

# ELECTORAL CYCLES AMONG U.S. COURTS OF APPEALS JUDGES

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**Abstract** We find field evidence consistent with experimental studies that document the contexts and characteristics making individuals more susceptible to priming. Just before U.S. Presidential elections, judges on the U.S. Courts of Appeals double the rate at which they dissent and vote along partisan lines. Increases are accentuated for judges with less experience and in polarized environments. During periods of national unity—wartime, for example—judges suppress dissents, again, especially by judges with less experience and in polarized environments. We show the dissent rate increases gradually from 6% to nearly 12% in the quarter before an election and returns immediately to 6% after the election. If highly experienced professionals making common law precedent can be politically primed, it raises questions about the perceived impartiality of the judiciary.

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## 1 Introduction

Whether U.S. judges are biased is subject to much debate. The view of judges as impartial has been questioned by studies showing that demographic characteristics of judges predict their decisions on a range of legal issues (Peresie 2005; Sunstein et al. 2006). Politics, race, and gender appear to affect judicial decisions (Schanzenbach 2005; Bushway and Piehl 2001; Mustard 2001; Steffensmeier and Demuth 2000; Albonetti 1997; Klein et al. 1978; Humphrey and Fogarty 1987; Thomson and Zingraff 1981; Abrams et al. 2012; Boyd et al. 2010; Bonica and Woodruff 2015; Bonica et al. 2016; Shayo and Zussman 2011; Guthrie et al. 2000, 2007; Rachlinski et al. 2009b, 2013; Simon 2012). Whether these correlations reflect per se bias or differences in legal philosophy is an open question (Kornhauser 1999). For example, a judge may hew to a strict Constitutional interpretation on first principles, rather than choose the preferred outcomes of a political party or group (Akerlof and Kranton 2000). In one judge's estimate, only 5–15% of cases are legally indeterminate and, even in these difficult cases, judges understand which legal reasonings have greater plausibility (Edwards and Livermore 2008)—an argument against an interpretation of per se bias. We test if experienced judges (mean experience = 10.3 y, SD = 7.5) on the U.S. Courts of Appeals are swayed by the partisan environment of elections. Since previous findings of partisanship in judicial decisions could be due to judges' adherence to different legal philosophies, this present paper advances the literature by utilizing the stability of judges' legal philosophies over short time horizons to eliminate legal philosophy as the source of temporarily altered patterns in judges' concurrences and dissents.

Prior research suggests that priming can increase temporarily the accessibility of knowledge units in the memory of an individual, thus making it more likely that these knowledge units are used in the reception, interpretation and judgment of subsequent external information (Bargh and Chartrand 2000; Storms 1958; Higgins and Chaires 1980). An activated concept becomes more likely than before to influence conscious judgments. One study documents priming effects as long as one week after the initial stimulus (Tulving et al. 1982). The greater the quantity

or concentration of primes, the stronger is the overall priming effect (Srull and Wyer 1979). However, conscious processing, directed by an individual’s intentions and goals, can override the usual or habitual response to priming (Bargh and Chartrand 2000); indeed, experienced individuals are less prone to priming, while novices are more easily primed by news coverage (Krosnick and Kinder 1990). We investigate whether, nearing the U.S. Presidential election, judges become more likely to vote along partisan lines, disagree when sitting with judges appointed by the opposite party, and issue decisions reflecting partisan views. If decisions are affected, this would have permanent effects on the establishment of precedent (Gennaioli and Shleifer 2007; Baker and Mezzetti 2012).

## 2 Data

Our data consists of 18,686 judicial rulings, collected over 77 years, by the 12 U.S. Circuit Courts, also known as Courts of Appeals or Federal appellate courts. Each Circuit Court presides over 3–9 states. Our case sample consisted of petitions related to economic activity (50.9%), criminal law (26.9%), civil rights (8.6%), labor relations (7.2%), first amendment, due process, and privacy (2.8%), miscellaneous (2.8%), and unable to be ascertained (0.8%). The 12 U.S. Circuit Courts process all cases that are appealed from the U.S. District Courts.

Circuit judges are appointed for life by the U.S. President. Three judges, out of a pool of 8 to 40 judges in a Circuit, are randomly assigned by a staffing office to each case. For each year we obtained a random sample of roughly 5% of cases. The majority of decisions were unanimous (92%). Our database includes the legal variables that have been hand-coded by prior researchers: variables include litigant type, litigant strategy, how many appellants or respondents were persons, businesses, public interest groups, or government actors, whether there was an issue of constitutionality, whether the court engaged in statutory interpretation, whether the issue involved state or local law, an executive order or administrative regulation, summary judgment, alternative dispute resolution, conflict of laws, international law, or agency discretion. There are over one hundred coded characteristics.<sup>1</sup>

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<sup>1</sup>Documentation and data for cases available at <http://www.cas.sc.edu/poli/juri/appctdata.htm>. Biographical information for the judges in the database was obtained from the Multi-User Data Base

When judges appointed by Democrats and Republicans vote in different ways, the legalist interpretation is that they differ because they simply follow different legal philosophies rather than demonstrating bias. For instance, a judge can derive from first principles an adherence to a legal school of thought, while not necessarily hewing to the preferences of a political party for a certain policy outcome. A variety of professional norms and institutional mechanisms are designed to limit the influence of extrajudicial factors. Federal judges are restricted from any semblance of impropriety. Judges are prohibited from receiving honoraria for speeches, appearances, or articles and are prohibited from receiving compensation for their service to a for-profit or non-profit organization.<sup>2</sup> They are also prohibited from making speeches for political organizations, publicly endorsing or opposing candidates, soliciting funds, making contributions, or attending or purchasing tickets for events sponsored by political organizations or candidates.<sup>3</sup> They are further prohibited from personally participating in any fund-raising activities, soliciting funds for any organization, or using or permitting the use of the prestige of their judicial office for fund-raising purposes.<sup>4</sup>

The judges' decisions are classified into two categories, "affirm" and "reverse." On average, 57% of cases were affirmed. The panels' decisions can be 3-0 (unanimous) or 2-1 (dissent). A judge who disagrees with the verdict must write a dissent explaining why. The judges' opinions were also classified into three categories: liberal = 1, conservative = -1, and mixed or unable

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on the Attributes of U.S. Appeals Court Judges. Documentation and data for judges available at <http://www.cas.sc.edu/poli/juri/auburndata.htm>. Random assignment in Courts of Appeals has been examined in other work (Chen and Yeh 2016a,b; Chen et al. 2015). Case assignment in Circuit Courts fall into two categories: 1) Once a case arrives, three randomly chosen judges are assigned to the case; 2) Once a year, judges are randomly assigned to panels and each panel is assigned a date to hear cases. Then, when a case arrives, it gets assigned to the next panel. It is well established and has been thoroughly tested that both procedures are indeed random. For example, Chen and Sethi (2016) use data from Boyd et al. (2010) and Sunstein et al. (2006), who code 19 case characteristics as determined by the lower court for 415 gender-discrimination Circuit Court cases, and find that case characteristics are uncorrelated with judicial panel composition. Other papers examine whether the sequence of judges assigned to cases in each Circuit Court mimics a random process. They find, for example, that the string of judges assigned to cases is statistically indistinguishable from a random string. This paper's appendix report omnibus tests of whether case and litigant characteristics vary over 4-year cycles, and Chen (2016) does the same for the caseloads and characteristics of judges authoring or sitting on the panel.

<sup>2</sup> *Guide to Judiciary Policy* Canon 4H. <http://www.uscourts.gov/uscourts/rulesandpolicies/conduct/vol02a-ch02.pdf>

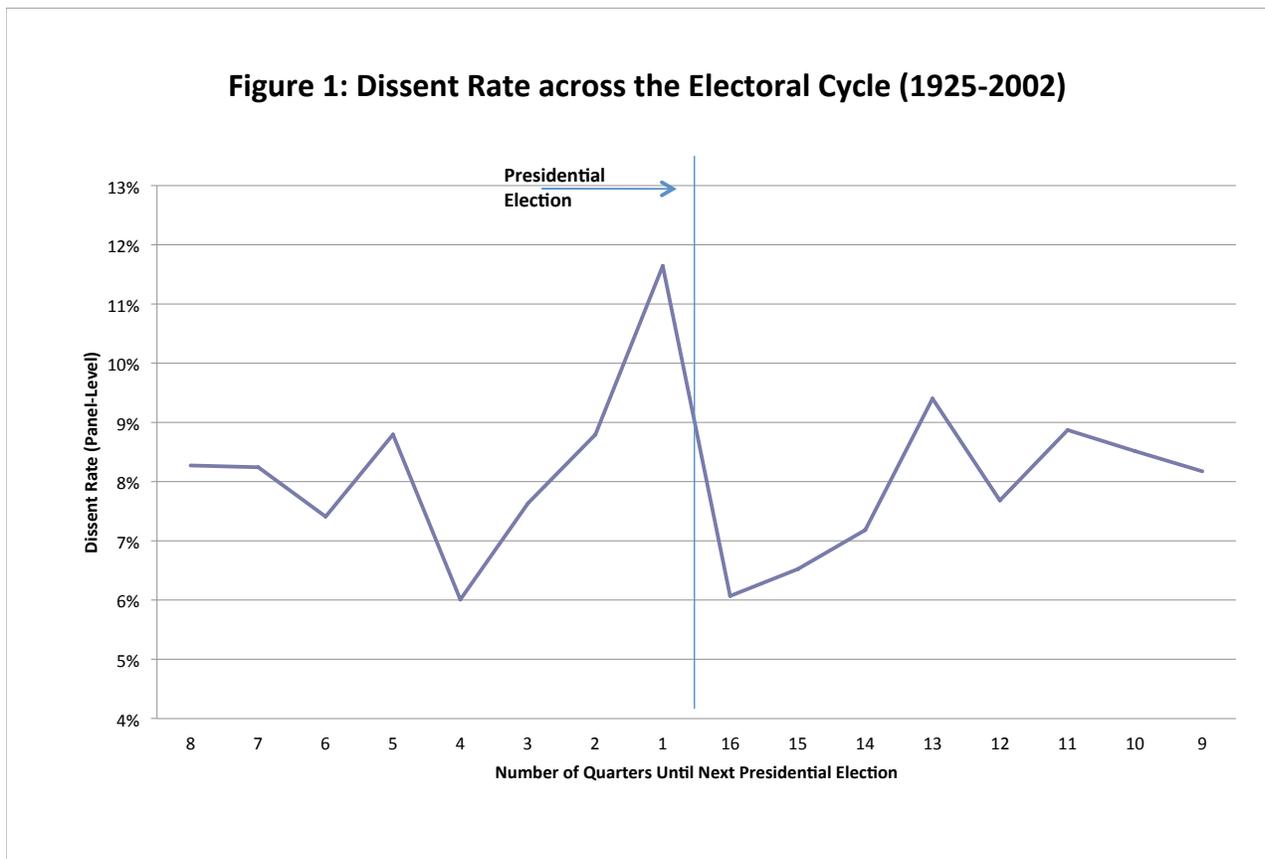
<sup>3</sup> *Guide to Judiciary Policy* Canon 5.

<sup>4</sup> *Guide to Judiciary Policy* Canon 4C.

to code = 0. For example, decisions supporting the position of the defendant in a criminal procedure case, the plaintiff who asserts a violation of her First Amendment rights, and the Secretary of Labor who sues a corporation for violation of child labor regulations are all coded as "liberal."<sup>5</sup>

### 3 Results

We find that the likelihood of a dissent is greater in the quarter preceding a Presidential election than after an election or in other quarters over the election cycle. This pattern is evident in Fig. 1, which graphs the proportion of dissents by quarter-to-election. The graph shows that the likelihood of a dissent spikes in the quarter before the election—the probability of a dissent steadily increases from 6% to nearly 12% and immediately returns back to 6% after the election.



<sup>5</sup>The Courts of Appeals Database Project states that for most, but not all issue categories, these will correspond to notions of "liberal" and "conservative" that are commonly used in the public law literature.

To account for the possible role of covariates in the patterns depicted in Fig. 1, we used a multivariate regression with dissent as the dependent variable and a legal-topic fixed effect to control for the idiosyncratic tendencies for dissent in each legal area, a calendar-quarter fixed effect to control for the tendencies for dissent that change by season, a year fixed effect to control for tendencies for dissent that change over time, Circuit Court fixed effect to control for tendencies for dissent that vary by Circuit, and a divided-panel fixed effect to control for the fact that dissents are more likely when judges appointed by Republicans and by Democrats sit together (Tab. 1).

We use the linear probability model (OLS) as our primary estimation method, and show that our results are robust to the use of probit models. There are two main reasons for this choice. The first is that our objective is to estimate the correlation coefficients rather than to develop a forecasting model of case outcomes, and OLS is superior for estimation purposes. And second, probit is not well-suited to the use of regressions with controls for fixed effects (e.g., dummies for quarter-to-election, legal topic, calendar-quarter, year, Circuit Court, and divided-panel) because of the incidental parameters problem (Angrist and Pischke 2008), and our analysis includes many controls for fixed effects.

The key predictors were indicators of a case’s temporal position: (i) dummy variables indicating the first three quarters before an election, included to examine how opinions immediately before an election differ from after an election; and (ii) dummies indicating whether Democratic and Republican appointees had been assigned on the same panel. To benchmark the findings, panels with judges appointed by Republicans and Democrats are 1.5 percentage points more likely to have a dissent relative to panels with judges appointed by only one party, but panels in the quarter before an election are 6.4 percentage points more likely to have a dissent relative to after an election. Thus the election effect is four times greater than the split panel effect.<sup>6</sup>

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<sup>6</sup>Chen (2016) reports that a linear model of proximity to an election would attribute 23% of dissents from unelected U.S. Courts of Appeals judges to the President’s electoral proximity. This suggests that—if

Table 1: Electoral Cycles in Dissents (2-1 Decision)

Mean of dep. var.	(1)	(2)
	0.079	
Divided (DRR or RDD)	0.0157*** (0.00452)	0.0154*** (0.00450)
Quartertoelect = 1	0.0637*** (0.0123)	0.0680*** (0.0135)
Quartertoelect = 2	0.0347*** (0.0121)	0.0341** (0.0145)
Quartertoelect = 3	0.0325*** (0.0123)	0.0343** (0.0133)
Quartertoelect = 4	0.00581 (0.0111)	0.00582 (0.0111)
Quartertoelect = 5	0.0209 (0.0152)	0.0251 (0.0159)
Quartertoelect = 6	0.0120 (0.0141)	0.0115 (0.0153)
Quartertoelect = 7	0.0226 (0.0141)	0.0238 (0.0153)
Quartertoelect = 8	0.00772 (0.0141)	0.00870 (0.0142)
Quartertoelect = 9	-0.0115 (0.0155)	-0.00718 (0.0157)
Quartertoelect = 10	-0.0114 (0.0160)	-0.0110 (0.0168)
Quartertoelect = 11	0.000311 (0.0162)	0.00269 (0.0167)
Quartertoelect = 12	-0.0102 (0.0128)	-0.00929 (0.0129)
Quartertoelect = 13	0.00115 (0.0148)	0.00451 (0.0151)
Quartertoelect = 14	-0.0157 (0.0134)	-0.0159 (0.0147)
Quartertoelect = 15	-0.0176 (0.0117)	-0.0154 (0.0121)
Year FE	Yes	Yes
Circuit FE	Yes	Yes
Season FE	No	Yes
Legal Issue FE	No	Yes
Observations	18686	18686
R-squared	0.019	0.021

Notes: Robust OLS standard errors clustered at the quarter-year level in parentheses (\*  $p < 0.10$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ). The omitted dummy variable indicating the number of quarters remaining before the presidential election is 16 quarters.

the estimate that 5–15% of cases are legally indeterminate is accurate (Edwards and Livermore 2008)—on average, all of them may be affected by elections.

The first three quarters before an election have coefficients that are positively signed and statistically significant, confirming that the pattern in Fig. 1 is robust to controlling for the legal attributes of the case. The results are extremely similar in analyses where we drop one Circuit at a time (Tab. S1). In addition, we rerun our basic specification with each quarter randomly assigned to a different quarter-to-election (a natural bootstrap with 200 draws); the 95% interval for t-statistics is between positive and negative 2.62. Fig. S1 shows that our true t-statistic of 4.01 lies far to the right of all the other simulated t-statistics. Several other simulated t-statistics are close to the true t-statistic, but this is to be expected since the second and third quarter before an election also display significant increases in dissents. Fig. S1 also displays the t-statistics for changes in the quarter before Presidential elections for over one hundred case and litigant characteristics. We find no increase or decrease before Presidential elections along these dimensions. We analyzed another statistical model that simply includes the linear trend that is apparent before elections in Fig. 1; regardless of the measure of electoral proximity we used, the trend was negative and significant and the results are nearly identical with probit estimates (Tab. S1).<sup>7</sup>

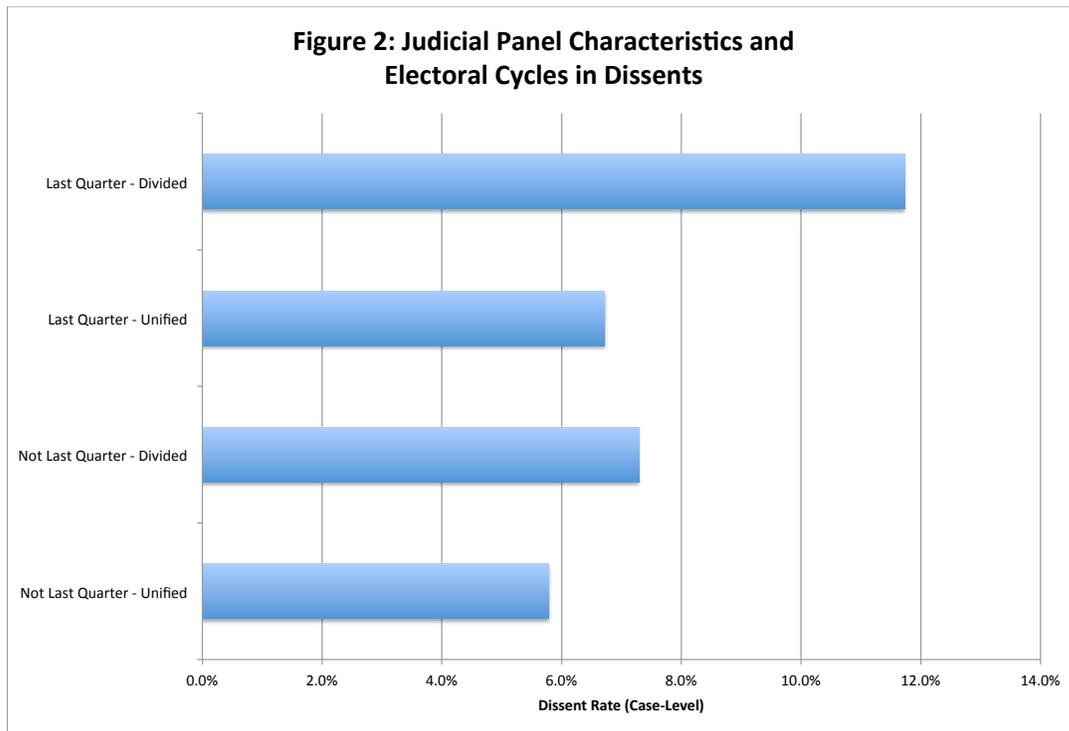
A separate paper by one of the authors replicates the electoral cycles in judicial dissents, at the monthly level, for the universe of 293,868 cases coded for dissents from 1950 to 2007 (Chen 2016). Unlike this paper, a noticeable increase in dissents also appears in the 9th quarter before a Presidential election; this time period is during the midterm elections, when all U.S. House and one third of U.S. Senate seats are up for election. The 5% sample used for this paper may be too small to observe significant midterm effects in dissents.<sup>8</sup>

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<sup>7</sup>The following robustness checks are reported in Chen (2016): 1) shifting seasonality controls by one month (January through March, etc.); 2) including dummy indicators for each type of panel composition (DDD, DDR, RRD, RRR); 3) controlling for the presence of a concurrence, which also display electoral cycles; 4) clustering standard errors at the Circuit level.

<sup>8</sup>Another replication by one of the authors uses machine learning to predict agreement between judges (Chen et al. 2016a,f). Random forest achieves the best classification and it shows that electoral proximity is one of the most important features predicting dissent. Moreover, consistent with the role of identity, dissent is roughly half-driven by shared biographical features of judges. Thus, while only a small portion of dissent is explained by political factors (Tabs. 1 and 2), electoral proximity and identity are both important in predicting dissent.

Next, we examine heterogeneity. In particular, we can examine whether the increase in dissents is larger for panels with judges appointed by Republicans and Democrats. Fig. 2 reports group means. A large proportion of the increase in dissents comes from ideologically divided panels. For unified panels, the dissent rate is 5.8% increasing to 6.7% before Presidential elections, while for divided panels, the dissent vote rate is 7.3% increasing to 11.7%.<sup>9</sup>



We next analyzed judges' vote ideology, specifically, whether judges appointed by Democrats

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<sup>9</sup>Several statistical tests for significant differences across groups are presented Chen (2016). The results of Fig. 2 are significantly different for divided panels. Behavioral changes are three times greater in close elections, non-existent in landslide elections, and reversed in wartime elections. Increases in dissents before elections are twice as large in the time period since the 1970s—a magnitude that is consistent with the increase in polarization found in studies of the U.S. Congress. Dissents are also elevated in swing states and in states that count heavily to winning the election, when these states are competitive.

were more likely to cast a liberal vote and judges appointed by Republicans more likely to cast a conservative vote. Vote ideology measures a behavior different from dissents. For example, if legal precedent dictates a liberal decision, a unified panel appointed by Republicans should cast a liberal vote. Before a Presidential election, however, such a panel may actually cast a conservative vote instead. There would be no dissent observed, but an alignment between the decision and the judges' party of appointment would be observed.

We observe that the ideological difference between Democratic appointees and Republican appointees doubles in magnitude in the quarter before an election (Tab. 2 Panel A). The positive coefficient on the dummy indicator for whether the judge was appointed by a Democrat indicates that Democratic appointees typically cast more liberal votes than Republican appointees. When the outcome measure is coded as liberal vs. not liberal, the interpretation of the regression coefficient is that Democratic appointees are 3.5 percentage points more likely than Republican appointees to cast a liberal vote relative to a neutral or conservative vote, but this difference increases by another 3.9 percentage points before the election.

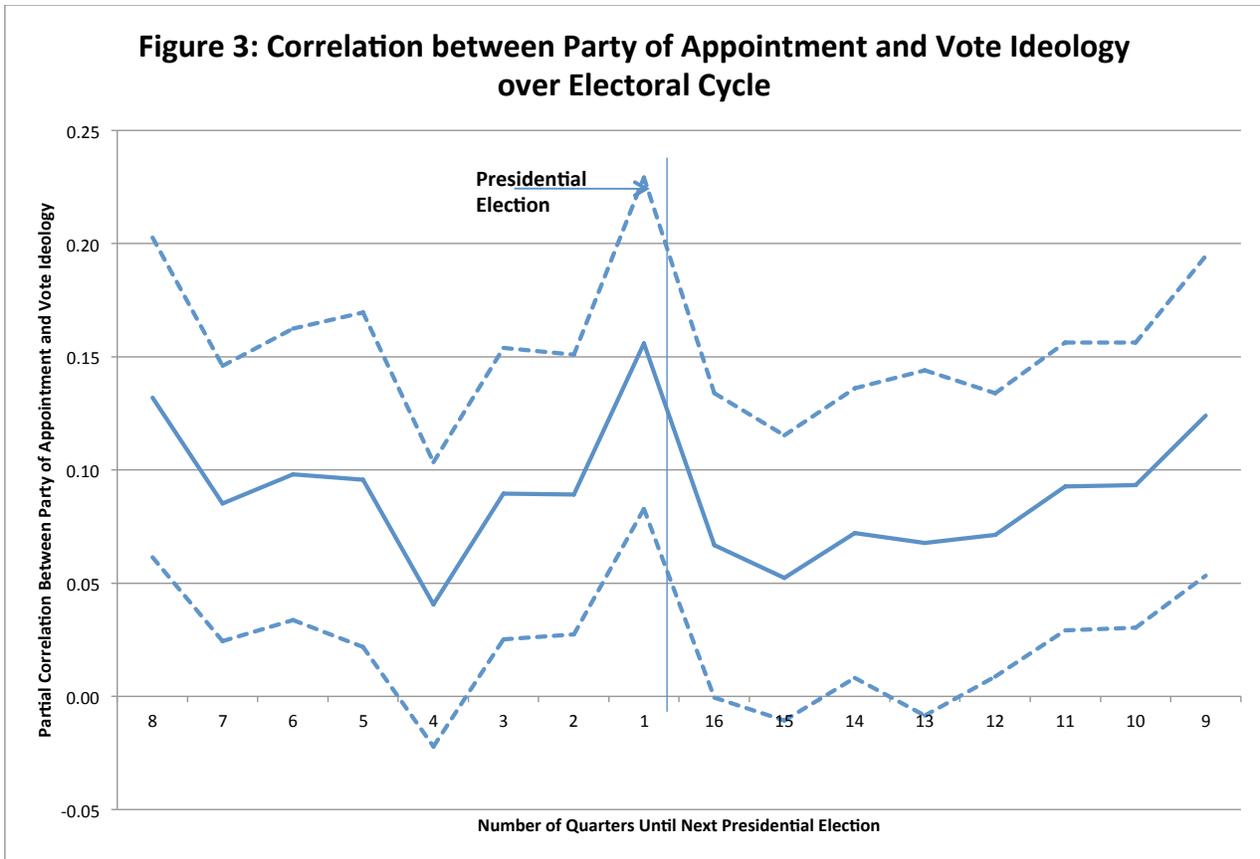
Decisions issued by unified panels (with three Republican or three Democratic appointees) are also more likely to reach partisan conclusions before Presidential elections. Panels with three Democratic appointees are 7.5 percentage points more likely to issue a liberal verdict than panels with three Republican appointees (Tab. 2 Panel B). Because case types should be evenly distributed across panel composition and across the electoral cycle, one might expect no increase in the correlation between the panel's party of appointment and the case outcome before elections. Precedent dictating a liberal outcome should be just as likely to appear before Democratic appointee panels as Republican appointee panels. In the quarter before a Presidential election, however, differences between panels with three Republican or three Democratic appointees double.

Table 2: Electoral Cycles in the Correlation between Party of Appointment and Judges' Votes

<i>Panel A</i>	(1)	(2)	(3)	(4)
	Vote Ideology			
Code	+1/0/-1		+1 vs. 0/-1	+1/0 vs. -1
Mean of dep. var.	-0.157		0.340	0.503
Judge appointed by Democrat	0.0849*** (0.00910)	0.0708*** (0.00821)	0.0348*** (0.00416)	0.0359*** (0.00462)
Judge appointed by Democrat * Last Quarter	0.0684** (0.0335)	0.0712* (0.0365)	0.0394* (0.0211)	0.0319* (0.0177)
Year FE	No	Yes	Yes	Yes
Circuit FE	No	Yes	Yes	Yes
Season FE	No	Yes	Yes	Yes
Legal Issue FE	No	Yes	Yes	Yes
Divided (RDD or DRR) FE	No	Yes	Yes	Yes
Quarter-to-Election FE	No	Yes	Yes	Yes
Observations	56058	56058	56058	56058
R-squared	0.002	0.087		
<i>Panel B</i>	Vote Ideology			
	Politically unified panels (DDD or RRR)			
Mean of dep. var.	-0.139		0.344	0.517
Panel appointed by Democrat	0.168*** (0.0257)	0.164*** (0.0302)	0.0753*** (0.0165)	0.0883*** (0.0160)
Panel appointed by Democrat * Last Quarter	0.217* (0.124)	0.207* (0.125)	0.0828 (0.0683)	0.124* (0.0633)
Year FE	No	Yes	Yes	Yes
Circuit FE	No	Yes	Yes	Yes
Season FE	No	Yes	Yes	Yes
Legal Issue FE	No	Yes	Yes	Yes
Divided (RDD or DRR) FE	No	Yes	Yes	Yes
Quarter-to-Election FE	No	Yes	Yes	Yes
Observations	5659	5659	5659	5659
R-squared	0.011	0.101		

Notes: Robust OLS standard errors clustered at the quarter-year level in parentheses (\*  $p < 0.10$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ). Panel A: Vote-level regression. The outcome variable is Liberal Vote, which is coded as 1 for liberal, 0 for mixed or not applicable, and -1 for conservative. Panel B: Case-level regression. The outcome variable is Liberal Precedent.

Fig. 3 plots the partial correlation between party of appointment and vote ideology for each quarter before an election. Before the election, the partial correlation is a little over 0.15, which is roughly twice the average partial correlation. This means that the ideological effect is similar whether comparing to the quarter after an election or comparing to all other quarters.



Changing the vote ideology of unified panels is one way for judges’ decisions to impact development of law, but the direct impact on the District Court decision is another. Since District Court judges are also politically appointed, we may expect, on average, that Circuit and District Court judges disagree more before elections and for this to be reflected in an increase in reversals and decrease in affirmations of the lower court decisions. We find that Circuit Courts are 5.9 percentage points less likely to affirm and 5.2 percentage points more likely to reverse the District Courts in the quarter before an election relative to after (Tab. 3).<sup>10</sup>

<sup>10</sup>Several additional aspects of behavioral change are considered in Chen (2016). Dissents occur shortly before publication, increase with monthly increases in campaign ads, and appear for cases whose legal topic, economic activity, is most heavily covered by campaign ads. Substituting the date of the publication with dates for any of seven earlier stages of a case (available in linked administrative data) suggests that the exact time at which a judge decides to dissent during Presidential elections occurs shortly before the publication of an opinion (not the date of oral argument, as conventionally assumed). The elevation of dissents correspond to the timing of Presidential primaries. Non-swing states—which are relatively less important during the

Table 3: Electoral Cycles in Treatment of Lower Courts

	(1)	(2)
	Affirm	Reverse
Mean of dep. var.	0.568	0.269
Last Quarter	-0.0588** (0.0251)	0.0519*** (0.0166)
Year FE	Yes	Yes
Circuit FE	Yes	Yes
Season FE	Yes	Yes
Legal Issue FE	Yes	Yes
Divided (RDD or DRR) FE	Yes	Yes
Quarter-to-Election FE	Yes	Yes
Observations	18686	18686
R-squared	0.054	0.025

Notes: Robust OLS standard errors clustered at the quarter-year level in parentheses (\*  $p < 0.10$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ).

We now turn to the role of experience to further investigate whether the characteristics that make individuals more susceptible to priming in the lab are found in the field. Experimental research has found that inexperience magnifies priming effects (Krosnick and Kinder 1990). Tab. 4 re-estimates our basic specification for sub-samples of judges grouped by the number of years they have served as Circuit judges. Overall, judges are 1.7 percentage points more likely to cast a dissenting vote before a Presidential election (Row 1). (This analysis differs from Tab. 1 because we analyze dissents at the individual vote level rather than at the panel level.) For judges with 1 or 2 years of experience, the magnitude of this effect is a considerably larger 3.4 percentage points. The point estimates are accentuated for inexperienced judges. The point estimates are also positive and occasionally statistically significant for other experience groups, e.g., 7–8 years of experience. Inexperienced judges being more likely to dissent before

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general election—are relatively more important during the primary season because many states allocate votes by proportional rule rather than by plurality. The relative elevation of the importance of non-swing states (further elevated due to the importance of momentum) early in the election cycle can be seen in data on campaign advertisements—and in the elevation of dissents. Thus, a contributing factor can be media affecting the behavior of judges (Lim et al. 2015; Arceneaux et al. 2016), though, to be sure, the election could directly affect workplaces rather than solely through media. Dissents before elections also appear on more marginal cases that cite miscellaneous discretionary issues and procedural (rather than substantive) arguments, which the Supreme Court appears to recognize and only partly remedy.

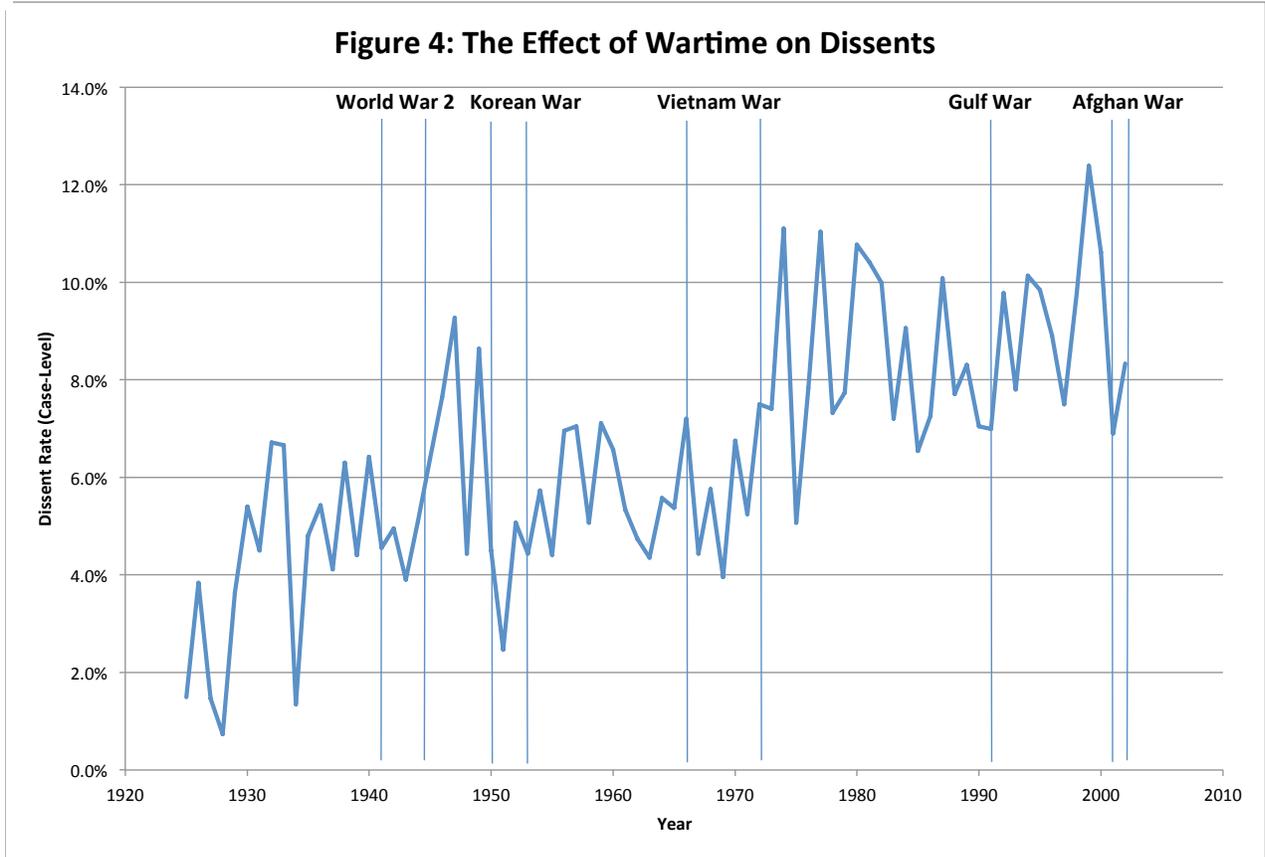
a Presidential election would be consistent with judges taking awhile to develop the strong professional, conscious commitments that would otherwise control the influence of unconscious bias (Rachlinski et al. 2009a).

Table 4: Judicial Experience and Electoral Cycles in Dissents

	(1)	(2)
	Dissent Vote	N
	Each coefficient represents a separate regression	
Last Quarter	0.0174***	56058
(All Experience)	(0.00415)	
Last Quarter	0.0343***	6314
(Experience = 1-2)	(0.0116)	
Last Quarter	0.00976	6526
(Experience = 3-4)	(0.0147)	
Last Quarter	0.0261	6075
(Experience = 5-6)	(0.0185)	
Last Quarter	0.0283***	5644
(Experience = 7-8)	(0.0106)	
Last Quarter	0.0173	5041
(Experience = 9-10)	(0.0166)	
Last Quarter	-0.0256	4390
(Experience = 11-12)	(0.0159)	
Last Quarter	0.0341*	3605
(Experience = 13-14)	(0.0192)	
Last Quarter	0.00159	3002
(Experience = 15-16)	(0.0166)	
Last Quarter	0.0212	2288
(Experience = 17-18)	(0.0256)	
Last Quarter	0.00878	2737
(Experience = 19-21)	(0.0134)	
Last Quarter	0.0188	3033
(Experience = 22-27)	(0.0135)	
Last Quarter	-0.00982	1292
(Experience = 28-35)	(0.0226)	
Year FE	Yes	
Circuit FE	Yes	
Season FE	Yes	
Legal Issue FE	Yes	
Divided (RDD or DRR) FE	Yes	
Quarter-to-Election FE	Yes	

Notes: Robust OLS standard errors clustered at the quarter-year in parentheses (\*  $p < 0.10$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ). The explanatory variable of interest is a dummy variable indicating whether the case was decided in the quarter immediately preceding a presidential election. Numbers in Column 3 do not sum up to the sample size in the first row as some cases have judges with years of experience outside the displayed range.

If elections prime partisan identities, what about wartime, which can prime national identity? Fig. 4 shows that dissents decrease during wars, whose official dates are indicated by the vertical lines.<sup>11</sup>



This result is robust to regression controls (Tab. 5). Notably, the decrease in dissent rates during wartime is largely attributed to divided panels (Col. 1) and inexperience (Col. 2).<sup>12</sup> The coefficient on the non-interacted term, war, is insignificant, while the coefficients on the interaction terms are large and negative. Since 70% of panels are divided, the average effect of

<sup>11</sup>Dates come from the International Crisis Behavior Project. Michael Brecher & Jonathan Wilkenfeld, International Crisis Behavior Project, 1918–2001 (ICPSR Study No. 9286, 2004), at <http://www.icpsr.umich.edu>. We consider the following wars: World War II: 12/7/41–8/14/45; Korea: 6/27/50–7/27/53; Vietnam: 2/7/65–1/27/73; Gulf: 1/16/91–4/11/91; Afghanistan: 10/7/01–3/14/02. Further references on the question of judicial decision making during war are provided in a separate paper (Chen 2016).

<sup>12</sup>We display results using 10 years of experience as the cut-off, but the finding is robust to other experience thresholds.

wartime is also negative. Moreover, divided panels, which are usually 2 percentage points more likely to dissent than unified panels, are 0.6 percentage points *less* likely to dissent during war. In sum, judges who are less experienced and sitting on divided panels are both more likely to dissent before Presidential elections and more likely to *not* dissent during wartime.<sup>13</sup>

Table 5: Judicial Decisions During Wartime

	(1)	(2)	(3)	(4)
	Dissent (2-1 Decision)	Dissent Vote	Affirm	Reverse
Mean of dep. var.	0.079	0.023	0.568	0.269
Divided	0.0198*** (0.00499)	0.00720*** (0.00150)	-0.0139* (0.00775)	0.0138* (0.00731)
War	0.00992 (0.00869)	0.00172 (0.00317)	0.0459*** (0.0113)	-0.0304*** (0.0102)
Divided * War	-0.0263*** (0.00972)			
Inexperience ( $\leq 10$ Years)	0.00469* (0.00264)			
Inexperience * War	-0.00835** (0.00395)			
Year (linear time trend)	Yes	Yes	Yes	Yes
Circuit FE	Yes	Yes	Yes	Yes
Legal Issues FE	Yes	Yes	Yes	Yes
Judge FE	No	Yes	No	No
Observations	18686	49374	18686	18686
R-squared	0.014	0.024	0.019	0.006

Notes: Robust OLS standard errors clustered at the quarter-year level in parentheses (\*  $p < 0.10$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ).

During wartime judges are also more likely to affirm and less likely to reverse lower court decisions (Col. 3-4) and these effects are statistically significant at the 1% level.

A key aspect for interpreting the association between the temporal position of a case and decisions is whether an unobserved factor determines case order in such a way that yields the pattern of results we obtain. For instance, if cases involving contentious issues were somehow more likely to appear before an election, we would naturally find a greater proportion of dissents occurring before the election as well. Two procedural factors preclude this possibility.

<sup>13</sup>Chen (2016) reports the effects of individual wars.

First and most critically, the cases are randomly assigned. Thus, the judge cannot decide to take contentious cases before an election. Second, displacing controversial cases to a later time can not explain the wartime results; wars can last for several years, and court guidelines limit the ability to delay cases for that long.

#### 4 Discussion

Alternative explanations of electoral cycles are explored elsewhere including career concerns, reputational capital, desire to impact the election, learning, and mood (Chen 2016). A combination of logic, empirical evidence, and institutional rules prevent these mechanisms from fully explaining the results. First, the fact that increases in dissents before elections are not matched one-to-one with decreases after elections means that the results are not due to time-shifting of dissents or cases. Second, the results are not due to career concerns. Judges elevated to the Supreme Court and potential Supreme Court nominees are neither more or less likely to dissent before Presidential elections. Nor are judges who are about to retire after the election differentially likely to dissent before elections. Third, dissenting before the election is uncorrelated with the candidate from the judge's party winning the election. Even if judges were motivated to spur additional voting, behavioral changes should be observed in all states within a Circuit since decisions are promulgated at the Circuit—not state—level. For example, consider the Sixth Circuit, which includes Tennessee, Ohio, and Michigan. Through random assignment to panels, judges from these three states may be assigned to the same case, and the judge from Ohio or Michigan is more likely to dissent than the Tennessee judge. Behavioral changes are greatest in states pivotal to the election where popular votes count heavily for the Presidential election and in media markets where campaign advertisements are greatest. Fourth, judges are not likely to be signalling to their state's electorate and politicians when they may be paying attention—no discernible effect is observed for elections of politician that is closest in physical proximity (the state governor) and newspapers are no more or less likely to report on Circuit decisions or their dissents before elections.<sup>14</sup> Fifth, the results are not

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<sup>14</sup>Furthermore, judges gain no benefit in likelihood of elevation to the Supreme Court.

about learning (Lenz 2009). If judges are learning from elections, then elevated dissents should persist after the election, but they do not. Judges are also not supposed to be learning from elections, nor are they supposed to base their decisions on what they learn about political parties.<sup>15</sup> Sixth, the results are not only about mood (Saunders Jr. 1993; Edmans et al. 2007; Simonsohn 2010; Card and Dahl 2011), since mood shifts would affect all judges. However, when judges who are close in ideology scores sit together but are from different parties, the rate at which they disagree triples before a Presidential election; and when judges from the same party sit together, if one dissents, the one with the ideology score more distant from the other party dissents more.<sup>16</sup>

## 5 Conclusion

This paper examines whether U.S. judges are illegitimately biased by political environment. Many studies have examined inter-judge differences in decision-making where the differences are attributed to politics (Peresie 2005; Sunstein et al. 2006). However, inter-judge differences can always be interpreted as being due to something else, like legal philosophy (Kornhauser 1999). This paper documents intra-judge differences and rules out legal philosophies as an explanatory factor since judges' legal philosophies should be stable over short time horizons. Increasing partisanship in recent years (McCarty et al. 2006) may have contributed to decreasing trust in political institutions, one consequence of which may be legal non-compliance (Tyler 2006; Tyler and Huo 2002). The findings that U.S. Courts of Appeals judges make decisions before Presidential elections in a partisan manner raise general questions about the primeability of highly trained professionals with strong commitments to be unbiased. We can-

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<sup>15</sup>Behavioral factors plausibly affect judicial outcomes in ways that need not be about learning. See, for example, snap or predetermined judgments (Chen et al. 2016c), gambler's fallacy (Chen et al. 2016e), mental accounting (Chen and Philippe 2017), masculinity (Chen et al. 2016h,g), mimicry (Chen et al. 2016b), visual cues (Chen et al. 2016d), implicit egoism (Chen and Prescott 2016), racial hierarchy (Chen and Prescott 2017b), and prosecutorial discretion (Chen and Prescott 2017a).

<sup>16</sup>That is not to say that mood does not generally affect judicial decisions. For example, asylum judges are more likely to grant asylum—and U.S. district judges are more lenient—on the day after a home-city Sunday NFL football game win instead of a loss and on days with good weather (Chen 2017b). Eren and Mocan (2016) also finds that emotions affect judicial decisions on juvenile defendants, especially minority defendants. The explanatory power of sports and weather persist after employing the best prediction models of asylum and sentencing decisions (Barry et al. 2016; Chen and Eigel 2016).

not rule out the possibility that highly trained professionals—who profess to be unbiased—are in fact biased, which would raise separate questions about pervasive bias. Indeed, less than 1% of federal judges report political motivations for retirements and resignations, but 13% of retirements and 36% of resignations are politically motivated (Chen 2017a),<sup>17</sup> raising the question of self-deception when claiming to be fair and impartial. Taken together, these results contribute to a theoretical discussion of balance of powers. Linz (1990) argues that conflicts arising in presidential systems between the president and congress can threaten democratic life. Our results raise the question of another conflict. If the judiciary becomes polarized and sclerotized in a manner that stymies the natural democratic churn of institutions, this can lead to additional conflicts between the judiciary and the other branches of government.

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<sup>17</sup>In order to calculate the share of judicial exits that are politically motivated, Chen (2017a) makes the following assumptions. First, assume that the benchmark is random exits spread evenly over 16 quarters between elections and evenly without regards to the party of the appointing President. On average, 0.14 judges voluntarily leave the bench (0.12 are retirements and 0.02 are resignations) each month. Next, calculate the deviation from the baseline in the quarters before or after an election when the party in power is such that it would be politically strategic to exit. In each of the three quarters before a Presidential election, the number of retirements for judges when the party in power is different drops by 0.08-0.10 per month. To interpret the magnitudes, assuming that random exits would render  $0.124 * 48 = 5.95$  judges to retire every 4 years, the comparison yields the abnormal number of judges not retiring before the election. Regression coefficients in the three quarters (each containing 3 months) prior to election indicates that  $(0.079 + 0.076 + 0.107) * 3 = 0.79$  judges are missing, which suggests 13% of judicial retirements are politically motivated. An analogous calculation yields 36% of resignations to be politically motivated.

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Appendix Table S1: Electoral Cycles in Dissents - Additional Robustness Checks

Mean of dep. var.	(1)	(2)	(3)
	OLS	Probit	Drop 1 Circuit
Quarters to Election	-0.00284*** (0.000709)	-0.00293*** (0.000765)	
Last Quarter All Circuits			Each coefficient represents a separate OLS regression.
Last Quarter Circuit 1			0.0686*** (0.0142)
Last Quarter Circuit 2			0.0679*** (0.0142)
Last Quarter Circuit 3			0.0639*** (0.0136)
Last Quarter Circuit 4			0.0715*** (0.0138)
Last Quarter Circuit 5			0.0729*** (0.0138)
Last Quarter Circuit 6			0.0627*** (0.0130)
Last Quarter Circuit 7			0.0706*** (0.0142)
Last Quarter Circuit 8			0.0714*** (0.0143)
Last Quarter Circuit 9			0.0674*** (0.0135)
Last Quarter Circuit 10			0.0713*** (0.0149)
Last Quarter Circuit 11			0.0685*** (0.0139)
Last Quarter Circuit 12			0.0603*** (0.0145)
Year FE	Yes	Yes	Yes
Circuit FE	Yes	Yes	Yes
Season FE	Yes	Yes	Yes
Legal Issue FE	Yes	Yes	Yes
Divided (RDD or DRR) FE	Yes	Yes	Yes
Quarter-to-Election FE	No	No	Yes

Notes: Robust standard errors clustered at the quarter-year level in parentheses (\*  $p < 0.10$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ ). The explanatory variables of interest is a dummy variable indicating whether it is the last quarter before an election (Column 3) or a continuous variable for quarters to election (Columns 1-2). Marginal effect from a probit specification of dissent on continuous variable for quarters to election in Column 2.

Appendix Figure S1: Randomization Inference and Randomization Checks

