# The Costs and Benefits of Court Curbing: Experimental Evidence from the United States

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Cannonical models of interbranch relations suggest that politicians attack courts at their own peril. When courts enjoy a store of diffuse support—so the logic goes—the public should punish incumbents who curb the judiciary. We call this widespread assumption into question. Drawing upon an experiment embedded in a survey of 2,500 Americans, we demonstrate that the public does punish, *but also rewards* politicians who attack the judiciary. Moreover, we demonstrate that institutional legitimacy does not have the shielding effect for courts so often assumed. The results have broad implications for our understanding of public support of democratic institutions, institutional legitimacy and interbranch relations.

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One only need open the newspaper to see that our understanding of the conditions under which mass publics are willing to support liberal democratic institutions have been openly challenged by recent events worldwide. From Donald Trump in the United States, to Evo Morales in Bolivia, to Rodrigo Duterte in the Philippines and Marine Le Pen in France, leaders have ascended to power who readily eschew norms that have long buttressed the democratic architecture of majority rule. These individuals have reached power with the support of vociferous followers who do not seem to value democratic institutions in the ways scholars have long suggested. The real world is challenging scholars' long-held assumptions, thereby opening the door for renewed examination of the public's support for political institutions in liberal democracy.

This especially true with regard to interbranch relations. With regard to judicial politics, traditional models of interbranch relations suggest that these populist leaders attack the courts at their own peril. Indeed, that the public is willing to punish incumbents electorally for attacks against high courts is an identifying assumption in many prominent theoretical models of comparative judicial independence and power (e.g., Staton 2006, 2010; Vanberg 2000, 2001; Helmke 2010*a*; Krehbiel 2016; Stephenson 2004; Rogers 2001). Faced with the threat of public backlash for non-compliance or inter-branch assaults, conventional logic suggests, incumbents should have no recourse but to respect the court as an institution and to comply with its decisions. This existing body of knowledge, therefore, would predict swift electoral punishment when leaders attack high courts.

We are not so sure. Theoretically, it is far from obvious why the public would come to the rescue of an unelected court, punishing an incumbent for transgressing a court's institutional integrity. After all, the public's interests may well be more aligned with the incumbent than the court by virtue of the electoral connection, and voters have been shown to adopt the policy positions of their elected officials, not vice-versa (Gabel and Scheve 2007; Lenz 2012). We therefore argue that incumbents might *benefit* from attacking the judiciary.

This theoretical lacuna is compounded by an empirical reality. We have scant evidence as to how (or if) citizens will punish elected leaders for attacks on courts. Moreover, nearly all research on public support for judicial institutions examines the United States Supreme Court (c.f. Walker 2016; Gibson, Caldeira and Baird 1998*b*; Driscoll and Nelson 2018*a*,*b*), an institution that is anomalous for the remarkably high level of public institutional commitment (Gibson 2007). Fundamentally, we do not know under what conditions citizens will stand up to an incumbent to protect a high court. Given the prevalence of these attacks worldwide, this is a question of both academic and popular interest.

We employ an experimental approach, randomly exposing respondents to the threat of court curbing. Relying on experiment embedded in a survey of 2,500 Americans, our data provide on of the most comprehensive the first experimental tests of the effects of court curbing.[1] We find that, in many cases, citizens approve of court curbing and reward legislators who propose court packing attempts. This is especially true when the proposers are copartisans, and when they frame their court packing attempts in bureaucratic language. What is more, we find no evidence that institutional legitimacy—respondents' unwillingness to support proposals to institutionally alter the courts—has the effect of motivating respondents to punish incumbents for putative attacks on courts. Instead, we find that those who hold the federal judiciary in the *highest* regards are actually more likely to *reward* copartisans for attempts at drastically changing the composition of the federal courts.

The results both affirm and challenge conventional wisdom, emphasizing that not all

[1]Experimental studies of public support for high courts are now commonplace (Bartels and Johnston 2013; Clark and Kastellec 2015; Christenson and Glick 2015; Armaly 2017; Gibson and Nelson 2017), with scholars typically querying respondents reactions to randomized features of judicial decision-making. Our approach takes a different tack, exposing respondents to incumbent attacks on courts, and randomizing the proposal content and identity of the putative court-curber. Further, our research is the first to examine the costs and benefits to incumbents, as opposed to the costs to the courts.

court curbing attempts are popularly controversial. Indeed, in many instances, the public may *reward* incumbents for their attempts to structurally alter the most fragile branch of government, especially if the proposal is advanced by a copartisan, or framed in politically neutral terms. The results also underscore the importance of further, and ideally cross-national testing. We therefore conclude this paper outlining our plan to expand this research agenda outside of the United States, examining how variation in democratic values, levels of democratic consolidation, and other various country-level characteristics affect the costs and benefits of attacking the judiciary.

## I The Costs (and Benefits?) of Attacking Courts

That the public is willing to punish incumbents electorally for attacks against high courts is a primary theoretical mechanism in many prominent theoretical models of judicial independence and power: because of the threat of public backlash for non-compliance or inter-branch assaults, implies incumbents should have no recourse but to respect the court as an institution and to comply with its decisions (Clark 2009, 2010; Staton 2006). A recent review article, Vanberg (2015) lays out the claim succinctly:

> Policy makers respect judicial authority not because doing so provides a positive benefit but because attacking the court or ignoring its decisions is too costly (e.g., Epstein et al. 2001; Vanberg 2001, 2005). *The most common explanation* of this type stresses public support for independent courts as the critical factor (Vanberg 2001, 2005; Staton 2006, 2010). The intuition behind this explanation is simple. Considerable empirical evidence suggests that citizens in democratic polities hold courts in high regard, often in higher regard than policy makers in other branches (e.g., see Gibson et al. 1998). If the integrity of the judiciary and respect for its decisions are values that a sufficient number of citizens are willing to defend by withdrawing support from policy makers who attack judicial independence, policy makers are likely to conclude that disciplining the court or resisting unwelcome decisions is not worth the potential costs of a public backlash. Public support provides a shield for judicial independence (176-7,

emphasis added).[2]

Thus, the electoral connection between citizens and incumbents, coupled with public intolerance for incumbents transgressions vis-a-vis the courts, ensures compliance and protects the judiciary from incumbent attacks on its structure and function.

Despite the centrality of this mechanism for many theoretical models of judicial behavior (Stephenson 2004; Rogers 2001; Staton 2006, 2010; Clark 2009, 2010; Vanberg 2000, 2001; Helmke 2010*a*; Krehbiel 2016), far less is known empirically about the conditions under which citizens punish incumbents for attacking courts.[3] Empirically speaking, Helmke's (Helmke 2010a, 2010b) work on inter-branch crises throughout the Americas shows that low public confidence in the judiciary is the strongest predictor of inter-branch conflict relative to other institutional factors, suggesting that public dissatisfaction might fuel incumbents willingness to target the judiciary (although she also acknowledges the converse causal claim may be true) (c.f. Driscoll & Nelson 2018a, 2018b). Beyond this cross-sectional effort to understand incumbents' calculus and the public reaction to court curbing, most of the systematic evaluations into the public backlash to court curbing outside of the United

[2]Elsewhere Vanberg discusses that the public's support for the judiciary may be a sufficient—but is not necessary—condition for staving off inter branch conflict (2001). This is just one among many explanations of the maintenance of an independent judiciary (Vanberg 2008).

[3]The formal theoretical literature is instructive on this point, identifying the structural conditions that would facilitate public backlash against incumbents for their anti-court aggressions. Vanberg's (2000, 2001) work stresses the importance of a transparent political environment in which incumbents might be monitored, while Weingast's model requires citizens' "consensus" regarding the appropriate bounds of constitutional rule. Others, such as Carrubba (2009) & Stephenson (2004) emphasize the role of elite opinions and actions in structuring the public's evaluation and possible backlash (c.f. Driscoll & Schorpp 2017).

States rests on qualitative accounts of single cases.[4] And even while Clark's canonical model of court curbing (2009, 2010) explicitly casts attacks on courts as incumbents' attempts to rally a base of electoral support, his empirical analysis centers entirely on judicial reactions to court curbing proposals, and does not speak to whether these proposals produce their intended electoral effects. Accordingly, we have much more to empirically understand.

Indeed, it is unclear why the public would choose to punish an elected incumbent to defend the institutional integrity of an unelected court. At root is an issue of electoral accountability: traditional theories suggest that attacks on courts inspire issue voting among the public, with the issue being the a matter of institutional commitment, rather than some policy issue like taxes, health care, or abortion. How said institutional commitments manifest in voting behavior remains an under-appreciated empirical puzzle, but one of great practical and normative import. Further complicating the broader theoretical logic, contemporary accounts of electoral accountability suggest that elections may not inspire the sort of policy-based voting that these traditional accounts require.

Traditional views of voter behavior has long assumed politicians are constrained by public opinion, and voters make decisions on the basis of politicians' policy platforms (Downs 1957; Mayhew 1974). Politicians therefore were constrained by public opinion, required either to align their positions with those of voters (Brody and Page 1972), or to provide persuasive justifications aimed at swaying voters' positions (Grose, Malhotra and van Houweling 2015). Not only is this a demanding theoretical view of democracy,

[4] Vanberg's (2001) account of inter-branch hostilities in Germany describes a case in which the potential for electoral backlash caused the German Prime Minster to back off his government's attempt to undermine the German Constitutional Court, but also underscores the role of elite opinions in preventing overt inter-branch conflict in other instances. Kapiszewski (2012) includes the public backlash as one possible factor that influenced incumbents' attacks on courts in Argentina and Brazil. Helmke singles out the constitutional crisis of Ecuador (2008) as an interesting case, wherein the president's aggressions against the court were met with widespread public protests, in spite of the fact that a very low proportion of Ecuadorians at the time reported confidence in their court (Helmke 2010*a*).

a prominent line of new empirical research on accountability suggests that issue voting is relatively scarce in practice. Instead, voters appear to make decisions on the basis of heuristics like partisanship, or when lacking the partisan heuristic base their decisions on candidates' ascriptive characteristics thought to proxy for the partisan cue (Driscoll and Nelson 2014). Moreover, as voters learn about their favored candidates' platforms, those issue positions tend to 'rub off' on them, as opposed to candidates adopting the stances to best appeal to constituents. In an early study, Abramowitz (1978) found that voters who watched the 1976 presidential debates adopted the positions taken by their preferred candidate rather than changing their candidate preference based on the extent to which that candidate's positions aligned with their own. Newer evidence from the U.S. and abroad, demonstrates that that voters often adopt the policy positions of their elected officials (Ladd & Lenz 2012). Broockman and Butler (2017), present field experimental evidence that in the case of state legislators: voters often adopted a state legislator's issue position after learning of it, even when the position was accompanied with little justification. This turns traditional notions of candidate position-taking—and the voter's reaction to it—completely on its head (Downs 1957; Mayhew 1974).

These findings have important ramifications for the theoretical account of electoral accountability as it relates to courts. If voters often adopt the views of politicians—sometimes even in the absence of persuasive justifications—politicians can make decisions relatively unencumbered by public opinion (Broockman and Butler 2017). What is more, if elites are the opinion *leaders* in this equation, then their proposals for high court reform may have the effect of actually *shaping* public opinion and support vis-á-vis the courts in politically relevant ways. For our purposes, this burgeoning evidence stands in stark contrast to the issue-voting-based-on-legitimacy perspective long held by scholars, and demands that it be both revisited and empirically tested. While we are not the first to suggest it,[5] these are provocative theoretical conjectures that motivate our current and future research.

Finally, the electoral connection provides another reason to expect the public to value their elected officials' opinions at the expense of an independent judiciary. Voting is an expressive act that binds voter and politician; with the exception of Bolivia and the American states, judges are not directly elected by the public (Driscoll and Nelson 2012, 2013, 2019). Indeed, regardless of whether one believes that politicians adjust their stances to appeal to the public—the logic of issue voting—or the more recent arguments about the public's propensity to adopt elites' issue stances, the electoral connection and process of representation may well result in a public that is more well-aligned with their elected officials than their unelected judiciary.

Thus, we expect that not all court curbing attempts will provide the sort of electoral backlash widely expected by traditional models of comparative judicial politics. There is ample observational evidence that the public supports judicial institutions in democracies the world over (Gibson, Caldeira & Baird 1998; Driscoll & Nelson 2018a, 2018b). At the same time, innumerable incumbents attempt to strip high courts of jurisdiction, to pack the courts with political lackeys, and to fundamentally undermine the separation of powers,

[5] Recent research by Clark and Kastellec (2015), suggests that the public is willing to accept some attacks on courts when they approve of the attacker. Armaly (2017), for example, shows that Americans react more favorably to attacks on judicial independence when they come from a presidential candidate the voter feels warmly about. Somewhat similarly, Nelson and Gibson (2018) demonstrate that President Trump's attacks on the judiciary are only threatening to the U.S. Supreme Court's legitimacy among the minority of the public who trust Trump; For the plurality of Americans that hold Trump in low regard, his attacks actually backfire and *increase* the Court's support. Perhaps more to the point, Nelson and Gibson experimentally manipulate agreement with criticisms of the Court, finding that—holding the content of the criticism constant—voters adopt or reject out of hand criticisms simply based on the identity of the speaker. This new evidence suggests that attitudes toward the judiciary have properties in line with the new evidence on electoral accountability: voters take their cue from elites about the appropriate bounds of constitutional order, and the role of course in modern constitutional systems.

ostensibly with the support of some part of the public. Perhaps the public is more likely to reward or punish an incumbent for court curbing attacks based on the shared partisanship of the proposer, or perhaps their reaction is rooted in the justifications would-be reformers give. These facts suggest we have much more to understand.

## II Of Proposals and Proposers

The forgoing discussion suggests two dimensions along which the effects of court curbing might vary: voter's attitudes toward the content of the proposal and their opinion of the proposer. Numerous recent studies have suggested that the public does not like attempts to politicize the judiciary (Johnston and Bartels 2010; Bartels and Johnston 2012; Gibson and Caldeira 2009; Hitt and Searles 2018). Conversely, more technocratic information about the judiciary—even if it relates to judges' ideology—does the Court much less harm (Gibson and Nelson 2017; Gibson and Caldeira 2011). Indeed, an array of evidence suggests that Americans dislike politicized processes and prefer more routinized, bureaucratic ones (Hibbing and Theiss-Morse 1995, 2001; Christenson and Glick 2015). Thus, we expect that *proposals that purport to be bureaucratic in nature will be evaluated positively, those those that aim to politicize the judiciary will purport to be evaluated negatively.*

Extant research also suggests that individuals dislike individuals who overtly politicize processes. Therefore, we expect the effects of the rationale for the proposal to also attach to the proposer. Thus, *proposers that purport to be seeking court reforms that are bureaucratic in nature will be evaluated positively, those those that aim to politicize the judiciary will purport to be evaluated negatively.*

At the same time, we expect that the identity of the proposer will also influence the public's response to threats made against courts. This is because (a) voters are particularly likely to support elites from their own party (e.g. Campbell et al. 1960) and (b) voters tend to adopt the positions taken by legislators they support (e.g. Lenz 2012). Therefore, we expect that *proposals made by copartisans should be evaluated more favorably.*

But, the effects of copartisanship should go beyond attitudes toward the proposal and also infect respondents' judgments of the proposer. Because Americans disproportionately favor copartisans over outpartisans, we expect them to reward copartisans and punish outpartisans who attack courts. Thus, we also expect that *respondents will judge a copartisan who introduces a court curbing bill more favorably than an outpartisan who does the same.*

## III The Conditioning Effects of Legitimacy

All institutions need public support in order to fulfill their roles in a democratic political system; without public support, institutions are unable to enforce their decisions, rendering them impotent. In his pioneering work on public support for the institutions of democracy, Easton (1965) differentiated short-term satisfaction with institutional decisions (specific support) from institutional legitimacy, otherwise known as diffuse support. To Easton (1965), diffuse support constitutes "a reservoir of favorable attitudes or good will that helps members to accept or tolerate outputs to which they are opposed or the effect of which they see as damaging to their wants" (273). This sort of institutional commitment is manifest in a fundamental unwillingness to tolerate fundamental changes to institutions (Caldeira and Gibson 1992). Where institutions are legitimate—enjoying a base of diffuse support from a broad cross-section of the public—attempts to undermine the institution's independent authority or to fundamentally change their structure should be met with widespread public resistance.

In this way, we expect legitimacy to act as both a shield and a sword.[6] First, legitimacy

[6]Notably, the institutional legitimacy of both the target institution and the aggressor are key parameters in Helmke's (2010, 2017) theoretical model of inter branch crises. Consistent with the "shield" analogy herein described, she envisions the public support as inflicting a "legitimacy cost" on the attacking institution. Empirically, she finds that higher public confidence in the target institution correlates with less frequent attacks from other branches of government."

can shield institutions from being attacked, acting as a deterrent to would-be inter branch conflict. An institution's preexisting level of support should enable it to weather attacks. When an institution is particularly legitimate, legitimacy might even inspire a backlash, harming those individuals who wish to harm the legitimate institution (Nelson and Gibson 2018). We therefore expect that *there is an inverse relationship between an an individuals' view of federal court legitimacy and their willingness to support a court curbing proposal.*

At the same time, legitimacy can also act as a sword, supercharging threats to other democratic institutions. When a proposer's institution is imbued with institutional legitimacy, it might be particularly effective; institutional legitimacy can therefore make threats that would otherwise be harmless quite potent. Thus, we expect *there is a positive relationship between an individuals' view of congressional legitimacy and their willingness to support a court curbing threat.*

## IV Research Design

We assess the extent to which the partisanship of the proposer and the rationale for the proposal affect respondents' evaluations of the proposer and the proposal using a survey experiment of about 2,500 Americans conducted on Amazon's Mechanical Turk platform in July 2018.[7]

### A Experimental Design

| Name | Proposer's Party | Rationale |
|---|---|---|
| Control | Not Stated | Not Stated |
| | | |
| Democratic Control | Democrat | Not Stated |
| Republican Control | Republican | Not Stated |
| Bureaucratic Control | Not Stated | Bureaucratic |
| Politicized Control | Not Stated | Politicized |
| | | |
| Democratic Bureaucratic | Democrat | Bureaucratic |
| Democratic Politicized | Democrat | Politicized |
| Republican Bureaucratic | Republican | Bureaucratic |
| Republican Politicized | Republican | Politicized |

Table 1: Summary of Experimental Treatments

After answering a series of demographic and political questions, respondents were presented with a brief vignette describing an incumbent U.S. senator's court packing proposal to the federal judiciary. The vignette varied (a) the partisanship of the proposer (not stated, Democratic, or Republican) and (b) the proposer's rationale (not stated, bureaucratic, or politicized). The bureaucratic rationale was "Legal experts from both parties

[7]While recent research suggests that MTurk samples are not representative of the national population, it also shows that they are more representative than many other convenience samples, such as college students (Clifford, Jewell and Waggoner 2015; Berinsky, Huber and Lenz 2012). In some dimensions MTurk samples can be remarkably similar to the general public (Huff and Tingley 2015). As a result of this, researchers have been able to replicate key findings in law and psychology using MTurk samples (Firth, Hoffman and Wilkinson-Ryan 2018). To the extent that the sample is not representative of the general public, though, this would limit the external validity (i.e. generalizability) of our results. It does not affect the internal validity of our causal inferences, though. As Crabtree and Fariss (2016) note, it is important to first verify the internal validity of theoretical claims before assessing the degree to which those claims extend to other samples. We think that a fruitful avenue for future work would be—and petitioning the NSF to—test how our findings travel to other populations.

have discussed the Senator's proposal and agree that this proposal is an attempt to enhance the efficiency of the federal judiciary, enabling courts to better manage a backlog of cases." Respondents who were a assigned the politicized rationale learned "Legal experts from both parties have discussed the Senator's proposal and agree that this proposal is an attempt to enhance the efficiency of the federal judiciary, enabling courts to better manage a backlog of cases." The two treatments were fully crossed. Table 1 displays the full set of 9 conditions in the experiment. An example treatment (the Republican Politicized treatment) read as follows:

> An incumbent Republican Senator from a nearby state who is seeking reelection in November, 2018, recently introduced a bill in the U.S. Senate that would expand the size of the federal judiciary, adding 64 new federal circuit court (appellate) judges (a 37% increase), and 189 new district court (trial) judges (a nearly 30% increase). Legal experts from both parties have discussed the Senator's proposal and agree that this proposal is an attempt to enhance the efficiency of the federal judiciary, enabling courts to better manage a backlog of cases.

Following the vignette, respondents indicated whether they would vote for the proposer if given the option, assessed the proposer's job performance, and indicated their level of support for the proposal. Because the vignette, while theoretically based on similar proposals percolating through academia, was somewhat deceptive, the survey ended by debriefing the respondents about the experiment.

Several design considerations deserve particular discussion. First, though survey experiments to evaluate public response to judicial decision-making are increasingly common (e.g., Mondak 1991; Baird and Gangl 2006; Zink, Spriggs and Scott 2009; Gibson, Lodge and Woodson 2014, 2012; Bonneau and Cann 2015; Clark and Kastellec 2015; Armaly 2017), existing experimental designs typically present respondents with a hypothetical court decision, randomizing the particulars of the procedure or outcome and evaluating the extent to which citizens support shifts as a result. Though a clear improvement on purely observational studies, these experimental designs presuppose a high level of understanding of politics and the business of high courts, an assumption that is highly questionable in the U.S., much less elsewhere where courts are less active, involved in politics or politically independent.

Second, we needed to craft a credible proposal that had some external validity. Because not every state has a senator from both parties, we were forced to discuss an incumbent "from a nearby state." This is similar to the approach taken by Butler and Powell (2014) who queried respondents about state legislative elections in "a nearby state" in order to randomize the partisanship of the party in control of the state legislature. We acknowledge that the hypothetical nature of the vignette is not ideal; however, such an approach was necessarily to be able to credibly and randomly assign the partisanship of the proposer.

Third, we based the vignette on court curbing proposals that attracted some public attention in the lead-up to our experiment. We modeled the proposal most closely after a well-publicized proposed judgeship bill by Northwestern Law Professor Steven G. Calabresi, which proposed "that Congress should — at a minimum — authorize 61 new circuit judgeships... and 200 district court judgeships" (Calabresi and Hirji 2017, 21). We designed the proposal in our experiment to mirror closely these numbers. Importantly, such proposals are not limited conservative elites. After Justice Kennedy announced his resignation in June 2018, liberal activists and academics also began discussing court packing (Ayres and Witt 2018; White 2018). Given the prominent discussions of the topic on both the left and the right, our vignette has a strong claim to external validity.

Finally, ours is the first experiment to explicitly examine the behavioral manifestation legitimacy theory implies: the public's willingness to vote for an incumbent, and their evaluation of incumbent performance in light of the court curbing attempt. At the heart of the theoretical models whereby widespread public support for courts deters inter-branch aggression, it is the public's ability to remove institutional assailants from their position that compels compliance and incumbent restraint (Vanberg 2001; Clark 2009). Though much contemporary experimental work considers possible shifts in institutional legitimacy (Christenson and Glick 2015; Gibson and Nelson 2016, 2017), it is the act of going to the ballot box to punish or reward and incumbent that is the heart of electoral accountability. Our outcome variables directly evaluate the public's reaction vis-á-vis the incumbent, an is therefore consistent with the framing of court curbing activities as largely 'position-taking' activities, meant to rally a base of electoral support (Mayhew 1974; Clark 2010; Driscoll 2012).

### *B Outcome and Explanatory Variables*

We have three major outcome variables. First, we measured respondents' hypothetical vote choice in the upcoming election with the question "If you were in this state, how would you vote in the next election?" 27.29% of respondents said they would vote for the incumbent.[8] Second, we asked respondents "To what extent do you approve of the incumbent's job performance?" 33.46% of respondents said they "Strongly Approve" or "Approve" of the incumbent's job performance. Finally, we measured respondents' approval of the proposal itself, asking respondents "To what extent do you approve of the incumbent's reform proposal?" 42.81% of respondents said they "Strongly Approve" or "Approve" of the proposal. The three measures are moderately correlated with each other. The relationship between vote choice and job performance is $r = 0.48$; for vote choice and proposal approval, it is $r = .55$; and for job performance and proposal approval it is $r = .70$.[9] In the analyses we present, we have rescaled all of the variables to vary from 0 to 1 for ease of comparison.

[8]To the question of vote choice, a plurality of our respondents (34%) said they would vote for someone other than the incumbent Senator, with another 38% reporting they would either abstain or were unsure.

[9]While we treat the three variables as separate dependent variables, it is worth noting that they form a fairly reliable scale, with $\alpha = 0.73$ and scale onto a single dimension with loadings of 0.62 (Vote Choice), 0.77 (Job Performance), and 0.81 (Proposal Support).

Though the random assignment to treatment mitigates the need to account for respondent-specific factors, a substantial portion of our analysis depends on the alignment of the respondent's partisanship with the proposer's. Analyses on this front therefore need to control for observable characteristics on which respondents may differ. We therefore included a battery of respondent-level characteristics. We measured the respondents' gender (50.1% female), age (38.7 years old, on average), race (9.0% black, 20.3% nonwhite), ethnicity (12.1% Hispanic), education (measured on an 8-point scale with 58.7% college graduates and 29.4% having completed some college), social class (55.79% own their home), ideology (51.4% describing themselves as liberal; 30.0% describing themselves as conservative), and partisanship (40.0% Democrat, 26.6% Republican). Finally, we included a 5-item political knowledge scale.[10] Befitting the high level of political knowledge typical of online convenience samples, the average respondent answered 3.9 of the 5 questions correctly.[11]

Finally, we were particularly interested in the ability of institutional support—legitimacy—to protect the federal courts against court packing attempts. To this end, we modified the standard battery of diffuse support questions suggested by Gibson, Caldeira and Spence (2003) to the broader federal judiciary:

- The right of the federal courts to decide certain types of controversial issues should be reduced. (25.74% Agree)
- Judges on the federal judiciary who consistently make decisions at odds with what the majority wants should be removed from their position. (28.12% Agree)
- The federal judiciary ought to be made less independent so that it listens a lot more to what the people want. (33.28% Agree)

[10]Full question wording is available in the Appendix.

[11]Importantly, there is no evidence that assignment to treatment was systematically related with any of these factors. Chi-squared tests of independence with gender (p=.15), race (p=.38), ethnicity (p=.84), education (p=.97), social class (p=.28), ideology (p=.78), partisanship (p=.80) and knowledge (p=.61) all render us unable to reject the null hypothesis of independence between our treatment and the respondent characteristic.

The three items are strongly reliable with $\alpha = 0.84$. Moreover, they scale on a single dimension with factor loadings of 0.72, 0.78, and 0.80. We therefore use as our measure of Federal Court Legitimacy the factor score from a unidimensional factor analysis. Scored from 0-1, the variable has a mean of 0.59 and a standard deviation of 0.27.

We made a similar index for congressional legitimacy:[12]

- Congress should be reformed by removing either the House or the Senate, making it a unicameral legislature (16.83% Agree)
- The right of Congress to oversee the executive branch should be reduced. (17.58% Agree)
- Members of Congress who consistently make decisions at odds with what the majority wants should be impeached. (13.18% Agree)
- The U.S. Congress ought to be subject to term limits so that it listens a lot more to what the people want. (70.26% Agree)

These four items form a slightly less reliable scale, with $\alpha = 0.65$. The items also load on a single dimension with an average factor loading of 0.55. Perhaps surprisingly, the term limits item is the item with the poorest performance. We use as our measure of Congressional Legitimacy the factor score from a unidimensional factor analysis. Scored from 0-1, the variable has a mean of 0.60 and a standard deviation of 0.22. Our two measures of legitimacy correlate at $r = 0.67$.

## V Results

We analyze the experiment in a series of steps. First, we consider the direct effects of rationale and partisanship treatments on each of the three outcome variables. Second, we analyze whether there is an interactive effect of the two treatments; that is, whether a proposal's rationale has a different effect when the proposer is a copartisan or an outpartisan.

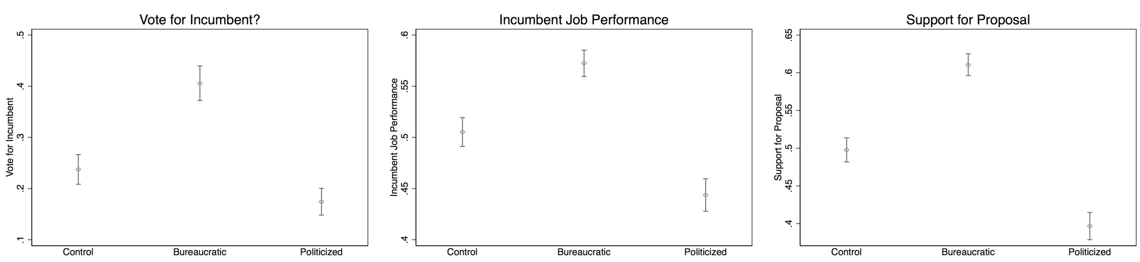
[12] Gibson, Caldeira and Spence (2005) also have a measure of congressional legitimacy. Both scales contain similar items.

Finally, we consider whether preexisting levels of institutional support mitigate or exacerbate the effect of the court curbing proposal on respondents' support for the proposer and the proposal.

## *A Direct Effects*

We begin our analysis of the experiment's effects by testing for differences across the two different *rationales* to which respondents were exposed. Recall that one-third of respondents were not provided a rationale for the court packing proposal, one-third of respondents read a bureaucratic rationale for the proposal, and the final one-third of respondents read a politicized rationale for the proposal.

Figure 1: Support for the Proposer and Proposal, by Rationale




The dots represent the average value of a dependent variable that is scale on the 0-1 interval. The whiskers represent 95% confidence intervals. Higher values of each outcome variable indicate more support for the proposer or the proposal.

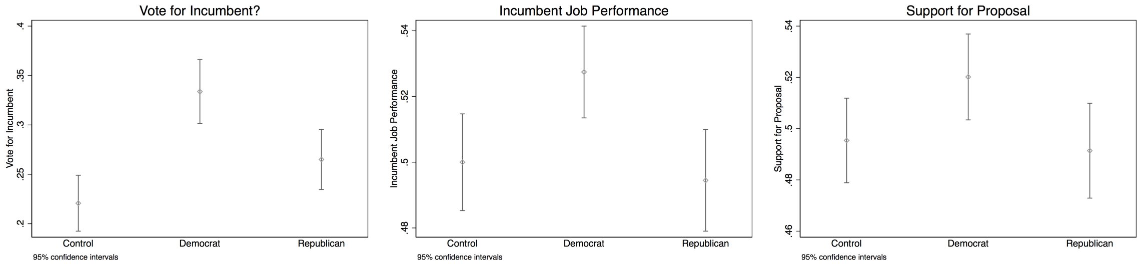
Figure 1 displays the average value of each of the three dependent variables across the three different rationale conditions. The conclusion from each panel of the figure is identical and unambiguous: compared to a control condition, respondents support incumbents and proposals that are rationalized for bureaucratic aims.[13] Conversely, respondents punish proposers and proposals that seek to politicize the judiciary. The wide vertical distance between the coefficients, as well as the relatively tight confidence intervals on each of these quantities of interest implies these effects are not only statistically significant from zero, but they are also statistically significant from each other. These results would suggest that incumbents who frame their efforts at judicial reform in bureaucratic or non-partisan terms are smart to do so. Describing these actions in political neutral terms is not only disarming to public opinion, but may in fact be a useful point on which to cultivate electoral support.[14]

It is important to note that the findings presented in Figure 1 do not account for partisanship; the figure presents the average value of the outcome variables, averaging across the partisanship of the proposer. That these results persist and are so clear given this potential confounding is further evidence of their strength.

[13]Whereas all respondents received a treatment, we cannot evaluate the counterfactual relative to a pure untreated group (c.f. Driscoll & Nelson 2018). At the same time, we are comforted by the quantities observed in the control group: across all outcome variables, the likelihood of voting for an incumbent, supporting the proposer or the proposal is about what you would expect it to be in a two party system with non-mandatory voting, taking into account that we do not control for partisanship.

[14]This is consistent with what information we have on court curbing proposals. Driscoll's (2012) classification of court reform proposals proposed in Chile and Argentina suggest that irrespective of their intended effects, court reform proposals are most commonly described in terms of their ability to enhance judicial administration or efficiency, followed by other laudable motives such as combating corruption, checking executive power or enhancing human rights (Driscoll 2012).

Figure 2: Support for the Proposer and Proposal, by Proposer's Partisanship

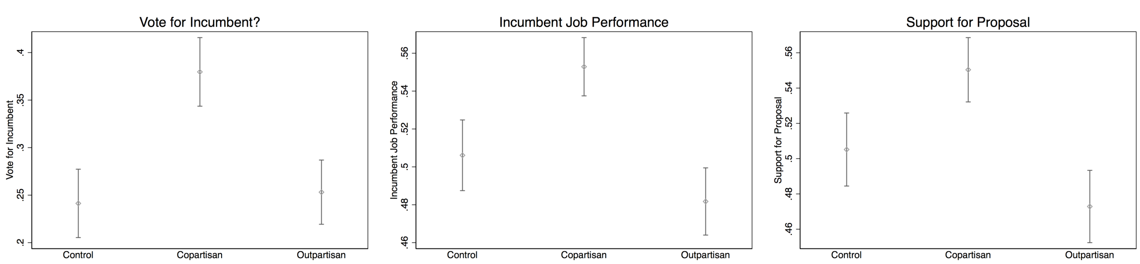



The dots represent the average value of a dependent variable that is scale on the 0-1 interval. The whiskers represent 95% confidence intervals. Higher values of each outcome variable indicate more support for the proposer or the proposal.

To begin to examine the effects of partisanship, we turn to Figure 2, which plots average values of the dependent variables by the partisanship of the proposer. The results provide some evidence that the respondents support Democrats and their proposals, all else equal.

Of course, all else is *not* equal. While respondents were randomly assigned to their treatment, they were not randomly assigned their own partisanship. Indeed, a plurality (40%) of our respondents were Democrats. It seems likely that the Democratic boost in Figure 2 is likely a result of this lopsideness in our sample. Were this the case, Figure 2 would actually *understate* the effectiveness of the partisanship cue due to heterogeneity in the respondent partisanship-experimental treatment pairings.

Figure 3: Support for the Proposer and Proposal, by Copartisanship




The dots represent the average value of a dependent variable that is scale on the 0-1 interval. The whiskers represent 95% confidence intervals. Higher values of each outcome variable indicate more support for the proposer or the proposal.

This is exactly the case. Figure 3 displays the average values of the outcome variable by whether respondents were in the control condition, were exposed to a proposal by a copartisan, or learned of a proposal made by an outpartisan.[15] Of those respondents assigned to learn of the proposer's partisanship, 52% learned of a proposal by a copartisan. Moreover, partisanship has a powerful effect on each of the outcome variables. Across the board, respondents are more likely to vote for and evaluate positively copartisans; there is no statistical difference between the control condition and evaluations of an outpartisan proposal or a proposer.[16]

### *B The Interplay of Copartisanship and Rationale*

Thus far, we have seen that both a court packing proposal's rationale and the partisanship of the proposer have powerful effects on respondents' evaluations of the proposal, and the voters' willingness to punish or reward incumbents for their actions taken against the courts. We now examine the interaction of the two sets of treatments to determine if copartisanship exacerbates or mitigates the effects of a proposal differently based on the proposal's rationale. Because copartisanship is not randomly assigned, we estimated a series of multivariate models including the multiplicative interaction of all of the treatments.[17]

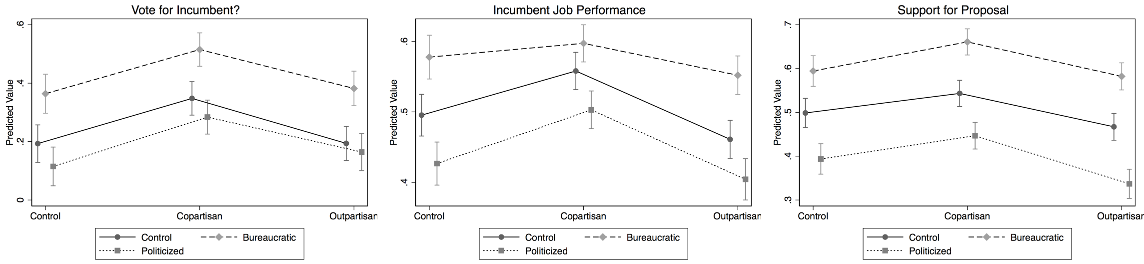
The answer is a resounding no. For no pair of treatments—and any of the three outcome variables—is there any evidence of an interactive effect. While both sets of treatments have a powerful additive effect on respondents' evaluations, we have absolutely no evidence that their effects are conditional on one another.

[15]For these analyses, we restrict our sample to Democratic and Republican respondents.

[16]Figure 3 displays average values of the outcome variables; we acknowledge that respondents were not assigned based on copartisanship, and so predictions from a multivariate model that holds respondent characteristics constant would be perhaps a more valid approach. Such models (shown in Table 3) suggest exactly the same pattern displayed in Figure 3.

[17]Full model results are provided in Table 4.

Figure 4: Predicted Values, By and Proposer Copartisanship



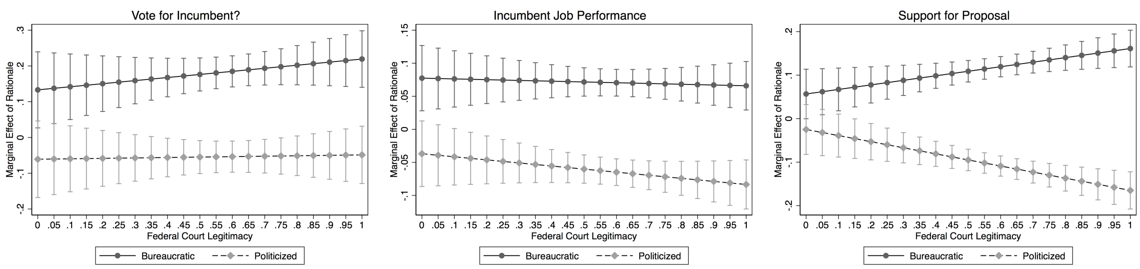

The figures plot the predicted value of the outcome variables for each combination of Rationale and Proposer Copartisanship. The whiskers represent 95% confidence intervals. Higher values of each outcome variable indicate more support for the proposer or the proposal. Model results provided in Columns 1-3 of Table 4.

Still, Figure 4 provides some new insights. For example, looking at the left panel of the Figure, giving respondents information that a court packing proposal was introduced for bureaucratic reasons boosts support for the proposal and the likelihood of favorable vote choice above that expected by copartisanship alone (the control condition). Bureaucratic rationales are always rewarded, politicized attempts to change the court composition are always punished. What is more, and consistent with other studies, source cues count (Armaly 2017; Clark and Kastellec 2015). The effects of copartisanship seem to be stronger than those of the rationale, as seen by the differences across the x-axis compared to those on the y-axis for a given value of copartisanship. This figure therefore emphasizes the important role that partisanship plays in understanding the effects of court curbing proposals.

### *C The Conditioning Effects of Legitimacy*

We further expected legitimacy to act as both a sword and a shield, with respondents' judgments of the federal court's legitimacy shielding the Court from an attack while increased beliefs in the legitimacy of Congress supercharging the effectiveness of the court packing proposal. We therefore estimated two sets of models: one that interacted federal court legitimacy with the treatments and another that interacted congressional legitimacy with the treatments. Both in the interest of parsimony and because of the lack of an interactive effect uncovered in the previous section, we consider the two sets of treatments separately. The full results of these models are found in Tables 5 and 6 in the Appendix.

Figure 5: Marginal Effect of Rationale on Support for the Proposer and Proposal, by Federal Court Legitimacy



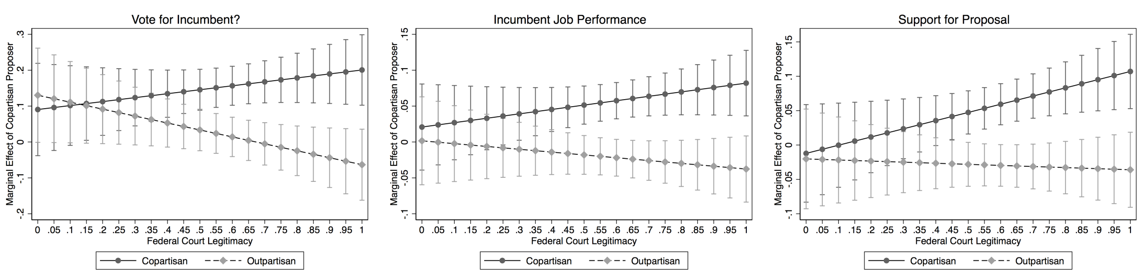

The figures plot the marginal effect of a bureaucratic or politicized rationale (compared to no stated rationale) as Federal Court Legitimacy varies. The whiskers represent 95% confidence intervals. Higher values of each outcome variable indicate more support for the proposer or the proposal. Model results provided in Columns 1-3 of Table 5

We begin by probing the protective effects of the federal judiciary's own legitimacy. The voluminous literature on comparative judicial politics expects that, as respondents' pretreatment beliefs that the judiciary to be legitimate increase, they should be increasingly willing to punish incumbents as a result of their proposal (Caldeira & Gibson 1992).

Figure 5 plots the marginal effect of receiving a bureaucratic or politicized court curbing on each of the three outcome variables. The results both challenge the traditional assumption in judicial politics, and illustrate the difference between support for a proposal and for the proposer. Beginning on the right-hand panel of the figure, we see exactly the expected result: as respondents view the federal judiciary as more legitimate, they are less likely to evaluate a politicized proposal favorably. However, respondents increasingly approve of bureaucratic proposals as their diffuse support for the judiciary increases. This appears a critical caveat to the Eastonian interpretation of institutional legitimacy as an unwillingness to support fundamental changes to institutional structures. Instead, it would appear that changes of a certain kind (those aimed at objectively improving institutional function) are warmly received by those who deem the courts legitimate.

However, *these same effects do not translate to the proposer*. There is *no* evidence that respondents are less likely to vote for or approve of incumbents who attack courts as their diffuse support for the judiciary increases, even when faced with an effort to politicize the courts. In the case of our vote choice outcome variable, the treatment effect for the politicized reform proposal is flat across all values of *Legitimacy*, and at no point is the coefficient differentiable from zero. Though this is at odds with what many theoretical accounts would lead us to expect, it is important to note here that we have not yet accounted for the partisan identity of the proposer, relative to the respondent's own. We now turn to the effects when we account for copartisanship.

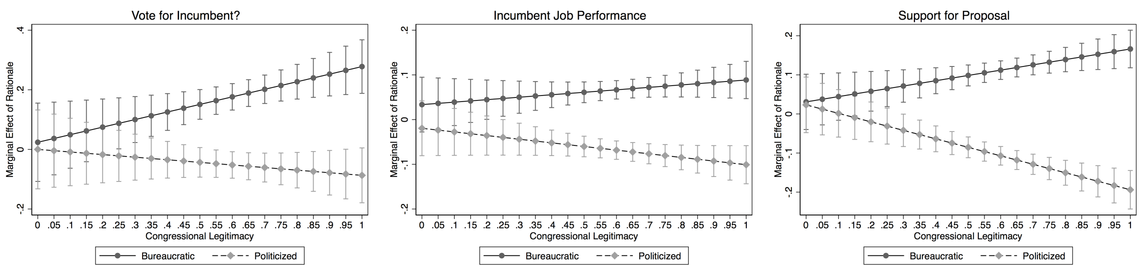
Figure 6: Marginal Effect of Copartisanship on Support for the Proposer and Proposal, by Federal Court Legitimacy




The figures plot the marginal effect of a copartisan or outpartisan proposer (compared to no stated proposer partisanship) as Federal Court Legitimacy varies. The whiskers represent 95% confidence intervals. Higher values of each outcome variable indicate more support for the proposer or the proposal. Model results provided in Columns 4-6 of Table 5

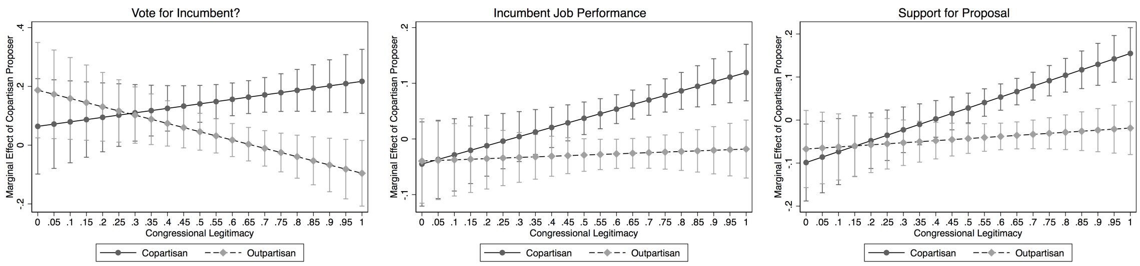
Generally, these same conclusions hold when we examine the effects of copartisanship, as seen in Figure 6. Far from punishing incumbents who attack courts, there is no evidence that increased legitimacy has a protecting effect on respondents' vote choice or evaluation of the incumbent. What's more, the effect we do observe is *contrary* to that suggested by many scholars of judicial politics: proposals by copartisans are evaluated *more favorably* when federal court legitimacy is high. At the high end of the legitimacy scale, we find that respondents were about 5-10% more likely to vote for the incumbent and approve of the proposal, conditional on having been proposed by a member of their own party. Legitimacy, it seems, is a weak and ineffective shield from court curbing proposals, and only serves to motivate incumbent punishment when the proposer is unaligned to respondents' partisanship.

Figure 7: Marginal Effect of Rationale on Support for the Proposer and Proposal, by Congressional Legitimacy




The figures plot the marginal effect of a bureaucratic or politicized rationale (compared to no stated rationale) as Congressional Legitimacy varies. The whiskers represent 95% confidence intervals. Higher values of each outcome variable indicate more support for the proposer or the proposal. Model results provided in Columns 1-3 of Table 6

Figure 8: Marginal Effect of Copartisanship on Support for the Proposer and Proposal, by Congressional Legitimacy




The figures plot the marginal effect of a copartisan or outpartisan proposer (compared to no stated proposer partisanship) as Congressional Legitimacy varies. The whiskers represent 95% confidence intervals. Higher values of each outcome variable indicate more support for the proposer or the proposal. Model results provided in Columns 4-6 of Table 6

Turning now to the legitimacy of the putative instigator of institutional aggression, we now consider whether congressional legitimacy confers any benefits to would be judicial attackers. Figures 7 and 8 show the marginal effects of the rationale and copartisan treatments as congressional legitimacy varies. The results in some sense mirror those for federal court legitimacy: bureaucratic proposals are evaluated more favorably among those who imbue the legislature with legitimacy; the converse is true for politicized proposals. Likewise, proposals made by copartisans are evaluated more favorably as congressional legitimacy increases.

Yet these results also suggest some new conclusions. Those who view Congress as particularly legitimate are particularly likely to evaluate copartisan incumbents favorably and to want to vote for them, and they are particularly likely to give an electoral benefit to legislators who make bureaucratic court curbing proposals. Thus, both the legitimacy of the federal courts and the U.S. Congress have an important role to play in conditioning the effectiveness of court curbing proposals. But, their role is not what traditional theory suggests. These results suggest that a reenvisioning of the role of institutional legitimacy might be called for, to better understand when it serves as an adequate shield from inter-institutional attacks.

## VI Discussion

The results of our experiment stand in strong contrast to assumptions that undergird prominent models of interbranch relations. Court curbing is not always costly. Rather, when legislators have the foresight to frame the attempt as bureaucratic in nature or share the partisanship of their constituents—and we have reason to think that they do (supra. 14, Driscoll 2012)—they might actually benefit from inter-institutional attacks.

A key implication of our findings is the vital importance of partisanship in understanding the consequences of court curbing. That copartisanship has such a strong effect on evaluations of both the proposer and the proposal underscores the dominating influence of party identification in modern, polarized American politics.[18]

Second, our findings suggest that an ambitious politician who seeks to attack the courts without political ramifications should frame her proposal as benign bureaucratic interventions. Such a frame, our results suggest, would not only stifle any electoral backlash *but would actually help the proposer's reelection chances.* While cynical, these teach an important lesson to both activists and academics; while the public does dislike politicizing courts—as Gibson and Nelson (2017) and others have shown—they *like* improving the bureaucratic functioning of the judiciary. This is an unsung point for judicial reformers throughout the country.

Still, the credibility of that rationale is one point about our experimental design that deserves additional discussion. We crafted the vignette to eliminate as many concerns among respondents as possible as to the effect of the court curbing proposal, though we

[18]An open question we cannot address here is the extent to which individuals' partisanship is rooted in their support for liberal democratic institutions. This is an open plausibility to which future research ought attend.

acknowledge that Americans' beliefs about the credibility of academics differ widely (Nelson and Gibson 2018).[19] Future work should vary the credibility of the rationale to determine when the public believes a legislator's intent is *actually* bureaucratic (or politicized) and when they view such attempts as cynical.

Another point that merits discussion is the difference between court curbing and court packing. Our experiment use a court *packing* attempt to test a claim about court *curbing*. In our view, this is justified: court packing is an attempt to dilute the work of current judges; it is an attack on judicial independence. However, in its attempt to expand the judiciary rather than reduce a court's powers, we acknowledge the possibility that the public draws a distinction between these two types of efforts. Future work should vary the type of court curbing threat to examine the generalizability of our findings.

A final point about our research design is the comparisons we are entitled to make given our experimental design. We designed the experiment such that all respondents were exposed to a court curbing attack. We made this decision believing that the validity of the experiment might suffer if respondents in a pure control condition—who were not exposed to any legislative proposal—were asked to evaluate a legislator who they were given no information about. However, other experimental designs might compare a court curbing proposal with some other sort of policy proposal or make some other, similar, comparison that would enable future efforts to compare the effects of a *court curbing* proposal to some other type of legislative proposal rather than, as we have done in this paper, examined differences in the effects of different types of court curbing proposals. This is ripe for future work.

The foregoing discussion takes for granted wide variation in attentiveness to the Courts among the American people. Lenz (2012) suggests that, as voters become more aware of a

[19]Indeed, though we do not discuss the results in full here, the data do suggest heterogeneous treatment effects for the rationale based upon respondents stated trust in academics. This is a caveat we will explore in future research.

politician's stance on an issue, they are more likely to adopt it. At the same time, Gibson and Caldeira (2009) suggests that knowing more about Courts inspires higher levels of institutional legitimacy. In this way, additional attention might have cross-cutting effects on voter behavior. We have sidestepped this issue in this paper because, as Barabas and Jerit (2010) note, survey experiments like ours mitigate to a large extent differences in information acquisition among the public. The consequence is that the effects we observe are likely to be maximal ones.

We conclude our paper with a call for more research. Despite our results we do not think that legitimacy *never* shields institutions from harm; indeed, there is a wealth of evidence that, in some cases, it can (e.g. Nelson and Uribe-McGuire 2017). Rather, more work—both in the U.S. and abroad—is necessary to delineate the conditions under which institutional support is effective at protecting institutions and when citizens' instrumental concerns dominate their belief in institutional legitimacy. We hope to soon conduct public opinion surveys in two dozen democracies worldwide to understand these dynamics. As more and more democratic leaders push the bounds of their institutions, understanding the trade-off between institutional legitimacy and instrumentalism is more pressing than ever.

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## VII Appendix

### *A Sample Information*

| | | Internet Samples | | | Face to Face |
|---|---|---|---|---|---|
| | Sample | Christenson and Glick | Berinsky, Huber, Lenz | ANES-P 2008-09 | ANES 2008 |
| % Female | 50.1 | 54.4 | 60.1 | 57.6 | 55 |
| % White | 79.7 | 79 | 83.5 | 83 | 79.1 |
| % Black | 9.0 | 7.9 | 4.4 | 8.9 | 12 |
| % Hispanic | 12.1 | 5 | 6.7 | 5 | 9.1 |
| Mean Age (Yrs) | 38.7 | 33.4 | 32.3 | 49.7 | 46.6 |
| Ideology (7 pt.) | 3.5 | 3.3 | 3.4 | 4.3 | 4.2 |
| Education | 59% Col Grad<br>29% Some Col | 50% Col Grad<br>37% Some Col | 14.9 yrs | 16.2 yrs | 13.5 yrs |

Table 2: Comparison of Sample Demographics. ANES-P is the American National Election Panel Study conducted by Knowledge Networks and the ANES is the American National Election Study. Data from the ANES are weighted. Data for Christenson and Glick (2015) comes from Table A1 of their article; data for the remaining columns comes from Table 3 in Berinsky, Huber and Lenz (2012).

*B Measurement of Independent Variables*

**Political Knowledge** Some judges in the U.S. are elected; others are appointed to the bench. Do you happen to Court Knowledge if the justices of the U.S. Supreme Court are

- Elected (1)
- Appointed to the Bench (2)

Some judges in the U.S. serve for a set number of years; others serve a life term. Do you happen to Court Knowledge whether the justices of the U.S. Supreme Court serve...

- For a Set Number of Years (1)
- For a Life Term (2)

Do you happen to Court Knowledge to which of the following institutions has the last say when there is a conflict over the meaning of the Constitution?

- The U.S. Supreme Court (1)
- The U.S. Congress (2)
- The President (3)

As you may know, the U.S. Supreme Court issues written opinions along with its decisions in most major cases it decides. We wonder if you Court Knowledge about how many decisions with opinions the Court issues each year. Would you say it writes

- Less than one hundred decisions with opinions each year. (1)
- Around five hundred decisions with opinions. (2)
- A thousand decisions with opinions or more per year. (3)

When the U.S. Supreme Court decides a case, would you say that

- The decision can be appealed to another court. (1)
- Congress can review the decision to see if it should become the law of the land. (2)
- The decision is final and cannot be further reviewed. (3)

| | Vote | Job | Proposal | Vote | Job | Proposal |
|---|---|---|---|---|---|---|
| Bureaucratic | 0.181* | 0.070* | 0.117* | | | |
| | (0.022) | (0.010) | (0.012) | | | |
| Politicized | -0.059* | -0.062* | -0.104* | | | |
| | (0.022) | (0.010) | (0.012) | | | |
| Copartisan | | | | 0.161* | 0.054* | 0.056* |
| | | | | (0.027) | (0.013) | (0.015) |
| Outpartisan | | | | 0.027 | -0.024 | -0.028 |
| | | | | (0.027) | (0.013) | (0.015) |
| Female | -0.072* | -0.004 | -0.004 | -0.076* | 0.001 | 0.004 |
| | (0.018) | (0.008) | (0.010) | (0.021) | (0.010) | (0.012) |
| Democrat | 0.062* | 0.005 | 0.010 | 0.063 | 0.002 | 0.012 |
| | (0.023) | (0.010) | (0.012) | (0.035) | (0.016) | (0.019) |
| Republican | 0.099* | 0.053* | 0.056* | 0.102* | 0.056* | 0.059* |
| | (0.026) | (0.012) | (0.014) | (0.038) | (0.018) | (0.021) |
| Ideo | -0.018 | -0.020 | -0.048* | -0.019 | -0.032 | -0.055* |
| | (0.039) | (0.018) | (0.021) | (0.045) | (0.021) | (0.025) |
| Knowledge | -0.107* | -0.128* | -0.146* | -0.120* | -0.138* | -0.166* |
| | (0.034) | (0.016) | (0.018) | (0.040) | (0.019) | (0.022) |
| Age | -0.002* | -0.001* | -0.002* | -0.001 | -0.001* | -0.002* |
| | (0.001) | (0.000) | (0.000) | (0.001) | (0.000) | (0.000) |
| Black | 0.000 | 0.026 | 0.034* | 0.009 | 0.033 | 0.043* |
| | (0.032) | (0.015) | (0.017) | (0.039) | (0.018) | (0.021) |
| Hispanic | 0.037 | 0.021 | 0.027 | 0.043 | 0.024 | 0.028 |
| | (0.028) | (0.013) | (0.015) | (0.034) | (0.016) | (0.019) |
| Education | 0.113* | 0.056* | 0.027 | 0.090 | 0.064* | 0.026 |
| | (0.042) | (0.019) | (0.022) | (0.051) | (0.024) | (0.028) |
| Own Home | 0.037* | 0.015 | 0.022* | 0.032 | 0.021* | 0.028* |
| | (0.019) | (0.009) | (0.010) | (0.023) | (0.011) | (0.012) |
| Intercept | 0.250* | 0.590* | 0.635* | 0.250* | 0.582* | 0.629* |
| | (0.051) | (0.024) | (0.027) | (0.067) | (0.031) | (0.037) |

Table 3: Multivariate Regression Results

## *C Additional Results*

| | Vote | Job | Proposal |
|---|---|---|---|
| Bureaucratic | 0.171* | 0.082* | 0.096* |
| | (0.047) | (0.022) | (0.024) |
| Politicized | -0.078 | -0.069* | -0.105* |
| | (0.047) | (0.022) | (0.024) |
| Copartisan | 0.155* | 0.063* | 0.045 |
| | (0.044) | (0.020) | (0.023) |
| Outpartisan | 0.001 | -0.034 | -0.032 |
| | (0.044) | (0.021) | (0.023) |
| Copartisan X Bureaucratic | -0.004 | -0.043 | 0.022 |
| | (0.062) | (0.029) | (0.033) |
| Copartisan X Politicized | 0.014 | 0.014 | 0.008 |
| | (0.063) | (0.029) | (0.033) |
| Outpartisan X Bureaucratic | 0.017 | 0.009 | 0.019 |
| | (0.063) | (0.029) | (0.033) |
| Outpartisan X Politicized | 0.049 | 0.012 | -0.025 |
| | (0.064) | (0.030) | (0.034) |
| Female | -0.081* | -0.002 | 0.002 |
| | (0.021) | (0.010) | (0.011) |
| Democrat | 0.064 | 0.004 | 0.017 |
| | (0.034) | (0.016) | (0.018) |
| Republican | 0.099* | 0.056* | 0.061* |
| | (0.037) | (0.017) | (0.019) |
| Ideology | -0.009 | -0.028 | -0.046* |
| | (0.044) | (0.020) | (0.023) |
| Knowledge | -0.115* | -0.136* | -0.161* |
| | (0.039) | (0.018) | (0.021) |
| Age | -0.002 | -0.002* | -0.002* |
| | (0.001) | (0.000) | (0.000) |
| Black | -0.000 | 0.027 | 0.033 |
| | (0.038) | (0.017) | (0.020) |
| Hispanic | 0.039 | 0.022 | 0.024 |
| | (0.033) | (0.015) | (0.017) |
| Education | 0.087 | 0.063* | 0.024 |
| | (0.050) | (0.023) | (0.026) |
| Own Home | 0.031 | 0.020* | 0.027* |
| | (0.022) | (0.010) | (0.012) |
| Intercept | 0.227* | 0.583* | 0.636* |
| | (0.071) | (0.033) | (0.037) |

Table 4: Multivariate Regression Results: Interacted Treatments

| | Vote | Job | Proposal | Vote | Job | Proposal |
|---|---|---|---|---|---|---|
| Bureaucratic | 0.133* | 0.077* | 0.057 | | | |
| | (0.054) | (0.025) | (0.029) | | | |
| Politicized | -0.061 | -0.037 | -0.025 | | | |
| | (0.055) | (0.025) | (0.029) | | | |
| Fed. Jud. Legit. | -0.091 | -0.027 | -0.022 | -0.063 | -0.075* | -0.083 |
| | (0.061) | (0.028) | (0.033) | (0.077) | (0.036) | (0.043) |
| Bureaucratic X Fed. Jud. Legit. | 0.086 | -0.012 | 0.104* | | | |
| | (0.083) | (0.039) | (0.044) | | | |
| Politicized X Fed. Jud. Legit. | 0.012 | -0.047 | -0.140* | | | |
| | (0.084) | (0.039) | (0.045) | | | |
| Copartisan | | | | 0.091 | 0.021 | -0.012 |
| | | | | (0.066) | (0.031) | (0.036) |
| Outpartisan | | | | 0.130 | 0.002 | -0.020 |
| | | | | (0.067) | (0.031) | (0.037) |
| Copartisan X Fed. Jud. Legit. | | | | 0.110 | 0.061 | 0.119* |
| | | | | (0.101) | (0.047) | (0.056) |
| OutpartisanX Fed. Jud. Legit. | | | | -0.193 | -0.039 | -0.016 |
| | | | | (0.103) | (0.048) | (0.057) |
| Female | -0.065* | -0.001 | -0.002 | -0.071* | 0.004 | 0.004 |
| | (0.019) | (0.009) | (0.010) | (0.022) | (0.010) | (0.012) |
| Democrat | 0.063* | 0.004 | 0.010 | 0.074* | 0.002 | 0.008 |
| | (0.023) | (0.011) | (0.012) | (0.036) | (0.017) | (0.020) |
| Republican | 0.099* | 0.055* | 0.059* | 0.112* | 0.060* | 0.061* |
| | (0.026) | (0.012) | (0.014) | (0.038) | (0.018) | (0.021) |
| Ideology | -0.011 | -0.012 | -0.048* | -0.012 | -0.027 | -0.059* |
| | (0.041) | (0.019) | (0.022) | (0.047) | (0.022) | (0.026) |
| Knowledge | -0.078* | -0.110* | -0.130* | -0.088* | -0.114* | -0.150* |
| | (0.037) | (0.017) | (0.020) | (0.043) | (0.020) | (0.024) |
| Age | -0.001 | -0.001* | -0.002* | -0.001 | -0.001* | -0.001* |
| | (0.001) | (0.000) | (0.000) | (0.001) | (0.000) | (0.001) |
| Black | 0.001 | 0.028 | 0.034 | 0.009 | 0.035 | 0.040 |
| | (0.032) | (0.015) | (0.017) | (0.039) | (0.018) | (0.022) |
| Hispanic | 0.003 | 0.008 | 0.016 | 0.008 | 0.009 | 0.019 |
| | (0.030) | (0.014) | (0.016) | (0.036) | (0.017) | (0.020) |
| Education | 0.121* | 0.067* | 0.038 | 0.115* | 0.083* | 0.039 |
| | (0.043) | (0.020) | (0.023) | (0.052) | (0.024) | (0.029) |
| Own Home | 0.029 | 0.008 | 0.017 | 0.022 | 0.012 | 0.023 |
| | (0.019) | (0.009) | (0.010) | (0.023) | (0.011) | (0.013) |
| Constant | 0.260* | 0.571* | 0.615* | 0.214* | 0.578* | 0.648* |
| | (0.060) | (0.028) | (0.032) | (0.080) | (0.037) | (0.044) |

Table 5: Federal Court Legitimacy Models

| | Vote | Job | Proposal | Vote | Job | Proposal |
|---|---|---|---|---|---|---|
| Bureaucratic | 0.024 | 0.034 | 0.031 | | | |
| | (0.067) | (0.031) | (0.036) | | | |
| Politicized | 0.000 | -0.019 | 0.023 | | | |
| | (0.067) | (0.031) | (0.036) | | | |
| Cong. Legit. | -0.037 | -0.030 | -0.025 | 0.035 | -0.127* | -0.177* |
| | (0.075) | (0.035) | (0.040) | (0.098) | (0.046) | (0.054) |
| Bureaucratic X Cong. Legit. | 0.254* | 0.055 | 0.135* | | | |
| | (0.103) | (0.048) | (0.055) | | | |
| Politicized X Cong. Legit. | -0.087 | -0.081 | -0.217* | | | |
| | (0.105) | (0.048) | (0.056) | | | |
| Copartisan | | | | 0.064 | -0.045 | -0.099* |
| | | | | (0.083) | (0.039) | (0.046) |
| Outpartisan | | | | 0.187* | -0.040 | -0.067 |
| | | | | (0.083) | (0.039) | (0.046) |
| Copartisan X Cong. Legit. | | | | 0.153 | 0.164* | 0.253* |
| | | | | (0.126) | (0.059) | (0.069) |
| Outpartisan X Cong. Legit. | | | | -0.283* | 0.021 | 0.049 |
| | | | | (0.127) | (0.059) | (0.070) |
| Democrat | 0.043 | 0.005 | 0.004 | 0.055 | 0.003 | 0.003 |
| | (0.024) | (0.011) | (0.013) | (0.037) | (0.017) | (0.020) |
| Republican | 0.109* | 0.058* | 0.055* | 0.104* | 0.056* | 0.046* |
| | (0.027) | (0.012) | (0.014) | (0.039) | (0.018) | (0.022) |
| Ideology | -0.035 | -0.021 | -0.052* | -0.036 | -0.033 | -0.057* |
| | (0.041) | (0.019) | (0.022) | (0.048) | (0.022) | (0.026) |
| Knowledge | -0.085* | -0.117* | -0.129* | -0.118* | -0.125* | -0.146* |
| | (0.037) | (0.017) | (0.020) | (0.044) | (0.021) | (0.024) |
| Age | -0.002* | -0.001* | -0.002* | -0.001 | -0.001* | -0.001* |
| | (0.001) | (0.000) | (0.000) | (0.001) | (0.000) | (0.001) |
| Black | -0.005 | 0.020 | 0.026 | -0.003 | 0.024 | 0.031 |
| | (0.033) | (0.015) | (0.018) | (0.041) | (0.019) | (0.022) |
| Hispanic | 0.030 | 0.016 | 0.018 | 0.027 | 0.018 | 0.018 |
| | (0.031) | (0.014) | (0.016) | (0.036) | (0.017) | (0.020) |
| Education | 0.131* | 0.067* | 0.031 | 0.108* | 0.083* | 0.035 |
| | (0.045) | (0.021) | (0.024) | (0.054) | (0.025) | (0.030) |
| Own Home | 0.038 | 0.012 | 0.021* | 0.032 | 0.016 | 0.027* |
| | (0.020) | (0.009) | (0.011) | (0.024) | (0.011) | (0.013) |
| Female | | 0.003 | 0.001 | -0.066* | 0.005 | 0.005 |
| | | (0.009) | (0.010) | (0.022) | (0.010) | (0.012) |
| Intercept | 0.230* | 0.590* | 0.634* | 0.224* | 0.630* | 0.725* |
| | (0.065) | (0.031) | (0.035) | (0.089) | (0.041) | (0.049) |

Table 6: Congressional Legitimacy Models