

The Costs of Court Curbing: Evidence from the United States

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Canonical models of interbranch relations assume that incumbents undermine well-respected courts at their own peril. Although court-curbing proposals are frequent in diverse political and institutional contexts, there have been few efforts to examine the electoral costs of interbranch aggression. Drawing upon vignette and conjoint experiments, we find some evidence that the public will punish incumbents for attacks on courts. However, the size of the effect varies: it is largest among individuals who hold the court in high esteem and can be mitigated by copartisanship with the proposer. Moreover, once information about partisanship and issue positions is available to respondents, the effect of supporting court curbing is smaller than other considerations. These results have implications for the public's willingness to safeguard the institutional separation of powers via the electoral connection and suggest that politicians may engage in activities that erode democracy without a broad loss of public support.

Political incumbents around the world often seek to undermine the judiciary, proposing reforms that would reduce a court's ability to decide certain cases or punish individual judges for their votes in contentious cases. In the United States, 2020 Democratic presidential candidates proposed limiting the terms of high court judges; Senate Republicans have advanced proposals that would subject the justices to periodic retention elections (Geraghty 2015; Scherer 2019). Such proposals are not new. Famously, the Supreme Court upheld the constitutionality of New Deal programs only after President Roosevelt threatened to increase the number of justices on that court (Caldeira 1987).

The regular presence of these proposals in consolidated democracies contradicts a foundational assumption in many prominent theories of interbranch relations. Scholars have long suggested that, provided a court has a sufficient store of legitimacy, the public will impose costs—such as withholding

electoral support or mobilizing on the court's behalf—on incumbents who propose weakening the judiciary (Vanberg 2015). This credible threat of electoral retribution transforms the electoral connection into a proverbial “shield” that protects an otherwise institutionally vulnerable court. This logic is prevalent in many prominent explanations of comparative judicial independence, compliance, and power (e.g., Caldeira and Gibson 1992; Helmke 2010; Krehbiel 2016; Staton 2006; Vanberg 2001).

Unfortunately, empirical tests of legitimacy's shielding quality are scant. While scholars have gone to great lengths to identify the correlates of institutional support (Bartels and Johnston 2013; Caldeira and Gibson 1992), less attention has been paid to determining the conditions under which institutional support affects mass political behavior (Clark 2009; Helmke 2010; Krehbiel 2016). Accordingly, despite the widespread assertion that high levels of institutional legitimacy

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protect courts from attempts to weaken them, we have little empirical evidence to suggest that individuals who support courts will mobilize to defend the judiciary at the next election.

Indeed, there is good reason to suspect that institutional legitimacy provides a feeble shield to protect high courts against attacks by incumbents. Attacks on courts do not happen in a vacuum, and the same considerations that color political behavior in other settings—like partisanship and shared issue positions—are absent from this conventional account. Withholding electoral support for the sake of protecting institutional integrity might seem reasonable in the abstract; but, once partisanship enters the picture, a desire to protect the judiciary might not be enough to convince a voter to abandon a copartisan incumbent and throw her support to an outpartisan. Thus, in practice, the costs to curbing a well-regarded constitutional court may well be slim.

We evaluate the extent to which incumbents face costs for their efforts to undermine a high court. Our analysis relies on two survey experiments conducted in the United States. Because the US Supreme Court is widely regarded as one of the most legitimate courts in the world, it is a case where electoral punishment is likely to occur (Gibson 2007). At the same time, the United States' high levels of political polarization present a case where citizens face real policy costs to defecting from a copartisan incumbent to an outpartisan, providing the sort of real-world consequences to political behavior that our theory suggests will mitigate the costs of court curbing for incumbents. Further, we take a wide view of the potential costs of court curbing, examining both incumbent support and respondents' willingness to mobilize on behalf of (or against) a court-curbing effort.

Our results both corroborate and challenge existing accounts regarding the electoral costs of interbranch antagonism. On the one hand, we find some evidence that the public retaliates against incumbents' court-curbing efforts. This effect is strongest when partisanship is absent from citizens' voting calculus and among those who hold courts in the highest esteem. Further, we conclude that Americans do not give incumbents a pass for their court-curbing proposals simply because of shared partisanship; the size of these effects is similar for copartisan and outpartisan incumbents. On the other hand, once information about partisanship and issue positions enters the picture, the effect of court curbing on vote choice is smaller than those considerations. Further, among the two-thirds of respondents who have low to moderate levels of legitimacy, we find—across two experiments—no evidence of a court-curbing penalty relative to an incumbent who defends the structural integrity of the judiciary. When the baseline is an innocuous proposal, the size of the court-curbing penalty is five times greater for respondents whose preexisting support for the

judiciary is in the highest tercile compared to those in the lower two terciles.

Minimally, our results raise concerns about the efficacy of the electoral connection to shield high courts against the whims of ambitious incumbents who seek to undermine the independence and integrity of the judicial branch. Our research contributes to the accumulating evidence that the electoral connection is not an ironclad safeguard against incumbent activities that decay democracy (Graham and Svolik 2020; Grillo and Prato 2022; Levitsky and Ziblatt 2018). Although the US context provides a strong test of our theory, these results raise questions about how well the electoral connection protects judicial integrity across political and institutional contexts.

COURTS, PUBLIC SUPPORT, AND THE ELECTORAL CONNECTION

Democratic political institutions depend on public support. An important type of support is legitimacy, often manifest in an unwillingness to tolerate fundamental changes to institutions (Caldeira and Gibson 1992). When the public broadly views an institution as legitimate, attempts to undermine its authority or structure carry the risk of widespread resistance (e.g., Helmke 2010; Krehbiel 2016; Staton 2006; Vanberg 2000, 2001).

Prominent theories of judicial power and independence suggest that institutional legitimacy may provide a credible threat of electoral punishment. As Vanberg (2015) summarizes in a review article:¹

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. *The most common explanation* of this type stresses public support for independent courts as the critical factor. 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. If the integrity of the judiciary and respect for its decisions are values that a sufficient number of citizens are willing to defend by withholding 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. (176–77; emphasis added and citations omitted)

1. Elsewhere Vanberg (2001) discusses the public's support for the judiciary as a possible sufficient—but not necessary—condition for staying off interbranch conflict.

Thus, it is “common” to suggest that incumbents who attack legitimate courts face potential costs to court curbing. Provided these conditions hold—a threat to court integrity is sufficiently important to a sufficient number of people—incumbents will conclude that the possible costs outweigh the benefits of undermining a legitimate high court.

While theoretically instructive, empirical evidence on these costs is thin, and several prominent gaps in our understanding remain. First, the electoral mechanism implied by these accounts is essentially a claim that citizens decide to support or oppose a candidate because of their support for an entirely different branch of government. Missing from this account are other well-established determinants of mass behavior and vote choice, such as partisanship or issue positions. While support for an institution may well inform the public’s willingness to punish (or possibly reward) incumbents for attacks on courts (Clark 2009; Helmke and Rosenbluth 2009), we do not know how this support compares to other, possibly countervailing, determinants of mass behavior.

Second, the electoral punishment logic implicates two sorts of “sufficiency”: provided a court is sufficiently supported by a sufficient number of people, incumbents will be incentivized to forgo interbranch interference. Yet the empirical record is silent on this matter of “sufficiency” in either sense: (a) we do not know what level of institutional commitment is enough to spur pro-court actions by the public and (b) we do not know what level of aggregate support is necessary to provide a credible threat of incumbent removal which, in turn, might deter incumbents from making these attacks.

Indeed, while the research on individual-level public support for high courts is witnessing a renaissance, these studies often focus on the correlates of judicial legitimacy (e.g., Bartels and Johnston 2013; Christenson and Glick 2015) rather than the consequences of that support. And, even in the rare cases where scholars have used institutional support as an independent variable, researchers have tended to use institutional support to explain elite, rather than mass, political behavior (e.g., Clark 2009; Helmke 2010; Krehbiel 2016; Nelson and Uribe-McGuire 2017). In short, we do not know how, or if, the public’s professed commitment affects mass behavior, including their willingness to withhold support from court-curbing incumbents, defend a court’s integrity, or vote against an incumbent for their court-curbing stance.

Even with these gaps in our understanding, these accounts provide a clear baseline hypothesis for evaluation:

H1. There is a cost to court curbing: the public will be less supportive of incumbents who seek to undermine a high court, relative to incumbents who do not.

THE POTENTIAL COSTS OF COURT CURBING

Whatever penalty the public may impose in response to a court-curbing proposal, the presence of other weighty considerations, such as partisanship or shared issue positions, might complicate a voter’s calculus. That voters cast their ballots on the basis of issues at all is hotly disputed (e.g., Achen and Bartels 2016). Further, in the portfolio of issues that might influence mass behavior, questions of institutional integrity lack the salience attached to pocketbook concerns or hot-button social policies and positions. Evidence from Mummolo, Peterson, and Westwood (2019) suggests that partisanship is a particularly powerful cue for voters on low-salience issues, a category that includes issues of institutional integrity for many voters. In sum, that institutional commitments might outweigh salient 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.

Indeed, when faced with a copartisan incumbent who seeks to curb the court, voters face a potentially serious trade-off. Court reform proposals aside, a vote against a copartisan incumbent may well contribute to the electoral success of a candidate with whom a voter has sincere disagreements. Take, for example, the 2020 US presidential contest, where several Democratic candidates campaigned on promises of expanding the US Supreme Court or imposing term limits on its jurists (Scherer 2019).² Had one of those candidates been nominated, a vote to punish them for this position would have been tantamount to a de facto vote in favor of Donald Trump, a candidate with whom many Democratic voters had little in common. This trade-off is especially stark in polarized political environments where a vote against a copartisan is tantamount to a vote in favor of a basket of political positions the voter abhors (Graham and Svolik 2020; Levitsky and Ziblatt 2018).

And, previous research shows that citizens’ attitudes regarding courts’ decisions and these proposals of institutional reforms are colored considerations of partisanship. Nicholson and Hansford (2014) demonstrate that partisan cues shape the public’s willingness to accept controversial decisions, while Collins and Eshbaugh-Soha (2019) suggest that the US president—a highly partisan figure—can shape public opinion on low-salience issues that come before the US Supreme Court. As to the question of actual reforms, Clark and Kestellec (2015) present experimental evidence that the public is accepting of some attacks on courts, provided they approve of the attacker, while Armaly (2018) suggests that Americans judge attacks on courts more favorably when they come from a preferred

2. Indeed, that candidates campaign on these proposals suggests that they doubt these proposals carry serious electoral costs and may instead mobilize supporters.

presidential candidate. Most recently, Bartels and Johnston (2020) demonstrate that, even beyond simple partisan cue taking, the mere presence of partisan conflict over judicial decisions can increase support for court-curbing proposals. Accordingly, although none of these studies scrutinize support for those who support court curbing in the way we do here, these studies give us reason to expect that the partisan identity of the proposer will inform public support for the proposal and, potentially, the public's evaluation of the incumbent. With this in mind, we advance our second hypothesis:

H2. The cost of court curbing will be smaller for copartisans than either for outpartisans or incumbents for whom no partisan information is available.

Turning now to the question of sufficiency, we expect that the extent to which a citizen is willing to withhold support from an incumbent is tied to their preexisting level of institutional support. A core premise of the existing accounts, but one largely untested, is that citizens who are more expressly supportive of the Supreme Court—who score comparably higher on this metric of diffuse support—will be more apt to translate that institutional commitment into electoral action. The first notion of “sufficiency” we address is the notion that, across individuals, those with higher levels of support for the court will impose greater costs on incumbents who seek to undermine the high court. Put differently, beyond expressing abstract support for democratic ideals and institutional integrity, the public must act in a way that puts these abstract principles into practice. Thus:

H3. The cost of court curbing will increase with a citizen's professed institutional commitment.

The second question of sufficiency relates to the aggregate, rather than an individual, level. Just because a court has a high aggregate level of institutional legitimacy does not mean that a sufficient number of citizens will be willing to hold an incumbent to account for an attempt to weaken the judiciary. Even courts with high levels of legitimacy have low levels of support among a sizable number of citizens (Driscoll and Nelson 2018), and people disloyal to an institution are unlikely to punish an incumbent for attacking it. Moreover, even among a public that is largely loyal to the judiciary, voter coordination problems mitigate the threat of electoral retribution. For voters to successfully remove an incumbent from office (the ultimate “cost” citizens might impose) they must engage in coordinated action to abandon an incumbent many of them previously cast a vote for and to coalesce around a credible challenger (Vanberg 2001).

Thus, preexisting support by the public affects both the presence of court-curbing cost as well as the size of that cost: citizens who profess little institutional commitment will be unwilling to enact electoral retribution in response to a court-curbing proposal. But, because there is some individual-level threshold of support necessary to trigger a backlash to these proposals and many citizens are disloyal to even the most legitimate judiciaries, only a segment of the population—those holding the judiciary in high esteem—may be willing to punish an incumbent who tries to weaken the judiciary.

In addition to testing hypotheses regarding the effects of copartisanship and preexisting support, we advance beyond existing accounts by examining different types of electoral costs an incumbent might pay. A loss of voter support is the most obvious—and perhaps most politically consequential—cost an incumbent may face. But, especially in a polarized political system like the United States, abandoning one's party to vote for the opposite party carries with it substantial costs to a voter as well (Graham and Svobik 2020). A vote against a copartisan incumbent on the basis of institutional integrity is often a vote for an outpartisan candidate who supports a bundle of other issue positions the voter finds noxious. For this reason and others, vote choice might be a particularly obdurate electoral outcome (Coppock, Hill, and Vavreck 2020).

Voters might punish incumbents in other ways. After all, citizens evaluate incumbent job performance, mobilize on behalf of favored candidates, and can signal their support or discontent by signing petitions or attending protests. Each of these behaviors has the potential to impose costs on an incumbent's support. We therefore take a broad conception of costs, considering evaluations of the proposal, judgments of incumbent job performance, and the public's willingness to countermobilize against the incumbent.

This last potential cost deserves additional discussion. Citizens could countermobilize against an incumbent in two different ways. First, voters could engage in direct countermobilization, transferring support from the incumbent to her challenger to contribute directly to her electoral defeat. This direct countermobilization might occur when a voter dislikes an incumbent's stance on court reform and takes explicit action—such as voting in favor of or donating to the challenger—intended to increase the probability of the incumbent's defeat. Second, indirect countermobilization is also possible: voters may withhold their support from the incumbent while also choosing to not support the challenger. We expect this sort of behavior when the incumbent aligns with a voter's preferences on key issues or positions: a voter may dislike the incumbent's stance on court reform, but his antipathy does not rise to the level where he wants to contribute to the incumbent's defeat, perhaps because he agrees with the incumbent

on other issues. Instead, the voter's dissatisfaction demobilizes him from participating in the race; he may instead choose to direct his resources and attention to a different electoral contest. What unifies these possibilities is the voter's decision to deprive the incumbent of resources she would have received if she had not taken a pro-court-curbing stance. We discuss—and explicitly evaluate—these two possibilities below.

To summarize, previous research has suggested that incumbents who support court curbing will face electoral peril when they undermine a well-respected high court. We argue that the presence and magnitude of these costs varies with the partisanship of the proposer and the voter's preexisting support for the judiciary. In a realistic political environment characterized by partisanship and at least some degree of polarization, there is unlikely to be a sufficient number of citizens willing to extract a sizable electoral penalty against the incumbent for their efforts to undermine a high court (Graham and Svobik 2020). Finally, we look for these costs broadly conceived, including possible costs to job performance evaluations, mobilization-related behavior as well as final vote intention. We join a chorus of researchers who suggest that partisanship and preexisting support may color citizens' attitudes regarding high courts (e.g., Bartels and Johnston 2020; Clark and Kestel 2015; Nelson and Driscoll 2023); we advance beyond previous accounts by emphasizing the effects of these considerations on incumbents rather than on support for the judiciary or the proposals themselves.

RESEARCH DESIGN

We use vignette and conjoint experiments with complementary strengths and weaknesses to test our expectations. The vignette enables a straightforward test of the consequences of court reform. But, the design limits the information that respondents are given about the incumbent, abstracting away many real-world determinants of political behavior. The conjoint experiment, by contrast, enables us to benchmark the effects of court curbing against other candidate-specific information (such as issue positions and partisanship), that voters would realistically encounter during an election. At the same time, the conjoint design might obscure relationships that appear clearly in the simpler vignette.

We preregistered both study 1 (https://aspredicted.org/blind.php?x=MC9_VXY) and study 2 (<https://aspredicted.org/blind.php?x=8xv2qi>). An experimental approach is particularly attractive because it mitigates issues of endogeneity that may arise in observational studies: Clark (2009) suggests that declining levels of public legitimacy may spur incumbents to advance court-curbing proposals yet leaves open the possibility that the converse could also be true. With this sort of observational research, threats of endogeneity loom large when

examining these relationships. Because we assign respondents to the presence or absence of a court-curbing proposal without regard to their judgments of institutional legitimacy, we need not worry that we are observing the presence of court curbing in a way that is correlated with respondents' levels of institutional support. A concern with any experimental approach is demand effects, that respondents will guess the purpose of the experiment and answer questions accordingly. However, Mummolo and Peterson (2019) demonstrate that demand effects are unlikely in survey experiments. Further, to the extent that demand effects appeared, they would likely overstate the effects we estimate, effects that we argue are already small in size.

We justify our decision to study the United States in three ways. First, existing theory suggests that electoral punishment should be most prominent among established courts that enjoy a deep reservoir of goodwill (Vanberg 2015). The US Supreme Court passes this test with flying colors (Gibson, Caldeira, and Baird 1998). Second, previous research has underscored the importance of public awareness of high courts as a critical determinant of the public's ability to punish incumbents for institutional transgressions (Vanberg 2001). The United States enjoys a very high level of public education and relatively high levels of public awareness of the Supreme Court (Gibson et al. 1998). It is among the most consolidated democracies in the world. And, despite some indications of democratic backsliding in recent years, the United States scores high on available metrics of electoral transparency and electoral accountability (Coppedge et al. 2017). Third, because the United States is highly polarized at the time of our studies, there are real potential policy costs for voters who choose to support an outpartisan candidate rather than a copartisan, providing a political environment sufficient to assess our expectations about shared partisanship. In short, the United States enables us to examine a case where there are clear trade-offs for voters who choose to support democratic institutions by voting for an outpartisan candidate but where the pinnacle court generally enjoys broad political support.

STUDY 1: JURISDICTION STRIPPING VIGNETTE

In partnership with YouGov, we fielded a vignette experiment to a nationally representative sample of 2,000 Americans in August 2021. The 3×3 design varied the partisanship of a US Senate incumbent and his stance on a proposal of court reform as part of a short vignette.³ We modeled the incumbent's

3. We focus on proposals because we cannot randomize the presence of actual reforms and because court-curbing positions communicate a threat meant to motivate the electorate or communicate with the court (Clark 2009).

positions on a common court-curbing tactic: limiting a court's jurisdiction over salient social issues. We chose this policy because it is a relatively common method of court curbing in the United States, rather than the more extreme types of court curbing that are less likely to actually become law. Chutkan (2008) reports that Congress passed 378 provisions removing jurisdiction from the federal courts between 1943 and 2005, with the number of proposals passed per Congress increasing over time. These proposals often involve salient social policies, such as school busing during the civil rights era, prayer in schools in the 1980s, and the courts' ability to decide cases involving gay rights, climate change, and voting rights in recent years (Clark 2009; Gillman 2015; Yaffe-Bradley 2020).

In the vignette, the incumbent either took a pro-court-curbing stance ("announced his support for a plan to stop the Supreme Court from deciding cases dealing with controversial issues, like abortion, school prayer, and LGBT rights"), opposed that same plan (defending the Court's jurisdiction over the same issues as above), or simply issued a statement honoring the Court (a court-related action unrelated to judicial reform: "issued a statement honoring the important role the US Supreme Court has played in American government"). This design allows us to compare the effect of supporting jurisdiction stripping against two baselines: explicitly opposing that action or taking a noncurbing, but court-related, action.

Together, the two baselines help us bound the effects of court curbing. The Curb-Defend comparison enables us to compare directly the costs of court curbing between incumbents who have taken unambiguous positions on court reform, providing a direct (and stringent) estimate of the cost of supporting court reform. The Curb-Statement comparison allows us to quantify the effect of supporting court curbing relative to taking a pro-court position that does not implicate institutional change, a more generous baseline.⁴

The usefulness of the Curb-Statement comparison is its utility in estimating a sort of upper bound to the effect of supporting court curbing by comparing the effect of explicitly supporting court curbing to avoiding the issue. Yet it is less useful as an attempt to judge the real-world effects of court-curbing proposals on incumbents' fortunes. As the 2020 presidential campaign demonstrated, politicians often need to

take a position on court reform once the issue is on the table (Scherer 2019). And, if the issue ever comes to a vote, incumbents would vote either for a proposal (taking the Curb position) or against it (essentially taking the Defend position). While future studies should examine the potential strategic benefits of ambiguity, we judge the Curb-Defend comparison as the one that mirrors real-world conditions more closely and therefore use a form of it in both experiments.

Each proposal was attributed to either an incumbent whose partisanship was unstated, an Outpartisan, or a Copartisan. In determining copartisanship, we code leaners as partisans and exclude pure independents from the analysis; their copartisanship cannot be determined.

After reading the vignette, respondents indicated their approval of the proposal, approval of the incumbent, their willingness to mobilize on behalf of the incumbent, and their vote intention in an upcoming election. We report here the result of a composite, Incumbent Support measure derived from a unidimensional factor analysis of respondents' answers to those four items. The items form a reliable scale ($\alpha = 0.79$) and load onto a single dimension with an average factor loading of 0.70. We rescale the measure from 0 to 1; higher values indicate more support for the incumbent. In appendix A, we report the results for the individual outcome variables.

Direct Effects

Figure 1A illustrates the average value of Incumbent Support by partisan treatment. As baseline evidence that copartisanship informs the public's evaluations, we note that respondents evaluated copartisans more positively than outpartisans: Incumbent Support for a court-curbing copartisan (0.42) exceeds that of an outpartisan (0.19) or an incumbent whose party is not stated (0.29). This same ranking holds also when an incumbent defends against a proposal to strip the court of jurisdiction or issues a statement honoring the Court.

We aim to determine whether respondents withhold support from an incumbent for their efforts to curb the court. Figure 1B shows the average difference in Incumbent Support between incumbents who support court curbing and those who defend the court; figure 1C shows this difference with the statement as the baseline. Negative values of this difference indicate a cost for supporting court curbing relative to defending the court or issuing a statement.

The first point estimates in figures 1B and 1C enable us to test hypothesis 1: is there a cost to court curbing? There is. Relative to both defending that Court (-0.14) or issuing a statement (-0.19), there is a penalty for curbing the court when there are no countervailing influences, such as partisanship or other issue positions, on respondents' political

4. The difference between the Defend and Statement conditions, which we report in the appendix, is the difference between supporting the Court in (and out) of a court-reform context. In both conditions, incumbents "support" the Court, yet those who do so in the context of court reform are judged more harshly by respondents. As table A2 shows, across all partisanship conditions, respondents support incumbents who issue a statement over those who defend the Court, demonstrating potential electoral costs to simply weighing in on court reform.

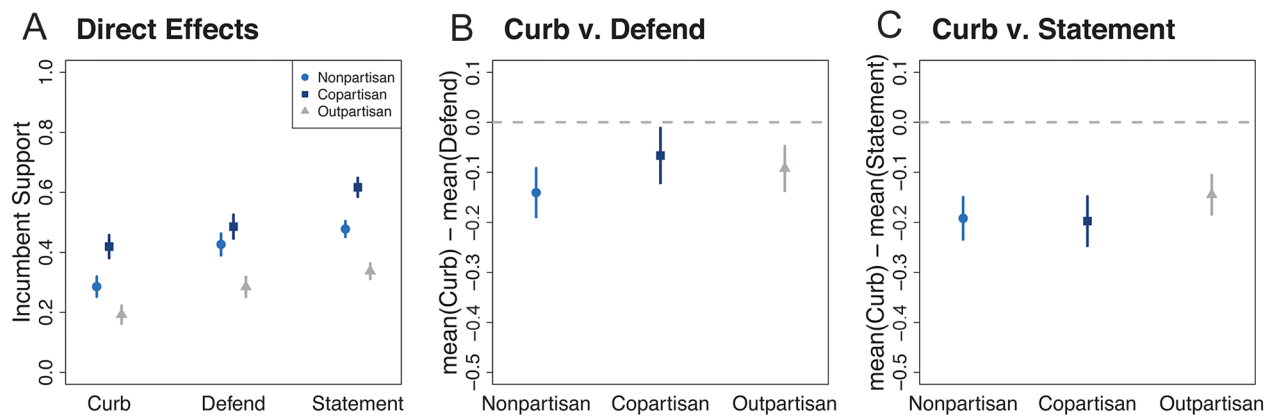


Figure 1. Direct effects and mean differences. In panel A, the point estimates represent the average value of Incumbent Support that is scaled on the 0–1 interval, with higher values indicating more support. Panels B and C display the average differences in stance conditions by each partisanship treatment. Negative values of this difference indicate a cost to supporting court curbing relative to defending the Court (panel B) or issuing a statement supporting the Court (panel C). The whiskers represent 95% confidence intervals.

calculus. These differences account for a change of about one-half of a standard deviation in the outcome.

There is mixed evidence regarding our second hypothesis, which proposes that the costs of court curbing should be weakest among copartisans. The remaining point estimates illustrate the average cost of court curbing for copartisan and outpartisan incumbents. Relative to defending the Court, there is a cost to court curbing for both copartisan ($p < .05$) and outpartisan incumbents ($p < .01$); there is no difference in the size of these two effects ($p = .47$). Compared to nonpartisan incumbents, however, copartisan incumbents are punished less severely ($p < .05$). Using the Statement as a baseline, there is also a cost to court curbing regardless of partisanship ($p < .01$ for both copartisan and outpartisan incumbents). Here, there is no difference in the cost felt by copartisans rather than outpartisans ($p = .10$) or between copartisan and nonpartisan incumbents ($p = .87$).

These results provide mixed evidence regarding hypothesis 2. In short, across both baselines, we observe minimal evidence that respondents engage in knee-jerk partisanship, blindly punishing outpartisans and rewarding copartisans. With the Defend baseline, respondents support copartisan incumbents at a lower level than nonpartisan incumbents; there are no partisanship-related differences with the Statement baseline. These results should provide some comfort to those who worry about the deleterious effects of partisanship for institutional reform efforts.

Preexisting support

Next, we turn to the question of sufficiency and our third hypothesis: we expect that the cost of court curbing will in-

crease with respondents' professed belief in the Supreme Court's legitimacy. Figure 2 plots the marginal effects of supporting court curbing as professed commitment to the institution increases. The solid black line and gray confidence band plots the standard, linear marginal effect; the point estimates and associated confidence intervals relax the linearity assumption, as suggested by Hainmueller, Mummolo, and Xu (2019), and plot the estimated marginal effect for each tercile of legitimacy. In appendix A, we show that these results hold when accounting for respondents' level of education, political interest, specific support, and demographic characteristics.

Figure 2 illustrates that, irrespective of the baseline, the cost of court curbing becomes increasingly large (and negative) in size as professed judicial legitimacy grows. For respondents with high levels of legitimacy, relative to the Defend treatment, the size of this effect is about -0.20 , an effect of about one-fifth of the range of the outcome variable; with the Statement condition as the baseline, this effect size doubles. When using the Hainmueller et al. binned estimator, the presence of this cost depends both on the respondent's preexisting level of legitimacy and the baseline of comparison. With the Defend treatment as the baseline, there is only evidence of this effect among about one-third of the public: there is no evidence of a penalty imposed by two-thirds of respondents. With the more generous baseline, the Statement, we observe a statistically significant cost to court curbing for all respondents. However, in both panels, the estimated cost to court curbing for those with low or middling levels of legitimacy is, at best, about 20% the size of the effect for those with the highest tercile of legitimacy. This evidence supports hypothesis 3.

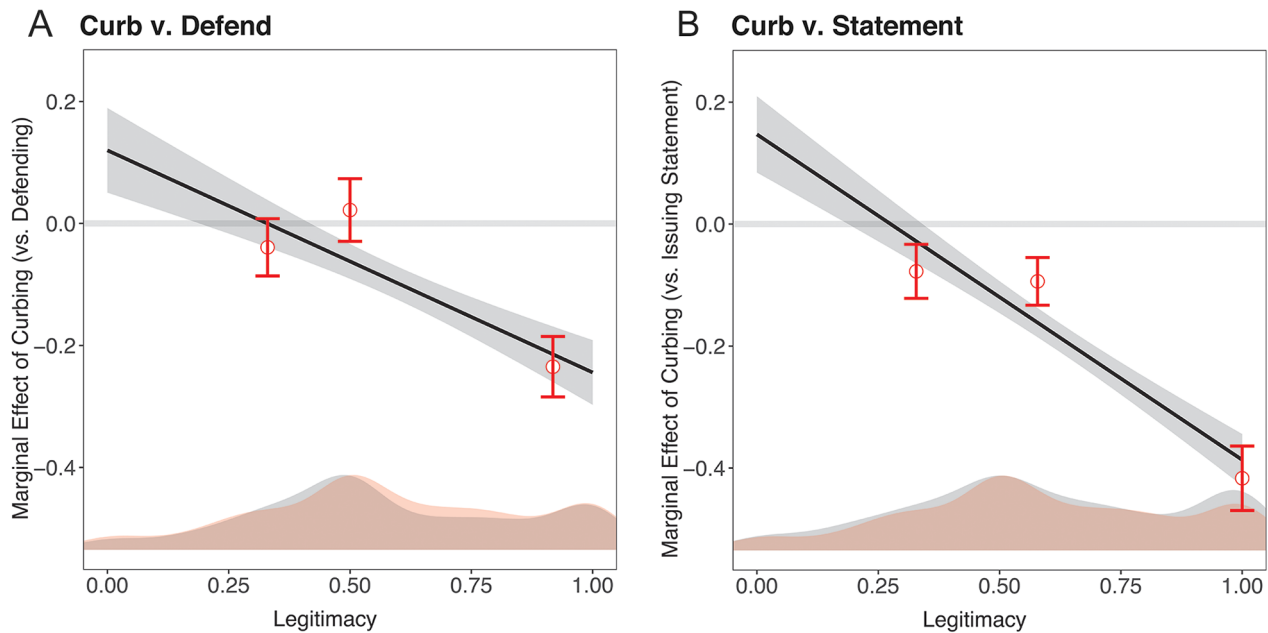


Figure 2. The conditional effect of court curbing. The marginal effect of curbing the court, as professed institutional legitimacy varies. The baseline in panel A is the Defend treatment; in panel B it is the Statement treatment. The solid line and associated 95% confidence interval is the traditional linear marginal effect; the point estimates and associated 95% confidence intervals are the Hainmueller et al. (2019) binned estimator, showing the estimated effect for each tercile of legitimacy. The density plot at the bottom of each panel shows the distribution of legitimacy for both those assigned to the Curb treatment and those assigned to the Defend treatment.

Mobilization and countermobilization

The costs of court curbing might extend beyond candidate support. The strength of our Incumbent Support outcome was its breadth, encompassing vote intention, mobilization efforts, and job approval; in this section, we train our focus on the effects of court curbing on (counter)mobilization. To this end, we asked respondents to divide \$1,000 in campaign donations between the incumbent, their challenger in the race from the other major party, or a different Senate race altogether.

We included this outcome for several reasons. First, because respondents have a fixed “budget,” we can assign a monetary cost to curbing the Court and compare the trade-offs across conditions. Second, by analyzing whether funds withheld from an incumbent are directed to either their challenger or a different Senate race, we can determine the conditions under which respondents countermobilize directly or indirectly against the incumbent.

The first row of figure 3 illustrates the average allocations of all respondents; the second and third columns report these average allocations for subsamples of respondents in the highest and lowest terciles of legitimacy (below 0.49 or above 0.74 on the 0–1 scale), following the binned estimator logic from above. The columns correspond to the three partisanship treatments. The medium color at the base of the bars represents the amount allocated to the incumbent, the darkest color

in the middle corresponds to the proportion allocated to the challenger, the lightest color at the top of the bars is the proportion diverted to a different Senate race. The average donation to the incumbent, challenger, and different Senate race was \$326, \$367, and \$306, respectively.

The top left panel of figure 3 shows allocation behavior among all respondents when they have no information about incumbent partisanship, enabling us to test hypothesis 1. Here, we observe a cost to court curbing: respondents allocated \$132 less on average to an incumbent who curbed a court than one who defends; this is a decrease of 36% in the size of the donation. At the same time, the donation to the incumbent’s other party challenger increases by nearly the identical amount: \$134. We see similar dynamics relative to the Statement baseline: a decrease of \$177 to the incumbent coupled with an increase of \$167 to the challenger. Thus, in the absence of partisanship, respondents withhold support for court-curbing incumbents and reallocate those funds to a challenger, evidence of direct countermobilization. There is a one-to-one relationship between the amount withdrawn from incumbents and that reallocated to the challenger. The out-of-state race does not see an increase, either statistically or substantively, in average allocations across either baseline. To reiterate, we observe direct countermobilization when no information about partisanship is available to respondents.

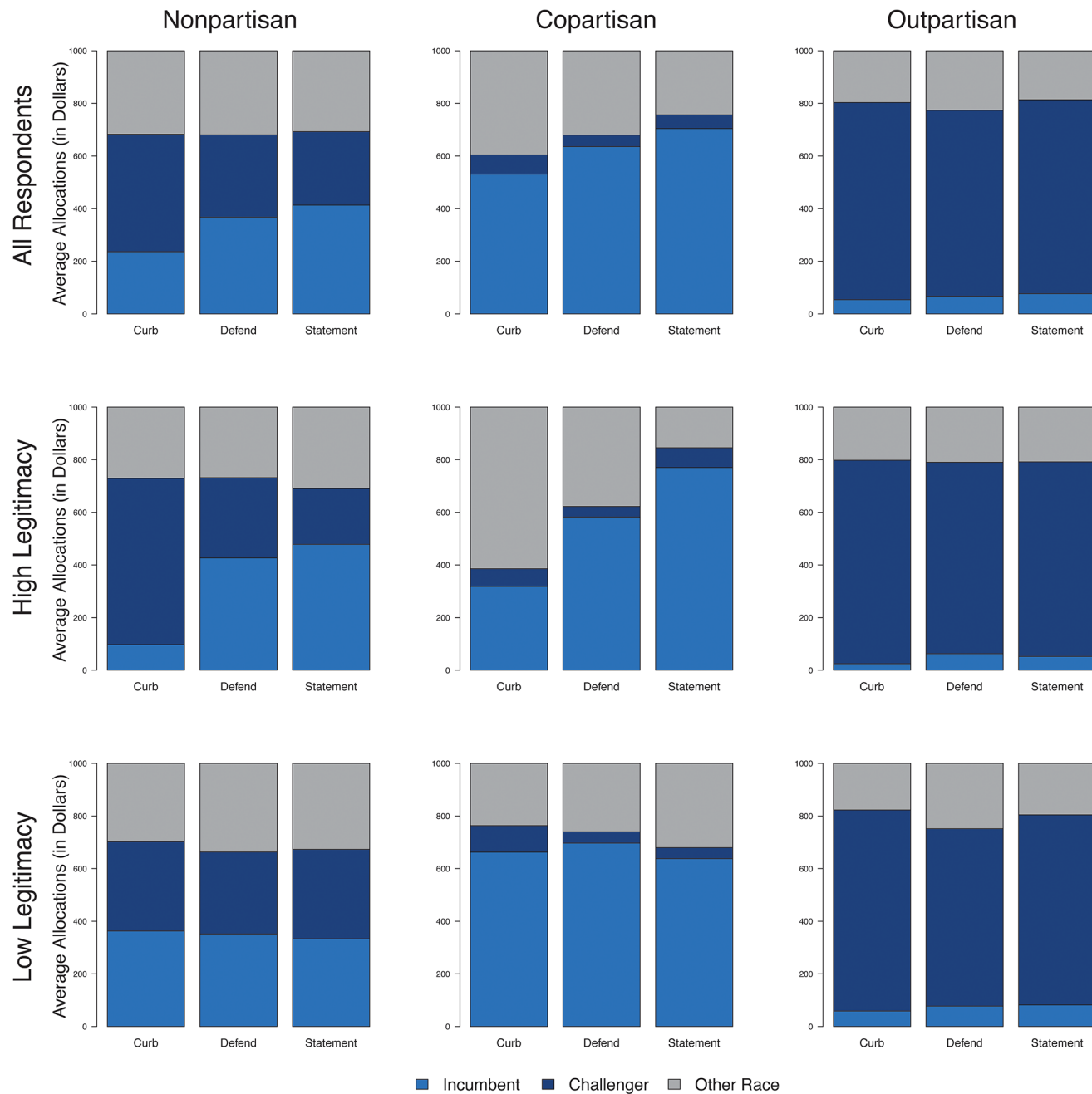


Figure 3. Average donation allocations. The figure shows the average allocations of \$1,000, by respondent, to the incumbent, that incumbent's challenger, and to a different US Senate race. The panels in the first row show the averages for all respondents; the second and third rows show the values for individuals with legitimacy scores in the highest and lowest third of the data.

Once partisanship is considered, allocation behavior changes dramatically, as seen in the middle and right-hand panels of the top row of figure 3. When presented with a copartisan incumbent who seeks to curb the court (relative to defending it), respondents withhold support from the incumbent, allocating \$104 less to the incumbent (a 16% decrease in their donation). But, only \$29 of these diverted funds, on average, are shifted to their challenger; instead most

of this difference (\$75) is reallocated to the out-of-state race. The dynamics are even more striking with the Statement as a baseline: a difference of \$173 allocated to the incumbent, with only \$21 of that money funneled to the challenger, compared to \$152 going to a different race. When the court-curbing incumbent is a copartisan, respondents countermobilize indirectly, redirecting their resources to the out-of-state race.

When we turn to outpartisan incumbents in the top right panel of figure 3, we observe no differences in allocation behavior according to the incumbent's court-curbing stance. The allocation to the outpartisan incumbent is so low in both conditions that there is no statistically significant decline in donations to outpartisan incumbents who curb the court nor is there a corresponding bump in donations to a copartisan challenger or the out-of-state race when the incumbent supports court curbing. Regardless of the baseline, none of the differences are statistically significant.

To summarize, citizens are willing to directly counter-mobilize against a court-curbing incumbent when partisanship is absent. When a copartisan supports court curbing, respondents punish the incumbent by modestly reducing the size of their donation; however, they redirect their donations to a different race, indirectly countermobilizing against the incumbent. And, we observe neither a cost to court curbing among outpartisan respondents—nor a benefit to their copartisan challengers—when an outpartisan supports reducing judicial jurisdiction.

In the remaining two rows of figure 3, we again consider the conditional effect of legitimacy. As before, we expect the cost of court curbing to be greater among individuals with higher levels of support for the Court. Beginning with the left-hand panel of the middle row (the high-legitimacy, nonpartisan condition), we see that respondents withdraw support from incumbents (a 77% decrease) who propose curbing (rather than defending) the Court and reallocate those funds to the challenger. This transfer of support is roughly one to one (a \$330 decrease for the incumbent and a \$326 increase to the challenger). As compared to the statement condition, we observe a \$381 decrease in donations to the incumbent and a \$420 increase to the challenger. Regardless of the baseline, there is no statistically significant change in the amount of funds sent to the other race. Thus, among these high-legitimacy respondents, we again observe direct countermobilization when partisanship is removed from the equation.

The central panel in figure 3 displays average allocations when high-legitimacy respondents are presented with a copartisan incumbent. Among high-legitimacy respondents, a copartisan court-curbing (rather than defending) incumbent inspires only indirect countermobilization. These respondents withdraw funds from the incumbent (\$264, a 45% decrease). However, rather than reallocating their donation to the challenger, nearly all of these funds (\$237) are diverted to another electoral contest. The same is true with the Statement baseline: a \$452 withdrawal in support for the incumbent with a \$460 increase in the amount sent out of state. The amount of money given to the challenger remains statistically indistinguishable across the two proposals. Strikingly, although high-legitimacy

respondents are willing to withdraw support for court-curbing copartisans, they are unwilling to mount a defense of the Court that would facilitate the removal of their copartisan from office. Instead, their countermobilization efforts are exclusively indirect.

Examining the allocations of high-legitimacy respondents when the incumbent is an outpartisan, we again observe no differences in respondents' allocation decisions (see the right-hand panel in the middle row). Perhaps surprisingly, there is also no benefit to a copartisan challenger when an outpartisan incumbent supports court curbing. Rather, allocations to the incumbent, challenger, and the outside Senate race remain static regardless of the outpartisan incumbent's court-curbing stance and the baseline condition.

Turning now to respondents with low levels of legitimacy (the bottom row in fig. 3), we observe neither type of countermobilization. Here, partisanship outweighs institutional considerations at every turn, regardless of the baseline. Examining a nonpartisan proposer (lower left panel), there is no difference in respondents' allocations according to the incumbent's position on court reform. In the remaining two panels, respondents contribute equally to incumbents regardless of their positions on court reform, and direct the majority of the donation to a copartisan incumbent.

Our foray into respondents' monetary allocations to candidates sheds important light on other costs of court curbing. In real-world-like situations where information candidate partisanship is available, we never observe respondents—even those who hold the Court in very high esteem—countermobilize directly against a copartisan who seeks to weaken a court. Instead, high-legitimacy respondents redirect their resources, neither assisting the copartisan's war chest nor directly contributing to their defeat. When the incumbent is an outpartisan, we find no evidence that the respondent's stance on court curbing has any effect on mobilization-related behavior. However, all of the effects we have discussed so far come from an environment in which other determinants of vote choice, such as the candidates' issue positions, are not provided to the respondents. Study 2 relaxes this constraint.

STUDY 2: CONJOINT EXPERIMENT

Our second experiment is a conjoint experiment embedded in a survey of 1,000 respondents on Mechanical Turk in November 2019. In the experiment, respondents were presented with two hypothetical candidates for office and asked to rate the profiles. These types of candidate selection conjoint have become common in political science (Hainmueller, Hangartner, and Yamamoto 2015; Hainmueller, Hopkins, and Yamamoto 2014). What makes our study unique is our use of partisanship,

policy positions, and court curbing to determine how voters make trade-offs among these three factors when selecting candidates. Because respondents in a conjoint experiment provide responses for multiple pairs of profiles, conjoint designs are particularly attractive because they allow researchers to probe respondent decision-making on far more dimensions than a single-shot vignette design allows.

Each respondent rated 15 pairs of profiles. In the first 10 pairs of profiles, one candidate was a Democrat and the other candidate was a Republican. For the final five profiles, we held constant the influence of partisanship by asking respondents to consider a primary election; for these five contests, both candidates were copartisans of the respondent.

In addition to partisanship, each pair of candidates took opposing positions on a series of issues. Although the Democratic and Republican parties have taken differing stances on most major issues in recent years, we attempted to choose issue positions we could imagine being credibly held by either a Democratic or Republican candidate. While rare, examples of such candidates exist: an anti-same-sex-marriage Democratic candidate, Dan Lapinski of Illinois, served in Congress until 2021; a handful of Democrats in the US House voted against expanded background checks for firearm owners in 2019, while eight Republicans voted in favor (HRC Staff 2018; Pramuk 2019). Respondents were asked about college tuition, Chinese tariffs, background checks for firearm purchases, and LGBT discrimination.

Each candidate took a pro- or anti-court-curbing stance. The wording of these treatments mirrored the Curb and Defend treatments from study 1. We incorporated five types of court curbing; one of which used the Curb and Defend treatments from study 1. This similarity provides a tight connection across the two experiments, enabling us to see how the size of the court-curbing costs fits into voters' constellation of electoral considerations.⁵ Appendix B reports the wording of the all of the treatments, disaggregated effects for different court-curbing proposals, alternative outcome variables, and sample characteristics.

To evaluate hypothesis 3, the ability of institutional support—legitimacy—to protect the Supreme Court against court-curbing attempts, we used a standard six-question diffuse support battery (Gibson, Caldeira, and Spence 2003). The scale is reliable: $\alpha = 0.88$; the items scale on a single dimension with factor loadings above 0.60; five of the six items load at 0.74 or higher. We use as our measure of Supreme Court Legitimacy the factor score from a unidimensional fac-

tor analysis. To make the analysis most similar to our discussion in study 1, we divide respondents into “high,” “moderate,” and “low” levels of legitimacy relative to the variable's terciles.

For ease of reporting, we present a simple outcome variable that centers the most electorally important cost an incumbent may face: respondents' binary decision about which of the two candidates they would be more likely to support. We follow the approach suggested by Hainmueller et al. (2014) and analyze the experiment by estimating the average marginal component effect (AMCE) of each of the attributes of the hypothetical candidate. The AMCE provides the marginal effect of each attribute over the joint distribution of the other included attributes, similar to estimating a regression with a suite of categorical variables. Indeed, Hainmueller et al. (2014) prove that estimated AMCEs are identical to the coefficients estimated from a multivariate linear regression. Thus, the estimated AMCEs are interpreted relative to an omitted baseline category. As is standard practice, we cluster our standard errors at the respondent level to account for the fact that each respondent rated multiple pairs of profiles.

To assess hypothesis 1—the overall effect of court curbing—we begin the analysis of the conjoint experiment by examining the unconditional AMCE estimates of each factor on the probability that a candidate was selected by the respondent in the primary election trials, where respondents were asked to consider two candidates who were from their political party, akin to a primary election (fig. 4). It is in these trials that the effects of partisanship are most attenuated, and we can examine the effects of court curbing when partisanship is held constant across the two candidates.

In figure 4, we observe a modest effect of court curbing, in line with hypothesis 1. Supporting court curbing is associated with a 6% reduction in the probability that the candidate was supported. In context, this effect size is roughly half of that of a candidate's position on background checks for gun purchases or protecting LGBT people from discrimination, both of which yielded considerable increases in candidate support.

What happens when partisanship is included as a factor alongside court-curbing positions? In figure 5, we display the results of the general election trials. Court-curbing candidates were, on average, penalized for their position: they were about 4% less likely to be chosen by respondents than those that did not. This effect is statistically differentiable from zero, yet substantively not very large, especially in light of the fact that a full 50% of open-seat congressional race victors win with more than 60% of the vote (Jacobson and Carson 2020). By contrast, candidates who proposed background checks for guns or who suggested that the government should protect LGBT people from discrimination were between 13% and 15% more likely to

5. The effect size across experiments is comparable: about a 10% reduction in support.

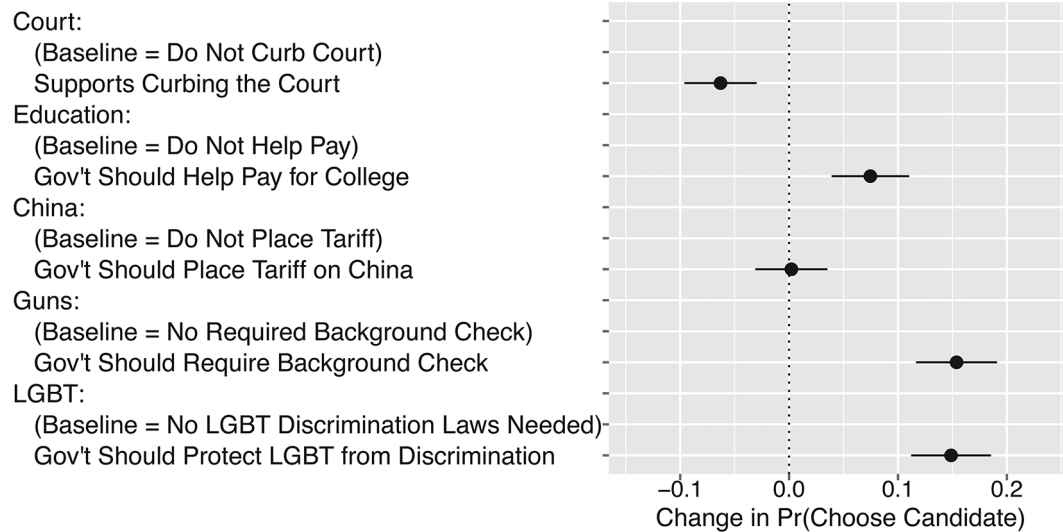


Figure 4. Conjoint results, primary elections. The point estimates represent the average marginal component effect (AMCE) from the conjoint experiment. The whiskers represent 95% confidence intervals.

be chosen. Further, this effect is much smaller than the effect of copartisanship. Copartisans are about 25% more likely to be chosen, meaning that the effect of court curbing is less than one-fifth the size of copartisanship. Thus, relative to other considerations, concerns regarding institutional integrity may not weigh heavily in voters' calculus.

Does the effect of court curbing vary according to copartisanship? To test hypothesis 2, we first examine the conditioning effects of partisanship by examining the percentage of profiles selected by respondents. Overall, respondents selected 61.75% of the copartisan profiles and 38.27% of the

outpartisan profiles. When a profile was both a respondent's copartisan and proposed court curbing, it was selected in 59.50% of trials; when a profile that was an outpartisan and proposed court curbing, it was selected 36.14% of the time. All told, this suggests that court curbing is associated with about a 2% decline in respondents' professed support for candidates, irrespective of shared partisanship. We formally tested this possibility by estimating the average component interaction effect for this pair of features; the effect is substantively and statistically insignificant ($ACIE = -0.001$, $p = .39$). This suggests that copartisanship does not condition the effect of

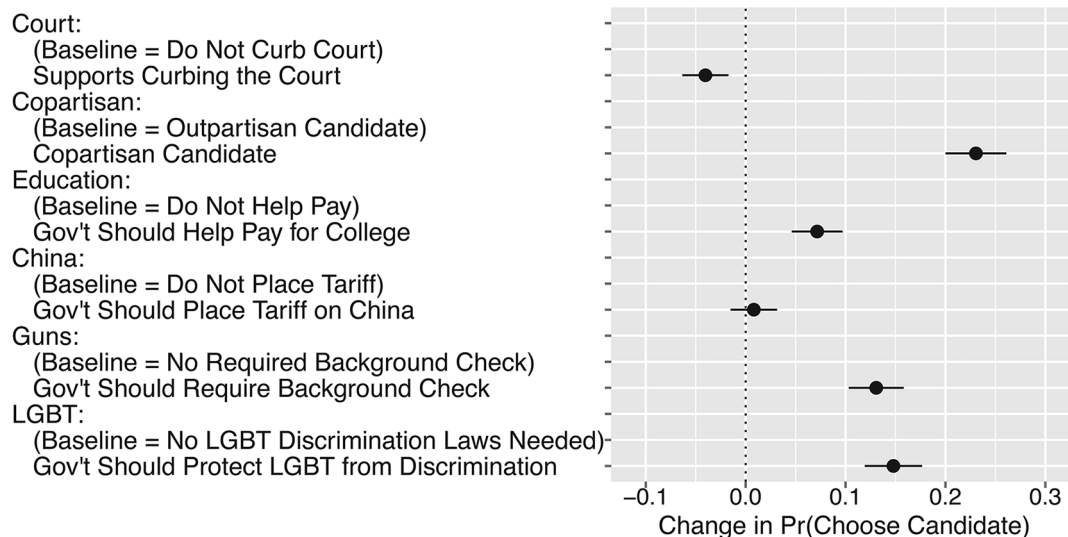


Figure 5. Conjoint results, general elections. The point estimates represent the average marginal component effect (AMCE) from the conjoint experiment. The whiskers represent 95% confidence intervals.

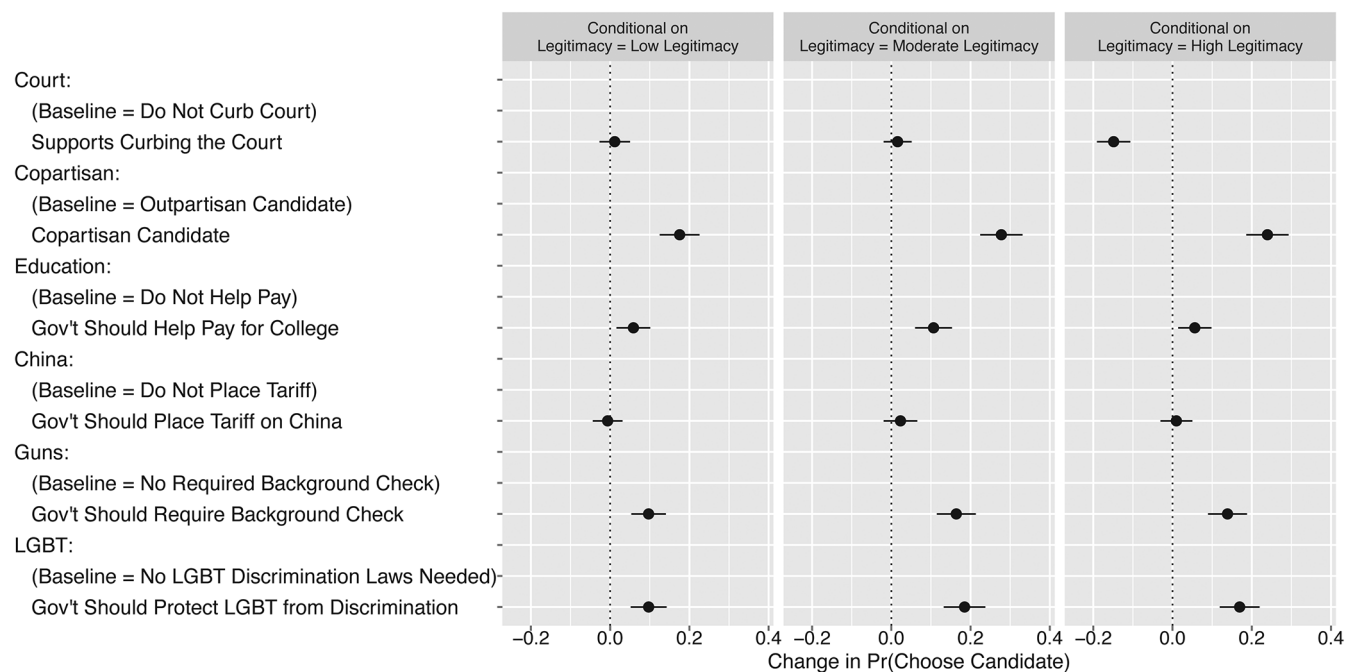


Figure 6. Conjoint results: general elections with legitimacy interaction. The point estimates represent the average marginal component effect (AMCE) from the conjoint experiment. The whiskers represent 95% confidence intervals.

court curbing. Instead, echoing findings for the Incumbent Support outcome in study 1, the small effect of court curbing is relatively constant for copartisans and outpartisans.

Finally, we turn to “sufficiency” and test the possible shielding effect of institutional legitimacy. Figure 6 displays conditional AMCE estimates, dividing respondents into “high,” “moderate,” and “low” legitimacy groupings based on the respondents’ professed institutional commitment relative to the sample’s terciles. These results mirror our conclusions from study 1. For respondents who hold the Court in high regard, the effect of court curbing is statistically significant and substantively large: a 15% reduction in the probability that the candidate is chosen. This effect is much smaller for respondents with low (1%) and moderate (2%) levels of legitimacy; in these cases, the estimated effect of court curbing is weakly positive, though not statistically distinguishable from zero. Thus, when respondents have high levels of Legitimacy, they display a willingness to enact a considerable electoral cost for a court-curbing incumbent; for low and moderate levels of Legitimacy (which, since we divide respondents at the terciles of Legitimacy, is two-thirds of respondents), the effects of court curbing are both statistically and substantively insignificant, mirroring exactly the Curb-Defend comparison from study 1.

DISCUSSION

Many accounts of interbranch relations hinge on the public’s willingness to punish politicians who attempt to un-

dermine or subvert judicial institutions. We evaluated whether the public withholds support for incumbents who seek to curb a high court, and we did so in an institutional context where this behavior is theorized to be most likely to occur: the US Supreme Court, which enjoys widespread and consistently high public support and has so for decades (Gibson et al. 1998; Nelson and Tucker 2021). Our research provides a novel look into a more fundamental question: whether professed institutional commitment informs voters’ incumbent evaluations, their willingness to mobilize on their behalf, and their willingness to punish (or possibly reward) incumbents for their attacks on the institutional integrity of the Supreme Court.

We find some support for the conventional accounts: across both experiments, we observe a cost imposed on incumbents for supporting court curbing: voters punish the incumbent by withdrawing their support. In some situations (e.g., where partisanship is absent, countervailing information on candidates’ issue positions is not available, and when citizens have high levels of professed institutional commitment), these costs can be considerable.

However, as we introduce more real-world considerations into respondents’ calculation, the presence and size of these costs diminish. For example, in the conjoint experiment, court curbing yields only about a 4% reduction in the likelihood of a vote for an incumbent, an effect smaller than those of partisanship or other salient issue positions. And, when we account

for pretreatment levels of institutional support, we see that the presence of this effect is concentrated among respondents who hold the court in very high esteem; in both experiments, court curbing (relative to defending the court) yields no effect among two-thirds of respondents; even with the generous Statement baseline in the vignette experiment, the size of the court-curbing cost is much lower for those low and middling levels of legitimacy than those who hold the Court in high esteem. Our research corroborates Helmke and Rosenbluth's (2009, 362) suggestion: "Even if the public has sufficient information about the role courts play and the threats they face, elections are a rather blunt instrument by which to secure judicial independence."

We employed an expanded conception of costs, emphasizing that a single-minded focus on vote choice risks overlooking other important ramifications of weakening democratic institutions. We were particularly interested in the extent to which the public would be willing to take actions that would increase the likelihood of the incumbent's removal from office (direct countermobilization). This is most directly seen in our analyses of campaign contribution (fig. 3). Here, we found limited evidence that court curbing inspires direct countermobilization. In our analyses, we only observe this behavior when partisanship is absent from respondents' calculations. Under more realistic conditions, respondents reduced donations to a copartisan incumbent but did not transfer those funds to the challenger in an attempt to aid in the incumbent's defeat. Instead, respondents diverted their contributions to another race, a behavior we termed indirect countermobilization. Continuing a theme seen throughout our analyses, these effects are driven by the respondents with the highest levels of preexisting institutional commitment.

These findings cast doubt on the extent to which the public imposes a cost to court curbing large and widespread enough to shield courts from ambitious politicians who see benefits from a weakened judiciary. Though we do observe some deleterious consequences associated with incumbents' support of court curbing, the effects are largest in the experimental contexts with the least external validity. The costs to incumbents for supporting court curbing diminish as more and more real-world considerations are available for voters' consideration. Moreover, because survey experiments may overestimate the size of treatment effects, our estimates might be considered upper bounds on the size of the court-curbing cost; the true, observationally observed effect of court curbing, once additional considerations are available to citizens, may be even smaller (Barabas and Jerit 2010).

Our findings regarding partisanship deserve special discussion to ensure that they are not overstated. Although the effects of partisanship on incumbent evaluations and vote

choice in studies 1 and 2 are scattered, we find that partisanship does structure respondents' mobilization behaviors. Consistent with hypothesis 2, we find that the cost of court curbing for copartisan incumbents is less than for nonpartisan incumbents (study 1, compared to incumbents who defend the Court), and respondents prefer copartisans to outpartisans in both studies. However, the differences between the size of the court-curbing costs paid by copartisan and outpartisan incumbents are not statistically distinguishable in either experiment. Copartisanship does not blindly intoxicate respondents' evaluations.

That we do not observe a decline in the cost of court curbing for copartisans relative to outpartisans in either study is perhaps reassuring in the face of concerns about democratic decay in the United States. This conclusion is in line with other research about democratic decay in the United States that demonstrates that the conditional effect of partisanship is negligible in these contexts (e.g., Carey et al. 2020). Americans, it seems, are not willing to give a "pass" to incumbents' attempts to undermine the institutional separation of powers due to shared partisanship.

We view our study as just one step in understanding these dynamics. One area that begs further investigation is the relationship between preexisting support, awareness, and electoral participation. We found repeatedly that the costs of engaging in court curbing are limited to and largest among respondents with the highest preexisting commitment to the judiciary. Because high-legitimacy citizens are likely to be politically sophisticated (Gibson and Nelson 2015) and are most likely to comprehend court-curbing proposals as efforts to undermine high courts. Political sophistication also predicts political participation, it may be the case that the costs of court curbing are limited but present among the respondents most likely to actually punish respondents. Further research should explore these dynamics (see Nelson and Driscoll 2023).

Finally, we acknowledge that the United States is a particularly stark case. 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 either an electoral benefit or an electoral retribution. This is ripe for further examination.

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