currently $8 million is appropriated for treatment courts.	Current appropriations fund such courts in 45 out of 46 circuits. Legislation would require one additional court to be funded.	$0.2
HB585	Coleman	This bill requires paid tax return preparers to sign any income tax return or claim for refund and provide the preparer's Internal Revenue Service preparer tax identification number. Failure to do either shall result in a fine of $50.	Projected that there are up to 1.4 million returns filed by preparers, and that 1% of such returns may be subject to $100 fine (for violating both signature and TPIN reporting requirements).	-$1.4
HB715	Lynch	Provides higher education financial aid grants to qualified survivors of qualified military members to attend college.	25 grants are allocated per year.	$0.3

(1) Negative numbers indicate a net contribution / savings to state budget.

assignment. Roll call analyses recover the effects of treatments among the subset of bills that would have reached a vote independent of treatment assignment.

Experimental Design: Voters

We conducted a paired survey experiment of voters to complement our study of legislators. The voter experiment was similar in design, but featured some important differences.

We recruited 7,560 individuals via Lucid in the Fall of 2018; 6,501 participated in the survey. We enrolled respondents in three states — Missouri, Colorado, and Michigan — as this experiment was included in a larger survey project.

The demographic profile of respondents is displayed in Table 3. The table includes demographics by state, as well as the raw demographic characteristics of respondents to the nationwide 2020 Congressional Election Study. Respondents to our survey skew away from national levels in several respects. They are more educated, Female, and ideologically moderate. Nevertheless, our survey includes a large number of respondents of diverse backgrounds and characteristics. The survey included a question on political attention, with most respondents claiming to pay attention to what is going on in government and politics between 'Some of the time' and 'Most of the time'.

Voters were presented with descriptions of two bills pending in their states and asked the following:

> we would like you to estimate how much this legislation would cost if rolled out in your state. Like legislators, you will only be given a few pieces of information about the legislation, but we would like you to give your best guess of its cost to the state government. All bills cost between 0−10 million. Please use the slider to indicate how much you expect the following legislation to cost per year (or how much the state will lose in revenue if the legislation were enacted).

After providing their cost estimate, voters were asked "Based on the information you

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Table 3: **Characteristics of Participants: Voters**

	Missouri	Michigan	Colorado	CES
Education				
Less than HS	3.2%	3.0%	2.3%	3.3%
HS Graduate	23.0	23.3	18.2	27.2
Some College	40.7	42.9	40.6	32.6
4-Yr Diploma	21.9	20.4	27.3	23.2
Masters+	11.3	10.4	11.5	13.7
Sex				
Female	63.1	62.3	64.1	57.7
Race / Ethnicity				
Non-Hispanic White	73.7	75.1	74.0	72.3
Black	13.1	12.8	9.4	11.4
Hispanic	4.3	3.9	5.1	8.5
Party				
Democrat	35.4	41.2	36.5	37.3
Independent	26.4	26.8	32.0	28.0
Republican	35.4	29.2	27.5	25.0
Ideology (7-point)				
Liberal (1-2)	16.3	16.9	19.6	26.8
Moderate (3-5)	60.2	61.9	61.4	43.4
Conservative (6-7)	22.6	19.9	17.7	23.3
Attention				
Never	3.9	4.3	4.5	
Some of the time	20.0	20.9	20.8	
About half of the time	19.9	22.3	20.9	
Most of the time	38.3	36.2	36.4	
Always	17.8	16.2	17.4	
N	1104	3607	1790	61000

have been provided, would you support or oppose this legislation?"

Table 4 describes the bill information given to voters, along with the cost of the legislation. Like legislators, they were all provided a description of what the legislation proposed to do, but only in the treatment condition were they given the supplemental information to help them more accurately determine bills' fiscal impact. The supplemental information again was derived from research reports prepared by each state's reference bureau or budget office.

The voter surveys differed from the legislator surveys in several ways. A relatively minor difference is that voters were not provided the sponsor of the bill or the bill number. More substantial differences concern the bills included. First, the bills themselves are different. Because of the timing of the voter survey and its inclusion of voters in multiple states, we did not ask legislators about the same bills as we had asked voters. If some bills are simply easier to forecast than others, legislators or voters may have been given a relatively easier task.

A second difference is that voters were given only bills that impacted the state budget negatively. Thus voters were told that bills' fiscal consequences fell in a $10 million range, while legislators were told a $20 million range. This difference makes it difficult to compare the magnitude of forecast errors between the two groups, and impossible to say if voters or legislators are better able to distinguish bills with negative or positive budget impacts.

Despite these differences we think the tasks given to voters and legislators were qualitatively similar. In both cases were they asked to forecast the fiscal impacts of, and then to support or oppose, bills on the state's political agenda. The bills' fiscal impacts themselves were roughly similar across the two studies. The mean and standard deviation of bills' impacts given to legislators was $1.7 and $3.3, and to voters $1.8 and $2.2 million, respectively.

Table 4: **Description of Bills: Voters**

Bill	Description	Supplemental Information	Cost
MO 1	Proposes a constitutional amendment authorizing $63 million in bonds for veterans' homes.	The primary cost is projected principal and interest payments to repay the bonds within 15 years of the issuance date.	$6.0 million
MO 2	Waives certain business fees with the Secretary of State's office for veterans-owned businesses.	The various fees - including articles of organization, creation of LLCs or nonprofits, and name registration - are about $50 per filing.	$1.0
MI 1	Would allow any veteran free access to any state park or state-operated public boating site.	There are more than 100 qualifying recreation areas in the state.	$2.1
MI 2	Would exempt families of veterans wounded or killed in action from vehicle registration fees for one vehicle.	Currently, there are approximately 3 million registered vehicles in the state.	$1.3
CO 1	Would create the State Women Veterans Office and Steering Committee in the Office of the Governor to ensure that women veterans are aware of and receive the benefits to which they are entitled.	The only cost of the legislation would be to hire two full time employees to staff the office.	$0.1
CO 2	Authorizes the Department of Military and Veterans Affairs to operate a one-stop veterans facility in an underserved part of the state. The facility would offer services including benefits, counseling, housing assistance, family support, identification cards and a service officer.	The only cost of running the facility are two full time employees to staff it	$0.1

Describing Uninformed Beliefs

We begin by describing legislators' and voters' baseline beliefs about legislation using the control condition in each survey. Figure 3a is a histogram of legislators' forecast errors - defined as the difference between their forecast and the CLR forecast - and Figure 3b voters' forecast errors. Positive values indicate overestimates of bills' cost; negative values underestimates. While the CLR forecast is not objective truth, it is a relevant benchmark. For legislators, like those in Missouri, the constitution requires them to pass a balanced budget, and that process relies on CLR forecasts. For practical budgeting purposes, the impact of a bill is what the CLR says.

Both distributions feature a peak around zero but noticeable skew toward positive errors. Both groups overestimate bill costs: legislators by about $1 million, and voters $2 million. The interquartile range for both groups is about $5 million. Legislators and voters were twice as likely to overestimate than underestimate costs; 67% of legislators' forecasts overstated costs and 74% of voters'. On these bills, legislators' and voters' beliefs are biased — they thought bills would cost more than they actually will — and imprecise.
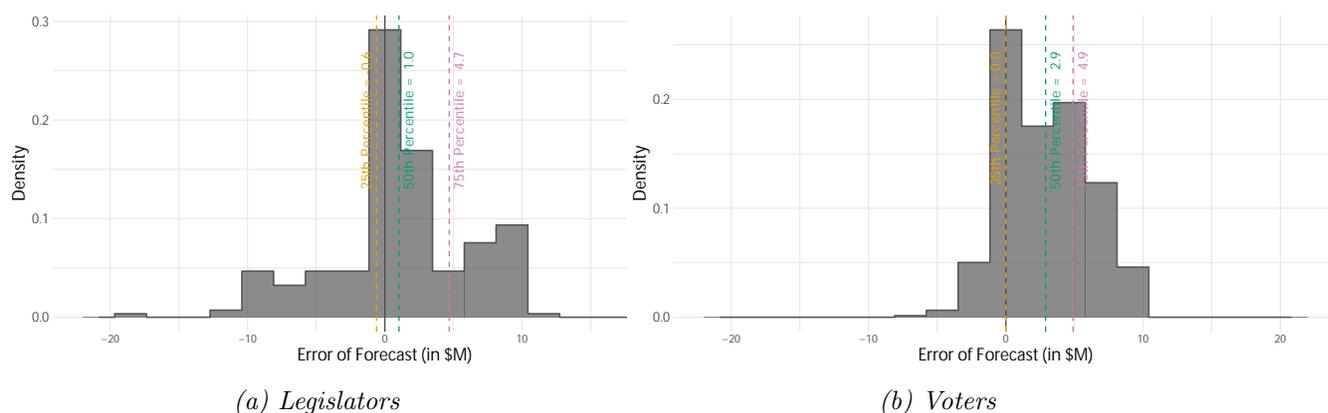

(a) Legislators   *(b) Voters*

Figure 3: Histograms of prior fiscal forecast errors (defined as respondents' estimates less official estimates).

There are several reasons individuals may overstate costs. Least interesting would be if these results are artefacts of the survey. Errors may depend on the ten bills in this study or on the particular wording of the surveys. There may be ten other bills where beliefs understate

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costs by $1.2 million on average, or it may be that the survey descriptions were misleading. A mechanical result whereby respondents overstated costs because bills were clustered in the lower end of the range we provided may explain these results. This explanation is more plausible for voters, since three of the six bills cost $1 million or less and five of the six $2.1 million or less, while legislators were presented a more balanced and varied selection of bills.

A more interesting explanation would be that individuals systematically overstate bill costs due to risk aversion, pessimism, or ideology. Conservatives who are opposed to government spending may manifest their fiscal hawkishness through excessively pessimistic forecasts. While we tend to think of hawkishness as meaning that an individual rates a bill with a given cost lower than a more dovish person, respondents may also provide pessimistic forecasts to align with their bill positions.

To provide stronger evidence that beliefs are biased in systematic ways, we examine the correlates of legislators' and voters' forecasts via regression. If beliefs were unbiased, the only characteristic that should predict forecasts would be the actual cost of the bill, proxied here by the estimate from the CLR. We also include in our regression covariates specific to the individuals — for legislators, their party, sex, and years of service in the legislature; for voters, their party, sex, education, and attention to politics — to see if some individuals overstate costs more than others. For legislators, we also included two bill-specific variables, the party of the sponsor and whether the party of the sponsor matches the party of the legislator, to see if legislators think one party sponsors more costly bills than the other or rate proposals as more costly if they come from in- or out-partisans. If expectations were rational, there would be a strong relationship between the bills' actual cost and respondents' forecasts, but no other variables would predict forecasts.

Table 5 shows the results of the regression. Legislators' forecasts weakly reflect the bills' forecasted costs. A $10 million increase in CLR's forecasted cost of a bill is matched by only a $2 million increase in legislators' estimates. There is a strong relationship between legislators' party and their fiscal estimates. When Republicans forecast Democrats' bills, or

Table 5: **Prior Belief Bias by Respondent and Bill Characteristics (DV in $M)**

	Legislators	Voters
Bill Characteristics		
Cost	0.21**	0.10***
	(0.09)	(0.02)
Republican Sponsor	3.46**	
	(1.50)	
Republican Sponsor and Respondent	−4.21**	
	(1.72)	
Respondent Characteristics		
Republican	3.69**	0.40***
$\widehat{\text{SE}}$	(1.58)	(0.09)
Female	0.68	−0.01
	(0.81)	(0.08)
Years in Leg	−0.07	
	(0.13)	
Education (4 Yr or Masters)		−0.40***
		(0.08)
Attention (Most or Always)		−0.05
		(0.08)
Black		1.14***
		(0.12)
Hispanic		0.72***
		(0.18)
N	240	5676

Robust, two-tailed p-values indicated at p < .1 (*), p < .05 (**), and p < .01 (***).

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vice versa, forecasts are about $3 million higher than when Democrats or Republicans judge co-partisans. Legislators hold pessimistic views about the spendthriftiness of their opposing partisans.

Voters also weakly reflect experts' forecasted costs. A $10 million increase in CLR's forecasted cost of a bill is matched by only a $1 million increase in voters' estimates. Though we do not examine an in-partisan bias to estimates, Republicans do appear to again overstate costs relative to Democrats, the excluded category. Though the self-declared politically attentive do not appear any more accurate than the inattentive, we do see a strong relationship between education and forecasts: respondents with a four-year or advanced degree forecasted bills as about $0.5 million less costly than other respondents. Black and Hispanic respondents overestimated costs relative to Non-Hispanic White respondents.

These findings of partisan and education-related bias are worth exploring further. For the purposes of this paper, they are evidence that beliefs are systematically biased, but in comparable ways between voters and legislators. Legislators' forecasts were more highly correlated with expert forecasts, but not overwhelmingly so. And in both groups Republicans overstated costs relative to Democrats. Within the survey framework, legislators and voters look to have about similar level of knowledge about legislation.

New Information, Learning, and Posterior Beliefs

We now turn to beliefs when respondents are provided with policy research. We expect that providing research will lead beliefs to converge to the expert forecasts. Figure ?? plots histograms of legislators' posterior forecast errors for all ten bills, and Figure ?? voters' beliefs.

The histogram of legislators' beliefs is noticeably more concentrated near zero; 42% of observations are off by less than $1 million, compared to only 27% of prior errors. The median error is $0.4 million, a 60% decline from prior levels, and the interquartile range has shrunk by 60%, to $2.2 million. Only 62% of observations overstate bill cost, down from

67%. Legislators' beliefs appear to have converged substantially to the CLR forecasts.

Voters' beliefs converge far less. The median error has fallen from $2.9 to $2.0 million, but the interquartile range remains about $5 million and 74% of observations overstate the expert forecasts. Voters appear not to have converged quite as much as legislators.
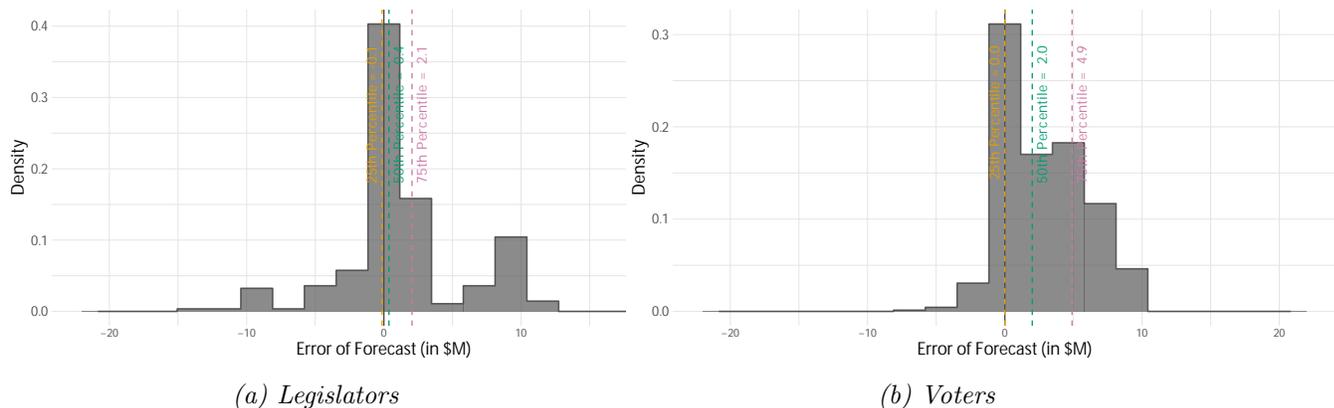

(a) Legislators   *(b) Voters*

Figure 4: Histograms of posterior fiscal forecast errors (defined as respondents' estimates less official estimates).

We more formally estimate learning via OLS regression. Table 6 displays four analyses for both legislators and voters. Each analyses regresses a dependent variable based on legislators' beliefs on an indicator variable for treatment. Regression (1) uses OLS and forecast error as dependent variable; (2) uses quantile regression and forecast error as dependent variable; (3) uses OLS and the absolute value of forecast error as DV; and (4) uses OLS and an indicator variable for whether forecasts overstate the truth.

We use a range of analyses to provide a more complete picture of learning. If respondents were rational learners, we would expect negative estimates for all four regressions. Forecasts should converge to the CLR forecasts and thus be less biased on average and at the median (1 and 2) and errors should be smaller and less positive (3 and 4). To improve precision of treatment effect estimates, OLS regressions include fixed effects for bills and legislators or for bills and voter characteristics, respectively.

Table 6 shows that the information treatment did make legislators' and voters' forecasts substantially more accurate. Forecasts were closer to the CLR forecast by $800,000 on

Table 6: **Learning: Estimated Effects of Information on Fiscal Forecasts**

	Legislators	Voters
	(1) DV: Error ($M, Difference-in-Means)	
Intercept[1]	1.22	2.81
$\widehat{\text{SE}}$	(0.32)	(0.04)
Treatment	0.06	-0.13^*
$\widehat{\text{SE}}$	(0.31)	(0.05)
	(2) DV: Error ($M, Difference-in-Medians)	
Intercept	1.03	2.88
$\widehat{\text{SE}}$	(0.14)	(0.07)
Treatment	-0.66^{***}	-0.88^{**}
	(0.23)	(0.37)
	(3) DV: Absolute Error ($M)	
Intercept	3.89	3.34
$\widehat{\text{SE}}$	(0.24)	(0.03)
Treatment	-0.77^{**}	-0.25^{***}
	(0.30)	(0.05)
	(4) DV: Error > 0 (in pp)	
Intercept	67.1	74.0
$\widehat{\text{SE}}$	(3.1)	(0.5)
Treatment	-6.0^*	-0.6
	(3.4)	(0.7)
N	480	12914

Robust, two-tailed p-values indicated at $p < .1$ (*), $p < .05$ (**), and $p < .01$ (***).

(1) Intercepts represent mean (or median) values in the control group, i.e. the intercept in a regression excluding covariates and fixed effects.

average, a 20% decline in error, for legislators and $250,000, a 7.5% decrease, for voters. Legislators and voters both converged to the fiscal forecasts of the experts when provided supplemental information. Estimated effects are larger for legislators.

Both legislators and voters also were less biased against the bills, although the evidence is slightly more mixed. Legislators' average forecast continued to overstate the truth by about $1.2 million, a minimal difference from control. However, the median forecast changed dramatically. Treatment caused the median estimate to fall from a $1 million overestimate to a $0.4 million overestimate. Further, treatment is estimated to have decreased the percentage of forecasts that overstated costs, from about 67% to 61% of observations. Treatment brought legislators' beliefs closer to the truth and reduced their bias. Voters' bias declined by a minor $130,000 on average but $880,000 at the median. Voters continued to overstate costs at a 74% rate, however.

Taken together, the evidence shows that legislators and voters converge largely as expected of rational learners. Beliefs move toward the evidence, are less likely to overstate costs, and, at the median, are substantially closer to the truth. Legislators appear to learn and respond more to information; estimated effects are larger in percentage terms for several outcomes and legislators, but not voters, become less likely to overestimate costs. But qualitatively, both legislators and voters appear to update similarly.

We conclude our analysis of posterior beliefs by returning to the analysis of forecast bias. Absent treatment, both legislators and voters overstated costs, with intuitive patterns based on respondents' partisanship and level of education. Did treatment not only reduce overall bias, but affect these patterns? Table 7 displays the correlates of post-treatment forecasts. The only covariate that predicts legislators' posterior forecasts is the CLR forecast. The strength of this relationship has slightly increased after treatment as well. No other covariate predicts legislators' forecasts. The magnitude of the partisan variables have declined dramatically in magnitude and statistical significane. Whereas legislators' had predictable prior bias, they no longer have systematic posterior bias.

Voters, on the other hand, continue to demonstrate the same biases post-treatment. Voters provide post-treatment estimates that are much more strongly correlated with expert forecasts than pre-treatment forecasts. Yet, Republican respondents continue to overstate costs and more educated respondents continue to understate them.

Table 7: **Posterior Belief Bias by Respondent and Bill Characteristics (DV in $M)**

	Legislators	**Voters**
Bill Characteristics		
Cost	0.33***	0.28***
	(0.09)	(0.02)
Republican Sponsor	1.04	
	(1.06)	
Republican Sponsor and Respondent	−1.22	
	(1.27)	
Respondent Characteristics		
Republican	0.78	0.39***
$\widehat{SE}$	(1.58)	(0.09)
Female	−0.68	−0.04
	(0.67)	(0.08)
Years in Leg	0.05	
	(0.12)	
Education (4 Yr or Masters)		−0.47***
		(0.08)
Attention (Most or Always)		−0.23
		(0.08)
Black		1.14***
		(0.12)
Hispanic		0.24
		(0.19)
N	240	5734

Robust, two-tailed p-values indicated at p < .1 (*), p < .05 (**), and p < .01 (***).

The prior section showed that legislators and voters held systematically biased beliefs about bills. This section showed that both respond reasonably to new information. Both converged to the truth as contained in expert forecasts. Legislators learned more, but this difference could be attributed to the different survey formats. The one important qualitative difference between legislators and voters is that voters retained bias post-treatment.

Legislators learned so much that their biases faded in importance, but voters did not. This result should be interpreted with caution. Because voters responded to an online survey and legislators were responding face-to-face with an enumerator, voters may simply not have taken the survey as seriously. However, the result is still important: legislators learn enough from new information that their biases fade in significance; voters do not. Voters may behave more like legislators if placed into legislators' shoes, but in their respective arenas, legislators appear to more responsive to information and less beholden to biases.

Taking Reasoned Positions

The final stage of decision-making is actually supporting or opposing a proposal. Since Miller and Stokes (1963), political scientists have defined reasonable decision-making in terms of the strength of the relationship between key dimensions of a policy, perceptions of those dimensions, and policy positions. For example, the quality of legislative decision-making in Miller and Stokes (1963) was deemed relatively strong, but not superb, in part because legislators were taking positions based on their inaccurate perceptions of constituent preferences moreso than constituents' actual preferences. Since we are looking at another dimension of legislation than constituent support, its fiscal cost, we examine the relationship between policy support and both real and perceived costs of legislation. We assume that, all else equal, a reasonable decision-maker prefers policies that cost less.

To what extent did legislators and voters take positions that reflected the fiscal consequences of legislation, or at least their perceptions of the bills' consequences? To what extent did treatment align positions with beliefs? We examine the relationship between bills' consequences and position-taking through two types of regressions. First, we regress support for a bill on bills' actual cost (as determined by the legislative research bureau), an indicator variable for treatment, and an interaction between these two terms. This regression describes the correlations between bill support and bills' actual cost in both the control and treatment conditions. In an ideal world, decision-makers already have access to this information and

incorporate it into their positions, so there would be a negative correlation between support and actual cost for all observations and no differential correlation in the treatment group specifically.

We also examine the relationship between bill support and perceived costs. We regress bill support on actual cost and perceived cost, interacting both with treatment. This regression shows whether there are relationships between positions and bills' actual cost and beliefs about cost in both control and treatment conditions. Again, we expect a negative relationship between support and actual cost, and possibly a negative relationship between support and perceived cost, if legislators or voters take positions based on their perceptions, rather than the actual, cost of legislation. A differential relationship in the treatment group would again indicate that treatment changes the correlation between support and either actual or perceived cost.

This second regression must be interpreted carefully, since fiscal estimates are provided post-treatment on the survey. There is post-treatment bias, as we showed in the last section that treatment changes perceptions of cost. We perform this regression despite the bias for an important reason. We do not want to argue that treatment influences support heterogeneously by beliefs; we cannot say that, in fact, because post-treatment bias means our treatment may have only changed beliefs and not support. We want to know, however, whether there is a descriptive relationship between beliefs and support, both absent treatment and with treatment. Even though we cannot say whether beliefs, positions, or both changed due to treatment, we can say with confidence whether the relationship between cost, beliefs, and positions is stronger among treatment observations because observations were assigned to treatment conditions at random.

Table 8 displays results from these two regressions for both voters and legislators. The first key result is that there is minimal relationship between bill support and cost absent treatment, for either group. Neither legislators nor voters are able, on the survey, to incorporate the cost of legislation into their policy positions. Equally striking are the large,

consistent, and statistically-significant relationships between bill support and cost in the treatment condition. When provided supplemental information, both legislators and voters are substantially less supportive of bills that cost more. Both groups are limited by their imperfect information to making decisions that do not reflect the true consequences of legislation.

There are additional results of interest in Table 8. First, legislators and voters differ in whether their positions reflect their perceptions of bill cost absent the supplemental information. Legislators' support do not reflect perceived costs, but voters' do. This finding is consistent with legislators knowing their perceptions are unreliable and discarding them in making decisions, while voters do rely on their uninformed beliefs. This interpretation is supported by results from the treatment condition: legislators' positions do reflect their perceptions once they've received supplemental information, while voters' positions are no further aligned with beliefs due to treatment.

A final result worth highlighting is that treatment appears to make voters, and perhaps legislators, more supportive of bills; the standalone estimated effect of treatment is $2-8$ percentage points and, for voters, reaches conventional levels of statistical significance. There are several reasons why treatment may make decision-makers generally more supportive of bills, regardless of their content. One possibility is that treatment conveys the valence of legislation. Perhaps legislators and voters are responding to a perception that researchers want them to support the bill. The most intriguing possibility is that the estimated standalone effect of treatment is related to the interaction effect between treatment and actual or perceived cost. Perhaps legislators and voters are becoming more supportive of bills that cost less, and less supportive of bills that cost more. This interpretation is supported by the fact that the standalone effect is observed even absent the post-treatment measures of beliefs; legislators and voters may be aligning their positions with the costs of bills by means of changing their positions.

The analysis so far shows that legislators and voters take positions on bills in impor-

Table 8: **Position-taking and Policy Consequences (in perc. points)**

	Legislators		Voters	
Intercept	59.0	57.6	56.0	60.4
$\widehat{\text{SE}}$	(3.6)	(4.1)	(0.9)	(1.3)
Treatment	2.4	7.9	3.5***	3.1*
	(5.1)	(5.7)	(1.2)	(1.7)
Actual Cost	−0.6	−0.7	−0.5	−0.4
	(1.0)	(1.0)	(0.4)	(0.4)
Actual Cost * Treatment	−3.3**	−2.6*	−1.9***	−1.7***
	(1.3)	(1.4)	(0.6)	(0.6)
Perceived Cost		1.0		−1.0***
		(1.1)		(0.2)
Perceived Cost * Treatment		−2.3**		0.0
		(1.1)		(0.3)
N		480		12914

Robust, two-tailed p-values indicated at p < .1 (*), p < .05 (**), and p < .01 (***).

tant, similar ways. Both groups are unable to take positions that reflect the true cost of legislation absent supplemental information, though both respond to such information when it is provided. These analyses, as do all that rely on survey measures of policy positions, leave an important question unanswered: would legislators (or voters) have learned about the policies' fiscal impact on their own, absent treatment, and been able to base decisions on their fiscal impact?

We examine the relationship between legislators' policy positions and the fiscal component of legislation using actual roll call votes. Because our survey asked about pending bills in the legislature, several of the bills actually reached the floor and required legislators to take a vote. Six of the ten bills reached a vote. Did treated legislators take positions differently than untreated legislators?

Table 9 displays regressions of legislators' bill support on measures of actual and perceived fiscal cost and an indicator variable for treatment. As in Table 8, we regress roll call vote on actual and perceived cost using observations in the survey treatment and survey control condition in Columns (1) and (2). Again, we are looking for descriptive relationships between fiscal impact and policy positions.

Table 9 includes two types of robustness checks. First, we utilize the pure control group to further examine the relationship between bill support and actual cost. Although we do not have measures of legislators' beliefs for pure control observations, we can nevertheless examine whether treatment caused legislators' positions to correlate more highly with actual fiscal cost, since that measure is available for all bills. The second type of robustness check is to replicate our analysis of survey responses in Table 8 but only for bills that reached a vote. This analysis will show whether legislators actually behaved differently on roll call votes than they did on the survey, or if any apparent differences result from the different bills included in each analysis.

Table 9: **Position-taking and Policy Consequences (in perc. points)**

DV:	Roll Call Votes			Survey (bills reaching a vote)	
	(1)	(2)	(3)	(4)	(5)
Intercept	92.6	93.4	88.0	60.2	56.3
$\widehat{\text{SE}}$	(2.1)	(2.4)	(2.5)	(4.5)	(5.1)
Treatment	1.2	1.1	5.9*	9.3	15.3**
	(3.0)	(3.5)	(3.2)	(6.3)	(7.2)
Actual Cost	−4.6***	−4.6***	−4.6***	0.1	−0.1
	(1.0)	(1.0)	(1.0)	(1.1)	(1.1)
Actual Cost * Treatment	−0.9	−0.9	0.9	−4.9***	−4.5***
	(1.4)	(1.5)	(1.4)	(1.5)	(1.5)
Perceived Cost		−0.3			1.2
		(1.1)			(0.8)
Perceived Cost * Treatment		0.0			−1.9
		(0.8)			(1.2)
Comparison Group	Survey Control		Pure Control	Survey Control	
N		289	311	289	

Robust, two-tailed p-values indicated at p < .1 (*), p < .05 (**), and p < .01 (***).

Table 9 shows that legislators' positions reflect the fiscal impact of legislation among both control and treatment observations, and there is no additional relationship in the treatment condition. A \$1 million increase in bill cost is associated with a five percentage point decrease in probability of supporting a bill ($p < 0.01$). All legislators appear to equally incorporate information about fiscal costs by the time they took a vote. This result is robust to alternative

specifications, including whether we add the post-treatment measure of beliefs or use the pure control group as comparison. Further, this result is not driven by the subset of bills that reach a vote: for observations that reached a vote, legislators' survey responses do not show any relationship between support and actual cost absent those in the treatment condition. This result strongly suggests that legislators are learning about the fiscal consequences of legislation outside the scope of the experiment. By the time they take a vote, they know how much a bill will cost and vote accordingly.

Table 9 shows once again that we estimate a positive standalone effect of treatment, though this effect is smaller in magnitude on roll call votes than survey responses. In addition to showing that legislators become more supportive of less costly bills, this result could indicate that legislators feel pressured to support treatment bills or feel more confident in the content or consequences of treated bills. Additional work is needed to determine the mechanism by which treatment influences legislators' positions.

Discussion

This paper fills an important gap in the literature on legislative persuasion by exploring how persuasion occurs. Several legislative experiments provided information treatments that influenced legislators' position-taking behavior, but they were focused on demonstrating influence rather than the mechanisms behind influence. Is persuasion driven by social pressure? Hawthorne or monitoring effects? Valence? This study showed that policy research is persuasive because it changes legislators' beliefs about the consequences of policy, in this case the fiscal impacts of legislation. Legislators resemble the rational learners of political economy models of legislatures who use information to condition their policy positions on the uncertain mapping of policies to outcomes.

The learning and responsiveness exhibited by lawmakers in this study may not generalize to other settings, of course. The Missouri legislature may be less partisan or accountable than other American legislatures, especially Congress. The bills selected for this study were

not overly partisan or salient, which may make learning relatively easier. Third, we observe persuasion on a subset of bills, those that reached a roll call vote, and those bills may be less contested, or of higher quality, than the broader legislative agenda. Fourth, legislators may have trusted information from their own Committee on Legislative Research more than they would research from lobbyists, thinktanks, or academics.

The experiment both elicited information from legislators and randomized an information treatment provided to them. Eliciting information is time-consuming and challenging — it is easier for legislators to listen to a spiel than to reveal their own opinions — but it provides a crucial link in the study of information and legislative behavior. With legislators' beliefs, we can show not only that information changes behavior, but that it likely does so by the mechanism of changing beliefs about the content of the policy itself.

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