How Private Politics Alters Legislative Responsiveness

James Druckman

Payson S. Wild Professor of Political Science
IPR Fellow
Northwestern University

Julia Valdes

Visiting Assistant Professor of Politics
Lake Forest College

Version: October 5, 2018

DRAFT

Please do not quote or distribute without permission.

ABSTRACT

Private politics occurs when citizens and activists seek policy change outside the democratic process. This includes boycotting companies and/or buycotting products so as to influence market practices (e.g., increased wages, more attention to environmental impact). The rise of private politics complicates our understanding of democratic responsiveness—legislators may be less incentived to respond to citizens' preferences. This occurs because legislators receive less credit for policy change and may view themselves as less necessary for policy-making. The researchers present a survey experiment with state legislators to explore how legislators react to private politics. They find that a constituent communication that references private politics vitiates legislative responsiveness. In particular, Republicans become less likely to say they would take policy action or move their positions. Moreover, reference to private politics decreases the likelihood of constituent engagement among both Republican and Democratic legislators. The results accentuate the importance of considering private politics in conversations about how democracies work.

The democratic ideal is that public policies reflect citizens' preferences. This works most straightforwardly when elected representatives enact policies that echo the desires of their constituents. However, alternative mechanisms for citizen input exist, such as direct democracy where citizens independently shape public policy (e.g., Gerber 1996). Another alternative, which has become increasingly impactful, involves citizens and activists expressing themselves in the private realm via boycotting businesses and/or buycotting products. They often do this to bypass formal democratic (legislative) practices and induce companies to alter their behaviors. This approach has become more feasible as non-governmental organizations (NGOs) exploit communication technologies to orchestrate boycotting and/or buycotting efforts (e.g., Baron and Diermeier 2007; Reid and Toffel 2009). Examples include consumers refusing to buy Nike products due to worker conditions, shunning Home Depot and Kinkos due to their deforestation practices, and boycotting Walmart due to low worker wages (e.g., Baron and Diermeier 2007; Copeland 2014; Kam and Deichert 2017).

How does private politics affect legislative responsiveness? Do actions outside the public sphere cause elected officials to become less directly responsive to their constituents? If so, this complicates our understanding of democratic governance. Decisions about public policy would lie increasingly in the hands of the interests that organize boycotts and buycotts rather than government that, in theory, represents citizens writ large. That said, practically speaking, citizens are not always equally represented via democratic legislative representation (e.g., Soroka and Wlezien 2010); it could be that extra-systemic private politics efforts offer a mechanism for "better" representation.[1] Either way, any effect of private politics on legislative behaviors

---

[1] For example, efforts to boycott Uber induced the company to change security practices that may be more in line with consumers' preferences—and this may not have happened through formal governmental processes.

accentuates the need for scholars to carefully consider alternative routes to policy outcomes, and democratic functioning more broadly.

Public Versus Private Politics

Public politics refers to activities taken by citizens or NGOs to push an agenda through governmental means such as the legislative process. This includes citizens contacting their representatives or attending public meeting, and interest groups lobbying legislators. These activities are meant to induce governmental responsiveness, such that policymakers enact laws that reflect citizens' preferences. The literature on the extent to which and to whom policymakers respond is large (e.g., Bartels 2008; Soroka and Wlezien 2010; Gilens 2014; Druckman and Jacobs 2015). Of particular note for our purposes is work that utilizes field experiments or surveys to identify how individual legislators—often at the state level—respond to constituents' messages (e.g., Bergan 2009; Butler and Broockman and 2011; Butler 2014; Butler and Dynes 2016). For example, Flynn (2018) asked state legislators to consider a sample e-mail from a constituent who appeared well-informed, uniformed, or misinformed about the state sales taxes. He finds legislators report that they would be more likely to respond to uninformed constituents and less likely to respond to misinformed constituents; legislators also report a higher likelihood of responding with factual information when the constituent is uninformed or misinformed.

Why would legislators respond to constituent requests at all? The answer is simple: 1) most legislators on the national and state levels aim to be re-elected, and 2) being responsive allows them to placate voters by taking positions preferred by voters and claiming credit for representing voters (e.g., Druckman, Kifer, and Parkin 2009). Private politics, however, is an alternative process that may alter legislator incentives. Private politics refers to "actions by private interests such as activists that target private agents, often in the institution of public

sentiment" (Baron and Diermeier 2007: 600; also see Baron 2003). As mentioned, in many cases, NGOs coordinate large scale boycotts of companies so as to induce firms to alter their practices (e.g., wages, climate change efforts, worker conditions).[2] Our interest is in the consequence of such practices on legislators' behaviors and responses. As far as we know, this question has gone unaddressed. Emergent literatures study citizens' decisions to engage in private politics (Zukin, Ketter, Andolina, Jenkins, and Delli Carpini 2006; Stolle, Hooghe, and Micheletti 2005; Benstead and Reif 2017; Endres and Panagopoulos 2017; Kam and Deichert 2017) and NGO-firm strategy (e.g., Baron and Diermeier 2007; Reid and Toffel 2009), but not legislative responsiveness.

Our focus is on legislators' responses to constituents. However, given private politics is most credible when an NGO is in play (Diermeier 2007), we attend to a situation where a constituent references NGO activities. Likewise, public politics efforts have more impact when reflecting collective opinion and/or campaigns (e.g., Bergan 2009), and thus we include an NGO reference when conceiving of a public politics communication. Without an underlying NGO campaign, it is not clear legislators would respond to a single or even a few e-mails: signaling the larger NGO effort is an important part of the communications. Our "private politics" therefore has the constituent reference an NGO campaign aimed at companies in the market place. Our "public politics" involve a constituent contacting a legislator in accordance with an NGO call to pursue change via governmental channels.

---

[2] Endres and Panagopoulos (2017: 7) report that, in January 2016, 35% of respondents (from a nationally representative survey) reported that they participated in a boycott and/or buycott during the previous 12 months. This is similar to the 2016 American National Election Study pilot that shows 32% of respondents engaging in boycotting and 22% engaging in buycotting within the past 12 months. As Kam and Deichert (2017) point out, these percentages rival or exceed the percentages of governmental and many forms of campaign participation (e.g., 28% report contributing to campaigns).

When an NGO coordinates a private politics campaign, it vitiates the extent to which legislators can claim credit for a policy change (because the change occurs outside of the public domain). This leads to our first prediction: relative to a constituent communication referencing public politics, a communication referencing private politics will decrease the extent to which a legislator believes he or she can claim credit for a policy, all else constant (Hypothesis 1).

It follows that an inability to claim credit means that legislators will be less incentivized to take action, such as pursuing legislation on the issue. That is, because the constituent (and accompanying NGO) will attribute credit to their own market actions, legislators will prove less likely to allocate their capital and resources to pursuing the issue. In contrast, when a public politics track is taken, the legislator has a clearer payoff to taking action and thus will be more likely to do so. Alternatively, it could be that reference to private politics leads a legislator to believe the market is providing an adequate policy solution and therefore there is no need for (state) legislative action, regardless of credit. Whether due to credit claiming opportunities or market efficiency, we expect that relative to a constituent communication referencing public politics, a communication referencing private politics will decrease the extent to which a legislator will take action on the issue mentioned (e.g., sponsor legislation), all else constant (Hypothesis 2).

Related to this prediction is whether the legislator will move his or her issue position in response to the constituent's communication. It could be that lost credit claiming opportunities and/or a belief in effective market solutions leads to less position taking movement in the private politics case. However, position taking, even sans credit, is an important re-electoral strategy (Mayhew 1974). It also does not entail the resource expenditure that comes with bill sponsorship: why not align a position with what constituents seem to want if it helps re-election,

even if there is no credit claiming opportunity or a need for government intervention? Thus, we expect equal position taking stances regardless of whether private or public politics are pursued—in both cases, we expect alignment in the direction of the position advocated, all else constant (Hypothesis 3).

A final area of responsiveness concerns legislator engagement with the constituent (e.g., Fenno 1979). As Neblo, Esterling, Kennedy, Lazer, and Sokhey (2010) make clear, many citizens value interactions, which can generate efficacy and increased trust (also see Neblo, Esterling, and Lazer n.d.). We suspect that when a citizen prioritizes private politics in communications, legislators will be less likely to respond, presuming such citizens are less likely to be supportive in the domain of public politics (vote, donate, etc.). Relative to a constituent communication referencing public politics, a communication referencing private politics will decrease the likelihood that a legislator will respond to the communication, all else constant (Hypothesis 4). When a legislator does, however, respond to the private politics communication, relative to the public politics communication, he or she will be more likely to cite legislation, all else constant (Hypothesis 5). A legislator does so to perhaps try to make clear that public politics is another possible channel and one where the legislator could obtain some credit.[3]

In sum, we suspect that private politics can alter political responsiveness by de-incentivizing legislators from engaging their constituents and from pursuing legislation. This then complicates the nature of democratic representation: citizens' policy input via the private politics route lessens their influence via the public politics route. This, in turn, might lead citizens to further avoid unresponsive legislators and continue to pursue private politics with greater frequency.

---

[3] That said, Hypothesis 5 may not hold if the legislator strongly believes that private politics should be continually pursued, with the legislature/state being left out of the process.

Survey Experiment

We tested our predictions with a survey experiment conducted with state legislators (for similar approaches, see Butler 2014: chapter 4; Harden 2013; Butler and Dynes 2016; Flynn 2018). State legislators face analogous electoral logic to members of Congress, are much larger in number, and are more likely to respond to requests for completing a survey.[4] Further, Anderson Butler, and Harbridge (2016: 615) offer evidence that state legislators' survey responses map onto their actual behaviors (i.e., roll call votes) (also see Anderson, Butler, and Harbridge-Yong 2018).

We sought to gauge response to constituent messages on the issue of raising the state's minimum wage to at least $15 an hour. Interest groups have actively targeted both businesses (e.g., McDonald's) and state legislatures to raise the minimum wage. In our treatments, we referenced one of the more notable groups, *Fight for $15* (https://fightfor15.org/); we do so because, as mentioned, private politics are most effective/realistic when orchestrated by a larger NGO (Baron and Diermeier 2007).

We conducted the survey experiment in the summer of 2016 by sending an e-mail invitation to every legislator in 48 states; we excluded New York and California because they had already passed laws to increase the wage in 2018 and 2023, respectively. We utilized an assembled database of 7,338 publicly listed email addresses for legislators (Williams 2015); 663 individuals responded (a 9% response rate, which compares with that found in Butler and Dynes 2016: 978, and Flynn 2018). Of those 663, though, roughly 109 exited the survey after answering no or only a few questions (i.e., none of our outcome variables). The median age in the sample is between 55-64, 63.3% reported being male, 12.52% reported being a racial minority, 48.8%

---

[4] We did not use an audit study approach because we hoped to capture responses on a number of key outcome variables and it was unclear how to do this with an audit approach.

reported being Democratic, and 48.1% reported being Republican. This makes for a fairly heterogeneous sample that is in-line with population figures on state legislators where, albeit from a time slightly before our survey, the average age is 56, about 76% are male, and 52% are Republican (Anderson et al. 2016; Kurtz 2015).[5]

We invited respondents to participate in a survey about "how state legislators interact with constituents who send them e-mails." After obtaining consent, respondents were randomly assigned to one of three conditions, as portrayed in Table 1. Each condition involved providing the legislator with an e-mail that could come from a constituent arguing that the state's minimum wage should be increased to $15. The public politics condition mentions *Fight for $15*, stating that the goal is to have constituents contact representatives as it is the most effective way to enact change on this issue (N = 227).[6] In contrast, the private politics condition accentuated how the *Fight for $15* is focused on encouraging boycotts as it is more impactful (N = 213). We included a third control condition that only had the constituent state that he or she believed the minimum wage should be increased to $15, with a vague reference to a group (N = 223). The control serves as an interesting baseline where there is no coordinated reference to public or private politics (a pure control sans an e-mail would not have worked given our interest in studying explicit legislator reactions). We utilized a constituent e-mail because prior work has shown e-mails can

---

[5] The percentage minority is, of those who responded to the race/ethnicity question, the percentage who cited being at least partially Black, Native American, Hispanic, and/or Asian-American. A total of 2.5% reported being Independents, and .66% reported being from an "other" party. Also, we asked respondents whether they were the legislator him- or herself, or a staff member. We find that 73.29% report being the legislator (also see Flynn 2018). Our main results reported below are similar if we look only at the self-reported legislators—while there are some differences in treatment significance relative to the baseline, the reported differences between the public and private politics conditions hold (and in fact the sponsorship result reported below becomes significant at the .10 level). When looking only at self-reported legislators, our heterogeneous party effects reported below are consistent but not quite as strong as those reported below (seemingly due in part to a loss of statistical power). Finally, as is often the case due to the large size of the state legislature, we have substantial over-representation of representatives from New Hampshire (about 8% of our sample). Our main results hold if we exclude this group.
[6] The Ns for conditions include the aforementioned respondents who dropped out; there is not a significant correlation between condition assignment and drop-out. Also, the conditions are balanced based on measured demographics.

be effectual (e.g., Bergen 2009), and we are directly interested in the impact of messages from constituents. As discussed, we included explicit reference to NGO activities so the message was not seen as the sentiment of just a single constituent (i.e., any treatment effects likely reflect reactions to NGO-led tactics and not the single e-mail).

[Insert Table 1 About Here]

The constituent e-mail message was followed by a series of questions that serve as our main outcome variables. We asked respondents how much credit they believe voters would attribute to the legislature if an increased minimum wage went into effect, on a 4-point scale from no credit to all credit. We similarly asked how much credit would be attributed to interest groups. To test policy effects, we asked legislators how likely they would be to sponsor new legislation in favor of an increase in the state's minimum wage, on a 7-point scale ranging from very unlikely to very likely. We further asked, on the same scale, how likely they would be to move their policy positions in support of the issue. Finally, using analogous 7-point scales, we queried how likely they would be to respond to the e-mail, and if responding, whether the response would include reference to legislation, and interest group activities on the minimum wage issue.[7] All question wordings are in the appendix.

Results

To test our hypotheses, we run a series of ordered probit regressions (using robust standard errors) with experimental treatment dummy variables. We exclude the control baseline so as to assess the impact of the private and public treatments independently. That said, recall our hypotheses involve relative comparisons between the private and public treatments. Thus, we

---

[7] We have item non-response on our outcome measures, leading to lower Ns for some of our models. (We also offered the option of "prefer to not answer" for questions about personal actions. We did this to minimize discomfort; these responses were treated as missing data.)

report whether those two conditions significantly differ from one another in the regressions in the row included below the main coefficients—this constitutes the key test.[8] We also include variables for the legislator's age (measured on a 7-point categorical scale), gender, minority status, and partisanship (whether or not the legislator is a Republican). We suspect that Republicans will be less likely to sponsor legislation and move their issue positions given it is not an issue they typically support.[9] Asterisks indicate statistical significance, using two-tailed tests.[10]

[Insert Table 2 About Here]

The first regression in Table 2 offers strong support for Hypothesis 1: relative to legislators who received the control message, legislators who received the public politics message are much more likely to believe voters will credit the legislature for a policy change. The opposite is true for those receiving the private politics message, meaning that the two treatment conditions significantly differ. The second regression shows that this trend flips when it comes to interest group credit: public (private) politics recipients are much less (more) likely to believe interest groups will receive credit. The substantive impact of the messages on credit attribution is large: an average respondent who receives a public politics message has .89 probability of reporting that the legislature will receive "a fair amount" or "a lot of credit." [11] This contrasts with an average private politics respondent who has a .73 probability. The respective probabilities for interest group attribution are .54 and (again) .73 (i.e., the public politics leads to much less credit being given to interest groups). Clearly, the private politics

---

[8] We use a chi-square test to the hypothesis that the two coefficients do not differ from one another.
[9] We do not include ideology as it highly correlates with party (r = .85).
[10] We use two-tailed tests because our excluded condition is the control and we have less clear predictions about the impact of the conditions relative to the control (i.e., our focus is on comparing the public politics versus private politics treatments—the differences between these two are revealed in the row near the bottom of tables).
[11] We compute probabilities using *Clarify* (King, Tomz, and Wittenberg 2000), holding other variables at their average values.

route vitiates perceptions that legislators will receive credit and increases interest group credit. Also, minority respondents generally believe that the legislature will receive less credit and interest groups will receive more credit, perhaps reflecting lower perceived efficacy among minority legislators (West 2017).

The next two rows of Table 2 show the results for the likelihood of sponsoring legislation that would increase the minimum wage and moving one's position to support an increased wage. We find that both messages increase the likelihood of a legislator sponsoring legislation, with the public politics message doing so a bit more (i.e., the difference between coefficients yields $p = .12$). This is consistent with but not strongly supportive of Hypothesis 2. An average public politics recipient has a .20 probability of being "somewhat likely" or "very likely" to sponsor legislation, while an analogous private politics legislator has a .15 chance of doing the same. The significant positive effect of the private politics message may stem from legislators' pre-empting policy that would otherwise be beyond their control (e.g., Gerber 1996). Specifically, if legislators allowed the minimum wage to be set entirely in the market place, they would have no control over the details (e.g., exact level, implementation). If they anticipate a change, want to claim some credit, and oversee the policy details, they may take action. This then also suggests legislators, on average, do not easily cede full control to the market on this policy.

When it comes to position movement, we find that, despite the relative ease with which legislators can adjust their positions to respond to constituents' demands, going the private politics route eliminates their incentives to do so. This is contrary to Hypothesis 3. Legislators report that they would move their positions in response to the public politics message but not the private politics message. Taken together, then, our policy results are not exactly what we predicted, but they make clear that private politics can lesson legislator responsiveness both in

11

terms of bill sponsorship and position-taking. This likely reflects the lost credit-claiming opportunities in the private politics scenario, a belief that private politics is a suitable alternative and thus no actions is necessary, or some mix of the two.[12] Otherwise, not surprisingly, the results reveal that Republicans are much less likely to sponsor legislation or move their positions.

[Insert Table 3 About Here]

In Table 3, we present the results for e-mail response. The first column shows that both the public politics message and the private politics message fall just short of significantly differing from the control ($p = .116$ and $p = .106$, respectively). More importantly, consistent with Hypothesis 4, the two treatment conditions significantly differ from one another—the average legislator in the public politics case has a .79 probability of being "somewhat likely" or "very likely" to respond to the e-mail, compared to a .66 probability for the private politics case. We also see that women are significantly more likely to respond, and minorities are less likely to do so. Further, Republicans are less likely to respond, although that result falls short of statistical significance—this may reflect legislators' presuming the constituent is likely to be more liberal given the policy stance taken (and thus Republicans feel less need to respond).

The last two columns of Table 3 show that the nature of the message alters the content of the legislators' responses; as Hypothesis 5 suggests, the private politics message increases the likelihood of a reference to possible legislation (perhaps as a reactive measure). Interestingly, it also increases the likelihood of the legislator mentioning interest groups—unfortunately, we have

---

[12] Our data do not allow us to isolate the causal mechanism. However, we find no suggestive evidence for the causal story that private politics affect credit claiming, which in turn influences legislative action. There are near 0 correlations between credit attributions and legislative actions. Thus, it seems that faith in market mechanisms or some other calculus is at work when it comes to policy decisions.

no way to know how interest groups would be invoked (e.g., to counter-argue their effectiveness or praise their actions). Further, Republicans are less likely to mention either approach.

A final question concerns the possibility of heterogeneous treatment effects. While we did not formally hypothesize such effects, it seems sensible that legislators from different parties could react differently to the messages. To explore this possibility, we added interactions between Republican and each treatment message to the regressions. We present the results in the appendix. The most relevant finding concerns sponsorship and position movement. In those cases, Republicans respond in qualitatively distinct ways from Democrats. Specifically, the results presented in Table 2 hold for Republicans insofar as they increase in likelihood of sponsoring legislation and moving their positions in the public politics conditions but not the private politics one.[13] For instance, Republicans who receive the private politics message have virtually no chance (i.e., a .01 probability) of being "somewhat likely" or "very likely" to sponsor legislation. The public message, though, prompts an increase to at least a marginal chance of .05. Perhaps more telling though is that, for Republicans the probability of being "very unlikely" to sponsor a bill for the private politics message is .80, as opposed to .52 for the public politics message. The private politics message then does seem to eliminate Republican legislation efforts, making it very unlikely that they will take action. In contrast, Democrats actually increase in their likelihood of being "somewhat likely" or "very likely" to sponsor a bill in the *private* politics condition, jumping to .52 compared to .44 when receiving a public politics message.[14]

---

[13] In the latter, they even move in a negative direction relative to the control, albeit not to a statistically significant degree.

[14] Their respective chances of being "very unlikely" are .06 in the private case and .08 in the public case. Technically, these probabilities are for non-Republicans since that it is what is included in our appendix regressions, but the results stand if we look only at Democrats (e.g., excluding the few Independents).

Similar probabilities hold for the position movement variable.[15] Put another way, when it comes to legislative activity, private politics vitiates responsiveness relative to public politics *only* for Republicans. Democrats engage in legislation regardless of the private or public politics route while Republicans are pushed only by the public route. These probabilities also accentuate that partisans, regardless of the treatment, differ dramatically on legislating on this issue: Democrats are much more likely to do so.

Our heterogeneous treatment finding is consistent with the idea that Republicans tend to be more free market oriented and thus prefer not to intercede in market processes when they take hold. We posited that as one possible reason why private politics would lessen responsiveness and it is sensible this would mostly apply to Republicans. In contrast, Democrats may view private politics as threat to government action/oversight and react accordingly. Alas, we are not in a position to make a definitive statement about what drives the heterogeneous effects; another possibility is that the finding is issue-specific such that Democrats acted in the private politics condition because the issue aligns more with their interests.[16]

In sum, we find clear evidence that a constituent's reference to NGO private politics tactics alters the nature of legislators' behaviors. Relative to public politics approaches, it leads

---

[15] Specifically, the probability of a Republican being "somewhat" or "very likely" to move his or her position in the public politics case is again .05 compared to .01 in the private politics case. Yet, the chance of him or her being "very unlikely" is .68 in the private politics case versus .44 in the public politics case. Thus, again the private case makes movement much less likely. The Democrats are similar in the public and private case. The chance of a Democrat being "somewhat" or "very likely" to move his or her position in the public politics case is .32 versus .28 in the private politics case. This contrasts a bit with the sponsorship variable since here the private case is less than the public case. The Democrat respective probabilities of being "very unlikely" to move positions are .09 and .11. We also should note another significant interaction: Republicans are less likely to mention legislation in the public politics case, but they still are likely to do so in the private politics case. This may reflect their partisan position in the public politics case of avoiding talking about legislating specifically on a bill they do not wholly endorse given their partisanship (in the private case, perhaps they aim to discuss failed legislation; our survey item does not allow us to isolate the content of what they would have said).

[16] It is an interesting question why there is no evidence of heterogeneous treatment effects for responding to the e-mail. Even Democrats are less likely to respond in the private politics case. This may be because they, like Republicans, view such voters as less engaged and less likely to vote, but they still want to pre-empt the private politics efforts by taking legislative action.

Republican legislators to be less likely to both pursue and support desired policies, and it leads legislators from both parties to be less likely to engage with constituents.

Conclusion

Technology has altered the course of politics, making engagement between elected officials and citizens much more feasible (Neblo et al. n.d.). At the same time, it has facilitated large-scale coordination efforts in the market place where NGOs generate boycotts and buycotts. What drives these private politics efforts, whether they fail or succeed, and how individual citizens decide to follow are all questions that have received recent attention. What has not been considered, however, is how private politics affects legislative responsiveness. In some sense, Diermeier (2007: 7) raised this question over a decade ago: "Private politics focuses on influencing markets and the practices of market participants. This influence moves markets and their participants in particular directions, but what are the consequences of these interactions? In other words, are private politics "good" or "bad" for society?

Answers to these questions are not straightforward, and our study accentuates the interconnectedness between what happens in the private and public domains. Private politics often proceed due to the many built-in barriers to the democratic process: its inefficiency. These activities, though, then dis-incentivize legislators, particularly Republicans, further and could consequently make it even less likely that NGOs and citizens will want to work through the legislative process. In this case, policy is being made based on the resources and skills of those who run private politics campaigns, rather than by legislators who answer to the larger citizenry. On the flip side, though, legislative responsiveness is far from perfect and brings with it distinct types of inequalities; thus, it is possible that extra-systemic efforts are sometimes needed to induce changes that are more in line with citizens' preferences. The larger point is that scholars

15

of democratic representation need to carefully consider the place of private politics and how to appropriately gauge when and whether citizens' preferences are being exercised in policy.

We of course recognize the limitations of our study—it focuses on a single issue and a hypothetical situation. The results also do not provide clear mediational processes for why legislators act as they suggest they will. Much remains to be done, and our hope is that ours is the first of many works that explicitly explore how private politics affects representation. It may very well be that private politics outcomes are more optimal than public politics outcomes, relative to some normative baseline. What that baseline should be is one of the many questions that remains to be addressed.

References

Anderson, Sarah E., Daniel M. Butler, and Laurel Harbridge. 2016. "Legislative Institutions as a Source of Party Leaders' Influence." *Legislative Studies Quarterly* 41: 605-631.

Anderson, Sarah E., Daniel M. Butler, and Laurel Harbridge-Yong. 2018. *Rejecting Compromise: Legislators' Fear of Primary Voters*. Book Manuscript.

Baron, David P. 2003. "Private Politics." *Journal of Economics and Market Strategy* 12: 31-66.

Baron, David P., and Daniel Diermeier. 2007. "Strategic Activism and Nonmarket Strategy." *Journal of Economics and Management Strategy* 6: 599-634.

Bartels, Larry M. 2008. *Unequal Democracy: The Political Economy of the New Gilded Age*. New York: Russell Sage Foundation and Princeton University Press.

Benstead, Lindsay J., and Megan Reif. 2017. "Coke, Pepsi or Mecca Cola?: Why product Characteristics Affect the Likelihood of Collective Action Problems and Boycott Success." *Politics, Groups, and Identities* 5: 220-241.

Bergan, Daniel E. 2009. "Does Grassroots Lobbying Work?: A Field Experiment Measuring the Effects of an e-Mail Lobbying Campaign on Legislative Behavior." *American Politics Research* 37: 327-352.

Butler, Daniel M. 2014. *Representing the Advantaged: How Politicians Reinforce Inequality*. New York: Cambridge University Press.

Butler, Daniel M., and David Broockman. 2011. "Do Politicians Racially Discriminate Against Constituents?: A Field Experiment on State Legislators." *American Journal of Political Science* 55: 463-477.

Butler, Daniel M., and Adam M. Dynes. 2016. "How Politicians Discount the Opinions of Constituents with Whom They Disagree." *American Journal of Political Science* 60: 975-989.

Copeland, Lauren. 2014. "Value Change and Political Action: Postmaterialism, Political Consumerism, and Political Participation." *American Politics Research* 42: 257-282.

Diermeier, Daniel. 2007. "Private Politics - A Research Agenda." *The Political Economist.* 14 (Summer): 1-10.

Druckman, James N. and Lawrence R. Jacobs. 2015. *Who Governs? Presidents, Public Opinion, and Manipulation*. Chicago, IL: University of Chicago Press.

Druckman, James N., Martin J. Kifer, and Michael Parkin. 2009. "Campaign Communications in U.S. Congressional Elections." *American Political Science Review* 103: 343-366.

Endres, Kyle, and Costas Panagopoulos. 2017. "Boycotts, Buycotts, and Political Consumerism in America." *Research and Politics.* 4: 1-9.

Fenno, Richard F., Jr. 1978. *Home Style: House Members in Their Districts.* Boston: Little, Brown.

Flynn, D.J. 2018. "How Do Politicians Respond to Misinformed Constituents?: Results from an Elite Survey Experiment." Working paper, Dartmouth University.

Gerber, Elisabeth. 1996. "Legislative Response to the Threat of Popular Initiatives." *American Journal of Political Science* 40: 99-128.

Gilens, Martin. 2014. *Affluence and Influence: Economic Inequality and Political Power in America*. New York: Russell Sage and Princeton University Press.

Harden, Jeffrey J. 2013. "Multidimensional Responsiveness: The Determinants of Legislators' Representational Priorities." *Legislative Studies Quarterly* 38: 155-184.

Kam, Cindy D., and Maggie A. Deichert. 2017. "Boycotting, Buycotting, and the Psychology of Political Consumerism." Working Paper, Vanderbilt University.

King, Gary, Michael Tomz, and Jason Wittenberg. 2000. "Making the Most of Statistical Analyses: Improving Interpretation and Presentation." *American Journal of Political Science* 44 (2):347–61.

Kurtz, Karl. 2015. "Who We Elect: The Demographics of State Legislators." *State Legislators Magazine* (December). [online]. Available at: http://www.ncsl.org/research/about-state-legislatures/who-we-elect.aspx [Accessed 19 Oct. 2016].

Mayhew, David R. 1974. *Congress: The Electoral Connection.* New Haven, CT: Yale University Press.

Neblo, Michael A., Kevin M. Esterling, Ryan P. Kennedy, David M.J. Lazer, and Anand E. Sokhey. 2010. "Who Wants to Deliberate - And Why?" *American Political Science Review* 104: 566-583.

Neblo, Michael A., Kevin M. Esterling, and David M.J. Lazer. N.d. *Directly Representative Democracy: Reconnecting People and Government for a 21st Century Republic*. New York: Cambridge University Press.

Reid, Erin M., and Michael W. Toffel. 2009. "Responding To Public and Private Politics: Corporate Disclosure of Climate Change Strategies." *Strategic Management Journal* 30: 1157-1178.

Soroka, Stuart N., and Christopher Wlezien. 2010. *Degrees of Democracy: Politics, Public Opinion, and Policy*. Cambridge: Cambridge University Press.

Stolle, Dietlind, Mark Hooghe, and Michele Micheletti. 2005. "Politics in the Supermarket: Political Consumerism as a Form of Political Participation." *International Political Science Review* 26: 245-269.

West, Emily A. 2016. "Descriptive Representation and Political Efficacy: Evidence from Obama and Clinton." *The Journal of Politics*. 79: 351-355.

Williams, R. Lucas. 2015. *Three Essays on Legislative Behavior in American Legislatures*. Doctoral Dissertation, University of Houston.

Zukin, Cliff, Scott Ketter, Molly Andolina, Krista Jenkins, and Michael X. Delli Carpini. 2006. *A New Engagement?*: *Political Participation, Civic Life, and the Changing American Citizen.* New York: Oxford University Press.

Table 1: Experimental Treatments

"We are interested in how your office manages and responds to constituent emails. Please read this sample (hypothetical) email from a constituent and answer the questions that follow."

"Dear [Representative/Senator],

"As your constituent, I'm writing about our state's minimum wage with the hope of getting it increased to $15. [TREATMENT]. We need a living minimum wage."

[Randomly assigned to one of the following conditions.]

[Control]

"… I am doing so because I have been contacted by a group that is attempting to raise the minimum wage across the United States."

[Public Politics]

"…I am doing so because I have been contacted by a group called *Fight for $15* that is attempting to raise the minimum wage across the United States. The group is encouraging people to contact their state representatives. The group argues that taking actions by contacting representatives is the most effective way to communicate my opinion, generate a change in the minimum wage, and engage in politics. I believe in their mission, and have decided to participate in their campaign by contacting my state legislator."

[Private Politics]

"…I am doing so because I have been contacted by a group called *Fight for $15* that is attempting to raise the minimum wage across the United States. The group is encouraging people to boycott local businesses that have not raised their minimum wage to at least $15. This group argues that taking actions against such businesses is the most effective way to communicate my opinion, generate a change in the minimum wage, and engage in politics. I believe in their mission, and plan to participate in their boycott. However, I also am asking for help from my state legislator."

Table 2: Legislators' Credit Attribution and Policy Responses

	(1) Legislature Credit	(2) Interest Group Credit	(3) Sponsor	(4) Move Position
Public Politics	0.250**	-0.266**	0.406***	0.391***
E-mail	(0.123)	(0.115)	(0.119)	(0.113)
Private Politics	-0.355***	0.268**	0.216*	0.071
E-mail	(0.122)	(0.123)	(0.122)	(0.124)
Age	0.001	0.069**	0.072**	0.059*
	(0.031)	(0.032)	(0.031)	(0.031)
Female	-0.023	-0.149	-0.005	0.178*
	(0.104)	(0.103)	(0.106)	(0.101)
Minority	-0.280*	0.487***	0.201	0.157
	(0.147)	(0.135)	(0.135)	(0.140)
Republican	-0.072	0.118	-1.799***	-1.354***
	(0.105)	(0.102)	(0.112)	(0.104)
Significant Difference	YES	YES	NO	YES
between Public/Private	$(p \leq .01)$	$(p \leq .01)$	$(p \leq .12)$	$(p \leq .01)$
Conditions?				
Log-Likelihood	-538.41	-566.13	-769.95	-802.57
N	499	496	527	519

Note: Entries are ordered probit coefficients with robust standard errors in parentheses. ***$p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$ for two-tailed test. A full table with constant cut-points is available in the appendix.

Table 3: Legislators' E-mail Responses

	(1) Respond	(2) Mention Legislation	(3) Mention Interest Groups
Public Politics	0.197	0.211*	-0.201*
E-mail	(0.126)	(0.118)	(0.108)
Private Politics	-0.194	0.561***	0.254**
E-mail	(0.120)	(0.136)	(0.123)
Age	-0.006	0.001	0.082***
	(0.032)	(0.032)	(0.030)
Female	0.234**	0.217*	0.115
	(0.112)	(0.112)	(0.101)
Minority	-0.362***	-0.120	0.188
	(0.135)	(0.166)	(0.134)
Republican	-0.139	-0.599***	-0.367***
	(0.104)	(0.107)	(0.101)
Significant Difference	YES	YES	YES
between Public/Private	($p \leq .01$)	($p \leq .01$)	($p \leq .01$)
Conditions?			
Log-Likelihood	-701.51	-662.67	-884.32
N	543	525	520

Note: Entries are ordered probit coefficients with robust standard errors in parentheses. ***$p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$ for two-tailed test. A full table with constant cut-points is available in the appendix.

Appendix for "How Private Politics Alters Legislative Responsiveness"

Survey Question Wording

Are you a staff member or legislator?

Staff member   *Legislator*
0   1

What state do you represent?_____

What is your age?

18-24   *25-34*   *35-44*   *45-54*   *55-64*   *65-74*   *75 years or older*

What is your gender?

Male   *Female*   *Other*

Which racial or ethnic group best describes you? You can check more than one.

White   *Black*   *Native American*   *Hispanic*   *Asian American*   *Other*

What is your party affiliation?

Democrat   *Republican*   *Independent*   *Other*

Which point on this scale best describes your political views?

Very liberal	*Mostly liberal*	*Somewhat liberal*	*Moderate*	*Somewhat conservative*	*Mostly conservative*	*Very conservative*

[TREATMENT]

How unlikely or likely would you be to respond to this e-mail?

Very unlikely	*Somewhat unlikely*	*Slightly unlikely*	*Neither unlikely nor likely*	*Slightly likely*	*Somewhat likely*	*Very likely*

If you responded to this e-mail, how unlikely or likely would you be to mention legislation that has already been passed, is pending, or is possible on the minimum wage issue?

Very unlikely	*Somewhat unlikely*	*Slightly unlikely*	*Neither unlikely nor likely*	*Slightly likely*	*Somewhat likely*	*Very likely*

If you responded to this e-mail, how unlikely or likely would you be to mention interest group activities?

Very unlikely	*Somewhat unlikely*	*Slightly unlikely*	*Neither unlikely nor likely*	*Slightly likely*	*Somewhat likely*	*Very likely*

How unlikely or likely would you be to sponsor new legislation in favor of an increase in your state's minimum wage?

Very unlikely	Somewhat unlikely	Slightly unlikely	Neither unlikely nor likely	Slightly likely	Somewhat likely	Very likely

How unlikely or likely would you be to move your policy position in support of increasing your state's minimum wage?

Very unlikely	Somewhat unlikely	Slightly unlikely	Neither unlikely nor likely	Slightly likely	Somewhat likely	Very likely

If there was a change in the minimum wage due to the group mentioned in the letter, how much credit do you think voters would attribute to the legislature?

No credit	Some credit	A good deal of credit	All credit

If there was a change in the minimum wage due to the group mentioned in the letter, how much credit do you think voters would attribute to interest groups?

No credit	Some credit	A good deal of credit	All credit

Table A-1: Legislators' Credit Attribution and Policy Responses with Cutpoints

	(1) Legislature Credit	(2) Interest Group Credit	(3) Sponsor	(4) Move Position
Public Politics	0.250**	-0.266**	0.406***	0.391***
E-mail	(0.123)	(0.115)	(0.119)	(0.113)
Private Politics	-0.355***	0.268**	0.216*	0.071
E-mail	(0.122)	(0.123)	(0.122)	(0.124)
Age	0.001	0.069**	0.072**	0.059*
	(0.031)	(0.032)	(0.031)	(0.031)
Female	-0.023	-0.149	-0.005	0.178*
	(0.104)	(0.103)	(0.106)	(0.101)
Minority	-0.280*	0.487***	0.201	0.157
	(0.147)	(0.135)	(0.135)	(0.140)
Republican	-0.072	0.118	-1.799***	-1.354***
	(0.105)	(0.102)	(0.112)	(0.104)
Significant Difference	YES	YES	NO	YES
between Public/Private	($p \leq .01$)	($p \leq .01$)	($p \leq .12$)	($p \leq .01$)
Conditions?				
Constant cut1	-2.218***	-1.524***	-0.783***	-0.689***
	(0.206)	(0.192)	(0.170)	(0.172)
Constant cut2	-1.037***	-0.000	-0.437**	-0.201
	(0.168)	(0.177)	(0.172)	(0.172)
Constant cut3	0.131	1.185***	-0.265	0.025
	(0.169)	(0.183)	(0.173)	(0.172)
Constant cut4			0.358**	0.821***
			(0.174)	(0.180)
Constant cut5			0.743***	1.129***
			(0.174)	(0.181)
Constant cut6			1.368***	1.555***
			(0.182)	(0.192)
Log-Likelihood	-538.41	-566.13	-769.95	-802.57
N	499	496	527	519

Note: Entries are ordered probit coefficients with robust standard errors in parentheses. ***p≤0.01, ** p≤0.05, * p≤0.1 for two-tailed test.

Table A-2: Legislators' E-mail Responses with Cutpoints

	(1) Respond	(2) Mention Legislation	(3) Mention Interest Groups
Public Politics	0.197	0.211*	-0.201*
E-mail	(0.126)	(0.118)	(0.108)
Private Politics	-0.194	0.561***	0.254**
E-mail	(0.120)	(0.136)	(0.123)
Age	-0.006	0.001	0.082***
	(0.032)	(0.032)	(0.030)
Female	0.234**	0.217*	0.115
	(0.112)	(0.112)	(0.101)
Minority	-0.362***	-0.120	0.188
	(0.135)	(0.166)	(0.134)
Republican	-0.139	-0.599***	-0.367***
	(0.104)	(0.107)	(0.101)
Significant Difference between Public/Private Conditions?	YES $(p \leq .01)$	YES $(p \leq .01)$	YES $(p \leq .01)$
Constant cut1	-1.752***	-1.602***	-0.126
	(0.191)	(0.184)	(0.168)
Constant cut2	-1.560***	-1.486***	0.258
	(0.184)	(0.182)	(0.167)
Constant cut3	-1.400***	-1.329***	0.389**
	(0.176)	(0.185)	(0.167)
Constant cut4	-0.916***	-1.010***	0.950***
	(0.174)	(0.183)	(0.171)
Constant cut5	-0.657***	-0.771***	1.232***
	(0.172)	(0.179)	(0.174)
Constant cut6	-0.320*	-0.187	1.788***
	(0.171)	(0.174)	(0.185)
	-1.752***	-1.602***	-0.126
Log-Likelihood	-701.51	-662.67	-884.32
N	543	525	520

Note: Entries are ordered probit coefficients with robust standard errors in parentheses. ***p≤0.01, ** p≤0.05, * p≤0.1 for two-tailed test.

Table A-3: Results with Republican X Treatment Interactions

	(1) Legislature Credit	(2) Interest Group Credit	(3) Sponsor	(4) Move Position	(5) Respond	(6) Mention Legislation	(7) Mention Interest Groups
Public	0.223	-0.160	0.284*	0.388**	0.188	0.448**	-0.163
Politics	(0.154)	(0.143)	(0.149)	(0.151)	(0.174)	(0.179)	(0.141)
Private	-0.262*	0.438***	0.480***	0.284*	-0.149	0.760***	0.187
Politics	(0.157)	(0.160)	(0.165)	(0.169)	(0.168)	(0.214)	(0.160)
Age	0.000	0.067**	0.075**	0.058*	-0.006	-0.003	0.082***
	(0.031)	(0.032)	(0.031)	(0.031)	(0.032)	(0.032)	(0.030)
Female	-0.028	-0.154	-0.021	0.165	0.230**	0.212*	0.122
	(0.104)	(0.104)	(0.107)	(0.102)	(0.113)	(0.112)	(0.101)
Minority	-0.269*	0.485***	0.242*	0.179	-0.356***	-0.137	0.177
	(0.147)	(0.137)	(0.135)	(0.141)	(0.135)	(0.167)	(0.135)
Republican	-0.024	0.306*	-1.720***	-1.209***	-0.114	-0.343**	-0.383**
	(0.169)	(0.158)	(0.165)	(0.162)	(0.167)	(0.164)	(0.163)
Rep.*Public	0.062	-0.228	0.282	0.003	0.022	-0.451*	-0.087
	(0.250)	(0.229)	(0.231)	(0.225)	(0.250)	(0.236)	(0.218)
Rep.*Private	-0.205	-0.369	-0.706***	-0.522**	-0.094	-0.377	0.149
	(0.245)	(0.249)	(0.269)	(0.258)	(0.243)	(0.273)	(0.249)
Constant cut1	-2.201***	-1.451***	-0.757***	-0.632***	-1.742***	-1.500***	-0.133
	(0.209)	(0.195)	(0.171)	(0.176)	(0.198)	(0.191)	(0.171)
Constant cut2	-1.018***	0.075	-0.403**	-0.141	-1.550***	-1.384***	0.251
	(0.175)	(0.180)	(0.174)	(0.177)	(0.190)	(0.188)	(0.170)
Constant cut3	0.151	1.265***	-0.226	0.086	-1.390***	-1.227***	0.382**
	(0.176)	(0.186)	(0.177)	(0.178)	(0.183)	(0.191)	(0.170)
Constant cut4			0.410**	0.886***	-0.905***	-0.905***	0.943***
			(0.179)	(0.187)	(0.182)	(0.191)	(0.173)
Constant cut5			0.799***	1.195***	-0.647***	-0.665***	1.226***
			(0.179)	(0.187)	(0.178)	(0.187)	(0.177)
Constant cut6			1.428***	1.621***	-0.310*	-0.078	1.782***
			(0.190)	(0.200)	(0.178)	(0.184)	(0.187)
Log-Likelihood	-537.78	-564.94	-762.07	-799.64	-701.39	-660.68	-883.81
Observations	499	496	527	519	543	525	520

Note: Entries are ordered probit coefficients with robust standard errors in parentheses. ***p≤0.01, ** p≤0.05, * p≤0.1 for two-tailed test.