State Politics & Policy Quarterly (2023), **23**: 3, 327–339
doi:10.1017/spq.2023.6

CAMBRIDGE UNIVERSITY PRESS

SHORT ARTICLE

Public Support for Professional Legislatures

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(Received 19 August 2022; revised 31 October 2022; accepted 03 January 2023)

Abstract

Evidence suggests that well-funded, professional legislatures more effectively provide constituents with their preferred policies and may improve social welfare. Yet, legislative resources across state legislatures have stagnated or dwindled at least in part due to public antagonism toward increasing representatives' salaries. We argue that one reason voters oppose legislative resources, like salary and staff, is that they are unaware of the potential benefits. Employing a pre-registered survey experiment with a pre–post design, we find that subjects respond positively to potential social welfare benefits of professionalization, increasing support for greater resources. We also find that individuals identifying with the legislative majority party respond positively to potential responsiveness benefits and that out-partisans do not respond negatively to potential responsiveness costs. In a separate survey of political elites, we find similar patterns. These results suggest that a key barrier to increasing legislative professionalism – anticipated public backlash – may not be insurmountable. The findings also highlight a challenge of institutional choice: beliefs that representatives are unresponsive or ineffective lead to governing institutions that may ensure these outcomes.

Keywords: Legislative professionalism; Survey experiment; State legislatures; Legislative staff; Public opinion

Introduction

The legislative endeavor requires substantial resources. Once a goal is identified, legislators must come to understand the relevant processes that may be reshaped to attain that goal, construct a policy device to incentivize desired behavioral change, and then translate this proposal into legal text allowing the preferred level of bureaucratic discretion in implementation. Given the nature of this process, it comes as little surprise that members working in chambers that provide greater resource support are more apt to learn their constituents' preferences and deliver their

constituents' preferred policy outcomes (Lax and Phillips 2012; Maestas 2000). The resources also enable legislatures to coerce greater effort from bureaucratic agents, which has been shown to improve standards at regulated nursing homes, decrease enrollment delays for welfare benefits, or improve drinking water quality (Boehmke and Shipan 2015; Drolc and Keiser 2020; Williamson, Morris, and Fisk 2021, respectively). Capacity may even condition individual-level legislative behavior by, for example, decreasing race-discriminatory responsiveness (Landgrave and Weller 2020). Despite the commonsense nature of the relationship between legislative capacity and democratic outcomes – and its persuasive documentation in the scientific literature – resource endowments in state legislatures have stagnated or declined in recent decades (Bowen and Greene 2014).

One potential explanation is that voters are averse to providing representatives with resources. A well-documented empirical regularity is that American legislatures enjoy lower levels of institutional support than courts and executives (Kelleher and Wolak 2007) and this perhaps contributes to the similarly well-documented disdain voters have toward the idea of increasing legislator compensation. We see this in the scholarly literature (Theriault 2004) and "in the wild" as voters repeatedly defeat referenda required to increase legislative salaries, for example, in Arizona (2006 and 2014) and Nebraska (2006 and 2012), but pass referenda to limit legislative salaries, for example, in Oklahoma (2006). Squire (2012) documents the consistently low levels of public support for increasing professionalism from 1955 to 1990, with the (relative) peak of support coming in the 1960s and 1970s noting that "41% of all the legislative pay raise measures passed by voters between 1955 and 1990 were adopted during the eight years from 1965 to 1972" (304). Failure of such ballot measures is far more common than success.

We argue that one reason voters are disinclined to support expanding resources for their legislature is because they simply do not know that these resources can redound to their benefit by increasing legislative responsiveness or potentially improving general social welfare.

In this short paper, we assess this argument with a simple, yet persuasive, pre-registered survey experiment[1] employing a pre–post design to test whether subjects change their support for providing representatives more salary and staff when presented with information describing the potential effects of professionalized legislatures. We find overall increases in support when respondents are given a prompt on the good governance, or social welfare, benefits of more professional legislatures and differential responses to a prompt on legislative responsiveness: support for increasing resources increases among in-group partisans, but is unchanged among out-group partisans. A combination of the two prompts was also broadly persuasive, and in a separate, smaller survey of political elites, we find results similar to those for the general public. The results suggest that the public's reticence to provide their representatives with higher salaries or other resources may not be set in stone, but also reveal a challenge of democratic choices over government capacity. Voters believing that their representatives will not be responsive or improve social welfare may be more likely to build governing institutions that ensure those outcomes. Our results, however, show that these beliefs may be

[1]This study was pre-registered through EGAP.

https://doi.org/10.1017/spq.2023.6 Published online by Cambridge University Press

malleable – 22% of treated respondents who previously opposed increasing legislative resources like salary or staff updated to supporting such resources.

A fact-value disconnect over legislative professionalism

Apart from a few articles on voter preferences for legislative salaries (e.g., Cooper 2021; Theriault 2004), most research on legislative capacity is not about the core components of Squire's (2012) professionalism index – salary, staff, and session durations – but rather regard legislative term limits, which reduce capacity by limiting experience and, by extension, expertise. This research suggests term limit imposition is driven by public preferences (Mooney 2009), and assesses the correlates of those preferences (Donovan and Snipp 1994; Karp 1995), but never asks whether or not voters *understand* how term limits may effect function of their government. This is an example of what Cain and Levin (1999) call a fact-value disagreement. Political scientists have attempted to back-out voters' instrumental, or value-based, preferences for institutional structures without first assessing voters' expectations about how those structures may shape outcomes. Without understanding outcome expectation, we cannot infer value-based preferences.

We extend this research in two ways. First, we consider public opinion on legislative staff and salaries, two resources central to facilitating legislative productivity – legislators earning a high wage need not pursue other income sources and can therefore invest more time in the legislative process, and legislative staff gather and process policy-relevant information. Second, we assess whether opinion is responsive to information on the potential effects of these resources in a registered, pre–post experimental design that resolves the fact-value disagreement manifest in much of the extant literature. This design allows us to estimate whether voters respond positively to a "good governance" prime (they do), whether in-partisans respond positively to a responsiveness prime (they do), and whether out-partisans respond negatively to a responsiveness prime (they do not), which in turn allows us to assess the relative value that voters place on good governance (or social welfare) and democratic responsiveness.

Experimentally assessing the value of legislative professionalism

Our survey instrument assesses whether providing voters information on the real-world effects of legislative capacity (as manifest in the political science research) can sway their support for these resources. We employ a "good governance" frame and a "responsiveness" frame, and a frame combining the two, reflecting evidence from recent empirical research (Fortunato and Parinandi 2022; Lax and Phillips 2012). These frames resolve the fact-value disconnect by providing subjects with information on the potential effects of the policy change that are either strictly positive ("good governance"), conditionally positive or negative given the subjects' partisanship ("responsiveness"), or a combination of the two. This allows us to assess whether or not public sentiment on legislative resources is malleable, and whether sentiment is more responsive to general welfare-enhancing action or ideologically aligned policy outcomes. Are voters more attracted to a government that "works" or a government that delivers partisan victories?

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Our experiment's value (like all experiments) is its *internal* validity – resolving the fact-value disconnect and assessing the relative weights subjects place on social-welfare and ideological payoffs with randomized informational treatments. This internal validity comes at some external validity costs – we cannot assess the impact of other types of information or simulate "campaign effects." Thus, any conclusions, we draw from the analysis will be limited to the boundaries of the instrument. Our pre-registered expectations, which differentiate in-party (identify with the legislative majority) and out-party (do not identify with the legislative majority) subjects are as follows:

1 Good governance: Subjects will be more supportive of increasing legislative resources when provided with information about how such resources aid legislators in responding to real problems in their communities (compared to subjects under control).

2a In-party responsiveness: In-party identifiers will be more supportive of increasing legislative resources when provided with information about how such resources aid legislative majorities in responding ideologically to voter preferences (compared to out-party identifiers within treatment and co-partisans under control).

2b Out-party responsiveness: Out-party identifiers will be less supportive of increasing legislative resources when provided with information about how such resources aid legislative majorities in responding ideologically to voter preferences (compared to in-party identifiers within treatment).

3a In-party combination: In-party identifiers will be more supportive of increasing legislative resources when provided with information about how such resources aid legislative majorities in both solving real problems and ideological responsiveness (compared to out-party identifiers within treatment and co-partisans under control, good governance treatment, and responsiveness treatment).

3b Out-party combination: Out-party identifiers will be less supportive of increasing legislative resources when provided with information about how such resources aid legislative majorities in both solving real problems and ideological responsiveness (compared to in-party identifiers within treatment and co-partisans under good governance treatment).

The intuition behind these expectations is straight-forward. Evidence abounds that voters want their government to solve "real problems" (e.g., Hibbing and Theiss-Morse 2002). It is also clear that voters prefer government to deliver ideologically aligned policy; the mixture of policy congruence and "wins" for social identification should be a relatively potent cocktail (e.g., Achen and Bartels 2017). What is unclear is how preferences for good governance and responsiveness 1) compare to one another and 2) stack up against voters' aversion to paying politicians, which new research (Cooper 2021), historical evidence (Squire 2012), and recent events – for example, sound defeat of referenda to increase legislative salaries in Arizona (2014) and Nebraska (2012)[2] – all show is intense. Our instrument allows us to learn both.

In keeping with recent research suggesting that pre–post designs increase precision and allow cleaner identification (or exploration) of heterogeneous treatment effects

[2]Referenda data from respective State Secretary databases.

https://doi.org/10.1017/spq.2023.6 Published online by Cambridge University Press

and are preferred for detecting learning/updating (Clifford, Sheagley, and Piston 2021), our experiment measures support for increasing legislative resources before *and* after treatment, allowing identification of treatment effects from within-subject changes from pre-treatment baseline. Readers will note that the comparison groups for H3a and 3b are more complex than for H1, 2a, and 2b. The comparison for H3a is structured to identify additive effects for in-partisans – the intuition being voter utility over legislative outcomes weighs responsiveness and social welfare benefits *positively* and *separately* such that in-partisans prefer responsiveness *and* solving real problems to *either* responsiveness *or* solving real problems. We further expect that in-partisans would respond more positively to the dual treatment than out-partisans, because the responsiveness treatment provides disutility to out-partisans. These comparison groups are pooled to avoid the multiple comparisons problem. For H3b, the focus is on identifying "crowd-out" effects for out-partisans – that the disutility of responsiveness would crowd-out positive utility from social welfare benefits relative to out-partisans in the good governance treatment or in-partisans receiving the dual treatment. These reference groups are also pooled to avoid the multiple comparisons problem. After assessing our pre-registered expectations, however, we estimate exploratory interacted linear models that allow for more direct comparison.

The outcome measure asks subjects whether they "support providing your state legislators with more resources like salary and staff" and is measured on a four-point Likert scale from strongly oppose to strongly support, where higher values indicate greater support for enhanced resources. Experimental manipulations, given in Table 1, add treatments as pretext to the outcome question wording. Our instrument leaves significant distance between the baseline measure and the experimental manipulations which is preferred for repeated measures design.

Examples of the research alluded to in the treatment frames are Fortunato and Parinandi (2022) for the good governance prime and Lax and Phillips (2012) for the responsiveness prime. The instrument was embedded in a representative survey of Michiganders fielded by YouGov in two waves in October 2020 and March 2021 as part of an omnibus survey by the Institute for Public Policy and Social Research (IPPSR) at Michigan State University.[3] YouGov interviewed a total of ~2,000 Michigan residents across the waves, producing a sample matched to the 2018 American Community Survey (ACS) subset of Michigan residents on gender, age, race, and education. Our sample is therefore representative of the Michigan population, and to the extent that Michigan is representative of the US population as a "purple" state with a diverse population, distributed across both rural and urban areas, our findings may be generalized to the US population. We also stress that, even if limited to a single state, this is a high quality, representative sample of typical Americans, rather than a convenience sample of college students or online taskers (e.g., MTurkers), and, even if Michiganders were somehow anomalous to the US in general (and they are not), our design and inference would still be *internally* valid.

We identify treatment effects and test our pre-registered expectations with *t*-tests comparing the groups identified in the hypotheses and provide further exploration of the results by regressing changes in support for legislative resources on treatment group indicators interacted with partisanship. Covariate-adjusted results that control for race, education, income, age, gender, employment, support for the Michigan

[3]Disclosure: We had access to the first wave data before the second wave was completed.

https://doi.org/10.1017/spq.2023.6 Published online by Cambridge University Press

Table 1. Treatment issue frames

Treatment	Outcome question pretext
Control ($n = 491$)	None
Good governance ($n = 507$)	"Research shows that, when state legislators have more resources like salary and staff, they are better equipped to deal with real problems. For example, states that provide their legislators with more resources had better responses to the opioid crisis and therefore fewer overdose deaths."
Responsiveness ($n = 504$)	"Research shows that, when state legislators have more resources like salary and staff, they are better equipped to give voters the policies they want. For example, among states (like Michigan) where voters elected Republicans to run the legislature, policy is more conservative in states where legislators had more resources."[a]
Combination ($n = 498$)	Combination of good governance and responsiveness issue frames

[a]We provide respondents the legislative majority identity as research shows most cannot identify the party in power in their statehouses (Fortunato and Stevenson 2021).

governor, trust in government, and confine effect estimates to within-wave variation are also provided.

Our instrument was additionally fielded on an elite sample of Michigan policymakers and we describe those data as well. Though we find these data interesting and worth presenting (elite samples are rare), we note: 1) the sample is quite small (roughly 1/9 the size of our primary sample) and leans heavily Democratic, limiting both statistical power and generalizability;[4] and 2) though the instrument is identical to our pre-registered design, this elite study is *not* part of the registration.[5] Therefore, the analysis of these data is purely "exploratory."

Results

Figure 1 describes the raw responses to the pre- and post-versions of the main outcome question by treatment group. There are two important points to take away from the figure. First, pre-treatment responses tend toward opposition, comporting with extant research and outcomes of recent ballot measures. Second, the figure shows that while the control group responses remain stable, all treatment groups show a net upward redistribution of responses to categories supporting an increase in resources.

Table 2 presents the (relevant) average group-level pre–post response differences and group-to-group comparisons (*t*-tests) testing our pre-registered hypotheses. We use pooled comparison groups to avoid the multiple testing problem.[6] The final three columns show the predicted directional difference, the actual cardinal difference, and the probability of no difference between the relevant groups. Note that each of the cardinal differences is signed in the predicted direction. However, while Hypotheses

[4]In 2019, IPPSR compiled this sample from lists of "political insiders" in the state of Michigan, including all state legislators and state legislative staff, executive branch officials in policymaking roles (according to the state), and people listed in the Truscott Rossman lobby guide.

[5]The opportunity to be part of the study was a pleasant, though unexpected, surprise.

[6]Table A5 in the Supplementary Material presents additional comparisons to evaluate our hypotheses based on comparing the relevant treatment group to only one other distinct group at a time. Results are consistent with Table 2.

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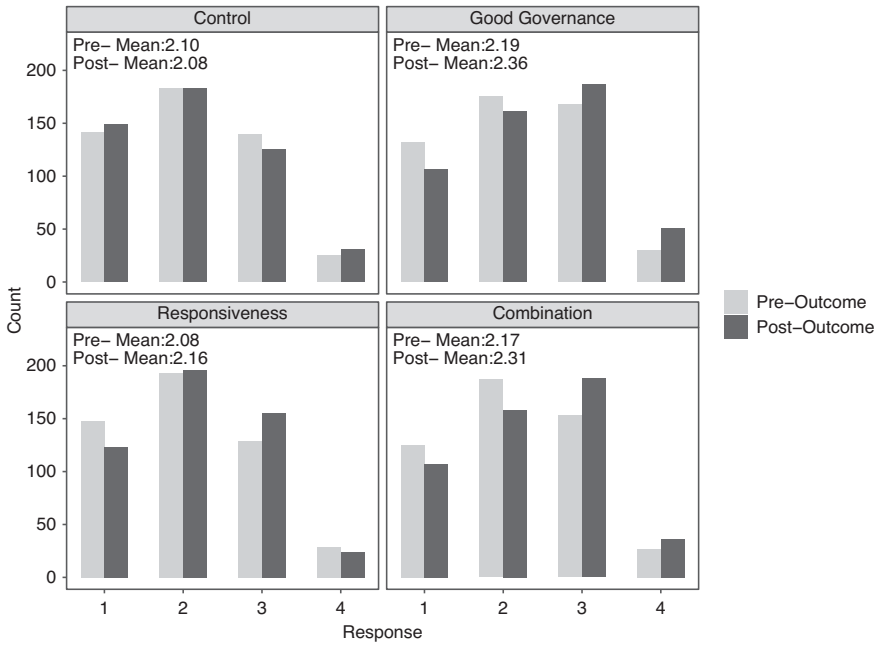

Figure 1. Pre and post response score distributions

1, 2a, and 2b, are supported by the data, Hypotheses 3a and 3b are not. More specifically, potential social welfare benefits of a professionalized legislature significantly increased support for increasing legislative resources, moving the average response about 5% of the total outcome scale. Potential responsiveness benefits had nearly identical effects (about 5% of the total outcome scale) for *in-partisans*, those aligned with the majority party. There was no change in support for *out-partisans*, those likely to see policy losses from the change. We parse the combination of treatments further below, but the data imply no additive effects for in-partisans, and, no crowd-out effects for out-partisans; therefore, no support for Hypotheses 3a and 3b.

Having tested our pre-registered expectations, we estimate a series of linear models to further scrutinize the data. These models allow the same comparisons as the *t*-tests shown in Table 2, but they also permit us to make other, exploratory comparisons (that we did not pre-register) in a framework that is more familiar to many readers. Table 3 presents the full results from basic and interactive model specifications for both the main sample as well as the elite sample.[7] Note that Republicans controlled both chambers of the Michigan Legislature at the time of administration, so we interact treatments with Republican (in-party) identification. The interactions between the treatments and in-party identification allow us to assess

[7]Tables A6 and A7 in the Supplementary Material present results for alternative binary outcome variables measuring movement from opposition to support and post-treatment support, respectively. The results are similar to the results in Table 3 and support the effectiveness of the good governance and combination treatments.

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Table 2. Results of hypothesis testing

Hypothesis	Treatment group with *n* and average change in support			Comparison group with *n* and average change in support			Difference		
							Expected	Actual	*p*-Value
1	Good governance	$n = 507$	0.171	Control	$n = 491$	−0.014	+	0.185	0.0001
2a	In-party responsiveness	$n = 169$	0.219	Out-party responsiveness + in-party control	$n = 490$	0.006	+	0.213	0.001
2b	Out-party responsiveness	$n = 335$	0.006	In-party responsiveness	$n = 169$	0.219	−	−0.213	0.003
3a	In-party combination	$n = 170$	0.148	Out-party combination + in-party control + in-party good governance + in-party responsiveness	$n = 816$	0.140	+	0.008	0.906
3b	Out-party combination	$n = 328$	0.148	In-party combination + out-party good governance	$n = 513$	0.163	−	−0.015	0.783

Table 3. Regression results

	Difference in support							
	General public				Elite sample			
	1	2	3	4	5	6	7	8
Good governance	0.185***	0.184***	0.193***	0.207***	0.209*	0.199	0.245*	0.264*
	(0.046)	(0.048)	(0.055)	(0.058)	(0.125)	(0.128)	(0.131)	(0.134)
Responsiveness	0.093**	0.090*	0.030	0.020	0.186	0.184	0.133	0.117
	(0.046)	(0.048)	(0.056)	(0.059)	(0.124)	(0.125)	(0.133)	(0.136)
Combination treatment	0.162***	0.175***	0.169***	0.179***	0.311***	0.329***	0.421***	0.438***
	(0.046)	(0.049)	(0.056)	(0.060)	(0.119)	(0.121)	(0.128)	(0.132)
In-party			0.030	−0.040			−0.114	−0.233
			(0.070)	(0.094)			(0.226)	(0.307)
Good governance × in-party			−0.025	−0.075			−0.423	−0.489
			(0.098)	(0.104)			(0.370)	(0.375)
Responsiveness × in-party			0.184*	0.206**			0.312	0.343
			(0.098)	(0.102)			(0.322)	(0.324)
Combination treatment × in-party			−0.022	−0.010			−0.532*	−0.565*
			(0.098)	(0.104)			(0.302)	(0.306)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
N	1,972	1,761	1,972	1,761	215	212	215	212
R^2	0.011	0.022	0.017	0.027	0.032	0.085	0.096	0.131

*$p < 0.1$.
**$p < 0.05$.
***$p < 0.01$.

whether, but not why, in-partisans respond differently to our informational treatments than out-partisans.

The estimates show the good governance treatment increased support for legislative resources, relative to control.[8] Among the general public, this frame significantly increased support for more resources by about 0.19 points on average (again, about 5% of the total scale).[9] The global effect of the responsiveness treatment is also positive and significant, but about half the size of the good governance treatment at 0.09 points. Finally, the combination of good governance and responsiveness also exerts a globally positive and significant effect, increasing support by about 0.16 points. These effects largely comport with those we see in the elite sample. All treatments in the elite sample are positive, although estimated with less precision (due to smaller sample size). While the good governance and combination treatments yield a statistically significant positive effect in the uncontrolled model, the responsiveness treatment does not.

The interactive model results in columns 3 and 4 of Table 3 assess all treatment effects by party group, where Republicans are the "in-party" and the comparison "out-party" group are all others.[10] There is little difference between party groups in responses to the good governance prime – though in-partisans appear slightly less moved, it is not statistically differentiable (and the larger difference in the controlled models is more a function of covariate missingness reshaping the sample). We report large partisan differences in reaction to the responsiveness prime, however. Out-partisans are unmoved by the treatment, but in-partisans, as expected, respond very positively, increasing support by about 0.21 points. This is the largest effect within and across groups and treatments. Of course, the treatment is partisan in nature and effects would likely differ with a neutral framing of responsiveness to voter preferences. A neutral frame, however, would not allow us to assess out-party respondents' willingness to accept potential partisan *losses* in exchange for potential social welfare benefits. As it turns out, both in-partisans and out-partisans respond positively to the combination treatment and at very similar levels. We find this result the most interesting as it implies 1) there is no additive effect for bundling social welfare benefits and party-aligned responsiveness and 2) there is no crowd-out effect for bundling social welfare benefits and ideological/partisan losses.

We plot the party-treatment effects for all groups in Figure 2 (recall these are "exploratory" analyses, though the effects are still causal). First, the plot reiterates that responses to the good governance prime are indifferentiable across party groups. Second, within the in-group, no treatment effect is statistically differentiable from another (though all three treatment effects are significantly larger than control at $p < 0.05$ in a directional test) and any apparent differences from visually comparing the estimates do not come particularly close to traditional significance thresholds. Looking across all respondents, it appears as though a legislature that can "solve real problems" is preferred to a legislature that is ideologically responsive, but that

[8]Results do not meaningfully change when including covariates, as expected given the balance table presented in the Supplementary Material (Table A1).

[9]The standardized effect sizes (using Glass's delta and model 2) for the treatments are the following: about 0.31 for good governance, about 0.27 for the combined treatment, and about 0.15 for responsiveness.

[10]We identify Republicans and lean Republicans as in-party, leaving strict independents, lean Democrats, and Democrats as members of the out-party comparison group. Moving independents to the in-group changes substantive results very little.

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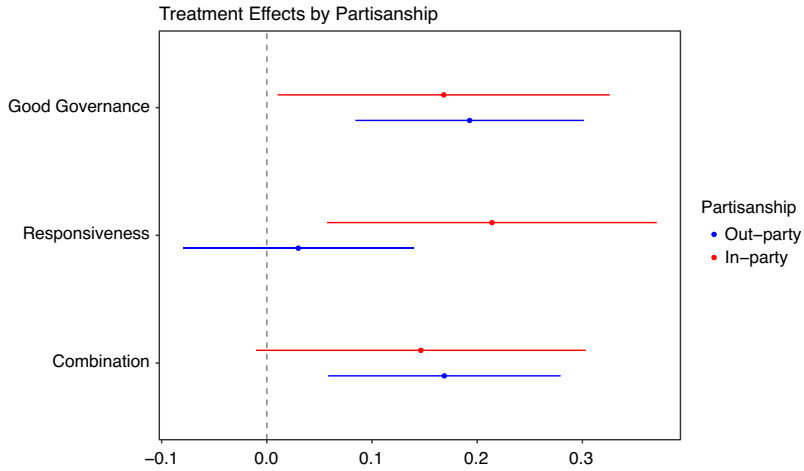

Figure 2. Conditional average treatment effects by partisanship

difference is driven by out-party ambivalence toward responsiveness, rather than an aggregate, bipartisan consensus on good governance vis-á-vis responsiveness.

A final point to note is that, because we draw from one state, we cannot distinguish in- and out-party effects from Republican and Democratic effects. Thus, while we can say that Democrats do not respond negatively to the responsiveness treatment when they are in opposition, and that there is no crowd-out effect of bundling out-party responsiveness with good governance for Democrats, we do not know if the same would hold with Republicans if they were the legislative minority. Likewise, we cannot say with confidence that Democratic identifiers would respond positively to responsiveness treatments if they were in the legislative majority (although the prima facie case for that expectation is strong).

The elite sample results, which, we reiterate, are exploratory, are given in models 5–8. These estimates broadly but imperfectly comport with the sample of the general public. The sign on all treatments, comparing global results, and all treatments and party-treatment interactions, comparing interactive results, match across samples. However, the elite sample effects are estimated with much less precision. Furthermore, the positive effects of the good governance and combination treatments are washed out within the in-party group (Democrats outnumber Republicans 2 to 1). Michigan's high level of existing legislative professionalism may explain reduced support for additional resources among elites, especially Republicans. While it is tempting to speculate on potential other reasons for these differences, we believe that would be inappropriate given the small, imbalanced sample. That said, these data do hint at interesting differences between (particularly Republican) elites and the general public and we hope that our colleagues will pursue them.

Discussion and conclusion

This research note has provided experimental evidence that voters are more supportive of increasing their state legislature's resources (members' salary and staffing) when shown this could yield social welfare benefits. We also learned that in-partisans

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respond positively to party-aligned responsiveness benefits, but that out-partisans do not respond negatively to misaligned responsiveness costs. There are a few important implications. First, given the strong positive effects (on average, across treatments), we may safely infer that the typical voter does not understand the potential effects of improving legislative capacity. This demonstrates, second, just how important it is to resolve fact-value disconnects in opinion research. How much less popular would term limits be if the typical voter were as familiar with their effects as the typical political scientist? This leads to a third implication: the evidence here shows that a central hurdle to increasing legislative capacity, anticipated voter backlash, is not insurmountable; voters are simply unacquainted with the potential effects of such a change.

This is all the more meaningful in light of recent research showing that demand effects are effectively nonexistent in survey experiments, particularly in high-quality (i.e., not convenience) samples of the type we analyze here (Mummolo and Peterson 2019). In other words, we can be confident that our respondents are not updating because they believe we want them to update; they are updating because they are learning something new about legislatures. Indeed, 22% (212 out of 960) of treated respondents who initially opposed increasing legislative resources (scores of 1 or 2 on the outcome scale) later supported such resources (scores of 3 or 4) – this is inclusive of out-party respondents who stand to bear costs of increased responsiveness.

Our findings also clarify a challenge of institutional choice. In contexts where cynicism over the goals and behaviors of representatives proliferates, voters are unlikely to believe that legislators want to deliver popular policies or efficiently manage real problems. In this case, voters may prefer slashing legislative resource endowments, including salary and staff. Because low levels of legislative capacity retard responsiveness and good governance, these cynical beliefs become self-fulfilling – those who do not believe government can work build institutions that ensure it will not work.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/spq.2023.6.

Data availability statement. Replication materials are available on SPPQ Dataverse at https://doi.org/10.15139/S3/E5CMA8 (McCrain 2023).

Acknowledgments. We are grateful for helpful comments from Matt Grossmann.

Funding statement. The authors received no financial support for the research, authorship, and/or publication of this article.

Competing interests. None.

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Cite this article: Fortunato, David, Joshua McCrain, and Kaylyn Jackson Schiff. 2023. Public Support for Professional Legislatures. *State Politics & Policy Quarterly* **23** (3): 327–339, doi:10.1017/spq.2023.6