

Electoral Cycles among US 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 US presidential elections, judges on the US 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, especially if they have less experience or are in polarized environments. We show that the dissent rate increases gradually from 6 percent to nearly 12 percent in the quarter before an election and returns immediately to 6 percent 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.

1. Introduction

Whether US 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. Politics (Sunstein et al. 2006), race (Shayo and Zussman 2011), and gender (Peresie 2005) appear to affect judicial decisions. Whether these correlations reflect per se bias or differences in legal philosophy is an open question (Kornhauser 2000). 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 percent of cases are legally in-

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determinate, and even in these difficult cases, judges understand which legal reasonings have greater plausibility (Edwards and Livermore 2009)—an argument against an interpretation of per se bias. We test if experienced judges (mean years of experience = 10.3, SD = 7.5) on the US 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 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 temporarily increase 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 1 week after the initial stimulus (Tulving, Schacter, and Stark 1982). The greater the quantity or concentration of primers, 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 a US 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 consist of 18,686 judicial rulings, collected over 77 years, by the 12 US circuit courts, also known as courts of appeals or federal appellate courts. Each circuit court presides over between three and nine states. Our sample consists of petitions related to economic activity (50.9 percent), criminal law (26.9 percent), civil rights (8.6 percent), labor relations (7.2 percent), first amendment, due process, and privacy issues (2.8 percent), miscellaneous (2.8 percent), and undetermined (.8 percent). The 12 US circuit courts process all cases that are appealed from the district courts.

Circuit judges are appointed for life by the president. Three judges, from a pool of eight 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 percent of cases. The majority of decisions were unanimous (92 percent). Our database includes legal variables that were hand coded by prior researchers and include litigant type; the litigant's 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; and 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 100 coded characteristics.¹

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 demonstrate 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 nonprofit organization (Judicial Conference of the United States 2014, canon 4H). 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 (canon 5). 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 (canon 4C).

The judges' decisions are classified into two categories, affirm and reverse. On average, 57 percent 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 are also classified into three categories: liberal = 1, conservative = –1, and mixed or unable 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

¹For documentation and data for cases, see Judicial Research Initiative at the University of South Carolina, U.S. Appeals Courts Database (<http://artsandsciences.sc.edu/poli/juri/appct.htm>). Biographical information for the judges is from Zuk, Barrow, and Gryski (2009). For documentation and data for judges, see Judicial Research Initiative at the University of South Carolina, Attributes of U.S. Federal Judges Database (<http://artsandsciences.sc.edu/poli/juri/attributes.htm>). Random assignment in courts of appeals has been examined in other work. Assignment of cases in circuit courts falls into two categories. In the first, once a case appears on the docket, three randomly chosen judges are assigned to it. In the second, once a year, judges are randomly assigned to panels, and each panel is assigned a date to hear cases. Then, when a case appears, it is 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, Epstein, and Martin (2010) and Sunstein et al. (2006), who code 19 characteristics of cases as determined by the lower court for 415 gender-discrimination circuit court cases and find that cases' characteristics are uncorrelated with the composition of judicial panels. Other papers examine whether the sequence of judges assigned to cases in a 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. The Appendix reports omnibus tests of whether characteristics of cases and litigants vary over 4-year cycles, and Chen (2016) does the same for the caseloads and characteristics of judges authoring or sitting on the panel.

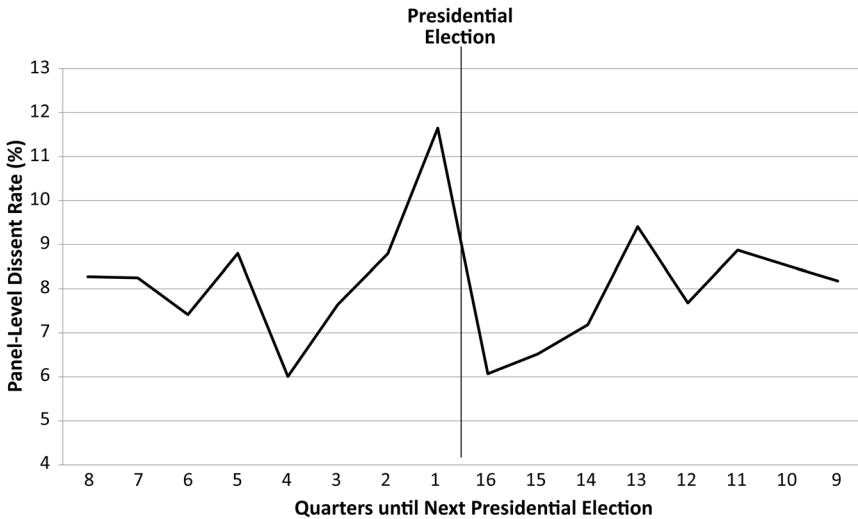


Figure 1. Dissent rate across the electoral cycle, 1925–2002

the secretary of labor who sues a corporation for violation of child labor regulations are all coded as liberal.²

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 Figure 1, which graphs the proportion of dissents by quarter to election. The figure shows that the likelihood of a dissent spikes in the quarter before the election—the probability of a dissent steadily increases from 6 percent to nearly 12 percent and immediately returns to 6 percent after the election.

To account for the possible role of covariates in the patterns depicted in Figure 1, we use a multivariate regression with dissent as the dependent variable and a legal topic fixed effect to control for the idiosyncratic tendencies to dissent in each legal area, a calendar-quarter fixed effect to control for the tendencies to dissent that change by season, a year fixed effect to control for tendencies to dissent that change over time, a circuit court fixed effect to control for tendencies to 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 on a panel (Table 1).

We use a linear probability model (ordinary least squares) as our primary estimation method and show that our results are robust to the use of probit models.

² The Appeals Courts Database project (Songer 1998) indicates that for most, but not all, categories of issues, these correspond to notions of liberal or conservative that are commonly used in the public-law literature.

Table 1
Electoral Cycles in Dissents

	(1)	(2)		(1)	(2)
Divided panel	.0157** (.00452)	.0154** (.00450)	Quarter 9	-.0115 (.0155)	-.00718 (.0157)
Quarter 1	.0637** (.0123)	.0680** (.0135)	Quarter 10	-.0114 (.0160)	-.0110 (.0168)
Quarter 2	.0347** (.0121)	.0341* (.0145)	Quarter 11	.000311 (.0162)	.00269 (.0167)
Quarter 3	.0325** (.0123)	.0343* (.0133)	Quarter 12	-.0102 (.0128)	-.00929 (.0129)
Quarter 4	.00581 (.0111)	.00582 (.0111)	Quarter 13	.00115 (.0148)	.00451 (.0151)
Quarter 5	.0209 (.0152)	.0251 (.0159)	Quarter 14	-.0157 (.0134)	-.0159 (.0147)
Quarter 6	.0120 (.0141)	.0115 (.0153)	Quarter 15	-.0176 (.0117)	-.0154 (.0121)
Quarter 7	.0226 (.0141)	.0238 (.0153)	Fixed effects:		
Quarter 8	.00772 (.0141)	.00870 (.0142)	Calendar quarter	No	Yes
			Legal topic	No	Yes
			R ²	.019	.021

Note. Results are from ordinary least squares regressions, with robust standard errors clustered at the quarter-year level in parentheses. The omitted variable is Quarter 16. All regressions include year and circuit fixed effects. Mean of the dependent variable = .079. $N = 18,686$.

* $p < .05$.

** $p < .01$.

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 cases' outcomes, and an ordinary least squares model is superior for estimation purposes. Second, a probit model is not well suited to the use of regressions with controls for fixed effects (here, 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).

The key predictors are indicators of a case's temporal position: dummy variables indicating the first three quarters before an election, included to examine how opinions immediately before an election differ from those after an election, and dummies indicating whether Democratic and Republican appointees were assigned to the same panel. As a benchmark for the findings, panels with judges appointed by both Republicans and Democrats are 1.6 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 divided-panel effect.³

³ Chen (2016) reports that a linear model of proximity to an election would attribute 23 percent of dissents from unelected courts of appeals judges to the president's electoral proximity. This suggests that—if the estimate that 5–15 percent of cases are legally indeterminate is accurate (Edwards and Livermore 2009)—on average, all dissents may be affected by elections.

The first three quarters before an election have coefficients that are positively signed and statistically significant, which confirms that the pattern in Figure 1 is robust to controlling for the legal attributes of the case. The results are very similar in analyses in which we drop one circuit at a time (Table A1). 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 percent interval for t -statistics is between 2.62 and -2.62 . Figure A1 shows that our true t -statistic of 5.05 lies far to the right of most of the simulated t -statistics. Several 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. Figure A2 presents the t -statistics for changes in the quarter before presidential elections for over 100 characteristics of cases and litigants. We find no increase or decrease before presidential elections along these dimensions. We analyze another statistical model that simply includes the linear trend that is apparent before elections in Figure 1; regardless of the measure of electoral proximity we use, the trend is negative and significant, and the results are nearly identical with probit estimates (Table A1).⁴

Chen (2016) 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. Unlike in the present paper, a noticeable increase in dissents also appears in the ninth quarter before a presidential election; this time period is during the midterm elections, when all House and one-third of Senate seats are up for election. The 5 percent sample we use here may be too small to observe significant midterm effects in dissents.⁵

Next we examine heterogeneity. In particular, we can examine whether the increase in dissents is larger for panels with judges appointed by both Republicans and Democrats. Figure 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 percent, increasing to 6.7 percent before presidential elections, while for divided panels, the dissent rate is 7.3 percent, increasing to 11.7 percent.⁶

⁴ The following robustness checks are reported in Chen (2016): shifting seasonality controls by 1 month (for example, shifting the definition of winter from being December through February to being January through March); including dummy indicators for each type of panel composition (indicating the numbers of Democrats and Republicans); controlling for the presence of a concurrence, which also displays electoral cycles; and clustering standard errors at the circuit level.

⁵ A replication in Chen et al. (forthcoming) uses machine learning to predict agreement between judges. The random forest method achieves the best classification and 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 (Tables 1 and 2), electoral proximity and identity are both important in predicting dissent.

⁶ Several statistical tests for significant differences across groups are presented in Chen (2016). The results in Figure 2 are significantly different for divided panels. Behavioral changes are three times greater in close elections, nonexistent 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 Congress. Dissents are

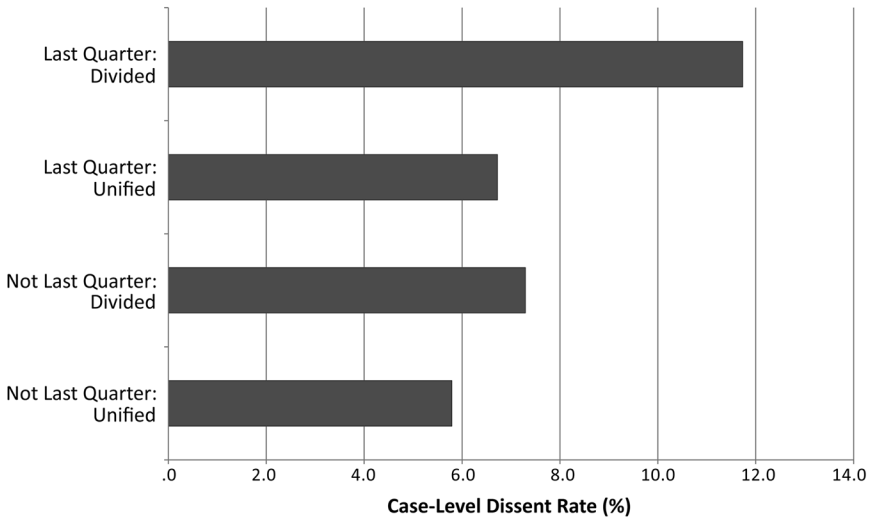


Figure 2. Judicial panels and dissents

We next analyzed judges' vote ideology, specifically, whether judges appointed by Democrats are more likely to cast a liberal vote and judges appointed by Republicans are 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 cast a conservative vote instead. There would be no dissent, but alignment between the decision and the judges' party of appointment would be observed.

We see that the ideological difference between Democratic appointees and Republican appointees doubles in magnitude in the quarter before an election (Table 2). 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 versus not liberal, 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 3.9 percentage points before the election.

Table 2 also indicates that decisions issued by unified panels are 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. Because case types should be evenly distributed across panels and the electoral cycle, one might expect no in-

also elevated in swing states and in states that count heavily toward winning the election, when these states are competitive.

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

	1/0/-1		1 versus 0/-1	1/0 versus -1
	(1)	(2)	(3)	(4)
Vote ideology (<i>N</i> = 56,058):				
Mean of dependent variable	-.157	-.157	.340	.503
Judge Appointed by Democrat	.0849** (.00910)	.0708** (.00821)	.0348** (.00416)	.0359** (.00462)
Judge Appointed by Democrat × Last Quarter	.0684* (.0335)	.0712+ (.0365)	.0394+ (.0211)	.0319+ (.0177)
Fixed effects:				
Year	No	Yes	Yes	Yes
Circuit court	No	Yes	Yes	Yes
Calendar quarter	No	Yes	Yes	Yes
Legal topic	No	Yes	Yes	Yes
Divided panel	No	Yes	Yes	Yes
Quarter to election	No	Yes	Yes	Yes
R ²	.002	.087		
Unified panel (<i>N</i> = 5,659):				
Mean of dependent variable	-.139	-.139	.344	.517
Panel Appointed by Democrat	.168** (.0257)	.164** (.0302)	.0753** (.0165)	.0883** (.0160)
Panel Appointed by Democrat × Last Quarter	.217+ (.124)	.207+ (.125)	.0828 (.0683)	.124+ (.0633)
Fixed effects:				
Year	No	Yes	Yes	Yes
Circuit court	No	Yes	Yes	Yes
Calendar quarter	No	Yes	Yes	Yes
Legal topic	No	Yes	Yes	Yes
Divided panel	No	Yes	Yes	Yes
Quarter to election	No	Yes	Yes	Yes
R ²	.011	.101		

Note. Results are from ordinary least squares regression, with robust standard errors clustered at the quarter-year level in parentheses. Results for vote ideology are from vote-level regressions, and the outcome variable is a liberal vote. Results for unified panels are from case-level regressions, and the outcome variable is a liberal precedent. Liberal = 1, conservative = -1, and 0 = mixed or not applicable.

+ *p* < .10.

* *p* < .05.

** *p* < .01.

crease in the correlation between the panel's party of appointment and the case's 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.

Figure 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 .15, which is roughly twice the average partial correlation.

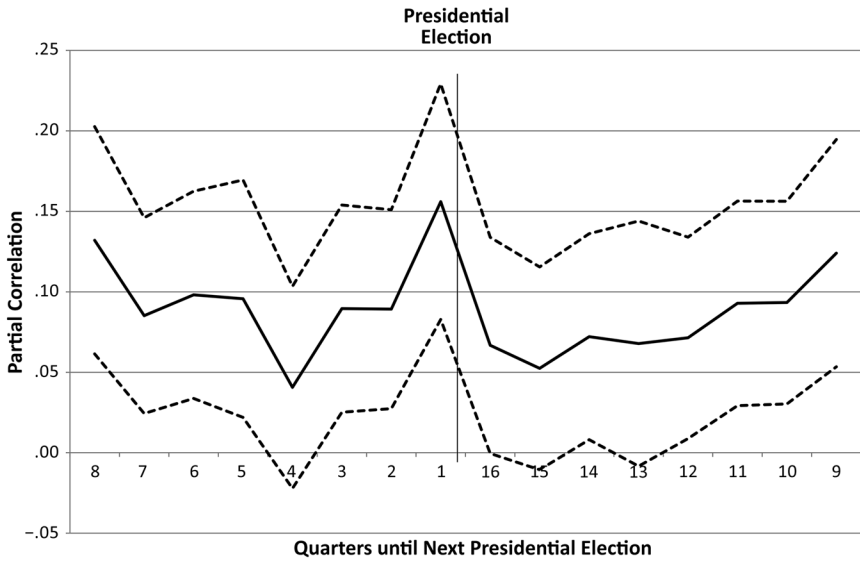


Figure 3. Party of appointment and vote ideology

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's 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 that this will be reflected in an increase in reversals and a decrease in affirmations of the lower courts' 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 an election (Table 3).⁷

We now turn to the role of experience to further investigate whether the char-

⁷ 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 of the opinion with dates of any of seven earlier stages of a case (available in linked administrative data) suggests that a judge decides to dissent during presidential elections shortly before the publication of an opinion (not the date of oral argument, as conventionally assumed). The elevation in dissents corresponds to the timing of presidential primaries. Nonswing states—which are relatively less important during the 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 nonswing states (further elevated because of 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, Snyder, and Strömberg 2015; Arceneaux et al. 2016), though, to be sure, the election could directly affect workplaces rather than solely through media coverage. Dissents before elections also oc-

Table 3
Electoral Cycles in the Treatment of Lower Courts

	Affirm	Reverse
Mean of dependent variable	.568	.269
Last Quarter	-.0588* (.0251)	.0519** (.0166)
R ²	.054	.025

Note. Results are from ordinary least squares regressions, with robust standard errors clustered at the quarter-year level in parentheses. Regressions include year, circuit court, calendar quarter, legal topic, divided-panel, and quarter-to-election fixed effects. $N = 18,686$.

* $p < .05$.

** $p < .01$.

acteristics 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). Table 4 reestimates our basic specification for subsamples 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. (This analysis differs from Table 1 because we analyze dissents at the individual vote level rather than at the panel level.) For judges with 1–2 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 groups, for example, those with 7–8 years of experience. Inexperienced judges being more likely to dissent before a presidential election would be consistent with judges taking a while to develop the strong professional, conscious commitments that would otherwise control the influence of unconscious bias (Rachlinski et al. 2009).

If elections prime partisan identities, what about wartime, which can prime national identity? Figure 4 shows that the number of dissents decreases during wars.⁸

This result is robust to regression controls (Table 5). Notably, the decrease in dissent rates during wartime is largely attributed to divided panels (first column) and inexperience (second column).⁹ The coefficient on War is insignificant, while the coefficients on the interaction terms are large and negative. Since 70 percent of panels are divided, the average effect of wartime is also negative. Moreover, divided panels, which are usually 2 percentage points more likely to dis-

cur in 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.

⁸ Dates of wars are from Brecher and Wilkenfeld (2007). We consider the following wars: World War II: December 7, 1941–August 14, 1945; Korean War: June 27, 1950–July 27, 1953; Vietnam War: February 7, 1965–January 27, 1973; Gulf War: January 16, 1991–April 11, 1991; and Afghanistan War: October 7, 2001–March 14, 2002. For further references on the question of judicial decision making during war, see Chen (2016).

⁹ We display results using 10 years of experience as the cutoff, but the finding is robust to other experience thresholds.

Table 4
Judicial Experience and Electoral Cycles in Dissents

Experience	Dissenting Vote	N	Experience	Dissenting Vote	N
All	.0174** (.00415)	56,058	13–14 Years	.0341+ (.0192)	3,605
1–2 Years	.0343** (.0116)	6,314	15–16 Years	.00159 (.0166)	3,002
3–4 Years	.00976 (.0147)	6,526	17–18 Years	.0212 (.0256)	2,288
5–6 Years	.0261 (.0185)	6,075	19–21 Years	.00878 (.0134)	2,737
7–8 Years	.0283** (.0106)	5,644	22–27 Years	.0188 (.0135)	3,033
9–10 Years	.0173 (.0166)	5,041	28–35 Years	–.00982 (.0226)	1,292
11–12 Years	–.0256 (.0159)	4,390			

Note. Results are from ordinary least squares regressions, with robust standard errors clustered at the quarter-year level in parentheses. The explanatory variable is a dummy indicating whether the case was decided in the quarter immediately preceding a presidential election. Observations do not equal the full sample, as some cases have judges with years of experience outside the displayed range. Each coefficient represents a separate regression. All regressions include year, circuit court, calendar quarter, legal topic, divided-panel, and quarter-to-election fixed effects.

+ $p < .10$.

** $p < .01$.

sent than unified panels, are .6 of a percentage point less likely to dissent during war. In sum, judges who are less experienced and sitting on divided panels are more likely to dissent before presidential elections and more likely to not dissent during wartime.¹⁰

During wartime, judges are also more likely to affirm and less likely to reverse lower-court decisions (Table 5). These effects are statistically significant at the 1 percent level.

A key aspect of interpreting the association between the temporal position of a case and decisions is whether an unobserved factor determines a case's 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. First and most critically, the cases are randomly assigned. Thus, a judge cannot decide to hear contentious cases before an election. Second, displacing controversial cases to a later time cannot explain the wartime results; wars can last for several years, and courts' guidelines limit the ability to delay cases for that long.

¹⁰ Chen (2016) reports the effects for individual wars.

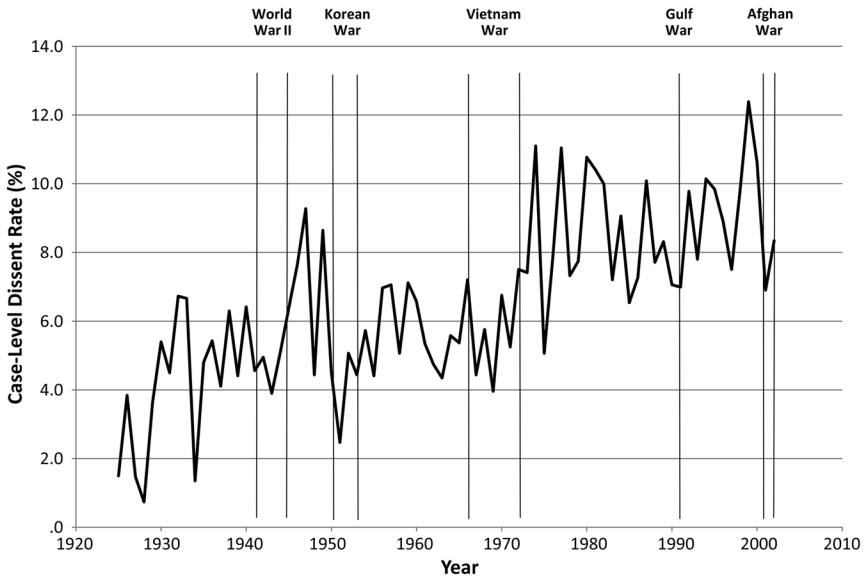


Figure 4. The effect of wartime on dissents

4. Discussion

Alternative explanations of electoral cycles including career concerns, reputational capital, desire to impact the election, learning, and mood are explored elsewhere (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 nor less likely to dissent before presidential elections. Nor are judges who are about to retire after an election differentially likely to dissent before the election. Third, dissenting before an election is uncorrelated with the candidate from the judge's party winning the election. Even if judges were motivated to encourage additional voting, behavioral changes should be observed in all states in 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 judge from Tennessee. 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 most frequent. Fourth, judges are not likely to be signaling to their states' electorate and politi-

Table 5
Judicial Decisions during Wartime

	Dissent Rate	Dissenting Vote	Affirm	Reverse
Mean of dependent variable	.079	.023	.568	.269
Divided	.0198** (.00499)	.00720** (.00150)	-.0139+ (.00775)	.0138+ (.00731)
War	.00992 (.00869)	.00172 (.00317)	.0459** (.0113)	-.0304** (.0102)
Divided × War	-.0263** (.00972)			
Inexperience		.00469+ (.00264)		
Inexperience × War		-.00835* (.00395)		
Judge fixed effects	No	Yes	No	No
N	18,686	49,374	18,686	18,686
R ²	.014	.024	.019	.006

Note. Results are from ordinary least squares regressions, with robust standard errors clustered at the quarter-year level in parentheses. Inexperienced judges have 10 years or fewer of experience. All regressions include year as a linear time trend and circuit court and legal topic fixed effects.

+ $p < .10$.

* $p < .05$.

** $p < .01$.

cians when they may be paying attention—no discernible effect is observed for elections of politicians who are closest in physical proximity (the state’s governor), and newspapers are no more or less likely to report on circuits’ decisions or dissents before elections.¹¹ Fifth, the results are not about learning (Lenz 2009). If judges are learning from elections, then elevated numbers of 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.¹² Sixth, the results are not only about mood (Saunders 1993; Edmans, García, and Norli 2007; Simonsohn 2010; Card and Dahl 2011), since mood shifts would affect all judges. However, when judges who are close in ideology scores are on the same panel but are from different parties, the rate at which they disagree triples before a presidential election, and when judges from the same party are on a panel, if one dissents, the one with the ideology score more distant from the other party dissents more.¹³

¹¹ Furthermore, judges gain no benefit in likelihood of elevation to the Supreme Court.

¹² Behavioral factors plausibly affect judicial outcomes in ways that need not be about learning. See, for example, the gambler’s fallacy (Chen, Moskowitz, and Shue 2016).

¹³ That is not to say that mood does not generally affect judicial decisions. For example, Eren and Mocan (2016) report that emotions affect judicial decisions on juvenile defendants, especially minority defendants.

5. Conclusion

This paper examines whether US judges are biased by the political environment. Many studies examine interjudge differences in decision making and attribute the differences to politics (Peresie 2005; Sunstein et al. 2006). However, interjudge differences can also be interpreted as due to something else, like legal philosophy (Kornhauser 2000). This paper documents intrajudge 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, Poole, and Rosenthal 2006) may have contributed to decreasing trust in political institutions, one consequence of which may be noncompliance with laws (Tyler 2006; Tyler and Huo 2002). The findings that US courts of appeals judges make decisions before presidential elections in a partisan manner raise general questions about whether highly trained professionals with strong commitments to be unbiased can also be primed. We cannot 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 percent of federal judges report political motivations for retirements and resignations, but 13 percent of retirements and 36 percent of resignations are politically motivated (Chen 2017),¹⁴ which raises the question of self-deception when judges claim to be fair and impartial. Taken together, these results contribute to a theoretical discussion of the 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.

¹⁴ To calculate the share of judicial exits that are politically motivated, Chen (2017) makes the following assumptions. First, assume that the benchmark is random exits spread evenly over 16 quarters between elections and evenly without regard to the party of the appointing president. On average, .14 judges voluntarily leave the bench (.12 are retirements and .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 .08 to .10 per month. To interpret the magnitudes, assuming that random exits would render $.124 \times 48 = 5.95$ 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 $(.079 + .076 + .107) \times 3 = .79$ judges are missing, which suggests that 13 percent of judicial retirements are politically motivated. An analogous calculation yields 36 percent of resignations to be politically motivated.

Appendix

Permutation Inference

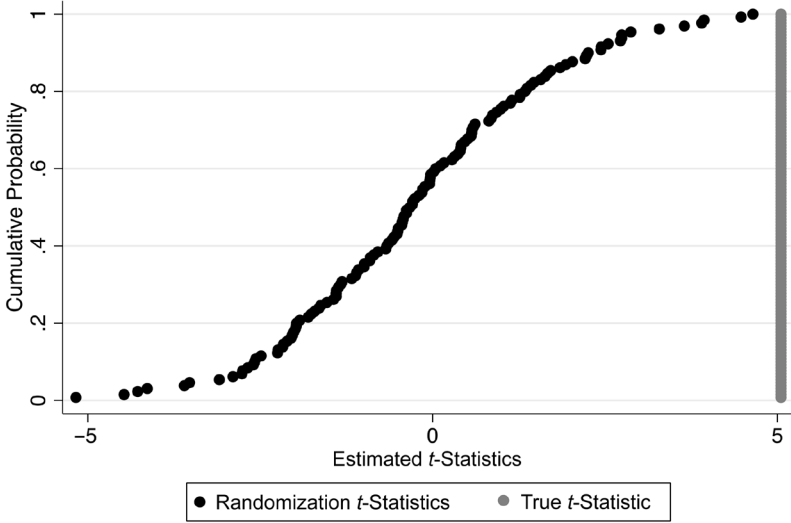


Figure A1. Randomization of quarter to election

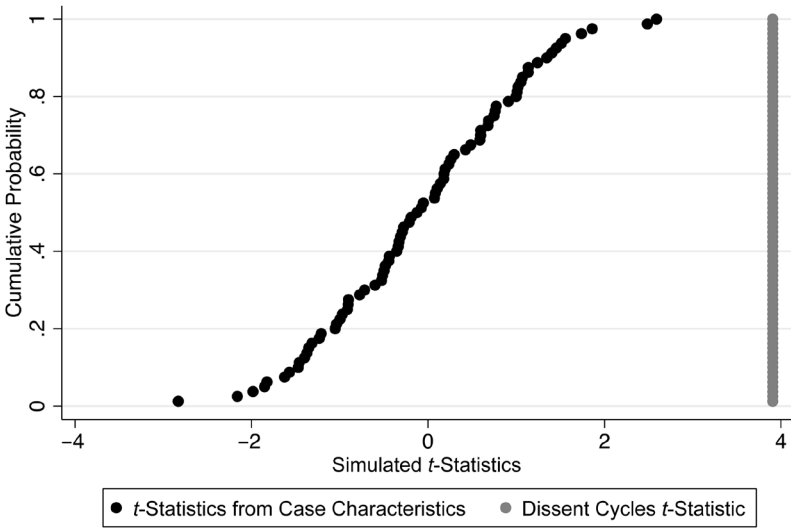


Figure A2. Randomization of case characteristics

Table A1
Electoral Cycles in Dissents: Additional Robustness Checks

Circuit	OLS	Probit	Drop One Circuit	Circuit	OLS	Probit	Drop One Circuit
All	-.00284**	-.00293**		Circuit 7			.0706** (.0142)
	(.000709)	(.000765)		Circuit 8			.0714** (.0143)
Circuit 1			.0686** (.0142)	Circuit 9			.0674** (.0135)
Circuit 2			.0679** (.0142)	Circuit 10			.0713** (.0149)
Circuit 3			.0639** (.0136)	Circuit 11			.0685** (.0139)
Circuit 4			.0715** (.0138)	Circuit 12			.0603** (.0145)
Circuit 5			.0729** (.0138)	Quarter-to-election fixed effects	No	No	Yes
Circuit 6			.0627** (.0130)				

Note. Robust standard errors clustered at the quarter-year level are in parentheses. The explanatory variables are either a dummy variable indicating whether it is the last quarter before an election (last column) or a continuous variable for quarters to election (first two columns). For the ordinary least squares (OLS) regressions, each coefficient represents a separate regression. Results for the probit regressions are marginal effects. All regressions include year, circuit court, calendar quarter, legal topic, and divided-panel fixed effects. Mean of the dependent variable = .079.

** $p < .01$.

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