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. Because courts generally enjoy a rich store of diffuse support—so the logic goes—the public will punish incumbents who curb the judiciary. We critically examine this widespread assumption. Drawing upon a survey experiment, we demonstrate that the public sometimes punishes, *but also rewards* politicians who attack the judiciary. Moreover, we demonstrate that institutional legitimacy does not have the shielding effect for courts so often suggested. These results call into question a central assumption upon which foundational models of judicial power are based and therefore have broad implications for our understanding of public support of democratic institutions, institutional legitimacy and interbranch relations.

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Our understanding of the conditions under which mass publics will support liberal democratic institutions has been openly and aggressively challenged by recent events worldwide. From Donald Trump in the United States, to Evo Morales in Bolivia, to Rodrigo Duterte in the Philippines and Jair Bolsonaro in Brazil, democratic leaders have ascended to power only to 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.

Many models of interbranch relations suggest that elected leaders attack the courts at their own peril. That the public may punish incumbents electorally for attacks against high courts is a key assumption in several prominent theoretical models of comparative judicial independence and power (e.g., Rogers 2001; Vanberg 2000, 2001; Stephenson 2004; Staton 2006, 2010; Carrubba 2009; Helmke 2010*a*; Krehbiel 2016), and is widely interpreted as a logical—though empirically unscrutinized—outcome of the high levels of public support enjoyed by the U.S. Supreme Court (Caldeira and Gibson 1992; Gibson, Caldeira and Baird 1998; Gibson 2007; Gibson and Caldeira 2009; Gibson and Nelson 2015). Faced with the credible threat of public backlash for non-compliance or inter-branch assaults, conventional logic suggests, democratic incumbents should have no recourse but to respect the judiciary's institutional integrity and to comply with its decisions.[1] This existing body of knowledge, therefore, suggests swift electoral punishment for populist leaders who attack independent courts, especially in countries where public support for national courts is widespread and the judiciary is widely regarded as an independent branch of government.

Empirical and theoretical concerns suggest this conventional wisdom merits closer scrutiny. Empirically, the frequency of incumbent attacks on courts, even in consolidated democra-

[1]Caldeira & Gibson 1992 speculate on this logic: "the mass public may wield a check on the Court-curbing activities of issue-oriented activists and other opinion leaders. For, if the public at large accords the Court a high level of diffuse support, not conditioned on the specific decisions of the justices, then the issue-oriented elites might run some political risks if they press vigorously to Court-curbing measures."

cies, suggests that this assumption deserves additional consideration. There is ample observational evidence that the public supports judicial institutions in democracies the world over (Gibson, Caldeira and Baird 1998; Driscoll & Nelson 2018b, 2018c, 2019). Yet innumerable incumbents attempt to strip high courts of jurisdiction, to pack the courts with political lackeys, and to fundamentally undermine the separation of powers, ostensibly with the support of at least some part of the public (Staton 2004; Clark 2009; Helmke 2010*a*; Driscoll and Nelson 2015; Helmke 2017).

Theoretically, we rely on foundational findings in the areas of public opinion, voting behavior, and institutional accounts of judicial independence to articulate at least five reasons why this conventional wisdom deserves renewed scrutiny. First, the public's vote choices are powerfully shaped by partisanship, an element absent from this conventional wisdom (Campbell et al. 1960). Second, on many issues, the public takes its cues on issues from political elites, not vice-versa (Lenz 2012). This suggests that the public would be more prone to adopt the incumbent's position, not punish him for holding it. Third, because they are connected to the incumbent (but not the court) through the electoral connection, the public's interests may well be more aligned with the incumbent than the court (Gabel and Scheve 2007). Fourth, even if the public would seek to punish incumbents for interbranch attacks, individuals face considerable barriers to collective action, such that acting together to punish incumbents may prove impossible (Weingast 1997; Vanberg 2015). Finally, even if the public would not tolerate interbranch aggression, it is hard for them to sufficiently monitor incumbents' institutional transgressions due to polarization, a lack of transparency, or clarity of responsibility (Powell 2000; Vanberg 2001; Tavits 2007; Carrubba 2009; Svolik 2018; Carey et al. 2018). With these reasons in mind, we consider the possibility that incumbents might actually *benefit* from attacking the judiciary, or minimally have reasons to believe their threats to high court will go unpunished.

To summarize, conventional wisdom hinges on two related but empirically distinct conditions that implicate public support in the maintenance and expansion of judicial power (Weingast 1997; Vanberg 2015; Carey et al. 2018). First, the public must be *willing* to punish incumbents for their threats or attacks on courts, converting expressed support for the judiciary into withdrawn support for incumbents at the ballot box. Second, and conditional on said willingness existing, the public must be *able* to hold incumbents into account, an ability which implicates questions of democratic accountability, transparency, collective action problems and electoral democracy. In this paper, we set aside the questions of ability for future research, to scrutinize directly the question of public willingness. As we describe in later pages, our broader research agenda aims to address the second condition as well, and in tandem with the first.

This theoretical lacuna is compounded by a lack of empirical testing. We have scant evidence as to how (or if) citizens will punish elected leaders for attacks on courts. Nearly all research on public support for judicial institutions examines the United States Supreme Court (c.f. Walker 2016; Gibson, Caldeira and Baird 1998; Gibson 2007; Driscoll and Nelson 2018*c*), drawing on a well-vetted battery of survey questions that probe respondents' support for fundamental changes to high courts (Caldeira and Gibson 1992; Gibson, Caldeira and Spence 2003). While the study of public support for U.S. courts has seen a resurgence in recent years (Bartels and Johnston 2013; Armaly 2017; Salamone 2018; Gibson and Nelson 2018), most of this attention has sought to identify when and why the public finds judges and their decisions legitimate. Far less attention has been paid to testing empirically the *consequences* of this support, the concept of interest which is central to many theoretical accounts of judicial independence and power. As a result, we know much more than we did 10 years ago about the correlates of judicial legitimacy in the United States; yet, we do not know whether this support actually provokes or informs citizens' behavior at the ballot box. Our goal in this research is to determine the conditions under which citizens will stand up to an incumbent to protect a high court, and to seriously consider the possibility that incumbents stand to benefit electorally from attacks on national courts. Given the prevalence of judicial attacks worldwide, this is a question of both academic and popular interest.

To causally identify the effects of court curbing, we employ an experimental approach in which our respondents are randomly exposed to incumbent threats of court curbing. In this paper, we present one of the first experimental tests of the effects of court curbing on incumbent support. We find that, in many cases, citizens do punish incumbents for their attacks on national courts, a fact which is consistent with common theoretical assumptions.[2] Yet we report here that in a non-trivial number of situations, citizens openly *approve* of court curbing, and reward incumbents who propose court threatening attempts. This is especially true when the proposers are copartisans, and when they justify their actions in bureaucratic rationale. What is more, we find no evidence that a respondent's preexisting store of judicial legitimacy, as measured by the standard battery of diffuse support questions, motivates respondents to punish incumbents for putative attacks on courts. Instead, we find that those who express the highest levels of institutional fealty are actually more likely to *reward* copartisans for attempts at drastically changing the composition of the national courts.

The results both affirm and challenge conventional wisdom, emphasizing that not all court curbing attempts are popularly controversial. Indeed, in many instances, attacking independent courts may be a winning strategy for incumbents, especially when an elite has many copartisan supporters and frames the proposal in mundane bureaucratic terms. The results also underscore the importance of further (and ideally cross-national) testing. We

[2]As we explain in the conclusion, we have run these experiments in Bolivia, Germany, and Argentina, as well as similar experiments that differ in the identity of the aggressor, the type of attack and the justification given to motivate the proposal. These pilot results reveal interesting points of commonality and contrast cross-nationally. The experiment presented here was also included on the 2018 CCES (the results of which we await), and we are currently seeking funding to expand our study to two dozen countries worldwide.

therefore conclude this paper outlining our plan to expand this research agenda outside of the United States, examining how variation in democratic values, party polarization, clarity of responsibility, levels of democratic consolidation, and various country-level characteristics affect the costs and benefits of attacking the judiciary.

I The Conventional Wisdom About Attacking Courts

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). According to Legitimacy Theory, 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.

Despite the centrality of this mechanism for many theoretical models of judicial behavior, the electoral consequences of court curbing for incumbents have not been subject to empirical scrutiny. This U.S.-centric literature has largely taken for granted the behavioral implication Legitimacy Theory implies, considering only variation in a now known battery of survey questions which is thought to capture diffuse support (Caldeira and Gibson 1992; Bartels and Johnston 2013; Gibson and Nelson 2015; Salamone 2018). Not only does this fail to address the question of electoral punishment, this also means that much of what we know about the public's support for national courts is based on analyses of an institution which is anomalous for its high levels of public backing (Gibson, Caldeira and Baird 1998, Gibson 2007). Although a renaissance of this literature has revealed new explanations for when the public finds judges, courts and their decisions legitimate (Bartels and Johnston 2013; Gibson and Nelson 2015; Salamone 2018), this research has never (to our knowledge) connected these findings to the behavioral implication Legitimacy Theory would imply: a willingness to withdraw support for incumbents for their attacks on independent courts.

Comparativists, by contrast, have focused more on the credible threat of electoral punishment, often used as a facilitating condition for judicial power and independence. In 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).[3]

In other words, incumbents should face electoral backlash in response to their court curbing attempts, especially where the public holds its court in high esteem.

Although theoretically instructive, empirical evidence on this point is scant: a small minority of these studies provide systematic evidence that voters actually *punish* incum-

[3]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).

bents who harm courts.[4] Where evidence exists, it tends to come in the form of qualitative accounts of single cases.[5] 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

[4]To the contrary, just as often comparative research highlights incidents where incumbents abuse national courts with widespread public *support* (Staton 2004; Driscoll and Nelson 2015; Helmke 2017). Taken with high profile incidents where the public takes to the streets to *defend* judicial institutions (e.g. Germany 1952, Ecuador 2008 & 2011, and Poland 2018, to name a few) underscores the variance in public reaction to court curbing offenses.

[5]We have identified only five cross-sectional empirical attempts to document the causes and consequences of court curbing, only one of which takes seriously the role of public opinion. Helmke's (2010a, 2010b) work on inter-branch crises throughout the Americas demonstrates 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). Although Clark's (2009, 2010) model of court curbing explicitly casts attacks on courts as attempts by incumbents to rally a base of electoral support, his empirical analysis focuses on judicial reactions to court curbing proposals and does not speak to whether these proposals produce their intended electoral effects. Driscoll (2012) extended Clark's (2009) model to explain court curbing as a function of legislators' electoral incentives, analyzing the court curbing behavior of legislators from six Latin American countries. Leonard's (2016; 2017) work considers court curbing across the U.S. states, explaining the variance therein as a function of political considerations and judicial assertiveness. Finally, the *Varieties of Democracy* dataset provides the most comprehensive look into interbranch attacks perpetrated against courts in a large number of countries and over time, based on a battery of questions that records judicial impeachments, incumbent slander of judges and courts, court packing attempts and successes, among other things (Coppedge et. al 2017). Nevertheless, the lack of comparative data on public support for courts has hindered direct consideration of its protective or enabling effects.

(Helmke 2010*a*). Generally speaking, more systematic consideration of public support for national courts outside of the United States is stymied by a lack of requisite data (Staton 2010, 171; Helmke 2010b; Kapiszewski 2012; c.f. Gibson, Caldeira and Baird 1998; Walker 2016, Driscoll & Nelson 2018c).

Thus, many scholars seem to agree that court curbing attempts should be met with widespread disdain by the public, culminating in an electoral backlash at the next opportunity. However, the existing empirical evidence about court curbing and public support does not examine voters' responses to these proposals. Moreover, these studies suggest a problem of endogeneity: low public support may lead to court curbing attempts, but court curbing attempts might also lower institutional support. Accordingly, we have much more to understand.

II Questioning the Conventional Wisdom

We entertain the possibility of an alternative hypothesis, wherein incumbents may benefit from interbranch attacks, or minimally perceive limited costs to undermining courts. We see at least five reasons why attacks on independent courts are unlikely to be met with severe public backlash at the polls, three of which derive from the literature on public opinion and voting behavior and two of which stem from institutional accounts of judicial independence. Here, we outline each reason in turn.

First, the conventional wisdom relies on a conception of voter behavior which implies that attacks on courts inspire issue voting among the public. Under the conventional wisdom, voters cast ballots for or against an incumbent *because of their support for an entirely different branch of government*. Even if voters cast their ballots on the basis of issues (a disputed assumption, e.g., Achen and Bartels 2016), that institutional commitments outweigh policy issues, like taxes, health care, or the economy, or the pull of valuable heuristics like partisanship, does not comport with the accumulated evidence on voter behavior. Instead, voters often make decisions on the basis of heuristics like partisanship (Campbell et al. 1960), or when lacking the partisan heuristic base their decisions on candidates' characteristics thought to proxy for the partisan cue (Koch 2000). Indeed, new evidence from Mummolo, Peterson and Westwood (2018) suggests that partisanship is a particularly powerful heuristic on low salience issues, a category that includes issues of institutional integrity for many voters. Were this the case, voters should be likely to support court curbing proposals introduced by copartisan incumbents. Indeed, Clark and Kastellec's (2015) recent work suggests that the public is willing to accept some attacks on courts when they approve of the attacker (Clark and Kastellec 2015).

Second, the conventional wisdom suggests that voters have discrete views about institutional commitments that in turn constrains elite behavior, specifically assuming that a court's preexisting level of support will enable it to weather attacks: public support acts as as a shield. Again, much of the research on diffuse support for the U.S. Supreme Court has made these claims based on the now-standard battery of support questions, which implicitly assumes that citizens' professed institutional commitments would translate directly into electoral behavior, without ever explicitly examining that claim (e.g., Caldeira and Gibson 1992; Gibson, Caldeira and Baird 1998; Gibson 2007; Gibson and Caldeira 2009; Gibson and Nelson 2015). Yet new research suggests that public support for the judiciary and other political institutions may well be linked: institutional legitimacy of both the target institution and the aggressor are key parameters in Helmke's (2010, 2017) theoretical model of interbranch crises. Consistent with the "shield" analogy, she envisions the public support as inflicting a "legitimacy cost" on the attacking institution, empirically, she finds that higher public trust in the target institution correlates with less frequent attacks from other branches of government. Nelson & Gibson's (2019) research substantiates the dynamics spelled out in Helmke's model; they 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. Accordingly, it would seem that public support for the judicial branch of government cannot be extricated from other sorts of institutional support, much less support for specific incumbents, but rather ought to be considered in tandem with other forms of institutional commitment (Hibbing and Theiss-Morse 1995).

That voters will react negatively to court curbing attempts because of their preexisting issue position—support for the judiciary—contradicts a burgeoning body of scholarship. This evidence suggests that candidates' or elites issue positions tend to 'rub off' on the public more broadly.[6] 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. More recent evidence from the U.S. and abroad demonstrates that that voters often adopt the policy positions of their elected officials (Ladd and 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. Work by Armaly (2017), for example, suggests that this logic extends to matters of the court, showing Americans react more favorably to attacks on judicial independence when they come from a presidential candidate the voter feels warmly about. More pointedly, Nelson & Gibson (2019) 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.

These findings turn traditional notions of candidate position-taking—and the voter's

[6]Scholars of the U.S. context and abroad have long acknowledged that the opinion structure of the mass public differs from elites or "opinion" leaders (Murphy and Tanenhaus 1968; Adamany and Grossman 1983; Caldeira and Gibson 1992; Vanberg 2000).

reaction to it—completely on its head (Mayhew 1974; Clark 2009).[7] If elites or institutions 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(Caldeira and Gibson 1992). This interpretation is consistent with several models of judicial power which endogenize the effect of public support for courts (c.f. Helmke 2010b, 749). Staton (2006, 2010), for example, explains the expansion of judicial power with the assumption that judges behave strategically in order to shore up institutional support, suggesting the public can pick up cues from elites and other institutions (Caldeira 1987). Other models, such as Carrubba (2009) and Stephenson (2004) emphasize the role of elite opinions and actions in structuring the public's evaluation and possible backlash, a possibility that Easton also explicitly entertained (1965, 279-80). In other words, it seems possible that voters might come to support judicial institutions, or court curbing proposals because their favored incumbents also do.

Finally, and more fundamentally, the electoral connection provides another reason to expect the public to value their elected officials' opinions at the expense of an independent

[7] Clark construes court curbing as 'position-taking,' meant to rally a base of electoral support, a signal which is interpreted by judges as a sign of declining public esteem. Although he does not explicitly consider the possibility that the threats of attack might influence public opinion regarding the court, his work implies that the threats are made in an effort to convince the public of their commitment to a substantive issue.

judiciary.[8] Even if interinstitutional assaults did inspire issue voting and voters' positions on institutional integrity do not stem from elite actions, it is unclear why the public would punish an elected incumbent to defend the institutional integrity of an unelected court. 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). The electoral connection enables voters to select incumbents who are more closely aligned than their policy preferences than an unelected judiciary, and said electoral connection incentivizes incumbents' responsiveness to constituents.

In sum, it has been widely assumed that the public will punish incumbents for threats or attacks on courts, but it is surprisingly rarely been empirically identified. The U.S.-centric literatures have rarely connected public's expressed support to courts to the behavioral outcome the theory would imply, and a lack of cross-national data has meant that this dynamic has been largely unobserved and untested outside of the United States. We have strong theoretical reasons to question this logic; we now explain our experimental design to do so.

[8] We acknowledge here, and then intentionally set aside, structural conditions that may impede the appropriate functioning of the electoral connection in this regard. First, a public "consensus" regarding the appropriate bounds of constitutional rule is a central feature of Weingast's model of the rule of law (1997), but one which also requires collective action against a sovereign's transgressions to truly bind the hands of incumbent rulers (c.f. Przeworski 2003; Vanberg 2015). Likewise, even if the public would not tolerate interbranch aggression, polarization, or a lack of clarity of responsibility, may hinder the public's ability to punish incumbents' attacks (Vanberg 2001; Powell 2000; Tavits 2007; Svolik 2018). For example, Vanberg's (2000, 2001) work stresses the importance of a transparent electoral environment in which incumbents might be monitored; lacking information regarding incumbents' malfeasance, the threat of electoral retaliation is undermined. These are considerations we intend to explore in future research; Before we can attend to the second order question of when these public backlashes will be effective deterrents, we must first be convinced of the willingness of the public to punish the incumbent.

III Of Proposals and Proposers

We consider the possibility that not all court curbing attempts will provide the sort of electoral backlash widely expected by many models of judicial politics. Given in the reality that most members of the public have relatively little information about these proposals, we expect their evaluation of proposers and proposals to be strongly guided by simple heuristics. We focus our discussion on two of these heuristics. First, perhaps the public is more likely to reward or punish an incumbent for court curbing attacks based on the shared partisanship of the proposer. Second, we consider whether public reaction might be rooted in the justifications would-be reformers advance to qualify their proposal.

First, we expect that the most obvious characteristic of a proposer—her partisanship—will play an outsized role in voters' evaluations of a court curbing proposal. Indeed, because (a) voters are particularly likely to support elites from their own party (e.g. Campbell et al. 1960; Iyengar and Westwood 2015) (b) voters tend to adopt the positions taken by legislators they support (e.g. Lenz 2012), and (c) partisanship is a particularly powerful heuristic on low salience issues (Mummolo, Peterson and Westwood 2018), we expect that *proposals made by copartisans should be evaluated more favorably.*

We also expect the effects of copartisanship to bleed beyond attitudes toward the proposal and to also infect respondents' judgments of the proposer. 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.*

Second, voters' evaluations should vary based on how proposal is framed or justified (Staton 2004). While evidence that framing effects affect public opinion are widespread (Chong and Druckman 2007), the burgeoning literature on democratic decay suggests that these sorts of framing effects are particularly useful to those incumbents who seek to weaken democratic institutions. Varol (2015) emphasized "stealth authoritarianism" as a key mechanism behind successful attacks on democratic institutions. Ginsburg and Huq

(2018) describe a similar process ("constitutional regression") which they characterize as "incremental (but ultimately substantial) decay in [the] basic predicates of democracy" (83); these authors note that this process often results in the erosion or elimination of institutional checks and balances. Finally, and particularly on point, Levitsky and Ziblatt (2018) note that many attempts to attack democratic institutions are posed as "attempts to improve democracy" such that "[d]emocracy's erosion is, for many, almost imperceptible" (5-6). Thus, while citizens might otherwise reject a court curbing proposal and punish an incumbent for suggesting it, creative justifications on behalf of those who would seek to curb courts might make citizens unable to recognize court curbing proposals, and, by extension, unable to punish incumbents for proposing such curbs (e.g., Svolik 2018; Carey et al. 2018).

In particular, Levitsky and Ziblatt (2018) single out efforts to make the judiciary "more efficient" as a common stealth authoritarian technique to attack the judiciary successfully (5). Because "efficiency" tends to be a popular goal, antidemocratic proposals framed as efficiency-enhancing may garner support from the population, leading to the adoption of antidemocratic proposals that the public does not realize could have deleterious consequences. By contrast, proposals that expressly aim to exacerbate political cleavages are more likely to encounter popular resistance.

Likewise, existing evidence on the politicization of the judiciary provides clear guidance about how the justification for a court curbing proposal could affect public reaction to it. 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). Even outside of the judicial branch, 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 (and proposers) 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.*

Finally, the conventional wisdom suggests that the effects of any court curbing proposal should vary systematically with voters' preexisting view of legitimacy of the attacked institution. Recall Vanberg's (2015) statement: "Public support provides a shield for judicial independence" (177). In other words, attacks against courts should have the largest deleterious effects among those individuals who have the strongest preexisting judgments of support for the institution under attack. *By this view, proposals should be evaluated more negatively (and proposers punished more severely) among voters with stronger preexisting commitments to the attacked institution.*

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. Although this is a convenience sample, our consideration of the U.S. case is justified as a focal yet difficult case.[9] Not only is the public's support for the American judiciary the most empirically studied case of its kind, the overwhelmingly positive pub-

[9]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). Appendix A compares the demographics of our sample to those of other prominent investigations.

lic evaluation of the U.S. judicial hierarchy implies this should be a difficult test of our experiment (Gibson and Caldeira 2009).

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.[10] 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 read "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." Respondents who were assigned the politicized rationale read "Legal experts from both parties have discussed the Senator's proposal and agree that this proposal is an ideological attempt to stack the federal judiciary with like-minded judges." The two treatments were fully crossed. 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 in

[10] Although we do not describe the full results in the interest of space, we have fielded similar experiments that vary in the identity of the proposer (executive), the type of proposed aggressions (judicial impeachment and jurisdiction stripping), and the justifications given to motivate the proposals (liberal and majoritiarian democratic values), and the respondent sample (Bolivia, Germany and Argentina) (see Driscoll and Nelson 2018*a*). Though different in kind, the results are similar to those presented here, in that we find evidence that the public generally punishes incumbents for their attacks on courts, but there are important exceptions to this rule.

a hypothetical upcoming elections, assessed the proposer's job performance, and indicated their level of support for the proposal.[11] Because the court curbing proposal, while theoretically based on similar proposals percolating through the political world, was somewhat deceptive, the survey ended by debriefing the respondents about the experiment.[12]

Three 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; Bonneau and Cann 2015), 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. Where scholars have used an experimental approach to study interbranch relations, they mainly examine on support for the curb or for the court as the outcome variable (Clark and Kastellec 2015; Armaly 2017; Nelson and Gibson 2019). By contrast, our outcome variables directly evaluate the public's reaction vis-á-vis the incumbent, and 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).

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

[11]We acknowledge the possibility that the treatment effects we find are owing to the relative innocuous nature of the court curbing proposal: constitutionally speaking, it is Congressional prerogative to move to staff and change the composition of the federal judiciary, so it is possible the public simply viewed our vignette as such. That said, we find similar effects in our related experiments which contained more extreme versions of court curbing initiatives (Driscoll and Nelson 2018a).

[12]All experiments herein described have been cleared by the IRB at both Penn State and Florida State Universities.

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. President Trump and his Republican majority have touted the confirmation of a historic number of federal judges as a noted political success, and Republican voters have openly professed their support for the President and his legislative delegation as a result (Johnson 2019; Schaul and Uhrmacher 2018). 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.

A Outcome and Explanatory Variables

We have three 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.[13] 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

[13]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.

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 $r = .70$.[14] In the analyses we present, we have rescaled all of the variables to vary from 0 to 1 for ease of comparison.

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.[15] Befitting the high level of political knowledge typical of online convenience samples, the average respondent answered 3.9 of the 5 questions correctly.[16]

Finally, we were particularly interested in the ability of institutional support—legitimacy—

[14]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).

[15]Full question wording is available in Appendix B.

[16]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.

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)

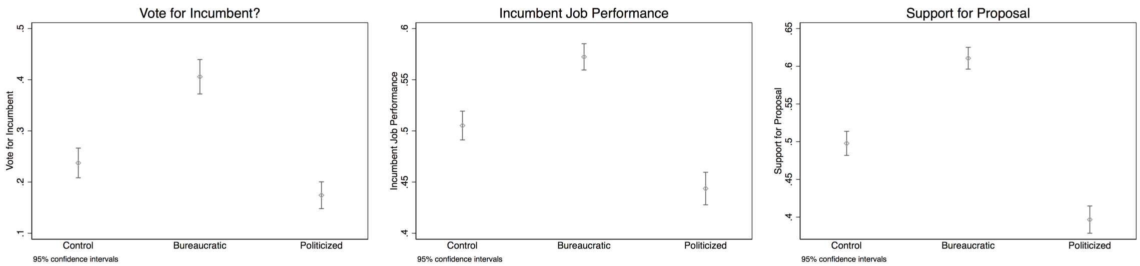
The three items are strongly reliable with $\alpha = 0.84$. Moreover, they scale on a single dimension with factor loadings of 0.72, 0.78, and 0.80. We therefore use as our measure of Federal Court Legitimacy the factor score from a unidimensional factor analysis. Scored from 0-1, the variable has a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

B 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. 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.

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 scaled 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.

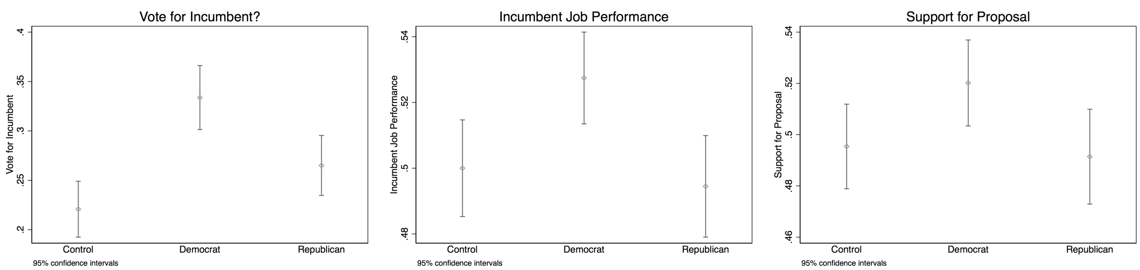
Figure 1 displays the average value of each of the three dependent variables across the three different rationale conditions. The conclusion from each panel of the figure is identical and unambiguous: compared to a control condition, respondents support incumbents and proposals that are rationalized for bureaucratic aims.[17] 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

[17] Because all respondents received a treatment, we cannot evaluate the counterfactual relative to a pure untreated group (c.f. Driscoll and Nelson 2018*a*). 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.

support.[18]

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.

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.

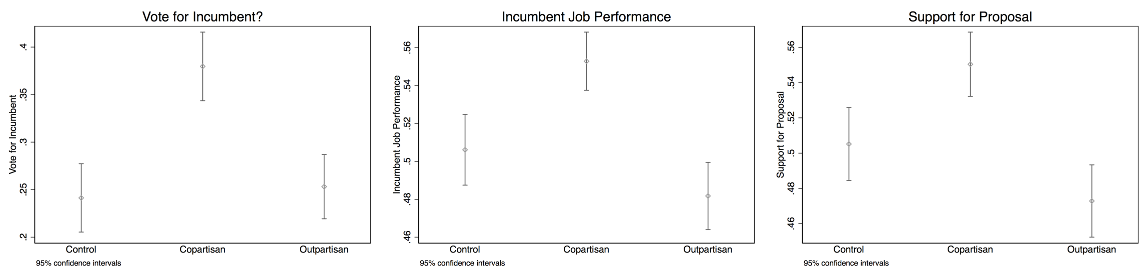
To begin to examine the effects of partisanship, Figure 2 plots average values of the dependent variables by the partisanship of the proposer. The results provide some evidence that the respondents support Democrats and their proposals, all else equal.

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

[18]This is consistent with data 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).

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.[19] 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.[20]

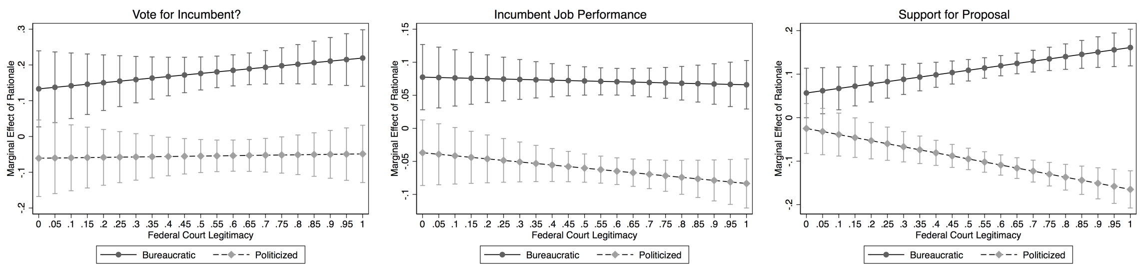
The Conditioning Effects of Legitimacy. We further probed the extent that preexisting views of legitimacy protect attacked institutions. To investigate this possibility, we estimated a series of models that interacted pretreatment judgments of federal court

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

[20]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 2) suggest exactly the same pattern displayed in Figure 3.

legitimacy with the treatments.[21] Both in the interest of parsimony and because of the lack of an interactive effect between the treatments, we consider the two sets of treatments separately. The full results of these models are found in Table 3 in the Appendix.

Figure 4: 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 3.

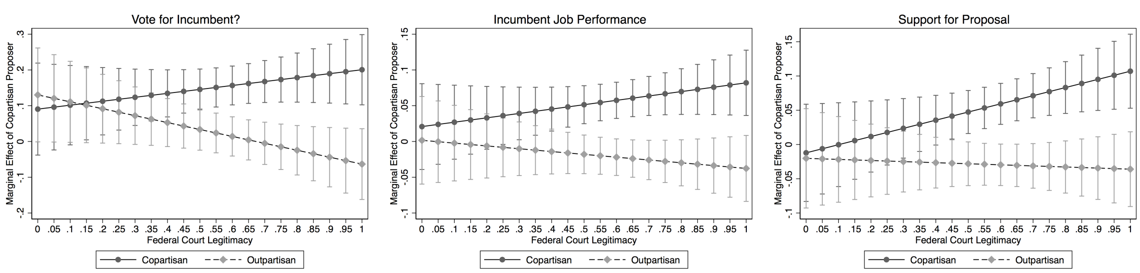
We begin by probing the protective effects of the federal judiciary's own legitimacy. Recall that the conventional wisdom expects that, as respondents' pretreatment beliefs that the judiciary is legitimate increase, voters should be increasingly willing to punish incumbents as a result of their proposal (Caldeira and Gibson 1992). Figure 4 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 result expected by the conventional wisdom: 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

[21]Building on the logic set out by Helmke (2010b, 2017), we also investigated the possibility that *congressional* legitimacy acts as a *sword*, supercharging the effectiveness of court curbing proposals; that investigation is detailed in Appendix D.

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 seek to pack the 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 5: 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 3.

Generally, these same conclusions hold when we examine the effects of copartisanship, as seen in Figure 5. 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 with respondents' partisanship.

V Discussion

The results of our experiment stand in strong contrast to assumptions that undergird prominent models of interbranch relations. The conventional wisdom we seek to engage hinges on two related assumptions regarding the public's support for judicial institutions: the public must be both willing and able to punish incumbents for their attempts to subvert the institutional separation of powers. In this research, we present one way in which we aim to evaluate the public's willingness to withdraw support from incumbents. The U.S. literature has long considered the standard battery of diffuse support questions as indicators of institutional legitimacy, but only rarely connected that measure to the behavioral implication Legitimacy Theory would imply. In so doing, we have shown that court curbing is not always costly, even among the American public where public support is widespread. Rather, when aggressors 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 (e.g., Driscoll 2012)—incumbent proposers 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 (e.g., Mummolo, Peterson and Westwood 2018). However, the United States is a particularly stark case in this regard; because its two major political parties are so polarized, it is easy for respondents to state which party they identify with and to determine whether or not the proposer aligns with their own party affiliation. In multiparty systems—especially where party polarization is less stark—these attributions might be more difficult for citizens to make, thereby decreasing both the likelihood of an electoral benefit or an electoral punishment. This is more than just speculation; we conducted versions of this experiment in Germany and Argentina in November 2018. The results for the partisanship treatment differ across countries, with the United States having the sharpest treatment effects. We are hopeful that we will eventually be able to field this experiment in more countries, probing how democratic consolidation (Weingast 1997), transparency (Vanberg 2001), clarity of responsibility (Powell 2000; Tavits 2007), and polarization (Svolik 2018) affect the magnitude of partisan punishment.

This finding is consistent with and a logical extension of the idea that instrumentalism—one's agreement with court decisions—has a potent effect on judicial legitimacy. While the conventional wisdom suggests that specific and diffuse support are only weakly correlated and only through repeated dissatisfaction might legitimacy eventually erode (Easton 1965; Gibson and Caldeira 1992), new studies suggest that support is more intertwined with satisfaction than previous research has appreciated (e.g., Bartels and Johnston 2013; Bartels, Johnston and Mark 2015; Christianson and Glick 2015a; Mondak 1992). Here, we find support for the notion that citizens' satisfaction with the aggressor also shapes their willingness to standup to defend a judicial institution (Helmke 2010b, 2017), and will exact less punishment when they are already predisposed to support the incumbent's proposals. In this way, we find some indirect evidence for these revisionist studies, perhaps suggesting that the increasingly polarized world of American politics today has altered the foundations of judicial legitimacy, increasing the extent to which it is built on an instrumental, rather than a values-based, foundation (c.f. Gibson and Nelson 2015, 2018).[22]

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 true beyond the United States. In both Germany and Argentina, the results for the justification treatment are broadly similar: proposals justified as bureaucratic tend to be supported; explicitly politicized proposals attract electoral rebuke. This is an unsung point for judicial reformers throughout the country and beyond.

The credibility of that rationale is one point about our experimental design that deserves additional discussion; the credibility of the threat is yet another. We crafted the vignette to eliminate as many concerns among respondents as possible as to the effect of the court curbing proposal, though we acknowledge that Americans' beliefs about the credibility of academics differ widely (Nelson and Gibson 2019).[23] 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. As

[22]This fact is particularly surprising given that both cultural and institutionalist approaches to democratic theory suggest that institutional support should be particularly divorced from instrumental concerns in consolidated democracies like the contetmporary United States (Weingast 1997; Easton 1965; Almond and Verba 1963; Carrubba 2009). Thus, as we continue cross-national testing, we hope to be particularly attune to the conditional effects of democratic consolidation on citizens' willingness to exact electoral retribution on incumbents in response to court curbing attempts.

[23]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.

to the credibility of the threat itself, we are encouraged by similar results in experiments where the aggressor is a unitary executive actor, although future research ought to contend with the possibility that the subjects view these vignettes as politically implausible or politically impossible. After all, member of Congress introducing a bill is not the same thing as a bill being passed, and empirical research into court curbing in the United States and elsewhere suggests that the vast majority of court curbing legislation ultimately goes nowhere (Driscoll 2012; Leonard 2016). Whether or not the public perceives this fact deserves additional future consideration.

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 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 to some degree 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. However, as we move beyond the United States, we are conscious that many national high courts are much less well-known than the United States Supreme Court; even if we inform respondents uniformly about a court curbing attempt, some proportion of respondents might still be unfamiliar with a country's high court. Understanding the effects of knowledge and awareness on the existence, magnitude, and direction of these effects is a high priority for us as we move forward.

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, in other, related work, we have analyzed a survey experiment that exposed respondents to a court curbing proposal from President Trump (Driscoll and Nelson 2018*a*). The results of that experiment demonstrate that President Trump is not punished for introducing a jurisdiction stripping bill; there is no difference in his support among those individuals who read a vignette about a court curbing proposal and those respondents who were assigned to a control condition without a vignette. Instead, among those respondents who identified themselves as Trump supporters, exposure to the treatment resulted in an *increase* in support for the President. The effect was sizable; the predicted probability of voting for Trump increased among those who received an experimental vignette about the President's threat to the Supreme Court, a 16-point increase over those in the control group. Among the majority of respondents who were not Trump supporters, exposure to the treatment continued to have no effect.[24]

We conclude our paper with a call for more research. Despite our results, we do not

[24]That vignette read:

> The U.S. Supreme Court is currently considering a case involving the limits of President Trump's powers under the U.S. Constitution. The decision has the potential to cut back on President Trump's ability to act swiftly without regard for the preferences of the legislature. President Trump recently threatened the Court, saying he would move to reduce the Court's powers to decide certain cases if the Court rules against the President. The public is divided about the court case.

The experiment contained two additional treatments that additionally provided a normative justification for the President's action at the end of the vignette. In the Liberal Democratic Values treatment, respondents are further told "Legal experts have discussed the President's actions, arguing that the courts and the legislature provide an important check on the President's power, and the President should respect the Court, even if he doesn't agree with its decisions." In the Majoritarian Democratic Values treatment condition, respondents are told "Legal experts have discussed the President's actions, arguing that courts should defer to the wishes of the majority, which is embodied in the President. Therefore, the President should resist the Court when he doesn't agree with its decisions." We defer analysis of these additional treatments for a later date.

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. For example, an open question we cannot address here, but which we intend to explore, is the extent to which individuals' legitimacy judgments are rooted in their support for liberal democratic institutions. This is an open plausibility—indeed, one widely theorized but not carefully tested (Gibson and Nelson 2015; Driscoll and Nelson 2018*a*)—to which future research ought attend. 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, citizens' values, and instrumentalism is more pressing than ever.

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VI 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	50% Col Grad	14.9 yrs	16.2 yrs	13.5 yrs
	29% Some Col	37% Some Col			

Table 1: 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)

C Full Model Results

	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 2: Multivariate Regression Results. The models are linear regressions. * indicates $p < .05$.

	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)

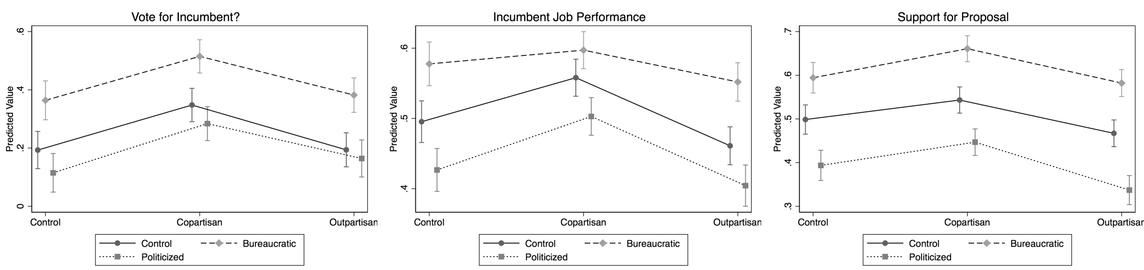
Table 3: Federal Court Legitimacy Models. The models are linear regressions. * indicates $p < .05$.

D The Interplay of Copartisanship and Rationale

In the body of the paper, we have saw 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. Full model results are provided in Table 4.

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.

Figure 6: 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 6 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.

	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. The models are linear regressions. * indicates $p < .05$.

E Congressional Legitimacy as a Sword

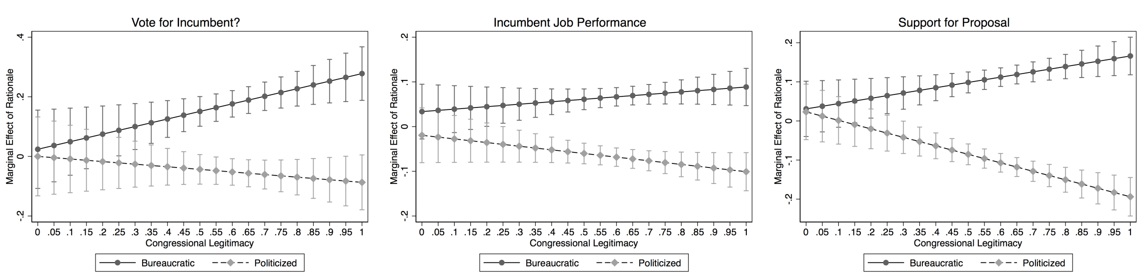
When the attacking institution is perceived as particularly legitimate, those high levels of public support might supercharge threats to other democratic institutions. Perhaps when a proposer's institution is imbued with institutional legitimacy, it is particularly effective. Were this the case, institutional legitimacy would therefore make threats that would otherwise be harmless quite potent. We address this possibility in this appendix.

This analysis is similar to the analysis of federal court legitimacy discussed in the body of the paper, relying on a pretreatment index of congressional legitimacy:[25]

- 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 $r = 0.67$.

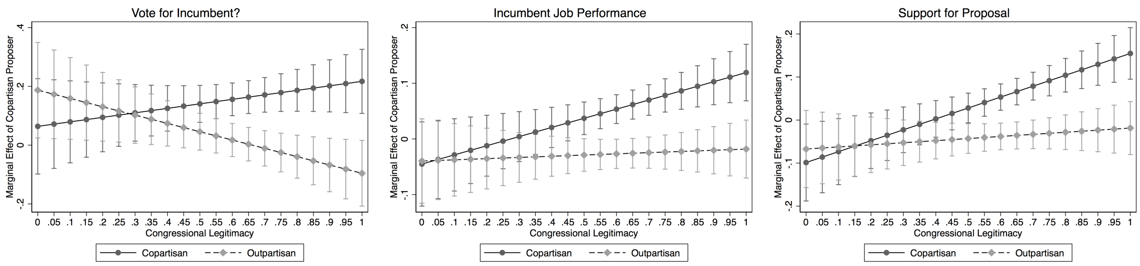
Figure 7: Marginal Effect of Rationale on Support for the Proposer and Proposal, by Congressional Legitimacy



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 5.

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

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 5.

We estimated linear models that interact this measure with our experimental treatments. Results are shown in Table 5. 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.

	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 5: Congressional Legitimacy Models. The models are linear regressions. * indicates $p < .05$.