Priming Ideology? Electoral Cycles Without Electoral Incentives Among Elite U.S. Judges

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February 2013

Abstract

Economists generally view incentives as determining behavior. We document large, economically important changes of behavior occurring even in the absence of incentives. Using all 293,868 cases from 1950-2007 on the U.S. Courts of Appeals and a detailed 5% random sample from 1925-2002, we show that setting precedent reflecting the political views of judge's party of appointment, partisan voting, and dissent rates, all double just before presidential elections. The changes in behavior are not attributable to shifts over the electoral cycle in case or litigant characteristics nor are they attributable to shifts in characteristics of judges either authoring or sitting on the case. We test and reject incentive-based reasons for these changes in behavior and find evidence consistent with priming. Changes in behavior are concentrated among judges sitting in electorally pivotal states and in media markets where campaign advertisements are greatest. Dissents by judges coincide with the monthly increase of campaign advertisements in their states of residence and with the closeness of the state's popular vote when that state has more electoral votes. Ideologically polarized environments and inexperience magnify the effect of proximity to presidential elections, while war has a unifying effect, especially in polarized environments and among inexperienced judges. The electoral cycles we document are significantly larger and more robust than previously-documented electoral cycles by politicians with electoral incentives.

Keywords: Priming, Incentives, Polarization, Normative Commitments, Identity
JEL codes: D72, D83, K40

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1 Introduction

Research on priming has reached a critical juncture. Priming is an implicit memory effect in which exposure to a stimulus influences a response to a later stimulus. For example, media information can temporarily increase (i.e., prime) the accessibility of certain 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). Economists have incorporated these cognitive effects into models (Laibson 2001; Bernheim and Rangel 2004). Yet, priming based on laboratory research in social psychology has come under a large amount of criticism.[1] Daniel Kahneman, even though he is a "general believer" in priming effects, goes so far as to say that a "train wreck" is looming about the robustness of priming results.[2] John List said, "I believe in priming. Psychologists have shown us the power of priming," and he tried "everything" to induce priming in field experiments, but could not.[3] This paper provides field evidence using a natural experiment for the existence and magnitude of priming. Detailed data—a complete sample of 293,868 cases from 1950-2007 and a random sample of 18,686 cases with detailed case and judge characteristics from 1925-2002—on the behavior of U.S. federal appellate judges allows us to study priming effects and whether the contexts and characteristics that make individuals more susceptible to priming in the lab (Srull and Wyer 1979; Iyengar et al. 1982; Krosnick and Kinder 1990) are also found in the field.

The U.S. Courts of Appeals are a relevant environment to study priming for two reasons: first, these courts are an environment where professional norms and institutional mechanisms are designed to limit the influence of non-relevant criteria (such as priming). If highly trained professionals in even these environments are susceptible to priming, then others may be as well. Second, these roughly 180 life-tenured elite judges, basically decide much of what the law is under the US common law system, so finding out whether they are susceptible to priming is important in and of itself.

Federal appellate judges are appointed by the U.S. President and confirmed by the Senate. They preside in one of 12 U.S. Circuits and appointed to a duty station, a particular geographic location within the circuit, where they do most of their writing. Three judges, out of the pool of judges in the circuit,[4] are randomly assigned to a panel for each case. As the party of the appointing President changes over time and judges are appointed for life, the

[1] "The Sin of Bad Science," *Financial Times,* December 21, 2012.

[2] "Nobel Laureate Challenges Psychologists to Clean Up Their Act," *Nature*, October 3, 2012.

[3] "Interview: John A. List," *Federal Reserve Bank of Richmond Region Focus*, Second/Third Quarter 2012.

[4] Judges are drawn from a pool of roughly 8 to 40 judges depending on the Circuit.

panels can be expected to be politically divided (70% of panels have both Republican and Democrat appointees), bringing together judges with different points of view. Judges only occasionally disagree—7.9% of panels have dissents (2-1 decisions) and these dissents occur among both politically divided and unified panels. These high courts do not have juries nor do they assign sentences; their role is to affirm or reverse the lower courts. Only 2% of federal appellate cases reach the U.S. Supreme Court, so these courts decide the vast majority of decisions that set new legal precedent.

We uncover that, despite the norms and mechanisms to limit outside influences, the establishment of precedent reflecting the political views of the judge's party of appointment, voting along partisan lines, and dissent rates, all double in the quarters leading up to a presidential election. Panels are less likely to affirm and more likely to reverse lower court decisions. Partisan behavior also increases before midterm elections. The changes in judicial behavior over the election cycle that we document are significantly larger and more robust than previously-documented electoral cycles by politicians with electoral incentives. For example, 5.9% of criminal sentencing lengths by elected state judges are attributed to the judges' proximity to their own elections (Gordon and Huber 2004, 2007)—we would attribute 23% of total dissents by non-elected judges to proximity to the presidential election. Behavioral shifts by elected state judges in election years are equivalent to a one-decile shift in judicial ideology score (Hollibaugh 2011)—we find a similar one-decile shift among non-elected judges. The changes in behavior are not due to shifts over the electoral cycle in any of over 100 case and litigant characteristics nor are they due to shifts in any of over 50 characteristics of judges either authoring or sitting on the case.

In the period preceding presidential elections, the media is saturated with political debate (see generally, Iyengar et al. 1984; Druckman 2005). We collect all 230,709 New York Times articles from 1900-2007 mentioning both "Republican" and "Democrat" and show that their numbers significantly increase in the months before presidential elections (and midterm) elections and remain elevated throughout the presidential primary season. Using the only database of daily presidential campaign advertisements in different states that we are aware of, we show that campaign advertisements are bimodal across time, peaking before presidential nominating conventions as well as before the general election.

We show that dissents are concentrated in circuits with electorally pivotal states whose popular votes count heavily in the presidential election (the winner of a state's plurality of votes takes all of that states' electoral votes, so states with more electoral votes receive more attention from campaigns). Using variation in where judges reside, we find that changes in

behavior are concentrated among judges in electorally pivotal states and in media markets where campaign advertisements are greatest. *Across* elections, dissents are elevated in states when their popular vote is close and their electoral vote count is high. Using variation in where judges reside and in *timing* of campaign advertisements in each state, we show that dissent rates of judges coincide with increases in campaign advertisements in the state of their duty station, particularly in the month of the increase.

Priming effects have been documented to occur up to one week after the stimulus (Tulving, Schacter, and Stark 1982; Ostergaard 1994; Hassin, Ferguson, Shidlovski, and Gross 2007), yet ours is a setting with lots of stimuli over a long time, unlike laboratory studies, which typically stimulate once. Dissents begin declining 3 months before the presidential election in electorally non-pivotal states, but remain elevated in electorally pivotal states. We show that campaign advertisements are more elevated during the period before the nominating conventions in electorally non-pivotal states than in electorally pivotal states. In the U.S. presidential primary system, candidates need to energize party loyalists before the nominating conventions, when delegates from the candidate's own party vote for their preferred candidate. During the primaries, unlike the general election, many states use a proportional system to allocate delegates, increasing the relative importance of states unlikely to be pivotal in the general election.

The exact time in which a judge makes the mental decision to dissent appears to occur shortly before the publication of an opinion. Using administrative data that provides important milestones for all federal court cases, we show that electoral cycles in judicial behavior do not appear when we substitute the publication date with dates for any of 7 earlier stages of a case. Moreover, cases with dissents take 10% less time to publish after oral arguments in the three quarters before a presidential election. Electoral cycles in disents with or without dissenting opinions are six times larger than electoral cycles in dissents with dissenting opinions. Taken together, electoral cycles in partisan behavior may result in more summary justice.

Campaign messages can activate latent partisan identities (Ansolabehere and Iyengar 1997), so if judges have partisan identities, priming may influence judges' perception of cases brought before them. For example, when judges who are close in ideology sit together but are from different parties, the rate at which they disagree triples before a presidential election. Experimental studies have, moreover, identified several factors that make individuals susceptible to priming, e.g., the number of primes, inexperience, and previous associative links. Inexperience magnifies priming effects—conscious processing, directed by an individual's intentions

and goals, can override the usual or habitual response to priming (Krosnick and Kinder 1990). Activated concepts will spread only if an associative link has been formed, and the stronger the association the wider and faster the activation will spread (Bargh and Chartrand 2000). These courts would be a setting to least expect priming, yet finding these factors at work in the field would lend support to priming as the underlying explanation for changes in behavior.

We show that changes in behavior before elections are 3.5 times larger in close elections, 2.5 times larger in politically divided panels, non-existent in landslide elections, and reversed in elections during wartime. They have also increased over time in tandem with increasing ideological polarization in the U.S. Experience mitigates changes in behavior, but not all kinds of experience: previous work has documented partisan bias by federal prosecutors in public corruption cases—and electoral cycles are larger among judges with previous federal prosecution experience. Notably, wartime reduces dissents, especially by judges sitting on politically divided panels and those with less experience. Wartime also increases affirmations and decreases reversals of lower court decisions.

It is natural to try to find incentive-based explanations for such large changes in behavior, but the facts we uncover are not reasonably explained by any plausible incentive-based mechanisms. First, judges sitting in electorally pivotal states are assigned with judges sitting in electorally non-pivotal states to cases and their decisions are promulgated at the circuit, not state level. Even if voters in electorally pivotal states respond to circuit court decisions, judges in all states would have an interest in getting out the vote. Moreover, dissents would not begin to decline for judges in electorally non-pivotal states after the nominating conventions. Second, judges in electorally pivotal and non-pivotal states alike should be equally affected by coordination breakdowns, collegiality norms, or, for that matter, shifts in unobservables. In particular, these mechanisms would equally affect judges in different duty stations who are randomly assigned to the same case, yet dissents by judges coincide with closeness of their state's popular vote when that state has more electoral votes and with increases in campaign advertisements in the state where a judge resides.

Judges also appear to dissent for highly discretionary, procedural reasons; and various citation measures for an opinion's impact or quality suggest that dissents attributable to electoral cycles are less likely to contribute to the development of law, which is inconsistent with judges seeking to build a reputation among their state's electorate. Perhaps the least outlandish incentive-based explanation is that judges seek promotion to the Supreme Court. Theoretically, it is not clear that dissenting is a good strategy since dissenting could also signal an inability to persuade colleagues and forge a majority coalition on the Supreme Court.

Judges elevated to the Supreme Court are less likely to dissent before elections, consistent with experience mediating the effects of priming.

The remainder of the paper is structured as follows: Section 2 provides background and description of data used, Section 3 outlines our main results, Section 4 examines priming mechanisms for our results, Section 5 considers incentive-based mechanisms, and Section 6 situates our findings in the literature and concludes.

2 Background and Data

2.1 Estimation Framework

Priming is a cognitive mechanism by which an activated concept becomes more likely than before to influence conscious judgments. We describe a simple model of priming of partisan identity (see generally, Akerlof and Kranton 2000 and Benjamin, Choi, and Strickland 2010) to motivate our empirical specifications. Consider latent ideology Q. A judge belongs to a social group C, such as Republican or Democrat, with strength of affiliation s. Assume that s has a steady-state value s but can be temporarily perturbed away from s by a treatment $\mathfrak{e} > 0$ that primes group identity. Let Q_0 denote the judge's ideology baseline without considerations of his/her identity, and let Q_C denote the ideology of social category group, C. Each judge chooses Q to maximize a weighted sum of squared differences, $U = -(1 - w(s))(Q - Q_0)^2 - w(s)(Q - Q_C)^2$, where $0 \leq w(s) \leq 1$ is the weight placed on strength of affiliation with social group C in the judge's utility function. Assume that $w(0) = 0$ and $w' > 0$ so that deviating from the ideology for one's group causes disutility that is increasing in s, the strength of one's affiliation with that group. The first-order condition gives $Q^*(s) = (1 - w(s))Q_0 + w(s)Q_c$, a weighted average of the ideology baseline and the ideology of group C. This condition implies that a group identity prime $\mathfrak{e}$ causes Q^* to move closer to Q_C, assuming that $s(\mathfrak{e})$ and $s' > 0$. In particular, one can see that $\Delta Q = Q^*(s) - Q_0 = w(s)(Q_c - Q_0)$.

Assume that utility of dissent is greatest when it is based on ideological principles and smallest when it is based on procedural quibbles. Assume further that the cost of dissent is constant. Dissents are costly as they typically require dissenting judges to write a separate opinion and can corrode judicial collegiality. Priming of latent ideology shifts the utility of dissent outwards or the cost of dissent downwards, either of which would lead to an increase in marginal dissents.

With this conceptual framework in mind, consider the following latent model for a stimulus to ideology (Chen 2010):

$$Q_{citp} = \alpha'_{0p} F(t) + \alpha'_{1p} \mathbf{Proximity}_t + \alpha'_{2p} \mathbf{Z}_{cit} + \varepsilon_{citp} \quad (1)$$

where Q_{citp} is ideology for judge i in time t on case c without and with the stimulus $p = 1$ *or* 2; $F(t)$ includes a set of year fixed effects; $\mathbf{Proximity}_t$, our explanatory variable of interest, is the set of quarter-to-election fixed effects;[5] $\mathbf{Z}_{cit}$ contains a dummy indicating whether the panel was divided (DRR or RDD), fixed effects for different types of legal issues (criminal, civil rights, constitutional, labor relations, and economic activity), and circuit fixed effects. The subscript p on α_{1p} and α_{2p} allows the relationship between ideology and **Proximity** or contextual factors to vary with the stimulus.

Taking first differences (implicit individual fixed effects) results in:

$$\Delta Q_{cit} = (\alpha_{02} - \alpha_{01})' F(t) + (\alpha_{12} - \alpha_{11})' \mathbf{Proximity}_t + (\alpha_{22} - \alpha_{21})' \mathbf{Z}_{cit} + \omega_{cit} \quad (2)$$

where ΔQ_{cit} represents changes in ideology due to the stimulus and $\omega_{cit} = \varepsilon_{cit2} - \varepsilon_{cit1}$. Recall that our theoretical model suggests that $\Delta Q_i = Q^*(s) - Q_0 = w(s)(Q_C - Q_0)$. Since electoral $\mathbf{Proximity}_t$ perturbs s, this expression motivates interaction specifications since $s'(\varepsilon)$ may be larger due to exogenous characteristics of the judge, such as whether the judge is inexperienced, a former federal prosecutor, or sitting in an electorally pivotal state with many campaign advertisements. Moreover, $Q_C - Q_0$ is greater when the panel is politically divided, decided during a close election, or occurred in the recent time period. $Q_C - Q_0$ is smaller during landslide elections or during wartime.

In practice, change in latent ideology, ΔQ_{cit}, is also latent and unobserved. Instead, our primary measure of judicial behavior is dissent, which is observed when the change in latent ideology is large enough. In particular, dissents increase with the distance between Q^* for the judges assigned to the 3-judge panel. This motivates the following linear probability specification:

$$Y_{cit} = F(t) + \beta'_1 \mathbf{Proximity}_t + \beta'_2 \mathbf{Z}_{cit} + \eta_{cit} \quad (3)$$

where, Y_{cit} , our outcome of interest, is an indicator variable equal to 1 if a dissent was filed on case c. Because Y_{cit} has the following ordered structure:

[5] In the regressions presented in the tables that follow, the omitted quarter is quarter 16, i.e., the quarter immediately following an election. In some specifications we use a more parsimonious measure, such as simply the last quarter before the election or a dummy indicator combining the last three quarters before the election.

$$Y_{it} = \begin{cases} 0 & if \quad \Delta Q_{cit} \leq \mu \\ 1 & if \quad \Delta Q_{cit} \geq \mu \end{cases}$$

we will also estimate the following probit specification to test whether electoral proximity primes ideology:

$$\begin{aligned} \Pr(\Delta Q_{cit} &= -1|\mathbf{I}_{cit}) = F_{\omega_{cit}}(\mu - (\alpha_{02} - \alpha_{01})F(t) - (\alpha_{12} - \alpha_{11})'\mathbf{Proximity}_t - (\alpha_{22} - \alpha_{21})'\mathbf{Z}_{cit}|\mathbf{I}_{cit}) \\ \Pr(\Delta Q_{cit} &= 1|\mathbf{I}_{cit}) = 1 - F_{\omega_{cit}}(\mu - (\alpha_{02} - \alpha_{01})F(t) - (\alpha_{12} - \alpha_{11})'\mathbf{Proximity}_t - (\alpha_{22} - \alpha_{21})'\mathbf{Z}_{cit}|\mathbf{I}_{cit}) \end{aligned}$$

where $\mathbf{I}_{cit} = (1, F(t), \mathbf{Z}_{cit})$ and $F_{\omega_{cit}}(.|\mathbf{I}_{cit})$ is normal. We show both probit and linear probability estimates. We cluster standard errors at the quarter-year level for case-level outcomes like dissent. We double-cluster standard errors at the quarter-year and case level for judge-level outcomes like voting valence or dissent votes.

Voting valence and dissents measure separate phenomena: for example, on a panel with 3 Democratic appointees, the panel may vote conservative when it is not before an election because of a prevailing precedent and choose to vote liberally when it is before an election, and in both situations, making a unanimous decision. In equation (3), Y_{it} then represents voting valence where 1 indicates liberal; -1, conservative; and 0, mixed. Priming social categories, such as party affiliation, shifts Q^* to be closer to Q_C, and will cause voting to be more aligned along partisan lines. The measure of vote valence captures latent ideology Q^* of judges on a panel moving further apart. That is, we would test:

$$Valence_{cit} = F(t) + \gamma_1'\mathbf{Proximity}_t + \gamma_2'\mathbf{Proximity}_t * Democrat_i + \gamma_3 Democrat_i + \gamma_4'\mathbf{Z}_{cit} + \nu_{cit} \tag{4}$$

We also run the same specification for the author of an opinion for cases assigned to 3 Democratic appointees or 3 Republican appointees. This test would indicate that proximity to a presidential election affects the establishment of precedent in partisan ways.

We consider additional outcomes for Y_{cit} like reason for dissent (procedural or substantive), subsequent citations to the opinion, length of opinion, time spent on opinion, affirmations of lower court decisions, reversals of lower court decisions, type of reversal (with or without elements of remand[6]), and number of days between oral argument and final judgment. We

[6]Reversals without remand require the appellate court to be more specific about the outcome of the case—it

also replace $\mathbf{Proximity}_t$ with a linear measure of time to election for comparability with other literature and replace $\mathbf{Proximity}_t$ with time-varying measures of stimulus—increases in campaign advertisements in different states. We use alternative dates t for the case representing every available significant event in a case's history to assess when the mental decision to dissent may have occurred. We assess robustness in various sub-samples (by circuit, by legal issue, by appointing president, by birth cohort as well as by experience). In some checks, $\mathbf{Z}_{cit}$ includes judge fixed effects and seasonality controls.[7] That is, in our most stringent tests, our identification comes from comparing judicial behavior just before—October—with just after—November and December, within legal case categories, within the set of divided or non-divided panels, within a circuit, within a year, and, potentially, within a judge. We also conduct randomization inference (randomizing cases to other quarters) and check whether case, litigant, or judge characteristics vary over the election. In both cases, we plot the true t-statistic in comparison to all the t-statistics from the other regressions.

We also estimate specifications with a prime for consensus rather than dissensus, namely wartime.[8] We interact wartime with $Polarization_{cit}$ factors where we might expect larger effects such as whether the judge was inexperienced or sat in a divided panel.

2.2 Data

Data on cases from 1925-2002 come from the U.S. Courts of Appeals Database Project.[9] This database includes information on opinion-specific variables (including the names of the judges sitting on each panel) for a roughly 5% random sample of cases. Biographical information for the judges in the database was obtained from the Multi-User Data Base on the Attributes of U.S. Appeals Court Judges.[10] Data on subsequent outcomes in the Supreme Court, if any, come from the Shepardized Courts of Appeals database. Election information come from CQ Voting and Elections Collection.[11] Significant dates for all cases filed in federal courts come

has not only decided on the law but also evaluated the facts and deemed it unnecessary for the lower court to do so, in effect determining the final outcome for the original plaintiff and defendant in the case. A remand means the lower court has to re-evaluate the facts of the case (perhaps with a new trial) to be sure that the outcome conforms with the law set out by the appellate court.

[7] I.e., we include a set of fixed effects for each calendar quarter of the year (e.g., January through March, April through June, etc.).

[8] 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.

[9] Documentation and data available at http://www.cas.sc.edu/poli/juri/appctdata.htm.

[10] Documentation and data available at http://www.cas.sc.edu/poli/juri/auburndata.htm.

[11] Table 30-1 Divided government, 1860–2006. (2008). In Guide to Congress, 6th ed. (Vol. 2). Washington: CQ Press. Retrieved May 12, 2010, from CQ Press Electronic Library, CQ Voting and Elections Collection,

from the Administrative Office of the U.S. Courts. Aggregate number of presidential campaign advertisements in the 75 largest media markets for the 1996 election and daily presidential campaign advertisements in the 100 largest media markets for the 2004 election come from the Wisconsin Ads project. Judicial ideology scores, a summary measure using the voting patterns of the appointing president and home state senators, come from the Judicial Common Space database (Epstein et al. 2007). We collect all 230,709 New York Times articles from 1900-2007 mentioning both "Republican" and "Democrat" in the same article. We collect all cases from 1950-2007 from Openjurist. From Openjurist, we have dates (97.5%), circuits (94%), and whether there was a dissenting opinion[12] (not all dissents have dissenting opinions).

Using these datasets, we construct the variables for dissent (the 1925-2002 dataset codes for the presence of a dissent, regardless of whether there was a dissenting opinion), reason for dissent (procedural or merit reasons),[13] electoral proximity (linear or nonlinear, i.e. a full set of indicator variables for each quarter to the upcoming presidential election), divided (panels having at least one Democratic appointee and one Republican appointee),[14] legal issue,[15] opinion quality (opinion length, citations, and citations by subsequent dissents),[16] judicial background (gender, age, previous work experience, judicial experience), political environment (incumbent President, electoral vote count), decision valence (liberal = 1, conservative = -1, and mixed or unable to code[17] = 0),[18] and treatment of lower court (affirm or reverse, reverse with remand, reverse without remand). For 1925-2002, we restrict our analysis to cases decided by panels composed of three judges. Some analyses are further restricted due to lack of biographical information for the judges sitting in the panel.[19] Our sample contains

http://library.cqpress.com/elections/g2c6e2-973-36489-1842592. Document ID: g2c6e2-973-36489-1842592.

[12]We split the text of the document for further analysis in current research.

[13]The database codes whether the dissent addresses a procedural issue or addresses a merits issue.

[14]The database assigns each judge to one of the following parties: republican, democrat, liberal, conservatives and independents. We group liberal judges with Democratic judges, conservative judges with Republican judges and independent judges with the party of the appointing president.

[15]We use the following 1-digit classification: criminal, civil rights, constitutional (i.e., First Amendment, due process and privacy cases), labor relations, economic activity and regulation, and miscellaneous. We also use finer 2-digit categories of legal issues for robustness checks.

[16]We winsorize this variable at the 1% level, that is, we censor all observations outside of 1% to 1%. Citation data come from Lexis's *Shephards* service.

[17]Our results are robust to dropping the votes that are unable to be coded.

[18]The Appeals Court 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. 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."

[19]Missing data include elevation to the Supreme Cout and previous service as Assistant U.S. Attorney or U.S. Attorney. The regressions that use this data have a reduction in sample size of 10%. In addition, we exclude the votes by judges sitting on cases when they have less than 0 years of experience or greater than 35

18,686 decisions (56,058 votes) for the period 1925-2002 and 293,868 decisions for 1950-2007. Summary statistics are displayed in Appx. Table A. Overall, 7.9% of opinions from 1925-2002 have dissents while 6.2% of opinions from 1950-2007 have dissents with dissenting opinions.

Our data contains only published decisions. Until the mid-1970s, all decisions were published (Cleveland 2010) and our results are robust to restricting the data to the time period when all decisions are published in both datasets.

3 Electoral Cycles in Judicial Behavior

3.1 Dissents

The electoral cycle can be observed in both the raw data and regression analyses. We present the fluctuation of the average dissent rate across the presidential electoral cycle. Dissents are most frequent in the months preceding a presidential election and lowest in the months that immediately follow it, ranging from a high of 11% to a low of 6% (Fig. 1A). We then control for a dummy for divided panels as well as year and circuit fixed effects (Table 1 Col. 1). The rate of dissent is highest during the period before an election, particularly in the quarter immediately preceding an election, a result that is both statistically and economically significant. The t-statistic on the quarter-before-election is 4.01. These results are robust to controlling for legal fixed effects and seasonality (Col. 2) and to a probit specification (Col. 3 and 4).[20]

Dissent rates remain elevated even if we compare the quarter immediately preceding an election to any other quarter in the two years immediately following an election. In fact, all the coefficients for the last 8 quarters in the presidential electoral calendar are greater in magnitude than those corresponding to the earlier half of the cycle. Coefficients for quarter-to-elect-15 through quarter-to-elect-9 all have negative sign in Col. 2 and 4.

This effect remains statistically significant when broken down to the monthly level (Fig. 1B and 1C) and is, in aggregate, particularly strong in the third month before the presidential election. This time period is when the news cycle is particularly amplified along partisan lines as party conventions are choosing the presidential candidates to compete in the general elections as evidenced by the number of New York Times articles mentioning both Republican and Democrat in the same article (Fig. 2A and 2B). These graphs and our regression results

years of experience as these experience measures are likely with error. These regressions have a 13% reduction in sample size.

[20]The OLS coefficients are similar in magnitude to the marginal effects of the probit specification.

display a prolonged period of elevation up to 10 months before the presidential election.

We replicate our findings using all cases from 1950-2007 (Col. 5-8). The magnitudes are roughly six times larger in the Appeals Court Database. To investigate this, we construct as sampling weights, the fraction of published cases by circuit-year in our 100% sample contained in the Appeals Court Database.[21] When we rerun Col. 1-4 using these weights, the effects become slightly larger. We attribute larger effects in the Appeals Court Database to dissents without dissenting opinions becoming more likely before presidential elections.[22]

A statistically significant elevation in dissents also appears before midterm elections in the 100% sample. Our results are robust to using a linear version of the quarters to election, dropping one circuit at a time, and parsimonious probit and logit models (Table 2). Our effects are stronger when we shift seasonality controls by one month (December through February, etc.).

3.2 Interpreting Magnitudes

In the quarter immediately preceding an election, the probability of a dissent increases by 6.4% percentage points (Table 1 Col. 1) when compared to the quarter following the election, an increase that represents over 75% of the average rate of dissent. The increase in dissents before the election is larger and more statistically significant than changes in other behaviors that have been attributed to electoral incentives. Among a sample of 276,119 decisions by Washington state judges, criminal sentencing lengths increase by 10% and deviations from criminal sentencing guidelines increase by 50% in the two quarters before a judicial election (Berdejo and Yuchtman 2010). Among our sample of 18,686 cases, the ratio of coefficients to standard errors is twice as large than in the Washington state sample.

A sizeable share of dissents also appears attributeable to electoral proximity and this share is larger than the share of prison time that has been attributed to electoral proximity. Using a linear measure of proximity to election, an estimated 5.9% of total prison time of sentenced criminals is attributed to electoral proximity (Gordon and Huber 2004 and 2007). In our sample, an estimated 23% of all dissents would be attributed to electoral proximity. Each quarter to an election reduces the dissent rate by 0.24% (Table 2 Col. 1). Since the average case is 7.5 quarters before the next election, multiplying 7.5 by 0.24% and dividing by the average dissent rate of 7.9% results in 23%. Our electoral cycles are also larger than

[21]The U.S. Courts of Appeals Database selects 15 cases per circuit-year before 1960 and 30 cases per circuit-year after 1960.

[22]Investigating the nature of dissent is a subject of present research.

political business cycles[23] and robust to controlling for these cycles.[24]

3.3 Voting Valence

Voting valence measures something different from dissents. For example, if a precedent dictates a liberal decision, a unified Republican panel should make 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 judge's party of appointment.

Democratic appointees typically cast votes that are more liberal than do Republican appointees (Table 3 Panel A Col. 1). The ideological difference between Democratic appointees and Republican appointees doubles in magnitude in the quarter before an election. These estimates are robust to keeping only a parsimonious set of controls (party of appointment, last quarter, and their interaction) and a saturated model (all quarter-to-election dummies in $\textbf{Proximity}_t$ and their interactions with party of appointment). The interaction coefficients of the saturated model are displayed in Fig. 3A. These partial correlations between voting valence and party of appointment for each individual quarter-to-election reveal that midterm elections also increase ideological polarization, an increase that we did not robustly detect in dissents, yet is a time period when there are also more newspaper articles mentioning both "Republican" and "Democrat" (Fig. 2B).

These effects are quite large in magnitude. Roughly 70% of panels are politically divided but only 2.3% of judicial votes are dissents. Moreover, only 5 to 15% of cases are legally indeterminate according to judges' estimates (Edwards and Livermore 2008). Partisan voting roughly doubles in the quarter before presidential elections: the correlation between party of appointment and voting valence increases by over 100%. The results survive a number of robustness checks. They are robust to examining the marginal effect of going from conservative/neutral (-1 or 0) to liberal (1) (Col. 3) and going from conservative (-1) to neutral/liberal (0 or 1) (Col. 4). The results are quantitatively similar when we include judge fixed effects.

Fig. 3B summarizes the role of ideology in a manner comparable to other studies. The

[23]Political business cycles are actually weak or non-existent (Drazen 2001, Alesina, Roubini, Cohen 1997, Alt and Chrystal 1983, McCallum 1978).

[24]We consider GDP, GDP growth, GNP, GNP growth, unemployment, and unemployment growth. Since the data is available by year and quarter, not quarter-before-an-election, our calculations are necessarily somewhat approximate. Cases decided in October before the election and November after the election would have the same economic data. Economic data one quarter before the presidential election is a weighted average that puts one-third weight on economic data from the last quarter of the year and two-thirds weight on data from the third quarter.

flatter line indicates the average voting valence by ideology score quintile when it is not the last quarter before a presidential election while the steeper line indicates the average voting valence by score quintile during the last quarter. Judges to the left of the median score are voting more liberally while judges to the right of the median are voting more conservatively. Using only quintiles 1-4, the electoral cycle appears to make judges ideology score equivalent to one decile away from the median. The most conservative quintile may be libertarian, which makes coding voting valence difficult (voting valence = 0). These judges, nevertheless, also become more conservative in the last quarter. The one-decile shift is similar to that found by Hollibaugh (2011), which finds that the extent to which state supreme court judges in competitive partisan elections exhibit polarizing behavior in election years is equivalent to a shift of 8.3%-10.3% in ideology score.

3.4 Legal Outcomes

Decisions issued by unified panels (3 Republicans or 3 Democrats) are more likely to be partisan before presidential elections (Table 3 Panel B Col. 1). Because case types should be evenly distributed across panel composition and across the electoral cycle, one might expect no correlation between the panel's party of appointment and the case outcome. Precedent dictating a liberal outcome should be just as likely to appear for Democrat panels as for Republican panels. In the quarter before a presidential election, however, unified panels are 125% more likely to issue partisan opinions. This pattern does not appear for decisions issued by divided panels, that is, RRD and DDR panels are not more likely to issue decisions that reflect the party of appointment of the majority of the panel.

In the quarter before an election, appellate courts decrease by about 10% the rate at which they affirm the lower court and increase by about 15% the rate at which they reverse the lower court above the baseline of 57% affirmations and 27% reversals (Table 3 Panel C Col. 1-2). We find that reversals without elements of remand do not increase before presidential elections, while remands do (Col. 3-4). Reversals without remand require the appellate court to be more specific about the outcome of the case (like dissents with dissenting opinions would), whereas a reversal with remand means the lower court has to decide the case again and conform with the law set out by the appellate court.

3.5 Randomization

First, 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. While high because the second and third quarter before an election also display increases in dissents, our true t-statistic of 4.01 lies far to the right of all the other simulated t-statistics (Fig. 4A).

Next, we test whether case type, caseload, and composition of the 3-judge panels vary over the electoral cycle. The proportion of ideologically divided panels is evenly distributed across the political cycle (Appx. Table B Col. 1). The number of cases in each of 5 broad legal categories are also evenly distributed across the political cycle (Col. 2-6). No case type is particularly likely to arise before presidential elections. Our results are robust to including these variables as controls. Col. 7 shows that caseload as measured by the number of cases does not systematically increase change in the quarter before presidential elections. The fraction of cases published does not significantly increase in the quarter before the presidential election (Appx. Table C Col. 5) (Berdejó 2010).

Fig. 4B displays the t-statistics for significant changes in the quarter before presidential elections for over 106 case and litigant characteristics coded in the database. We find no increase or decrease before presidential elections along substantive legal issues, including 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, agency discretion. We also find no difference before elections in litigant type or strategy, including how many appellants or respondents were persons, businesses, public interest groups, or government actors, and so on. Along four procedural issues—issues in the "other" category—we find some evidence of an increase before the presidential election. Our results are robust to the inclusion of controls for these four procedural issues.

Fig. 4C and 4D conduct the same randomization check for over 50 characteristics coded for the opinion writer and for the two other panelists. In all of these tests, the t-statistic of the dissent is far to the right of the other t-statistics. These results are consistent with related work finding that the sequence of judges assigned to cases in each appellate court is like a random process and uncorrelated with case characteristics (Chen and Sethi 2011).

4 Priming Mechanism

4.1 Campaign Advertisements

In the period preceding presidential elections, the media is saturated with political debate, but some regions of the country are more likely to be saturated with political debate than others. Dissent rates are concentrated in circuits with electorally pivotal states and the Washington, D.C. (Table 2 Col. 3-4). Circuit 6, which appears robustly in both data samples and measure of dissent, includes Ohio and Michigan (Fig. 5C), while Circuit 11 includes Florida. Circuit 3 includes Pennsylvania and Circuit 7 includes Illinois. Large states such as these count heavily in the presidential election since the winner of a state's plurality of votes takes all of that states' electoral votes, making these states' media markets an attractive target for campaigns (Fig. 5A).

Judges who do their writing in states likely to have greater amounts of political debate are more likely to dissent (Table 4 Panel A). Col. 1-2 compare D.C. and the 8 states ranked highest in electoral vote count (CA, TX, NY, FL, IL, PA, OH, MI) during the recent time period, when polarization has been greatest, with other states. Col. 3-4 compare states likely to be electorally pivotal, such as MI, OH, PA, FL, and CA, with other states. These states have 1,790 more campaign advertisements and 900 more negative campaign advertisements per media market and the relationships are statistically significant at the 1% level. We use the earliest date, 1996, of the campaign advertisement data, while our judicial behavior database ends in 2002. Col. 5-6 compare states in the top quartile of campaign ads vs. the remainder. The magnitude of the electoral cycle is up to 6 times larger in the regions of the country likely to have political debate and the dissent cycle is generally not statistically significant in other regions of the country.

Judges who do their writing in states *when* they are likely to have greater amounts of political debate are also more likely to dissent. We construct state-by-election electoral dissent spikes by taking the difference between the dissent rate in the three quarters before an election with the dissent rate in the three quarters after an election. We construct popular vote tightness of a state by taking the absolute difference in the fraction of votes received by the Republican candidate and the Democratic candidate for the presidential election. Judges sitting in states with high electoral vote counts have greater dissent spikes when the election is close (Panel B). For a large state with 30 electoral votes, going from a popular vote tightness from 5% to 0% (statistical tie) would result in an increase of 1.7% points in the dissent rate.

Using both variation in where judges reside and variation in the timing and magnitude

of campaign advertisements, we show that dissent rates of judges coincide with increases in campaign advertisements in the state of their duty station. As we only have daily campaign adverisement data for the 2004 election, we assign the monthly increase in campaign advertisements for some month before November 2004 to the same month before the respective prior elections. The intuition is that the importance of different states at different points in time during the 2004 electoral cycle predicts the months of stimulus in different states for other elections. We find that an increase in 10,000 campaign advertisements in the previous month corresponds to 0.7 percentage point increase in dissent rate by the judge in that state (Table 5 Col. 1). This coefficient is significant at the 5% level and is robust, becoming larger, when adding lags and leads (Col. 2-7). The one-month lag displays quantitatively similar associations, though the point estimate is not statistically significant and becomes smaller with the inclusion of controls. Col. 4 includes circuit and year fixed effects and Col. 5 includes judge fixed effects.

4.2 Summary Justice

A robust finding we uncover is that the dissent rate peaks in the third month before the presidential election (Fig. 1B and 1C). Dissents begin declining 3 months before presidential elections in electorally non-pivotal states (Fig. 6B) but remain elevated in electorally pivotal states (Fig. 6A). Campaign advertisements are similarly more elevated during the period before nominating conventions in electorally non-pivotal states (Fig. 5B) than in electorally pivotal states (Fig. 5A). As candidates need to energize party loyalists before the nominating conventions and, unlike the general elections, many states use a proportional system to allocate delegates for the nominating convention, the relative importance of electorally non-pivotal states is likely to decrease precipitously after the nominating conventions. Indeed, campaign advertisements only about double from its peak before the nominating convention for non-pivotal states while they increase roughly 5-fold for pivotal states. Priming effects have only been documented one week after the stimulus (Tulving, Schacter, and Stark 1982; Ostergaard 1994; Hassin, Ferguson, Shidlovski, and Gross 2007), so the effects of campaign advertisements may be relatively fleeting as well.

Using administrative data on all important milestones of a federal court case's development, we find evidence suggesting that the exact time in which a judge makes the mental decision to dissent may be shortly before publication of an opinion. We are able to match the Appeals Court database (1925-2002) to the Administrative Office of the U.S. Courts database to obtain the actual oral argument date for 3,517 cases between 1971-1999 (38% match rate).

This match disproportionately reflects cases docketed in the same year as the publication date because, for the fraction of opinions that did not record the 2-digit portion of the docket number (2 digit year followed by 5 digit number), the Appeals Court database assigned the 2-digit year of publication. For our 100% sample, we match 164,591 cases from 1971-2006 out of 218,683 cases (75% match rate). We achieve a much higher match by successively attempting to match with the 2-digit portions of docket numbers assigned to years preceding the publication date.

First, we substitute the publication date with dates for any of 7 earlier milestones of the case. These earlier milestones include the docketing date, the date of filed in district courts, the date the notice of appeal was filed, the date the original notice of the brief got issued, the date of the last brief filing, the date the appeal was submitted on the merits, and the date the appeal was orally argued. We find no electoral cycles for those dates (Table 6).

Second, we show that the average number of days between oral argument and final judgment[25] (the time taken to write the opinion) falls for cases with dissents in the 3 quarters before a presidential election (Table 7 Col. 1), while the number of days between oral argument and final judgment does not fall for cases without dissents (Col. 2). The average time spent writing the opinion was 132 days (190 days for cases with dissents and 125 days for cases without dissents). The magnitude of the decline in the 3 quarters before the election is substantial and equivalent to the resulting decision taking, on average, 44% less time to publish after the hearing.

In the 100% sample, the average time spent on opinions was 124 days (174 days for cases with dissents and 121 for cases without dissents). The decline in days spent writing an opinion appears statistically significant at the 5% level in the second and third quarter before the election and is equivalent to the resulting decision taking, on average, 10% less time (Col. 3). Cases without dissents take longer in the quarter before an election (Col. 4). The increase may be seasonal because an increase of the same magnitude appears every four quarters.

These results are robust to winsorizing at the 1% level. One reason for the larger magnitudes in the Appeals Court 5% sample is that the manner in which the Appeals Court Database assigned docket numbers may result in a disproportionate share of fast cases, docketed in the same year as the publication.

[25]We assign a value of zero to the few cases where this difference is negative.

4.3 Latent Partisan Identities

Campaign messages can activate latent partisan identities (Ansolabehere and Iyengar 1997), so if judges have partisan identities, priming may influence judges' perception of cases brought before them. The raw data indicates that a large proportion of the increase in dissents comes from ideologically divided panels (Fig. 7A Rows 1-4). Moreover, a significant proportion of these increases come from minority judges (D on DRR panels or R on RDD panels), who almost double their rate of dissent (Rows 6 and 8). Majority judges are also more likely to dissent (Rows 5 and 7). Analyses confirm when these judges dissent, their vote expresses the ideological commitments of the appointing president. The majority judge with the more extreme ideology score (ranked first or third on the three-judge panel) is the one more likely to dissent (Fig. 7B Rows 1-4). Finally, we show that when the minority judges share the same ideology score as a judge in the political majority, in the quarter before a presidential election, this minority judge triples the dissent rate (Fig. 7B Rows 5 and 7). These judges generally do not dissent, but in the quarter before the presidential election, they dissent at the same rate as those with a different ideology score (Row 6).

4.4 Type of Election

Close elections (defined as the winning party achieveing less than 55% of the electoral college vote) increase by 250% the rate of dissent during the contested period before an election (Table 8 Panel A Col. 1). Landslide elections (when the winning party achieves more than 95% of the electoral college vote) reduce by 90% the rate of dissent before an election (Col. 2). During war, elections are unifying, and cases are 1.1 percentage points less likely to have a dissent (Col. 3). In Panel B, we use the universe of cases from 1950-2007 and construct circuit-by-election electoral dissent spikes by taking the difference between the dissent rate in the three quarters before an election with the dissent rate in the three quarters after an election. As the electoral college percent drops from 100 to 50, the dissent spike increases 1.3 percentage points, roughly twice the average dissent spike, which is 0.6 percentage points.

4.5 Political Environment

There is growing evidence that politics in the United States has become more polarized in recent decades (McCarty et al. 2006; Bernhard et al. 2012). Cross (2003) finds evidence that the Reagan and Bush judicial appointees have been the most ideological relative to any judicial appointee since the late 1940's. We show that Reagan, Bush, and Clinton appointees

are the ones most likely to display electoral cycles (Appx. Table D). The increase in the dissent rate three quarters before a presidential election has been growing sharply: close to 0 and statistically insignificant for every appointee before Reagan, then increasing to 2%, 3%, then 6% for the last three set of appointees, statistically significant at the 1%, 5%, and 5% level, respectively. Some interesting patterns emerge when using all the dummy indicators for each quarter: Reagan appointees display an elevated level of dissent in *every* quarter except the quarter right *after* an election. Some electoral cycling appears for earlier appointees but with decreasing intensity and significance as one goes further back in time. We later control for birth cohort as further evidence that the time period, not judge age, drives the electoral cycles.

To begin the time period analysis, we display the 18-year moving average correlation between last quarter and dissent (Fig. 8A). Each year on the x-axis represents the center point for 4 elections. Electoral cycles in dissents are actually quite small during the 1940s, but the cycles increase around 1960. After 1975, these 4-election moving averages of the electoral cycle become statistically significant. Accordingly, we divide the cases into two groups, those cases decided on or prior to December 31, 1975 and those decided afterwards. The electoral cycle before 1975 is quite strong and statistically significant, but the electoral cycle is roughly 100% larger in the recent time period (Table 9 Col. 1).

Since *recent* may be correlated with birth cohort, we also include an interaction between $forties$, a dummy indicator for whether the vote was cast by a judge was born after the 1940s, roughly 15% of the sample. The priming effect of *lastquarter* on dissents lies primarily on *recent* and not birth cohort (Col. 2). This result suggests that the political primeability of judges is not due to cohort-specific experience. Rather, all judges, regardless of birth, become more primeable in the recent time period.

The timing of increase (and decrease) in electoral cycles over time coincide with the correlation between party of appointment and voting valence (Fig. 8B). Each year on the x-axis represents the mid-point a 10-year moving average correlation. Regression analyses including controls indicate that the influence of party of appointment on voting valence is 143% larger in the recent time period (Col. 3) and robust to controls for birth cohort (Col. 4).

To give historical perspective, Fig. 8 suggest that polarization may be present at the very beginning of the dataset, as early as 1925, when President Roosevelt attempted to stack the judiciary with judges that would vote for his New Deal programs. Even so, the contemporary time period is more polarized than during that historical time period. We

replicate the increase in polarization over time in the 100% sample (Fig. 8C). We document additional evidence of growing polarization of the judiciary in the Appendix.

4.6 Previous Experience

We now turn to the role of judicial experience, as experienced individuals are thought to have greater ability at controlling unconscious biases. Experimental research have found that inexperience magnifies priming effects (Krosnick and Kinder 1990). To investigate this hypothesis, we re-estimate our basic specification for sub-samples of judges grouped by the number of years they have served as appellate judges (Table 10).

Overall, judges are 1.3% points more likely to cast a dissenting vote before a presidential election (Row 1). For judges with 1 or 2 years of experience, the magnitude of this effect is a considerably larger 3.3% points. Our estimates are robust to the inclusion of judge fixed effects and triple-clustering at the quarter-year, case, and judge level. Since we control for year fixed effects, the effect of judicial experience is not spuriously due to the recent time period, e.g., the secular increase in dissents over time (Fig. 9). Our estimates are also robust to the exclusion of all controls and keeping only a dummy indicator for the period before the election.

Although the other age groups do not exhibit large cycling behaviors, for the most part, the point estimates are positive and sometimes statistically significant (7-8 years of experience). The fact that inexperienced judges are more likely to dissent before a presidential election is consistent with judges taking awhile to develop the strong professional, conscious commitments that would otherwise control the influence of unconscious bias (Rachlinski, Johnson, Wistrich, Guthrie 2009).

Some types of court experience may increase bias. Federal prosecutors, i.e. those advocating on behalf of the government, have displayed behavior reflecting party politics in federal public corruption prosecutions (Gordon 2009). These kinds of positions are highly political and legal in the sense that they can choose to enforce or not enforce different aspects of federal law; giving priority to specific types of criminal categories—health care fraud, obscenity, immigration cases—is often a political choice (Perry 1998). Moreover, working in this office frequently lead to higher office (Engstrom 1971). For well-known contemporary examples, Rudy Giuliani was U.S. Attorney for the Southern District of New York before becoming Mayor of New York City and ran for U.S. President in 2008. Judges in our study came of age in a time when even Assistant U.S. Attorneys would leave with a change in presidential administration, and 23% of U.S. Attorneys eventually became federal judges (Lochner 2002).

We find that judges who are previous U.S. Attorneys or Assistant U.S. Attorneys are more likely to display electoral cycles and this effect is statistically significant at the 10% level (Table 11 Col. 8) and 5% level with the inclusion of judge fixed effects (Col. 9). Parsimonious specifications that excludes all controls and keeps only previous federal prosecution experience, last quarter, and their interaction reveal the same relationship.

4.7 Wartime

Dissents decrease during wars, whose official dates are indicated by the vertical lines, especially during the onset of wars (Fig. 9). This result is robust to regression controls (Table 12 Col. 1) and in the 100% sample (Col. 2) and somewhat stronger when including only the first half of wartime as the indicator variable. Notably, the decrease in dissent rates during wartime is primarily observed in divided panels (Col. 3). Less experienced judges are, moreover, particularly likely to decrease dissent rates during wartime (Col. 4) and the finding is robust to other experience thresholds. 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.

During wartime judges are also more likely to affirm and less likely to reverse lower court decisions (Col. 6-7) and these effects are statistically significant at the 1% and 5% level, respectively. Moreover, the effect of wartime is about half the size of the effect of electoral proximity in absolute magnitudes. Our results are robust to randomization inference, where we randomly assign the dates of the case to another date (Fig. 9).[26]

4.8 Type of Case

Analysis by 1-digit legal category indicate that electoral cycles are somewhat more pronounced for criminal cases, civil rights cases, and economic activity cases (Appx. Table E Col. 1-7). First amendment cases may also be characterized as having electoral cycles insofar as the period after an election sees a persistent reduction in dissents (Col. 3). Part of this apparent disparity on the category level may be due to the larger sample size for these case categories. When we break the sample into finer case categories, however, it turns out that electoral cycles are greatest for federal criminal cases, civil rights cases about discrimination (in particular, voting rights and discrimination on the basis of race or sex), commercial cases (e.g. contract

[26]We also conduct randomization checks of judicial and case characteristics. Some characteristics are statistically significant. Accordingly, we check and confirm the wartime results are robust to controlling for these characteristics.

breach), and property cases (e.g. eminent domain). Some of the case categories have a small sample size (Col. 8-11).

4.9 Development of Law

We explore the potential contribution of electoral cycles to development of law. Following Choi et al. (2010), we proxy for the quality (or importance) of the opinion using the length of the majority opinion (in pages) and the number of times the opinion has been cited in subsequent cases. The average length of a majority opinion and the average number of citations for cases our sample (after winsorizing at the 1% level) is 4.6 pages and 6.0 citations, respectively.[27] Opinions written when there are dissents are over 40% longer than opinions without dissents (Table 13 Panel A Col. 1),[28] and opinion length increases slightly, albeit insignificantly, before the presidential election.

Furthermore, cases decided when there are dissents are cited more often in subsequent opinions and dissents (Col. 2 and 3). Citations by subsequent dissents suggest controversy or ambiguity in the law. This could occur when precedent begins to spread across circuits and legally innovative cases are cited by dissenting opinions in other courts seeking to follow the new precedent. Opinions written in the quarter preceding an election, however, are cited less often by subsequent dissents than cases decided in other quarters. This reduction in influence on the evolution of legal precedent appears particularly accute for opinions in which a dissent is filed, reducing by 70% the typical association between dissents and citations by subsequent dissents (Col. 3). Similar results are found with log citation counts.

Judges also appear to dissent on procedural, not merit issues, before a presidential election (Col. 4 and 5). On average, 9% of dissents mention procedural but not merit reasons for the dissent. In the quarter immediately before an election, this motivation increases by 8% points when compared to all other quarters.[29] For the entire 1925-2002 time period, we find that only four case characteristics significantly varied with the electoral cycle: (1) whether there were "other issues" related to juries, (2) whether some "other evidence" besides confession and evidence obtained through search and seizure was inadmissible, (3) whether the attorneys' fees favored the appellant, and (4) whether there was some "other issue" of civil law. About 1 to 6% of the cases mention these issues. What is remarkable about all

[27] Appendix Table A.

[28] One may expect majority opinions to be longer if there is a dissent either because cases in which a judge dissents tend to deal with more complex issues or simply because the majority opinion must address the arguments raised by the dissent.

[29] Column 4 does not include quarter-to-election fixed effects because we only have 227 observations, however, the result is robust to the inclusion of these controls.

four of these issues is that they are highly discretionary (e.g., attorneys' fees are only to be awarded to the appellant in exceptional circumstances (*Rolax v. Atlantic C. L. R. Co.* 186 F2d 473)), suggesting that judges may be consciously or subconsciously looking for easier, less direct reasons to dissent before election.

Dissents can also create crowding in the judicial docket, which decreases the attention that judges are able to spend on cases (Huang 2010). We find no significant differences in the rate at cases are reheard en banc (i.e. by the entire court) for opinions with dissents published before presidenial elections. Next, we link 607 of the 18,686 cases to subsequent Supreme Court cases. Cases with dissents are generally more likely to be appealed to the Supreme Court, more likely to be heard by the Supreme Court, and less likely to be reversed by the Supreme Court (Table 13 Panel B). No significant differences for these three outcomes are observed for cases with dissents published in appellate courts before presidential elections. These findings are robust to aggregating to the last three quarters instead of just the last quarter. Despite the apparent procedural or miscellaneous reason for dissent or lower quality of the opinion, cases with dissents before presidential elections appear equally likely to crowd the court dockets as cases with dissents published at other times.

5 Rejection of Incentive-Based Mechanisms

It is natural to think about incentive-based mechanisms for such large changes in behavior, but the facts we uncover are not reasonably explained by any plausible incentive-based mechanism. First, judges sitting in electorally pivotal states are assigned with judges sitting in electorally non-pivotal states to cases and their decisions are promulgated at the circuit, not state level. Even if voters in electorally pivotal states respond to circuit court decisions, judges in all states would have an interest in getting out the vote. Moreover, dissents would not begin to decline for judges in electorally non-pivotal states after the nominating conventions.

Second, judges in electorally pivotal and non-pivotal states alike should be equally affected by coordination breakdowns, collegiality norms, or, for that matter, shifts in unobservables. In particular, these mechanisms would equally affect judges in different duty stations who are randomly assigned to the same case, yet dissents by judges coincide with closeness of their state's popular vote when that state has more electoral votes and with increases in campaign advertisements in the state where a judge resides.

Judges also appear to dissent for highly discretionary, procedural reasons; and various citation measures for an opinion's impact or quality suggest that dissents attributable to

electoral cycles are less likely to contribute to the development of law, which is inconsistent with judges seeking to build a reputation among their state's electorate. Perhaps judges shift their attention to cases that require dissent and away from other cases before an election. However, the dissent rate decreases equally after the election for close elections and landslide elections (where there was no pre-election prime). Moreover, displacing less controversial cases to a later time does not apply to the wartime results, which can last for several years, and court guidelines limit the ability to delay cases.

Perhaps the least outlandish incentive-based explanation is that judges seek promotion to the Supreme Court. Theoretically, it is not clear that dissenting is a good strategy since dissenting could also signal an inability to persuade colleagues and forge a majority coalition on the Supreme Court. In our 5% sample, not a single judge elevated to the Supreme Court chose to dissent before the election, consistent with experience mediating the effects of priming. No positive correlation between dissenting before election and subsequent elevation is found (Table 11 Col. 5). Using a shortlist of appellate judges considered for the Supreme Court (Nemacheck 2008), there is also no partial correlation between dissenting before election and being a potential nominee, consistent with experience mediating the effects of priming. Nor are judges who are about to retire or resign after the election significantly more likely to dissent (Col. 6).

6 Conclusion

Using two datasets from 1925-2007, we present evidence that in the quarters leading up to a presidential election, judges are roughly twice as likely to dissent, vote along partisan lines, and set precedent along partisan lines. We show that dissents coincide with increase in campaign advertisements in states where judges' duty station resides and with closeness of the state's popular vote when that state has more electoral votes. Ideologically polarized environments, inexperience, and previous associative links magnify the electoral cycle. Wartime reduces dissents, and this reduction is exhibited especially in ideologically polarized environments and by judges with characteristics associated with susceptibility to priming. We test and reject incentive-based mechanisms. The electoral cycles we document are larger and more robust than previously-documented electoral cycles.

Our results makes contributions to several literatures. First, group polarization has received increasing attention since 9/11 (Montalvo and Reynal-Querol 2005; Benabou 2012; Golub and Jackson 2012). In recent years, economic experiments have documented that group

identity causes differences in social preferences (Chen and Li 2009; Fong and Luttmer 2009), economic decisions (Benjamin, Choi and Strickland 2010), and public opinion (Gerber, Huber, and Washington 2010), particularly in expressing preferences that favor in-group members. These experiments identify causal links in a laboratory setting by priming group identity. This paper examines the causal link between group identity and ideology, an outcome more closely aligned with the concerns raised by 9/11 about sacred values (Atran et al. 2007), and does so in the field.

Second, a growing literature in economics examines media and political persuasion (Enikolopov et al. 2010; DellaVigna and Gentzkow 2010). Prior results about the priming effects of elections have been re-interpreted to be simply about learning from campaigns (Lenz 2009). The literature primarily examines whether an increase in the prominence of an issue leads individuals to increase the weight given to the issue in regressions of presidential approval or vote choice. When the coefficients increase over the course of a campaign or in lab experiments, researchers have concluded that priming has occurred. The increase, however, could simply be about learning, since the increases have been found to only occur for individuals who learn about parties' positions. Our research design is completely different. Learning and persuasion are not likely to apply to federal appellate judges. In the first instance, judges are not supposed to be learning from elections nor are judges supposed to base their decisions on what they learn about political parties. Even if judges learn from elections, then changes in behavior should persist, but they do not.

Third, our paper contributes to a tradition examining how psychology matters in the field (Edman, Garcia, Norli 2007; Bertrand, Karlan, Mullainathan, Shafir, and Zinman 2010; Card and Dahl 2011), but does so in a setting less subject to economics critique. While market pressures may drive savings and risk preferences towards the rational model (List 2003) and drive out other forms and sources of ideological bias, such as media slant (Mullainathan and Shleifer 2005; Groseclose and Milyo 2005), behavioral anomalies in these courts can have indelible consequences on the development of law (Gennaioli and Shleifer 2007), legitimacy of law, and law-abiding behavior (Tyler 2006).

Fourth, correlations between judges' decisions and their demographic characteristics are poorly understood and are a subject of controversy among economists, political scientists, and legal scholars (Babcock, Loewenstein, Issacharoff, and Camerer 1995; Posner 1973; Cameron 1993; Kornhauser 1999). When judges vote along partisan lines (see, e.g., Sunstein et al. 2006; Segal and Spaeth 2002; Fischman 2011; Shayo and Zussman 2012), are judges simply following legal philosophy (e.g., different formal rules) rather than demonstrating *per se* bias? For

instance, a judge can adhere to a strict interpretation of the Constitution while not necessarily simply hewing to the preferences of a political party. Legal philosophy is generally difficult to distinguish from partisan preferences. In our research design, however, legal philosophy should not be changing with the electoral cycle.

Fifth, even if there is judicial bias, whether the bias is automatic (unconscious) or reflective (conscious) (Kahneman 2011) is another unanswered question. A sizeable experimental literature on judges using vignette studies and convenience samples suggests that unconscious heuristics (e.g., anchoring, status quo bias, availability) play a large role in judicial decision-making (see, e.g., Mussweiler and Strack 2000; Englich, Mussweiler and Strack 2005; Guthrie, Rachlinski, Wistrich 2007; Englich and Soder 2009; Guthrie, Rachlinski, Wistrich 2006; Mussweiler and Englich 2005). Several recent studies suggest that *changes* in behavior may not be conscious. In electroencephalogram (EEG) studies of political priming, people do not recall the stimulus (Morris et al. 2003). Neurocorrelates of behavioral change in individuals are activated by advertisements that affect population behavior, even when individuals do not believe these advertisements to be effective at changing behavior (Falk et al. 2012). Most importantly, when judges are made aware of priming, they control the influence of unconscious bias (Rachlinski, Johnson, Wistrich, Guthrie 2009).

Sixth, a burgeoning literature in economics examines endogenous normative commitments (Benabou 2012; Benabou and Tirole 2010). Social scientists have long speculated on whether people choose the same group because of a shared set of ideas or whether groups impart a set of ideas. Priming group identity isolates the second channel in a naturally occurring setting. Our research design tracks individual behavior over time as well as individual demographic characteristics before a prime to see if individuals from different groups diverge after the prime in their views of the moral or "just" thing to do.

Seventh, we also contribute to the literature on political polarization. Others study the U.S. House and Senate (McCarty et al. 2006), while we show that judicial polarization has increased markedly since the 1970s in the form of heightened electoral cycles in dissents.

Almost without exception, federal appellate judges believe that there is no evidence for ideological bias. In a recent attack on empirical scholarship of judicial decision-making, one prominent judge stated that only a small fraction (5 to 15%) of federal appellate cases are legally indeterminate (i.e., without legal precedent) and that, as experienced members of the legal community, judges recognize which arguments have greater plausibility within the legal community (Edwards and Livermore 2008). Our results raise questions regarding the independence or partisanship of the federal judiciary. U.S. judges have a strong profes-

sional, conscious commitment to be unbiased, yet display behavior indicating that judicial partisanship is not simply about differences in legal philosophy. If elite U.S. judges are in fact susceptible to priming via the partisan nature of electoral cycles, then highly trained individuals may be susceptible to other forms of priming regardless of their professional commitments to be unbiased.

Part of the effect we find might be attributable to priming of people around the judge such as family members or clerks, which have indirect effects on the judge. This does not diminish the economic importance of priming. If even half the priming was due to such indirect pathways, the direct priming of judges would still be statistically and economically significant. Further research should investigate how individuals and institutions can avoid or compensate for priming. For example, is mere awareness of primings effects' enough or can one counter-prime oneself by uniting stimuli (such as those that occur in wartime)?

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A Evolution of the Judiciary Over Time

We document additional evidence of polarization in the judiciary growing over time. First, we show that appellate judges are *less* likely to retire in each of the three quarters preceding a presidential election when the party of the President at the time the judge leaves is *different* from the party of the President that appointed the judge. Retiring (taking senior status) results in a reduced caseload for the judge assuming such status and, most importantly, allows an incumbent President to appoint a new judge. Judges are also *more* likely to resign in each of the four quarters after a presidential election, when the party of the President at the time

the judge leaves is the *same* as the party of the President that appointed the judge. The sclerotization of the normal churning of judges to reflect the preferences of the electorate may cause the judiciary to become more polarized over time. Recent studies of the relationship between politics and judicial retirements in appellate courts have conducted the analysis at a yearly level rather than quarter-to-election dummies, so they have not found electoral cycles in judicial turnover rates (Yoon 2006, Spriggs and Wahlbeck 1995, Stolzenberg and Lindgren 2010, Zuk et al. 1993).

For our analysis in this section we use the entire data from 1802-2004 from the Multi-User Data Base on the Attributes of U.S. Appeals Court Judges to sum up the number of retirements (resignations) per month. The rate for retirements fluctuates across the political cycle. In particular, this rate is relatively low in the months immediately preceding a presidential election and relatively high in the months immediately following such election. On average, 0.14 judges voluntarily leave the bench each month in our sample; of these 0.12 are retirements and 0.02 are resignations (Appx. Table A). 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 (Appx. Table F Col. 2). These effects are statistically significant at the 1% or 5% level and much larger in magnitude than the other quarters.[30] In each of the four quarters after a presidential election, the number of resignations for judges when the party in power is the same increases by 0.02-0.04 per month (Col. 3).[31] These effects are therefore substantial relative to the average rate of judicial exits per month. The patterns are robust to the same set of checks as in Table 2. These checks include alternative measures of electoral proximity, dropping one circuit at a time, and disaggregating the data to the number of retirements per month and by circuit and including circuit fixed effects and clustering the standard errors at the circuit level.[32]

These patterns in the number of judges voluntarily leaving at the beginning or end of a presidential electoral cycle is likely to be driven by political considerations (e.g., a judge may expect the President-elect to appoint someone from the President's own party). We find that these electoral cycles have been increasing after 1975. We estimate:

[30]There is one other quarter that is significant at the 10% level.

[31]It is important to note that quarter 16, which contains parts of November, December, January, and part of February is the omitted quarter, which has a coefficient of 0. Thus the coefficients on quarters 12-15 are estimated to be significant relative to the quarter right after, not relative to the election date. When we omit quarter 1 instead of quarter 16, the coefficients on quarters 12-15 are still statistically significant and increase somewhat in magnitude.

[32]These patterns are slightly more pronounced for Republican appointees.

$$Retire_i = F(t) + \beta_1 After_i * Recent_i + \beta_2 After_i + \beta_3 Recent_i + \varepsilon_i \quad (5)$$

where $F(t)$ are year and quarter fixed effects; $After_i$ is an indicator equal to 1 for the three months immediately following a presidential election; and $Recent_i$ is an indicator variable equal to 1 for the period of time after 1975. The higher rate of voluntary retirements following an election appears entirely attributable to the post-1980 period. In fact, the 20-year moving average correlation between retirement decision and whether it is after the election (Appx. Fig. A) suggests that the electoral cycles we observe in judicial retirement decisions may be entirely a recent phenomenon.

Figure 1A: Dissent Rate across the Political Cycle (Quarterly)
5% Sample (1925-2002)

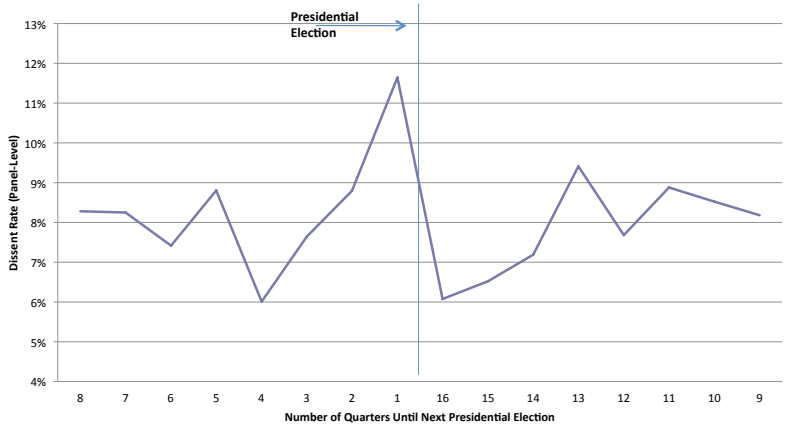


Figure 1B: Dissent Rate across the Political Cycle (Monthly)
5% Sample (1925-2002)

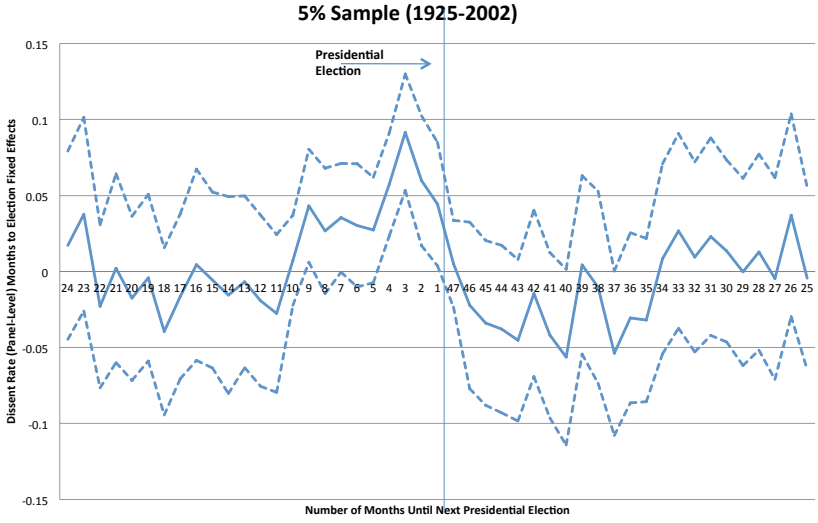
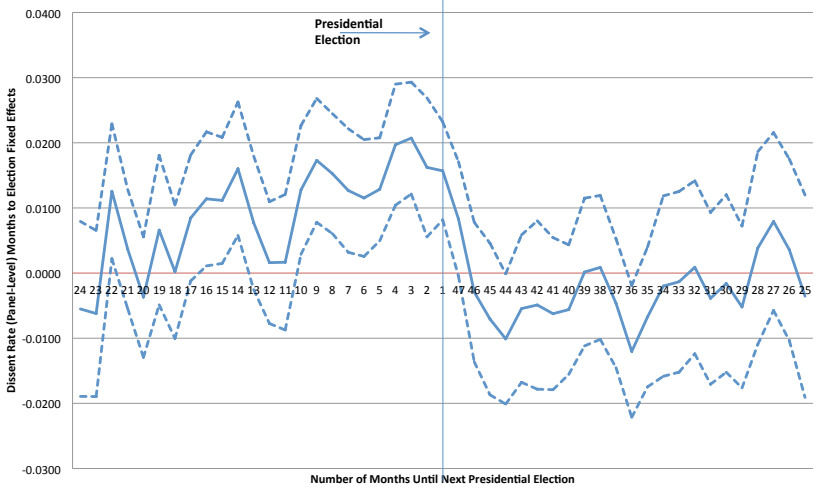


Figure 1C: Dissent Rate across the Political Cycle (Monthly)
100% Sample (1950-2007)



Notes: Figures 1A and 1B display dissents with or without dissenting opinions
Figure 1C displays dissents with dissenting opinions

Figure 2A: Political News Articles across Political Cycle (Quarterly)
New York Times 1900-2007

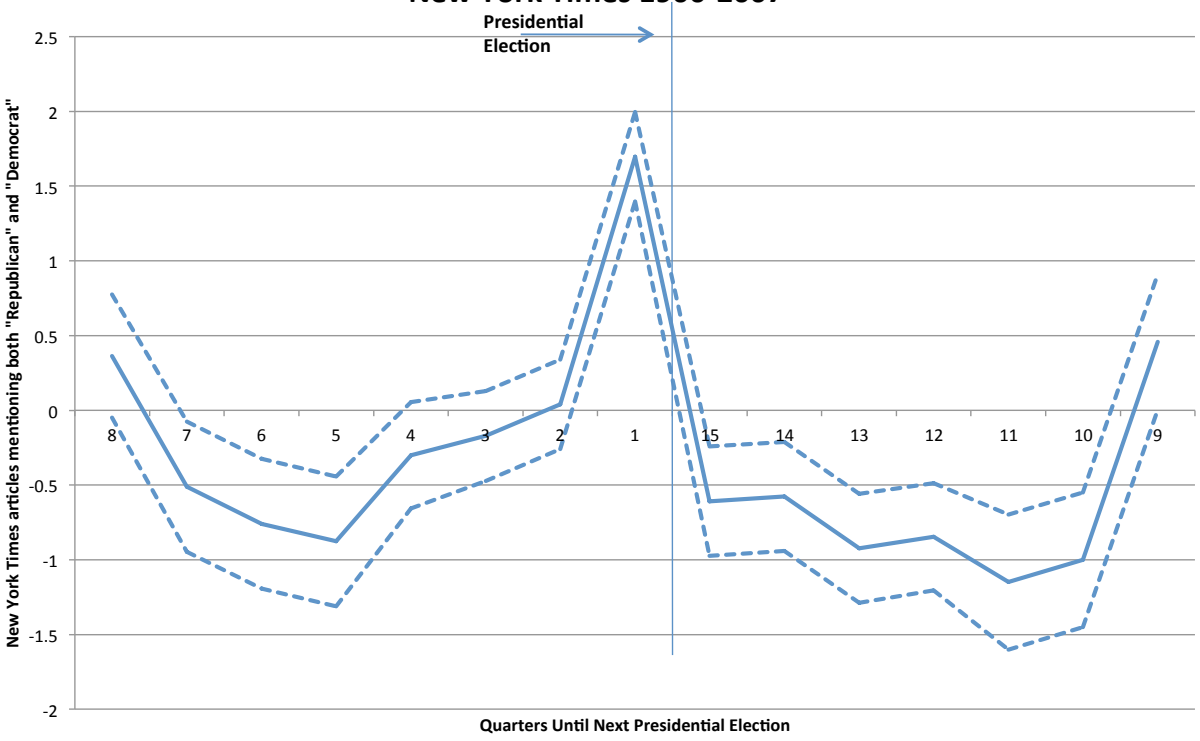


Figure 2B: Political News Articles across Political Cycle (Monthly)
New York Times 1900-2007

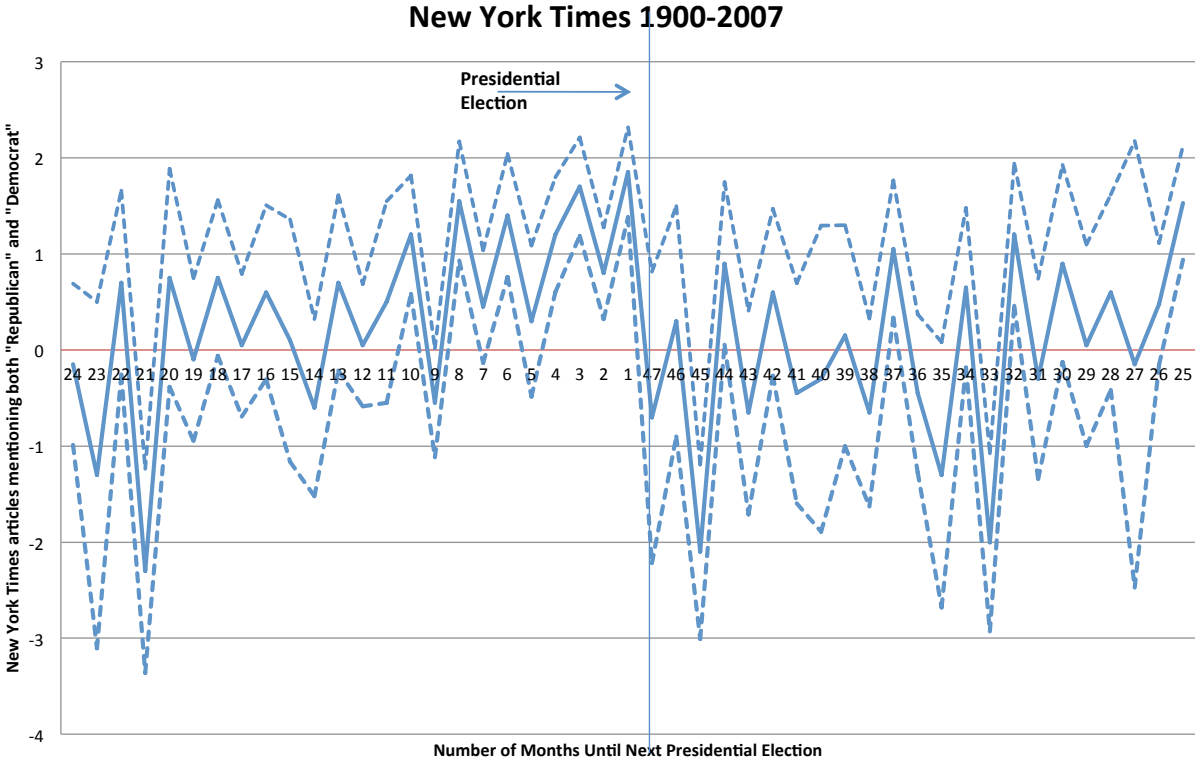


Figure 3A: Influence of Party of Appointment on Voting Valence across Political Cycle (Quarterly)

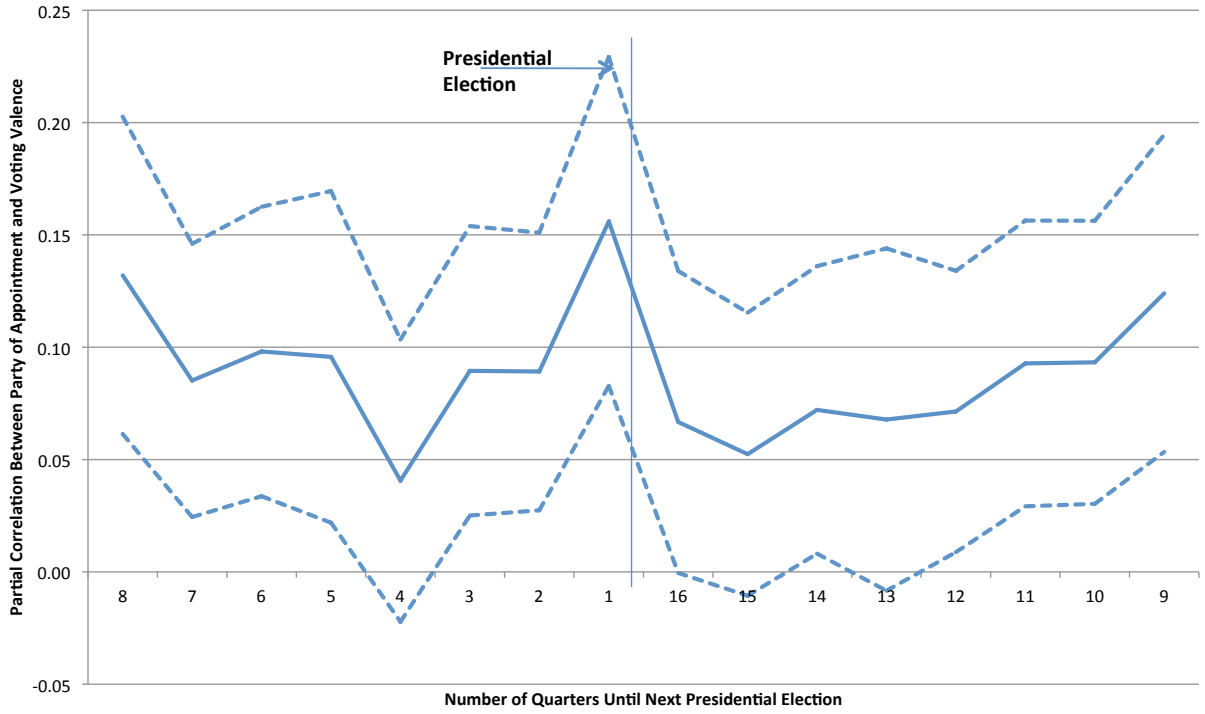


Figure 3B: Electoral Cycles in Voting Valence by Ideology Score

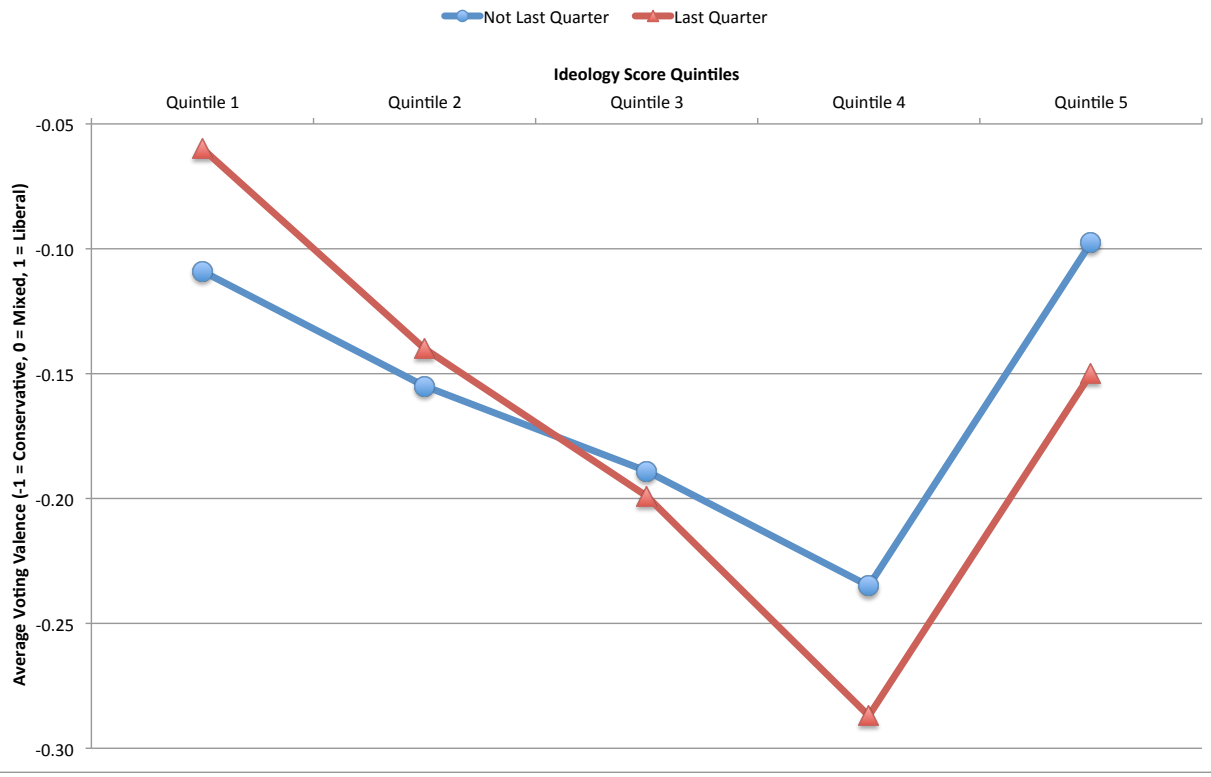
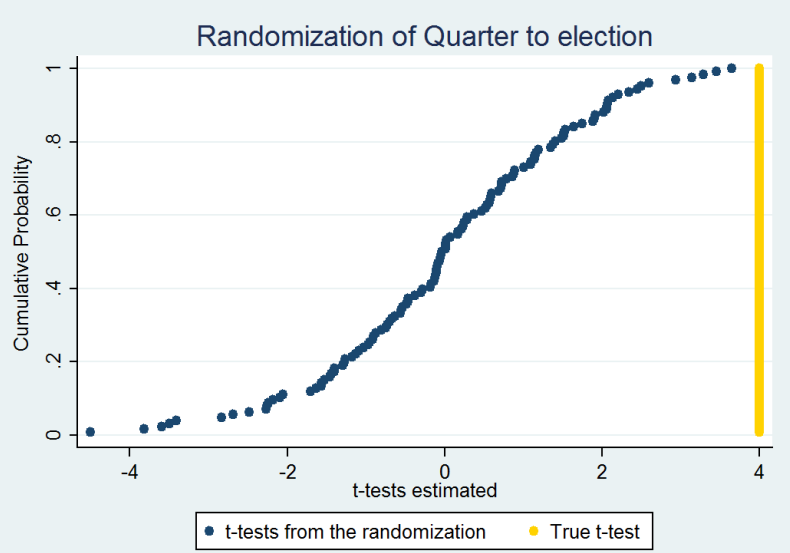
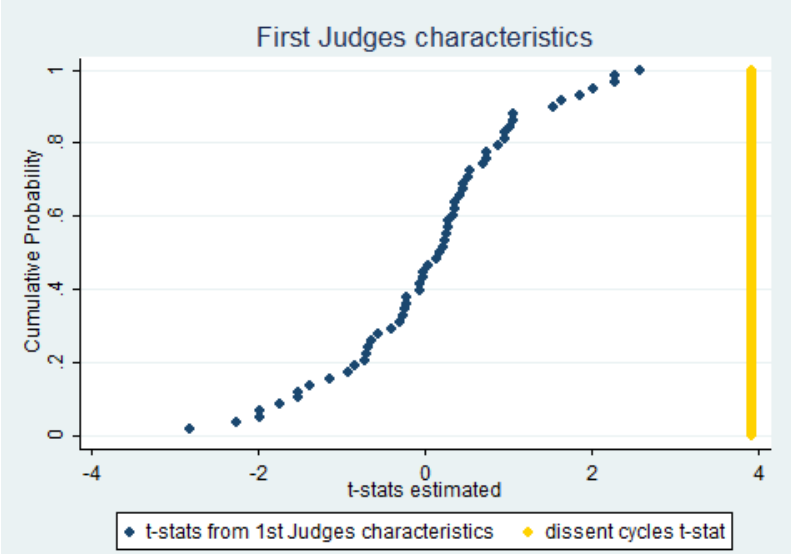
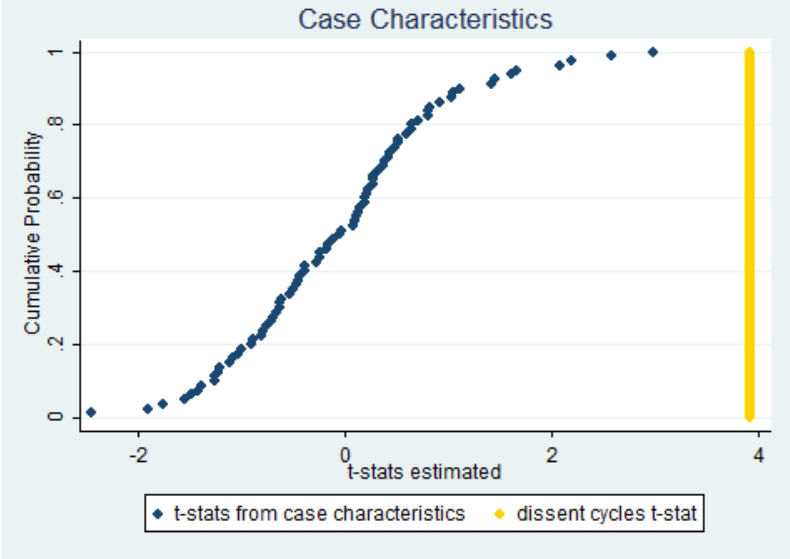


Figure 4: Randomization Inference and Randomization Checks







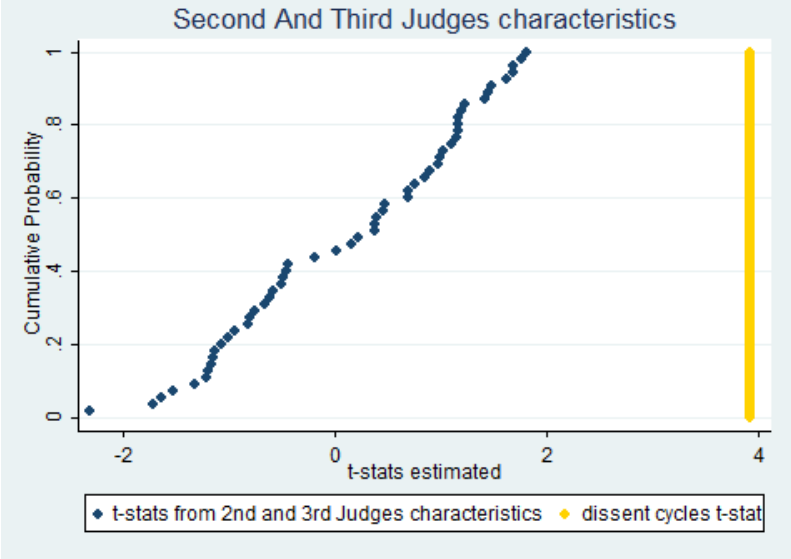


Figure 5: Campaign Advertisements across Political Cycle (Weekly)

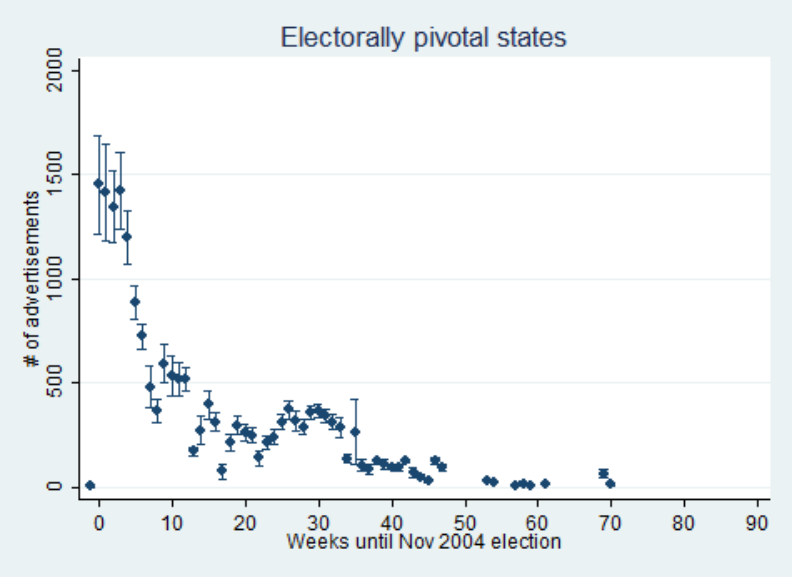


Figure 5A

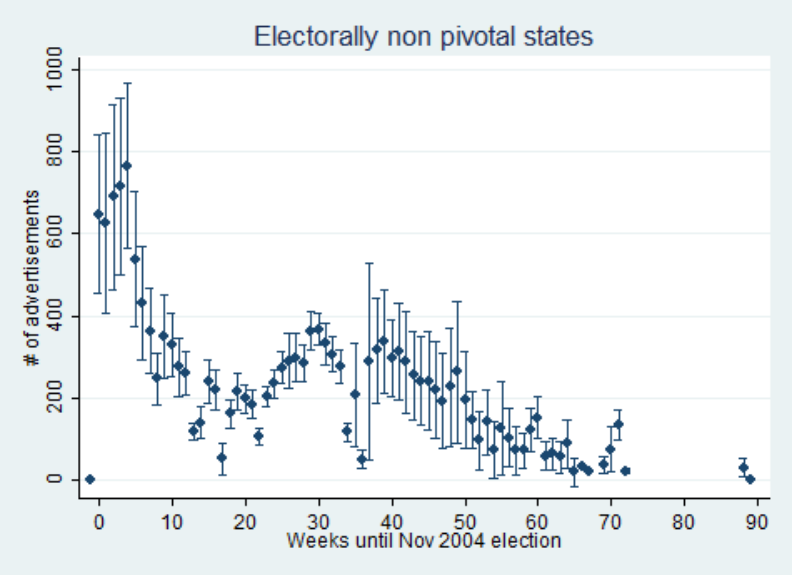


Figure 5B

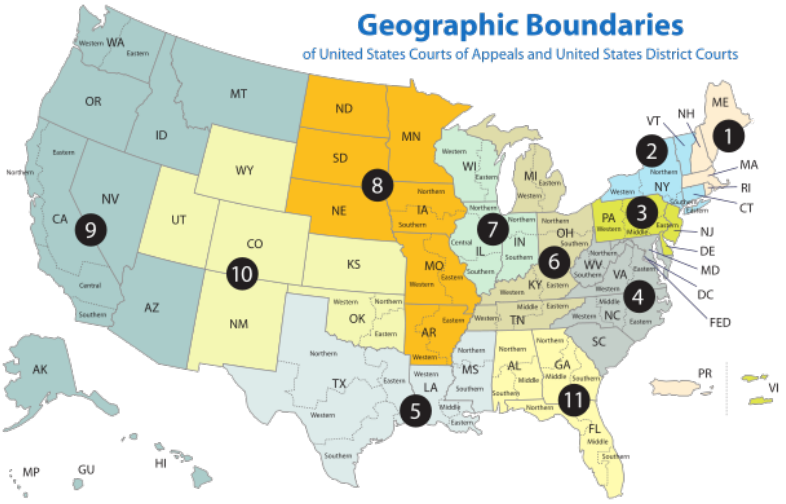


Figure 5C

Figure 6A: Dissent Rate across the Political Cycle (Monthly)
Electorally Pivotal States

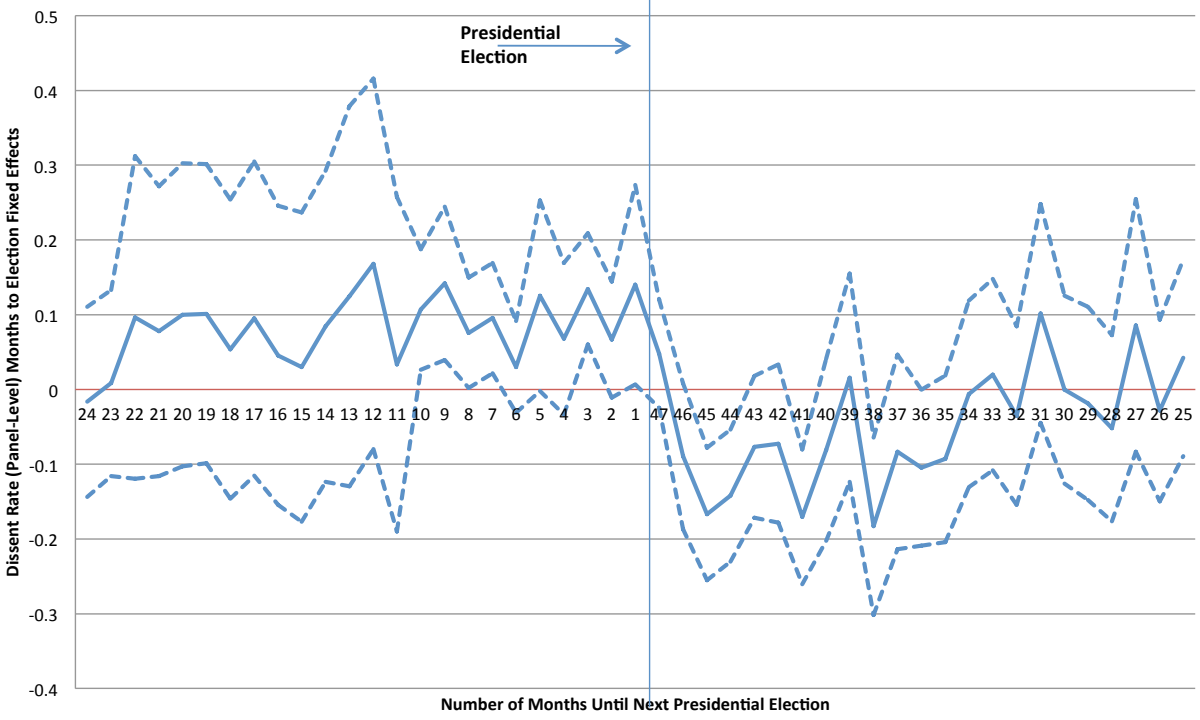


Figure 6B: Dissent Rate across the Political Cycle (Monthly)
Electorally Non-Pivotal States

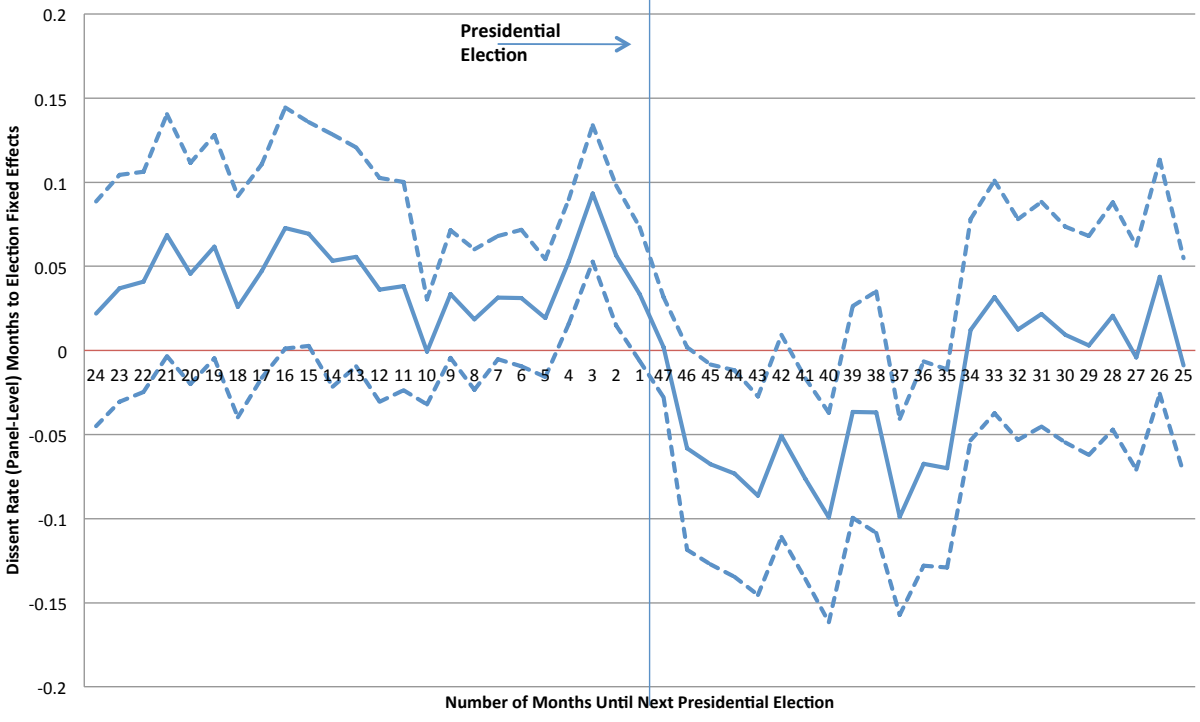


Figure 7A: The Role of Judicial Panel Characteristics in Electoral Cycles in Dissents

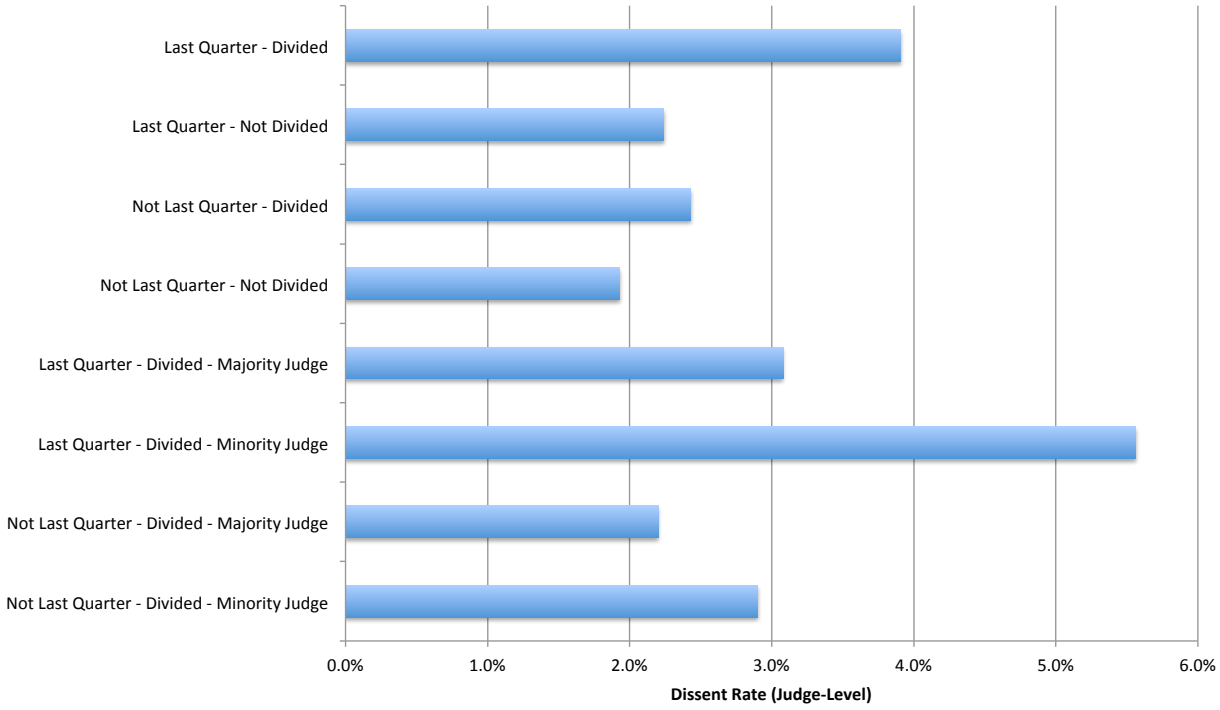
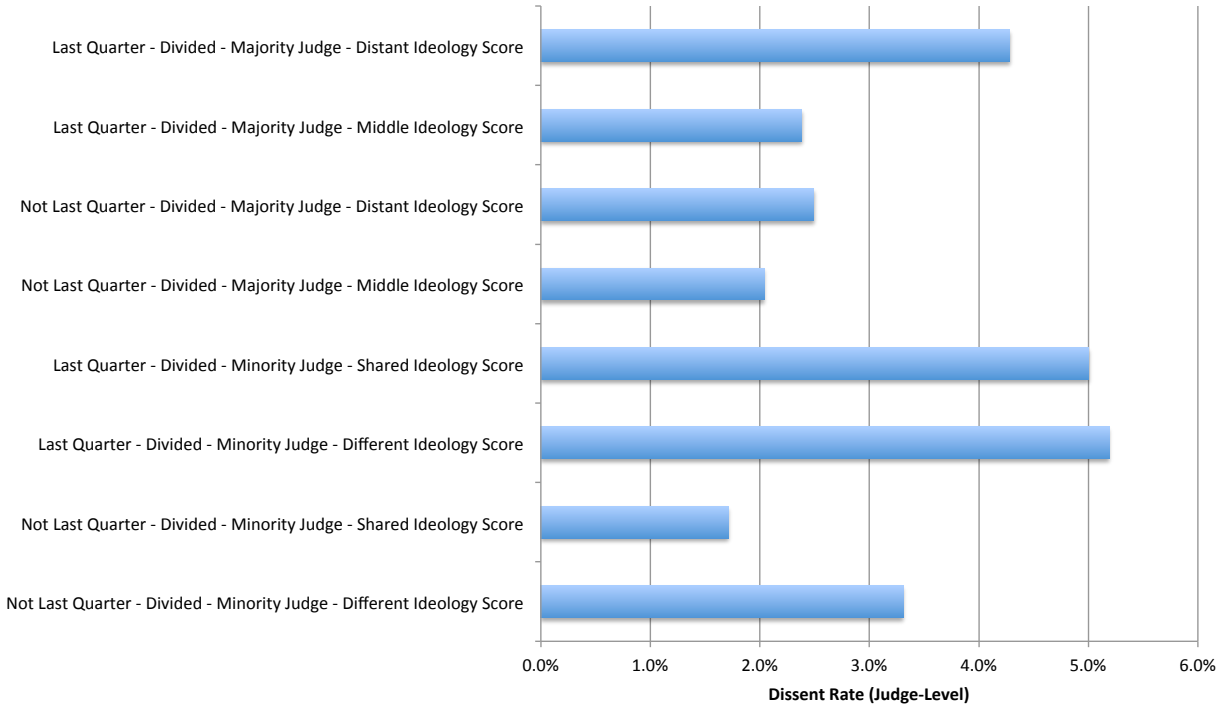


Figure 7B: The Role of Judicial Ideology Score in Electoral Cycles in Dissents



Notes: Politically Divided Panels are RRD or DDR; Non-Divided Panels are DDD or RRR.
Majority Judge is R in RRD panels and D in DDR panels; Minority Judge is D in RRD and R in DDR panels.

Figure 8A: Increase in Electoral Cycles in Dissents over Time (5% Sample)

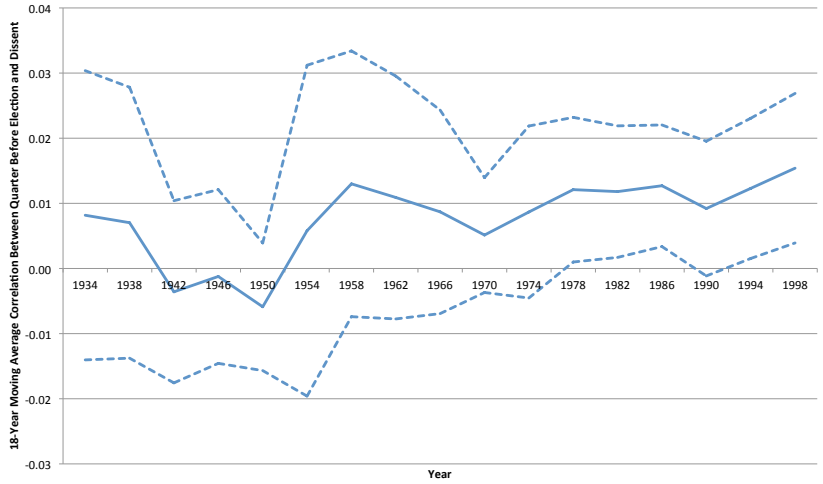


Figure 8B: Influence of Party of Appointment on Voting Valence Over Time (5% Sample)

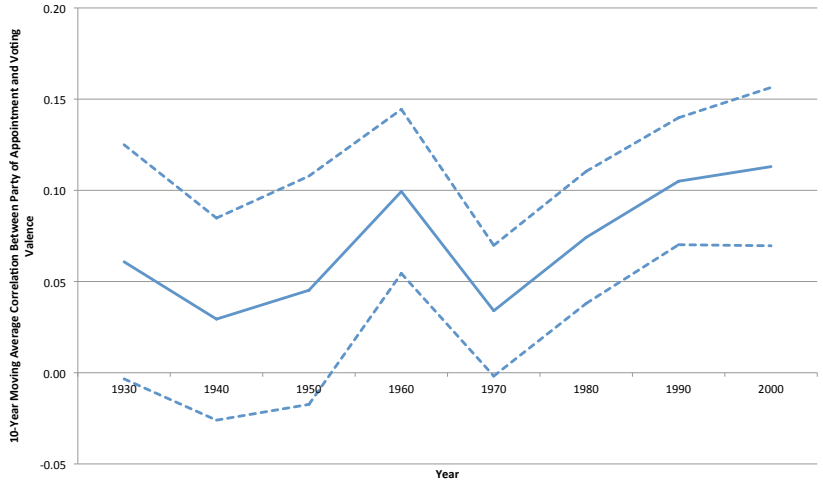


Figure 8C: Increase in Electoral Cycles in Dissents over Time (100% Sample)

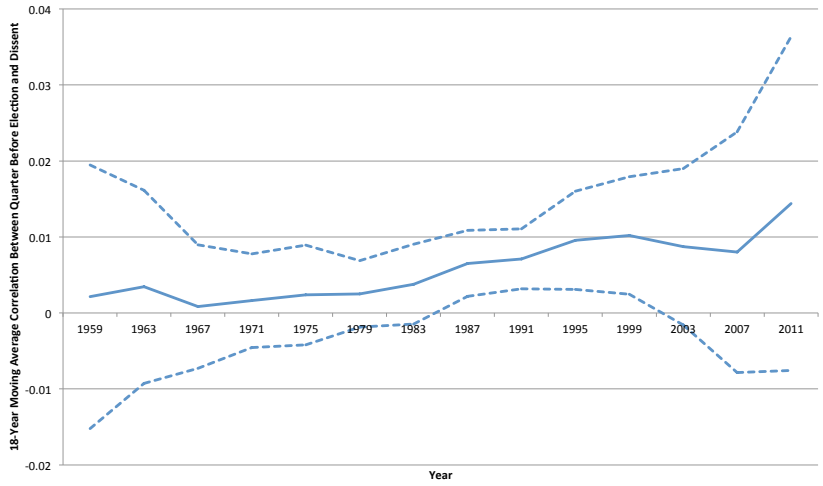
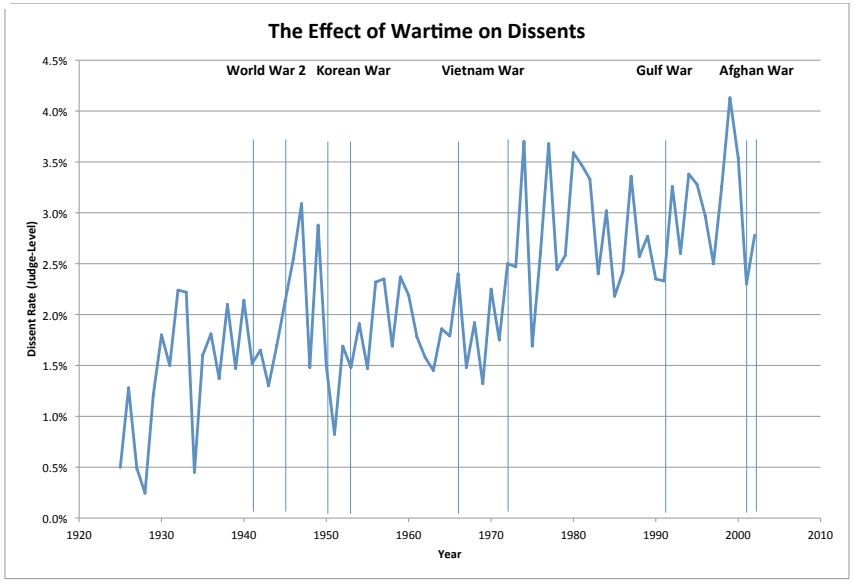
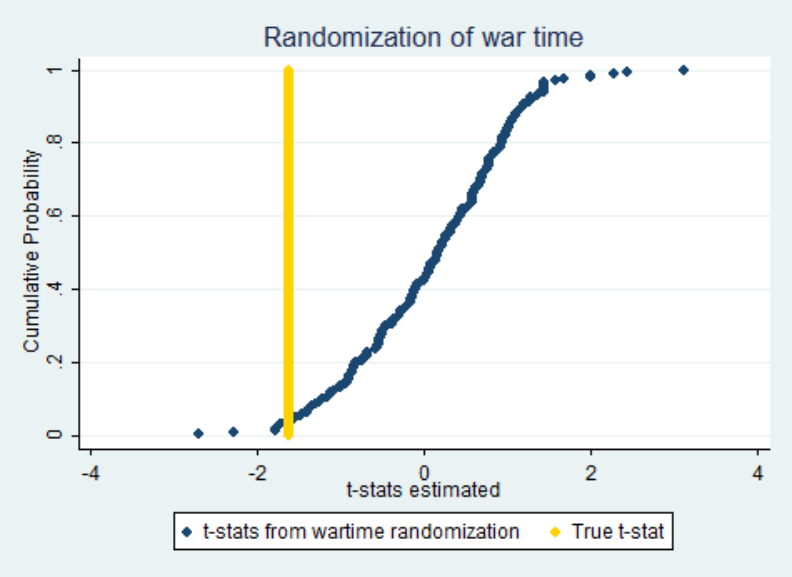


Figure 9: Dissents During Wartime



Case-Level



Vote-Level

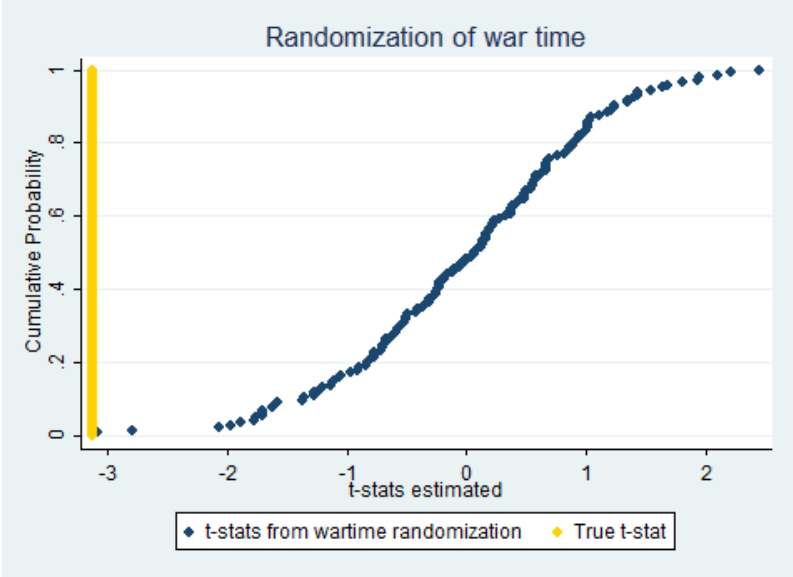


Table 1: Electoral Cycles in Dissents

	Dissent (2-1 Decision) with or without Dissenting Opinion				Dissent (2-1 Decision) with Dissenting Opinion			
	Ordinary Least Squares		Probit		Ordinary Least Squares		Probit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Divided (DRR or RDD)	0.0157	0.0153	0.114	0.111				
	[0.00452]***	[0.00451]***	[0.0327]***	[0.0328]***				
Quartertoelect = 1	0.0637	0.0527	0.448	0.377	0.0113	0.00847	0.0962	0.0724
	[0.0123]***	[0.0132]***	[0.0857]***	[0.0936]***	[0.00323]***	[0.00337]**	[0.0286]***	[0.0303]**
Quartertoelect = 2	0.0347	0.0255	0.284	0.224	0.00785	0.00474	0.0705	0.0441
	[0.0121]***	[0.0138]*	[0.0960]***	[0.105]**	[0.00292]***	[0.00318]	[0.0263]***	[0.0285]
Quartertoelect = 3	0.0325	0.0302	0.270	0.256	0.00782	0.00445	0.0704	0.0414
	[0.0123]***	[0.0134]**	[0.0982]***	[0.103]**	[0.00318]**	[0.00331]	[0.0284]**	[0.0295]
Quartertoelect = 4	0.00581	0.00578	0.0444	0.0481	0.00153	0.00158	0.0138	0.0153
	[0.0111]	[0.0111]	[0.0963]	[0.0962]	[0.00399]	[0.00368]	[0.0365]	[0.0333]
Quartertoelect = 5	0.0209	0.0102	0.170	0.101	0.00747	0.00454	0.0684	0.0449
	[0.0152]	[0.0156]	[0.119]	[0.122]	[0.00465]	[0.00450]	[0.0422]	[0.0407]
Quartertoelect = 6	0.0120	0.00302	0.0970	0.0418	0.00496	0.00185	0.0451	0.0196
	[0.0141]	[0.0155]	[0.114]	[0.118]	[0.00460]	[0.00455]	[0.0419]	[0.0407]
Quartertoelect = 7	0.0226	0.0194	0.178	0.159	0.0000166	-0.00330	-0.000524	-0.0280
	[0.0141]	[0.0150]	[0.111]	[0.113]	[0.00470]	[0.00448]	[0.0431]	[0.0403]
Quartertoelect = 8	0.00772	0.00859	0.0521	0.0644	0.00519	0.00528	0.0455	0.0464
	[0.0141]	[0.0141]	[0.107]	[0.106]	[0.00446]	[0.00415]	[0.0405]	[0.0370]
Quartertoelect = 9	-0.0115	-0.0218	-0.0717	-0.138	0.0120	0.00891	0.103	0.0759
	[0.0155]	[0.0157]	[0.112]	[0.114]	[0.00500]**	[0.00490]*	[0.0443]**	[0.0427]*
Quartertoelect = 10	-0.0114	-0.0193	-0.0779	-0.128	0.00647	0.00326	0.0581	0.0301
	[0.0160]	[0.0174]	[0.115]	[0.122]	[0.00482]	[0.00490]	[0.0434]	[0.0430]
Quartertoelect = 11	0.000311	-0.00142	0.00509	-0.00295	0.00706	0.00364	0.0623	0.0323
	[0.0162]	[0.0171]	[0.116]	[0.118]	[0.00499]	[0.00497]	[0.0450]	[0.0438]
Quartertoelect = 12	-0.0102	-0.00912	-0.0628	-0.0521	-0.00102	-0.00117	-0.0101	-0.0100
	[0.0128]	[0.0129]	[0.0900]	[0.0903]	[0.00382]	[0.00351]	[0.0336]	[0.0302]
Quartertoelect = 13	0.00115	-0.0101	0.00433	-0.0726	0.00450	0.00141	0.0347	0.00970
	[0.0148]	[0.0148]	[0.0961]	[0.0980]	[0.00385]	[0.00374]	[0.0330]	[0.0316]
Quartertoelect = 14	-0.0157	-0.0243	-0.105	-0.157	0.000920	-0.00234	0.00590	-0.0210
	[0.0134]	[0.0151]	[0.0940]	[0.103]	[0.00382]	[0.00391]	[0.0330]	[0.0329]
Quartertoelect = 15	-0.0176	-0.0194	-0.121	-0.131	-0.000372	-0.00386	-0.00561	-0.0348
	[0.0117]	[0.0127]	[0.0788]	[0.0832]	[0.00391]	[0.00377]	[0.0335]	[0.0312]
Controls	N	Y	N	Y	N	Y	N	Y
Observations	18686	18686	18686	18686	263388	263388	263388	263388
R-squared	0.019	0.022			0.012	0.013		

Notes: Robust standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Data in columns (1)-(4) come from U.S. Courts of Appeals Database (1925-2002) and data in columns (5)-(8) come from our 100% data collection from 1950-2007. Standard errors are clustered at the quarter-year level. The outcome variable is a dummy variable equal to 1 if there was a dissenting opinion in the case. The explanatory variables of interest are dummy variables indicating the number of quarters remaining before the presidential election (16 quarters to the election is the omitted dummy variable) and a dummy variable equal to 1 if the panel deciding the case was divided along ideological lines. All regressions include year fixed effects and circuit fixed effects. Columns (2) and (4) include legal issues fixed effects and quarter fixed effects. Columns (6) and (8) include quarter fixed effects.

Table 2: Electoral Cycles in Dissents - Robustness Checks

	Dissent (2-1 Decision)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Each coefficient represents a separate OLS regression				Probit	Logit
		Drop 1 Circuit		Keep 1 Circuit		
Quarters to Election	-0.00242					
	[0.000700]***					
Lastquarter		0.0527			0.0405	0.0405
(Entire Sample)		[0.0132]***			[0.0113]***	[0.0113]***
Lastquarter		0.0532	0.0545	-0.00368		
Circuit 1		[0.0142]***	[0.0398]	[0.0108]		
Lastquarter		0.0548	0.0383	0.00156		
Circuit 2		[0.0139]***	[0.0589]	[0.0107]		
Lastquarter		0.0444	0.164	0.0119		
Circuit 3		[0.0134]***	[0.0651]**	[0.0137]		
Lastquarter		0.0573	0.0102	0.0127		
Circuit 4		[0.0136]***	[0.0552]	[0.0153]		
Lastquarter		0.0586	0.0128	0.00888		
Circuit 5		[0.0137]***	[0.0286]	[0.00812]		
Lastquarter		0.0492	0.102	0.0348		
Circuit 6		[0.0125]***	[0.0500]**	[0.0115]***		
Lastquarter		0.0523	0.0567	0.0208		
Circuit 7		[0.0139]***	[0.0378]	[0.00871]**		
Lastquarter		0.0568	0.00822	0.0122		
Circuit 8		[0.0140]***	[0.0453]	[0.0110]		
Lastquarter		0.0521	0.0669	-0.0121		
Circuit 9		[0.0130]***	[0.0371]*	[0.00689]*		
Lastquarter		0.0578	0.00819	0.00254		
Circuit 10		[0.0147]***	[0.0320]	[0.0100]		
Lastquarter		0.0503	0.192	0.0211		
Circuit 11		[0.0136]***	[0.0590]***	[0.00822]**		
Lastquarter		0.0471	0.135	0.0124		
Circuit 12		[0.0145]***	[0.0614]**	[0.0135]		

Notes: Robust and clustered standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Data come from U.S. Courts of Appeals Database (1925-2002) except column (3), which comes from our 100% data collection (1950-2007). The explanatory variables of interest is a dummy variable indicating whether it is the last quarter before an election (columns 2-5) or a continuous variable for quarters to election (column 1). The regression also includes quarter to election fixed effects (columns 2-5 only), circuit fixed effects, legal issues fixed effects, year fixed effects, and seasonly quarter fixed effects. Marginal effects from a probit and logit specification of dissent on a dummy variable displayed for the last quarter only in columns (4) and (5).

Table 3: Electoral Cycles in the Influence of Party of Appointment on Judges' Votes and Case Outcomes

	Liberal Vote			
Code:	(+1/0/-1)		(+1 vs. 0/-1)	(+1/0 vs. -1)
	Ordinary Least Squares	Ordered Probit	Probit	Probit
Panel A: All Judges	(1)	(2)	(3)	(4)
Lastquarter	-0.0337	-0.0507	-0.0528	-0.0472
	[0.0348]	[0.0497]	[0.0527]	[0.0571]
Appointed by Democrat	0.0707	0.0988	0.100	0.0993
	[0.00820]***	[0.0115]***	[0.0119]***	[0.0127]***
Appointed by Democrat	0.0707	0.0955	0.113	0.0867
* Lastquarter	[0.0367]*	[0.0497]*	[0.0577]**	[0.0488]*
Controls	Y	Y	Y	Y
Observations	56058	56058	56058	56058
R-squared	0.087			
Panel B: Politically Unified Panels (DDD or RRR)		Liberal Precedent		
Lastquarter	-0.194	-0.282	-0.225	-0.325
	[0.105]*	[0.154]*	[0.164]	[0.161]**
Appointed by Democrat	0.163	0.232	0.217	0.247
	[0.0303]***	[0.0423]***	[0.0468]***	[0.0447]***
Appointed by Democrat	0.208	0.288	0.237	0.345
* Lastquarter	[0.126]*	[0.178]	[0.193]	[0.183]*
Controls	Y	Y	Y	Y
Observations	5659	5659	5659	5659
R-squared	0.100			
Panel C: Electoral Cycles in the Affirmations and Reversals of Lower Courts				
	Affirm	Reverse	Reverse and Remand	Reverse No Remand
		Ordinary Least Squares		
Lastquarter	-0.0515	0.0414	0.0503	-0.00489
	[0.0240]**	[0.0163]**	[0.0218]**	[0.0125]
Controls	Y	Y	Y	Y
Observations	18686	18686	18686	18686
R-squared	0.054	0.024	0.036	0.016

Notes: Robust and clustered standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Panel A: Vote-level regression. OLS standard errors are double-clustered at the quarter-year and case level; probit and ordered probit standard errors are clustered at the quarter-year level. The outcome variable is Liberal Vote, which is coded as 1 for liberal, 0 for mixed or not applicable, and -1 for conservative. The explanatory variables of interest are a dummy variable indicating whether the case was decided in the quarter immediately preceding a presidential election, a dummy variable indicating whether the judge was appointed by a Democratic President and an interaction between these two variables. Regressions include quarter-to-election fixed effects, circuit fixed effects, legal issues fixed effects, year fixed effects, quarter fixed effects, and a dummy variable indicating whether the panel is divided. Panel B: Case-level regression. The outcome variable is Liberal Precedent. Standard errors clustered at the quarter-year level. The explanatory variables of interest are a dummy variable indicating whether the case was decided in the quarter immediately preceding a presidential election, a dummy variable indicating whether all three members of the panel were appointed by a Democratic President and an interaction between these two variables. Control variables same as in Panel A. Panel C: Case-level regression. Robust standard errors, clustered at the quarter-year level. In column (1) the outcome variable is a dummy variable equal to 1 if the case affirmed the decision being reviewed; while the outcome variable in column (2) is a dummy variable equal to 1 if the case reversed the decision being reviewed; the outcome in column (3) is a dummy variable equal to 1 if the court requested the lower court to re-evaluate (perhaps with a new trial); the outcome in column (4) is a dummy variable equal to 1 if court determined the final outcome of the litigants in the original case. The explanatory variables of interest is a dummy variable equal to 1 if a case was decided in the quarter immediately preceding a presidential election. Controls are quarter-to-election fixed effects, year fixed effects, circuit fixed effects and legal issues fixed effects.

Table 4: Electoral Cycles in Dissents by State of Judge's Duty Location

	Dissenting Vote					
Sample:	Top 8 States in Electoral Votes Count and DC	States with Fewer Electoral Votes	States Likely to be Electorally Pivotal and DC	States Not Likely to be Electorally Pivotal	States in Top Quartile of Campaign Ads	States below Top Quartile of Campaign Ads
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
Divided (DRR or RDD)	0.00925	0.00411	0.0108	0.00403	0.00571	0.00517
	[0.00238]***	[0.00183]**	[0.00304]***	[0.00166]**	[0.00300]*	[0.00158]***
Lastquarter	0.0236	0.00400	0.0316	0.00503	0.0291	0.00762
	[0.00670]***	[0.00548]	[0.00986]***	[0.00501]	[0.00850]***	[0.00404]*
Controls	Y	Y	Y	Y	Y	Y
Observations	20145	31315	12642	38818	11246	36672
R-squared	0.010	0.009	0.016	0.007	0.015	0.007

Panel B	Dissent Rate in Three Quarters Before Election - Dissent Rate in Three Quarters After Election	
	(1)	(2)
Electoral Vote Count	0.00160	0.000786
	[0.00114]	[0.00126]
Popular Vote Tightness	-0.0801	-0.0845
	[0.0772]	[0.0947]
Electoral Vote Count	0.0118	0.0121
* Popular Vote Tightness	[0.00622]*	[0.00702]*
Controls	N	Y
Observations	593	593
R-squared	0.007	0.026

Notes: Robust and clustered standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Panel A: The outcome variable is a dissenting vote. The top 8 states in electoral vote count are CA, FL, IL, MI, NY, OH, PA, and TX. States likely to be electorally pivotal are: MI, OH, PA, FL, and CA. States in top quartile of campaign ads for 1996 were CA, FL, MI, OH, and TN. Standard errors double clustered at the case level and quarter-year level. The explanatory variable of interest are a dummy variable indicating whether the case was decided in the quarter immediately preceding a presidential election. Regressions include quarter-to-election fixed effects, circuit fixed effects, legal issues fixed effects, year fixed effects, quarter fixed effects, and a dummy variable indicating whether the panel is divided. Panel B: Each election-state is a separate observation. Electoral Vote Count is the number of electoral college votes for a given state in a given election, which ranges from 3 to 55. Popular Vote Tightness is the negative of the absolute value of the difference in the popular vote fraction won by Republicans vs. won by Democrats. Values that are more positive indicate a tighter election. Mean dependent variable, the "electoral spike" in dissents, is 0.035, and winsorized at the 1% level. Column (2) includes circuit fixed effects.

Table 5: Dissent of Judges and Campaign Advertisements by State of Judge's Duty Location

	Dissent Vote						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ΔCampaign Ads (t0)	0.00725	0.00998	0.0100	0.00810	0.00871	0.0223	0.0251
	[0.00316]**	[0.00475]**	[0.00487]**	[0.00479]*	[0.00551]	[0.0103]**	[0.0156]
ΔCampaign Ads (t1)		0.00824	0.00877	0.00430	0.00469		
		[0.00817]	[0.00870]	[0.00910]	[0.0116]		
ΔCampaign Ads (t2)			-0.00500	-0.00285	-0.00455		
			[0.0125]	[0.0127]	[0.0127]		
ΔCampaign Ads (f1)						0.00775	0.00893
						[0.00538]	[0.0112]
ΔCampaign Ads (f2)							0.00329
							[0.00535]
Controls	N	N	N	Y	Y*	N	N
N	7410	6674	5864	5864	5864	6674	6036
R-sq	0.000	0.001	0.001	0.012	0.086	0.001	0.001

Notes: Robust and clustered standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Campaign ads come from the Wisconsin Ads project for the November 2004 election. Appellate court data for the corresponding months before the election and state were linked to the campaign ad data in the same month and state before the November 2004 election in the Wisconsin Ads database. OLS standard errors are double-clustered at the quarter-year and case level. The outcome variable is a dummy variable equal to 1 if there was a dissenting vote. The explanatory variable of interest in column (1) is the change in number of campaign advertisements (10,000s) in the state of the judge's duty station in the month the decision was published. The explanatory variables of interest in columns (2)-(7) are lags and leads of changes in campaign advertisements. Controls in column (4) include circuit fixed effects and year fixed effects. Column (5) also includes judge fixed effects.

Table 6: Electoral Cycles in Dissents with Placebo Dates (Other Key Milestones of Cases)

	Dissent (2-1 Decision) - 100% Sample (1971-2006)									
	Publication Date	Docket Date	Date Filed in District Court	Notice of Appeal Filed	Date Brief Notice Issued	Date of Last Brief Filing	Submitted on Merits	Date of Oral Argument	Final Judgment Date	Publication Date
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)*
Quartertoelect = 1	0.00847	-0.00239	0.00467	0.00436	-0.00503	0.00695	0.0102	0.00323	0.00721	0.00908
	[0.00337]**	[0.00357]	[0.00335]	[0.00342]	[0.00688]	[0.00429]	[0.00911]	[0.0101]	[0.00330]**	[0.00328]***
Quartertoelect = 2	0.00474	-0.00469	0.00387	-0.00208	-0.00664	0.00557	0.00662	0.00474	0.00390	0.00504
	[0.00318]	[0.00446]	[0.00345]	[0.00442]	[0.00716]	[0.00571]	[0.00888]	[0.0138]	[0.00341]	[0.00351]
Quartertoelect = 3	0.00445	-0.00131	0.00292	0.00166	-0.00295	0.00736	0.00485	-0.00134	0.00418	0.00282
	[0.00331]	[0.00557]	[0.00359]	[0.00556]	[0.00914]	[0.00773]	[0.00780]	[0.0129]	[0.00356]	[0.00386]
Quartertoelect = 4	0.00158	-0.00238	0.000658	0.00182	0.00412	0.0108	0.0104	0.0105	0.00116	0.000715
	[0.00368]	[0.00583]	[0.00363]	[0.00612]	[0.0104]	[0.00727]	[0.00799]	[0.0126]	[0.00411]	[0.00428]
Quartertoelect = 5	0.00454	-0.000143	0.00170	-0.000972	0.000219	0.0124	0.0146	0.0106	0.00314	0.00340
	[0.00450]	[0.00585]	[0.00368]	[0.00579]	[0.00979]	[0.00763]	[0.00918]	[0.0130]	[0.00482]	[0.00483]
Quartertoelect = 6	0.00185	-0.0000619	0.00402	0.00383	0.00431	0.00877	0.00580	0.00368	0.000993	-0.000504
	[0.00455]	[0.00600]	[0.00376]	[0.00610]	[0.0111]	[0.00769]	[0.00986]	[0.0153]	[0.00494]	[0.00502]
Quartertoelect = 7	-0.00330	0.000717	0.000956	0.00129	0.00366	0.00979	0.0155	0.0104	-0.000730	-0.00470
	[0.00448]	[0.00617]	[0.00349]	[0.00602]	[0.0107]	[0.00817]	[0.0101]	[0.0147]	[0.00554]	[0.00523]
Quartertoelect = 8	0.00528	-0.000674	-0.00253	0.00239	0.00613	0.0152	0.00950	0.0134	0.00181	0.00409
	[0.00415]	[0.00625]	[0.00346]	[0.00615]	[0.0119]	[0.00896]*	[0.00979]	[0.0144]	[0.00465]	[0.00481]
Quartertoelect = 9	0.00891	0.00591	-0.00000849	0.00630	0.0150	0.0167	0.0125	0.0113	0.00730	0.00970
	[0.00490]*	[0.00642]	[0.00363]	[0.00630]	[0.0128]	[0.00840]**	[0.00936]	[0.0139]	[0.00540]	[0.00574]*
Quartertoelect = 10	0.00326	0.00416	0.00439	0.00931	0.00871	0.0125	0.0169	0.00350	0.00284	0.00313
	[0.00490]	[0.00632]	[0.00400]	[0.00633]	[0.0122]	[0.00811]	[0.00986]*	[0.0145]	[0.00567]	[0.00564]
Quartertoelect = 11	0.00364	0.00571	-0.00111	0.00935	0.00754	0.0115	0.00604	0.00836	0.00587	0.00332
	[0.00497]	[0.00610]	[0.00353]	[0.00588]	[0.0129]	[0.00820]	[0.0101]	[0.0147]	[0.00509]	[0.00529]
Quartertoelect = 12	-0.00117	0.00160	0.000268	0.00460	-0.000817	0.0140	0.00692	0.00992	-0.00753	-0.00750
	[0.00351]	[0.00631]	[0.00346]	[0.00585]	[0.0114]	[0.00881]	[0.00826]	[0.0145]	[0.00411]*	[0.00406]*
Quartertoelect = 13	0.00141	0.00417	-0.00498	0.00425	-0.000679	0.00650	0.00857	0.00764	-0.00392	-0.00222
	[0.00374]	[0.00599]	[0.00305]	[0.00543]	[0.00948]	[0.00752]	[0.00633]	[0.0111]	[0.00442]	[0.00466]
Quartertoelect = 14	-0.00234	0.00455	0.00616	0.00996	-0.00595	0.00914	-0.000736	-0.00389	-0.0112	-0.0124
	[0.00391]	[0.00513]	[0.00320]*	[0.00515]*	[0.0105]	[0.00625]	[0.00732]	[0.00904]	[0.00462]**	[0.00511]**
Quartertoelect = 15	-0.00386	-0.00271	0.00139	0.00289	-0.00577	0.00681	0.00153	-0.00901	-0.00748	-0.0101
	[0.00377]	[0.00333]	[0.00347]	[0.00422]	[0.00558]	[0.00487]	[0.00548]	[0.00608]	[0.00446]*	[0.00452]**
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	263388	164545	150293	151246	58773	155695	27231	134116	164545	164545
R-squared	0.013	0.019	0.019	0.019	0.026	0.019	0.018	0.019	0.019	0.019

Notes: Robust standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Data come from our 100% data collection from 1950-2007 merged with the Administrative Office of the U.S. Courts (1971-2006). Standard errors are clustered at the quarter-year level. The outcome variable is a dummy variable equal to 1 if there was a dissenting opinion in the case. The explanatory variables of interest are dummy variables indicating the number of quarters remaining before the presidential election (16 quarters to the election is the omitted dummy variable). All regressions include year fixed effects, circuit fixed effects, and quarter fixed effects. Column 10 restricts to data also containing final judgment dates.

Table 7: Electoral Cycles in Time Between Oral Argument and Publication

Outcome:	Number of Days Between Oral Argument and Final Judgment			
	Appeals Database (1925-2002)		100% Sample (1950-2007)	
Sample:	Cases With Dissents (2-1 Decisions)	Cases Without Dissents (3-0 Decisions)	Cases With Dissents (2-1 Decisions)	Cases Without Dissents (3-0 Decisions)
	(1)	(2)	(3)	(4)
Divided (DRR or RDD)	-12.10	0.603		
	[18.71]	[4.945]		
Quartertoelect = 1	-87.83	12.17	-0.480	19.19
	[43.82]**	[14.51]	[7.665]	[2.828]***
Quartertoelect = 2	-84.67	6.080	-16.47	1.227
	[42.35]**	[13.72]	[8.099]**	[1.833]
Quartertoelect = 3	-80.05	12.24	-20.12	1.882
	[43.66]*	[14.04]	[8.071]**	[2.152]
Quartertoelect = 4	51.65	-19.54	1.058	1.533
	[49.99]	[25.56]	[11.62]	[2.976]
Quartertoelect = 5	61.32	-4.137	10.83	19.93
	[55.78]	[30.47]	[13.06]	[3.664]***
Quartertoelect = 6	17.62	-10.18	-4.236	4.407
	[62.41]	[31.55]	[12.65]	[3.798]
Quartertoelect = 7	81.76	29.95	-6.682	1.679
	[68.65]	[31.02]	[14.15]	[3.594]
Quartertoelect = 8	-69.36	25.17	0.631	6.185
	[62.26]	[40.71]	[13.84]	[3.522]*
Quartertoelect = 9	16.01	11.71	7.377	21.27
	[70.31]	[45.25]	[14.87]	[3.858]***
Quartertoelect = 10	17.15	5.138	-7.276	6.832
	[71.27]	[40.94]	[14.80]	[3.908]*
Quartertoelect = 11	23.29	12.53	-7.905	0.893
	[63.88]	[39.07]	[16.82]	[3.542]
Quartertoelect = 12	-36.30	11.40	-15.39	-0.336
	[70.75]	[34.81]	[11.35]	[3.330]
Quartertoelect = 13	-66.43	-6.774	-2.708	16.64
	[72.93]	[30.95]	[11.65]	[2.650]***
Quartertoelect = 14	-42.33	-7.125	-7.842	2.381
	[103.0]	[30.96]	[12.38]	[2.729]
Quartertoelect = 15	-56.98	16.97	-21.19	-1.550
	[70.56]	[23.19]	[14.52]	[2.476]
Controls	Y	Y	Y	Y
Observations	368	3149	7971	119353
R-squared	0.230	0.071	0.064	0.040

Notes: Robust standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Standard errors are clustered at the quarter-year level. The outcome variable is the number of days between oral argument and final judgment, winsorized at the 1% level. The explanatory variables of interest are dummy variables indicating the number of quarters remaining before the upcoming presidential election (16 quarters to the election is the omitted dummy variable). All regressions include quarter-to-election fixed effects, year fixed effects, circuit fixed effects. Columns (1) and (2) also include legal issues fixed effects.

Table 8: The Role of Context in Electoral Cycles in Dissents

	Dissent (2-1 Decision)			
Panel A: U.S. Courts of Appeals Database (1925-2002)	(1)	(2)	(3)	(4)
Divided (DRR or RDD)	0.0153	0.0153	0.0152	0.0153
	[0.00451]***	[0.00450]***	[0.00450]***	[0.00451]***
Last Three Quarters	0.0332	0.0447	0.0491	0.0464
	[0.00793]***	[0.00967]***	[0.00904]***	[0.00879]***
Close Election (Electoral Count is Less than 55%)	0.0801			0.0767
	[0.0187]***			[0.0175]***
Close Election (Electoral Count is Less than 55%)	0.0846			0.0713
* Last Three Quarters	[0.0381]**			[0.0383]*
Landslide Election (Electoral Count is More than 95%)		0.0252		0.00958
		[0.0173]		[0.0167]
Landslide Election (Electoral Count is More than 95%)		-0.0400		-0.0203
* Last Three Quarters		[0.0139]***		[0.00948]**
War			0.0203	0.0204
			[0.0131]	[0.0132]
War			-0.0605	-0.0499
* Last Three Quarters			[0.0111]***	[0.0108]***
Controls	Y	Y	Y	Y
Observations	18686	18686	18686	18686
R-squared	0.021	0.021	0.021	0.021

Panel B: 100% Sample (1950-2007)	Dissent Rate in Three Quarters Before Election - Dissent Rate in Three Quarters After Election	
	(1)	(2)
Electoral Count (%)	-0.000248	-0.000250
	[0.000124]**	[0.000124]**
Controls	N	Y
Observations	149	149
R-squared	0.027	0.098

Notes: Robust and clustered standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Panel A: Standard errors are clustered at the quarter-year. The explanatory variables of interest are a dummy variable indicating whether a case was decided in the three quarters immediately preceding a presidential election and the interaction with as well as, in column (1), a dummy variable indicating whether the margin of victory in the electoral count was less than 10%, in column (2), a dummy variable indicating whether the margin of victory in the electoral count was more than 90%, and in column (3), a dummy variable indicating whether the decision was made during wartime. Column (4) includes all of these explanatory variables. The regressions also include circuit fixed effects, legal issues fixed effects, quarter fixed effects, year fixed effects, and a dummy variable indicating whether the panel was divided ideologically. Panel B: Each election-circuit is a separate observation. Electoral Count is the percentage of the electoral college achieved by the winning political party, which ranges from 50 to 100. Mean dependent variable, the "electoral spike" in dissents, is 0.006, and winsorized at the 1% level. Column (2) includes circuit fixed effects.

Table 9: Electoral Cycles in Dissents and Influence of Party of Appointment on Voting Valence Across Time

	Dissent		Liberal Vote	
	(1)	(2)	(3)	(4)
Divided (DRR or RDD)	0.0153	0.0153	-0.0141	-0.0150
	[0.00450]***	[0.00466]***	[0.0141]	[0.0141]
Year > 1975	0.0469	0.0454	0.0324	0.0319
	[0.0146]***	[0.0152]***	[0.0541]	[0.0590]
Lastquarter	0.0299	0.0265		
	[0.0170]*	[0.0171]		
Year > 1975 * Lastquarter	0.0409	0.0421		
	[0.0202]**	[0.0208]**		
Democrat Appointee			0.0520	0.0509
			[0.0106]***	[0.0110]***
Democrat Appointee			0.0466	0.0403
* Year > 1975			[0.0159]***	[0.0170]**
Born on or after 1940		0.00235		-0.0329
		[0.00624]		[0.0200]*
Born on or after 1940		-0.00479		
* Lastquarter		[0.0340]		
Born on or after 1940				0.0714
* Democrat Appointee				[0.0321]**
Controls	Y	Y	Y	Y
Observations	56058	51460	56058	51460
R-squared	0.021	0.022	0.087	0.088

Notes: Robust standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Standard errors are double-clustered at the quarter-year and case level for the vote-level regressions, otherwise they are clustered at the quarter-year level. Regressions also include quarter to election fixed effects, year fixed effects, quarter fixed effects, circuit fixed effects and legal issues fixed effects.

Table 10: The Role of Judicial Experience in Electoral Cycles in Dissents

	Dissent Vote			
	Ordinary Least Squares	N	Probit	N
	(1)	(2)	(3)	(4)
	Each coefficient represents a separate regression			
Last Quarter	0.0129	56058	0.231	56058
(Entire Sample)	[0.00413]***		[0.0734]***	
Last Quarter	0.0331	6314	1.133	5314
(Experience = 1-2)	[0.0110]***		[0.452]**	
Last Quarter	-0.000455	6526	-0.0559	5641
(Experience = 3-4)	[0.0155]		[0.272]	
Last Quarter	0.0206	6075	0.239	5051
(Experience = 5-6)	[0.0188]		[0.301]	
Last Quarter	0.0182	5644	0.542	4788
(Experience = 7-8)	[0.0106]*		[0.235]**	
Last Quarter	0.0150	5041	0.251	3911
(Experience = 9-10)	[0.0178]		[0.224]	
Last Quarter	-0.0196	4390	-0.383	3553
(Experience = 11-12)	[0.0142]		[0.252]	
Last Quarter	0.0308	3605	0.493	2466
(Experience = 13-14)	[0.0203]		[0.304]	
Last Quarter	-0.00230	3002	-0.264	1597
(Experience = 15-16)	[0.0165]		[0.575]	
Last Quarter	0.0173	2288	0.456	1312
(Experience = 17-18)	[0.0292]		[0.409]	
Last Quarter	-0.00166	2737	0.636	1850
(Experience = 19-21)	[0.0129]		[0.441]	
Last Quarter	0.00948	3033	2.237	1917
(Experience = 22-27)	[0.0124]		[1.716]	
Last Quarter	-0.0280	1292	-0.641	702
(Experience = 28-35)	[0.0235]		[0.571]	
Last Quarter	0.0251	2984	10.55	1704
(Experience = 1)	[0.0110]**		[2.014]***	
Last Quarter	0.0439	3330	0.887	2411
(Experience = 2)	[0.0208]**		[0.450]**	

Notes: Robust and clustered standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Standard errors are double-clustered at the quarter-year and case level. The outcome variable of interest is an indicator variable equal to 1 if the judge filed a dissenting opinion in the case. The explanatory variable of interest is a dummy variable indicating whether the case was decided in the quarter immediately preceding a presidential election. All regressions include a dummy variable equal to 1 if the panel deciding the case was divided along ideological lines, circuit fixed effects, legal issues fixed effects, year fixed effects, and quarter fixed effects. We exclude votes by judges sitting on cases when they have less than 0 years of experience or greater than 35 years of experience as their experience may be mismeasured.

Table 11: The Role of Judicial Characteristics in Electoral Cycles in Dissents

	Dissent Vote								
	Ordinary Least Squares							Probit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Divided (DRR or RDD)	0.00545	0.00479	0.00282	0.00598	0.00600	0.00545	0.00325	0.0639	-0.0275
	[0.00139]***	[0.00139]***	[0.00146]*	[0.00148]***	[0.00148]***	[0.00139]***	[0.00151]**	[0.0299]**	[0.120]
Lastquarter	0.0129	0.00381	0.00814	0.00764	0.0116	0.0126	-0.00287	-0.0240	0.0819
	[0.00413]***	[0.00595]	[0.00457]*	[0.00370]**	[0.00396]***	[0.00402]***	[0.00565]	[0.110]	[0.0305]***
Divided * Lastquarter		0.0130					0.00612	0.139	0.135
		[0.00680]*					[0.00204]***	[0.159]	[0.165]
Minority (D of DRR			0.00686				0.0199	0.101	0.152
or R of RDD)			[0.00193]***				[0.0125]	[0.0332]***	[0.0367]***
Minority * Lastquarter			0.0206				-0.00261	0.191	0.179
			[0.0100]**				[0.00188]	[0.144]	[0.154]
Federal Prosecution Experience				-0.00251			0.0189	-0.0436	0.346
				[0.00188]			[0.0126]	[0.0357]	[0.445]
Federal Prosecution Experience				0.0188			-0.00297	0.224	0.287
* Lastquarter				[0.0129]			[0.00510]	[0.127]*	[0.143]**
Elevated					-0.00207		-0.0306	-0.0357	1.265
					[0.00510]		[0.00677]***	[0.0963]	[0.686]*
Elevated * Lastquarter					-0.0304		-0.0306		
					[0.00624]***		[0.00677]***		
Retire or Resign Next Year						0.00126	0.000822	0.0105	0.0129
						[0.00416]	[0.00415]	[0.0735]	[0.0789]
Retire or Resign Next Year						0.00908	0.0105	0.144	0.0818
* Lastquarter						[0.0230]	[0.0225]	[0.253]	[0.249]
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y*
Observations	56058	56058	56058	51086	51460	56058	51086	51052	42719
R-squared	0.006	0.006	0.007	0.007	0.007	0.006	0.007		

Notes: Robust standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Standard errors are double-clustered at the quarter-year and case level. The outcome variable of interest is a dummy variable equal to 1 if the judge voted to dissent. The explanatory variables of interest in columns (1) and (2) are a dummy variable equal to 1 if a case was decided in the quarter immediately preceding a presidential election (lastquarter), a dummy variable equal to 1 if the panel deciding the case was divided along ideological lines and an interaction term between these. Column (3) includes an indicator variable equal to 1 if the other members of the panel were appointed by a different political party and the interaction of this variable with lastquarter. Column (4) includes an indicator variable equal to 1 if the judge was previously a U.S. Attorney or Assistant U.S. Attorney and the interaction with lastquarter. Column (5) includes an indicator variable equal to 1 if the judge is elevated to higher office and the interaction with lastquarter. Column (6) includes an indicator variable equal to 1 if the judge leaves the bench in the following year and the interaction with lastquarter. Columns (7)-(9) combine the explanatory variables contained in columns (1)-(6). All regressions include year fixed effects, quarter fixed effects, circuit fixed effects and legal issues fixed effects. Column (9) also includes judge fixed effects.

Table 12: The Effect of Wartime

Outcome:	Dissent (2-1 Decision)			Dissent Vote		Affirm	Reverse
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Divided (DRR or RDD)	0.0150		0.0198	0.00579	0.00737		
	[0.00442]***		[0.00499]***	[0.00148]***	[0.00166]***		
World War 2	0.0240						
	[0.0132]*						
Korean War	-0.0187	0.00272					
	[0.0101]*	[0.00486]					
Vietnam War	-0.0126	-0.0208					
	[0.00565]**	[0.00185]***					
Gulf War	0.0257	-0.0109					
	[0.0197]	[0.00255]***					
Afghan War	-0.0157	0.00294					
	[0.0229]	[0.00623]					
War			0.00992	-0.0000581	0.00578	0.0389	-0.0207
			[0.00869]	[0.00308]	[0.00364]	[0.0107]***	[0.00993]**
Divided * War			-0.0263		-0.00810		
			[0.00972]***		[0.00321]**		
Inexperience (<= 10 Years)				0.00189	0.00194		
				[0.00163]	[0.00164]		
Inexperience * War				-0.00744	-0.00779		
				[0.00367]**	[0.00364]**		
Year (linear time trend)	Y	Y	Y	Y	Y	Y	Y
Observations	18686	265868	18686	49374	49374	18686	18686
R-squared	0.014	0.012	0.014	0.005	0.005	0.045	0.014

Notes: Robust standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Data come from U.S. Courts of Appeals Database (1925-2002) except column (2), which is our 100% data collection from 1950-2007. Standard errors are clustered at the case level for column (1), double clustered at the quarter-year and case level in the other vote-level regressions, and clustered at the quarter-year level in the case-level regressions. All specifications include circuit and case type fixed effects except column (2), which only has circuit fixed effects.

Table 13: Quality of Opinion with Dissents before Presidential Elections

	Length of Majority Opinion	Citations	Dissenting Citations	Conditional on Dissenting, Dissent for Procedural, not Merits Issue	
Panel A	(1)	(2)	(3)	(4)	(5)
Lastquarter	0.216	0.289	-0.0809	0.0785	0.0962
	[0.227]	[0.536]	[0.0456]*	[0.0289]**	[0.0535]*
Dissentdummy	1.902	2.115	0.413		
	[0.123]***	[0.262]***	[0.0363]***		
Lastquarter *	0.387	-0.232	-0.292		
Dissentdummy	[0.428]	[1.206]	[0.111]***		
Controls	Y	Y	Y	N	Y
Observations	18686	18649	18649	227	227
R-squared	0.223	0.168	0.077	0.038	0.207
	Appeal Made to	Supreme Court	Supreme Court		
Panel B	Supreme Court	Takes Case	Reverses		
Lastquarter	-0.000359	0.00102	-0.00316		
	[0.0208]	[0.00869]	[0.00404]		
Dissentdummy	0.139	0.0445	-0.0102		
	[0.0131]***	[0.00729]***	[0.00357]***		
Lastquarter *	0.0116	0.00997	-0.0274		
Dissentdummy	[0.0431]	[0.0323]	[0.0248]		
Controls	Y	Y	Y		
Observations	18686	18686	18686		
R-squared	0.040	0.019	0.013		

Notes: Robust standard errors, clustered at the quarter-year level, in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Panel A: The outcome variable in columns (1) is the length of the majority opinion in pages (winsorized at the 1% level). In column (2), the outcome variable is the total number of times the case has been cited in subsequent opinions (winsorized at the 1% level), and in column (3) is the total number of time the case has been cited in subsequent dissents (winsorized at the 1% level) respectively. The outcome variable in column (4) is a dummy variable equal to 1 if the dissenting opinion focused on procedural (rather than merit) issues. The explanatory variables of interest are a dummy variable equal to 1 if a case was decided in the quarter immediately preceding a presidential election, a dummy variable equal to 1 if there was a dissenting opinion in the case and an interaction term between these. The regressions in columns (1)-(3) also include quarter-to-election fixed effects, year fixed effects, quarter fixed effects, circuit fixed effects and legal issues fixed effects. Column (4) includes a dummy for whether the panel was divided and column (5) also includes year fixed effects, quarter fixed effects, circuit fixed effects and legal issues fixed effects. Panel B: The outcome variable is whether the appellate case was subsequently appealed to the Supreme Court in column (1), whether the Supreme Court actually took the case in column (2), and whether the Supreme Court reversed any part of the case in column (3).

Appendix Table A: Summary Statistics

Panel A: Case Level	
Dissent (with or without Dissenting Opinion)	0.0786
	[0.00197]
Divided (DRR or RDD)	0.697
	[0.00336]
Criminal	0.269
	[0.00324]
Civil Rights	0.0859
	[0.00205]
First Amendment, Due Process, Privacy	0.0281
	[0.00121]
Labor Relations	0.0723
	[0.00189]
Economic Activity	0.509
	[0.00366]
Dissent for Procedural, not Merit Reasons	0.0925
	[0.0193]
Length of Majority Opinion (in pages)	4.565
	[0.0245]
Citations	6.060
	[0.0690]
Dissenting Citations	0.547
	[0.0079]
Affirm	0.568
	[0.00362]
Reverse	0.269
	[0.00303]
N	18686

Panel B: Judge-Vote Level	
Dissent (with or without Dissenting Opinion)	0.0234
	[0.000638]
Majority	0.768
	[0.00178]
Previous US Attorney or Assistant US Attorney	0.154
	[0.00160]
Democrat	0.510
	[0.00211]
Appointed by Democrat	0.491
	[0.00211]
Opposing Party Wins Election	0.504
	[0.00211]
Close Election Electoral Count < 55%	0.048
	[0.00090]
Age	62.19
	[0.0393]
Experience	10.33
	[0.0335]
Inexperience Experience <= 10 years	0.600
	[0.00221]
Elevated	0.0161
	[0.000555]
Retire Next Year	0.0309
	[0.000731]
N	56147

Panel C: Judge Level	
Resignation	0.023
	[0.00310]
Retirement	0.124
	[0.00870]
Resignation, when Same Party in Power	0.0152
	[0.00248]
Resignation, when Different Party in Power	0.00781
	[0.00178]
Retirement, when Same Party in Power	0.0506
	[0.00521]
Retirement, when Different Party in Power	0.0732
	[0.00611]
N	2433

Voting Valence if Democratic Appointee	
Conservative	47%
Liberal	36%
Mixed	6%
Could not be determined	11%
N	27550
Voting Valence if Republican Appointee	
Conservative	52%
Liberal	32%
Mixed	7%
Could not be determined	9%
N	28597

Panel D: Openjurist 100% sample	
Dissent (with Dissenting Opinion)	0.0619
	[0.00045]

Note: Coefficients shown are from an OLS regression on a constant.

Appendix Table B: Panel Composition, Case Type, and Case Load Over Political Cycle

	Divided Panel	Criminal	Civil Rights	First Amendment, Due Process, Privacy	Labor Relations	Economic Activity	Number of Published Cases
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Quartertoelect = 1	-0.0193	0.000959	-0.00626	-0.0141	-0.00179	-0.0120	-0.0942
	[0.0191]	[0.0216]	[0.0151]	[0.00987]	[0.0117]	[0.0314]	[0.0909]
Quartertoelect = 2	0.0227	0.00380	-0.00820	0.000377	-0.0133	0.00868	0.0919
	[0.0220]	[0.0211]	[0.0144]	[0.00930]	[0.0129]	[0.0270]	[0.0972]
Quartertoelect = 3	0.0252	-0.00355	-0.0133	-0.00814	0.00968	0.00820	0.108
	[0.0221]	[0.0189]	[0.0134]	[0.00820]	[0.0122]	[0.0240]	[0.0932]
Quartertoelect = 4	0.00376	0.00901	-0.0161	0.00653	0.00698	-0.0214	0.255
	[0.0214]	[0.0214]	[0.0147]	[0.00871]	[0.0114]	[0.0262]	[0.107]**
Quartertoelect = 5	-0.0393	-0.00654	-0.00261	-0.00933	-0.0202	0.00645	-0.139
	[0.0303]	[0.0286]	[0.0172]	[0.0106]	[0.0145]	[0.0354]	[0.131]
Quartertoelect = 6	-0.00114	-0.0106	-0.00242	-0.00599	-0.00163	0.0108	0.180
	[0.0292]	[0.0272]	[0.0180]	[0.00992]	[0.0154]	[0.0318]	[0.138]
Quartertoelect = 7	0.0240	-0.00843	0.00551	-0.00306	0.00810	0.00177	0.146
	[0.0297]	[0.0257]	[0.0179]	[0.00928]	[0.0162]	[0.0298]	[0.136]
Quartertoelect = 8	0.0266	-0.0222	0.000457	-0.00704	-0.00398	0.0220	0.140
	[0.0266]	[0.0241]	[0.0162]	[0.00853]	[0.0131]	[0.0263]	[0.124]
Quartertoelect = 9	-0.00273	0.00741	-0.0159	-0.0131	-0.0216	0.0324	-0.251
	[0.0304]	[0.0303]	[0.0197]	[0.0109]	[0.0152]	[0.0309]	[0.137]*
Quartertoelect = 10	0.0161	-0.00125	0.00727	-0.0164	-0.0322	0.0344	-0.0200
	[0.0307]	[0.0299]	[0.0187]	[0.01000]	[0.0149]**	[0.0326]	[0.144]
Quartertoelect = 11	0.0133	-0.0302	0.000967	-0.0171	-0.0126	0.0546	0.00925
	[0.0295]	[0.0300]	[0.0162]	[0.00947]*	[0.0147]	[0.0320]*	[0.141]
Quartertoelect = 12	-0.00647	0.0123	-0.0126	-0.0109	-0.0162	0.0313	0.126
	[0.0265]	[0.0219]	[0.0145]	[0.00699]	[0.0110]	[0.0216]	[0.115]
Quartertoelect = 13	-0.0347	0.0264	-0.0102	-0.00574	-0.0252	0.0138	-0.273
	[0.0308]	[0.0240]	[0.0170]	[0.00856]	[0.0111]**	[0.0242]	[0.107]**
Quartertoelect = 14	0.00176	0.0180	0.00296	-0.0104	-0.0148	-0.00979	0.0126
	[0.0316]	[0.0237]	[0.0164]	[0.00860]	[0.0120]	[0.0240]	[0.114]
Quartertoelect = 15	0.0180	0.00532	-0.00417	-0.0172	-0.0167	0.0359	-0.0191
	[0.0323]	[0.0237]	[0.0170]	[0.00776]**	[0.0118]	[0.0250]	[0.110]
Controls	Y	Y	Y	Y	Y	Y	Y
Observations	18686	18686	18686	18686	18686	18686	10479
R-squared	0.048	0.050	0.053	0.022	0.021	0.099	0.154

Notes: Robust standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). In columns (1)-(6) standard errors are clustered at the quarter-year level. The outcome variable in column (1) is a dummy variable equal to 1 if the case was heard by an ideologically divided panel. The outcome variables in columns (2)-(6) are dummy variables indicating the type of legal issue addressed in the case. The outcome variable in column (7) is the number of opinions with observations at the month-year level. The explanatory variables of interest are dummy variables indicating the number of quarters remaining before the upcoming presidential election (16 quarters to the election is the omitted dummy variable). Controls are year fixed effects, quarter fixed effects, and circuit court fixed effects.

Appendix Table C: Workload, Fraction of Opinions Published, and Time Spent per Case Overall Over Political Cycle (Ninth Circuit only)

Quarters to Election from Judgement	Number of Months in sample for Correspondin g Quarter-to-Election	Total Frequency	Frequency per Month	Fraction of Opinions Published	Average Number of Months Between Docket and Judgment Dates	Median Number of Months Between Docket and Judgment Dates	Average Number of Months Between Docket and Judgment Dates (Winsorized at 1%)	Average Number of Months Between Hearing and Judgment Dates	Median Number of Months Between Hearing and Judgment Dates	Average Number of Months Between Hearing and Judgment Dates (Winsorized at 1%)
1	12	52746	4396	0.2495	8.5920	7	8.4868	3.7137	3	3.6145
2	12	53391	4449	0.2813	8.9610	7	8.8485	3.3079	2	3.2679
3	12	54443	4537	0.2517	8.6393	7	8.5670	3.1136	2	3.0737
4	12	47668	3972	0.2466	8.7825	7	8.5154	3.0224	2	2.9523
5	12	50326	4194	0.2707	8.5823	7	8.4547	3.5885	3	3.5145
6	10	44171	4417	0.2721	8.6311	7	8.5164	3.1347	2	3.0906
7	9	41154	4573	0.2342	8.6963	7	8.5808	2.9408	2	2.9010
8	9	38264	4252	0.2345	8.6380	7	8.5039	2.9515	2	2.8933
9	9	40306	4478	0.2743	9.2000	7	8.8493	5.0413	3	4.1418
10	11	51733	4703	0.2525	8.8967	7	8.7849	3.1818	2	3.1350
11	12	58064	4839	0.2242	9.0300	7	8.8910	3.0743	2	3.0056
12	12	53912	4493	0.2186	8.7906	7	8.6921	3.0238	2	2.9656
13	12	55714	4643	0.2534	8.9531	7	8.8551	3.6881	3	3.6047
14	12	57527	4794	0.2568	8.7720	7	8.5901	3.2822	2	3.2325
15	12	55131	4594	0.2343	8.6848	7	8.5649	2.9590	2	2.9247
16	12	51019	4252	0.2326	8.6368	7	8.5363	2.9959	2	2.9382
Obs				494686	805569		805569	178613		178613
Mean			4474	0.2566	8.7818		8.6446	3.3074		3.2053
Std. Dev.			228.6077		8.0387		7.1462	4.0422		3.3524

Appendix Table D: Electoral Cycles in Dissents by Presidential Appointment

	(1)
Keep 1 set of presidential appointees at a time	Separate Regressions on Last 3 quarters
T. Roosevelt	-0.00712
[N = 315]	[0.00668]
Taft	-0.0214
[N = 154]	[0.0353]
Wilson	0.000862
[N = 1567]	[0.0144]
Harding	-0.00107
[N = 353]	[0.00614]
Coolidge	-0.00670
[N = 2366]	[0.00939]
Hoover	-0.000782
[N = 2585]	[0.0101]
F. Roosevelt	-0.00561
[N = 6055]	[0.00590]
Truman	0.00666
[N = 3006]	[0.00817]
Eisenhower	-0.00503
[N = 6109]	[0.00588]
Kennedy	0.0104
[N = 2585]	[0.0130]
Johnson	0.00376
[N = 5563]	[0.00902]
Nixon	0.00955
[N = 4836]	[0.00958]
Ford	-0.00340
[N = 1239]	[0.0286]
Carter	0.00496
[N = 5320]	[0.0112]
Reagan	0.0198
[N = 6185]	[0.00690]***
Bush	0.0304
[N = 2011]	[0.0126]**
Clinton	0.0609
[N = 1091]	[0.0268]**

Notes: Robust and clustered standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). The explanatory variables of interest is a dummy indicator for last 3 quarters before an election. The regressions also include circuit fixed effects, legal issues fixed effects, year fixed effects, and seasonly quarter fixed effects.

Appendix Table E: Electoral Cycles in Dissents by Case Type

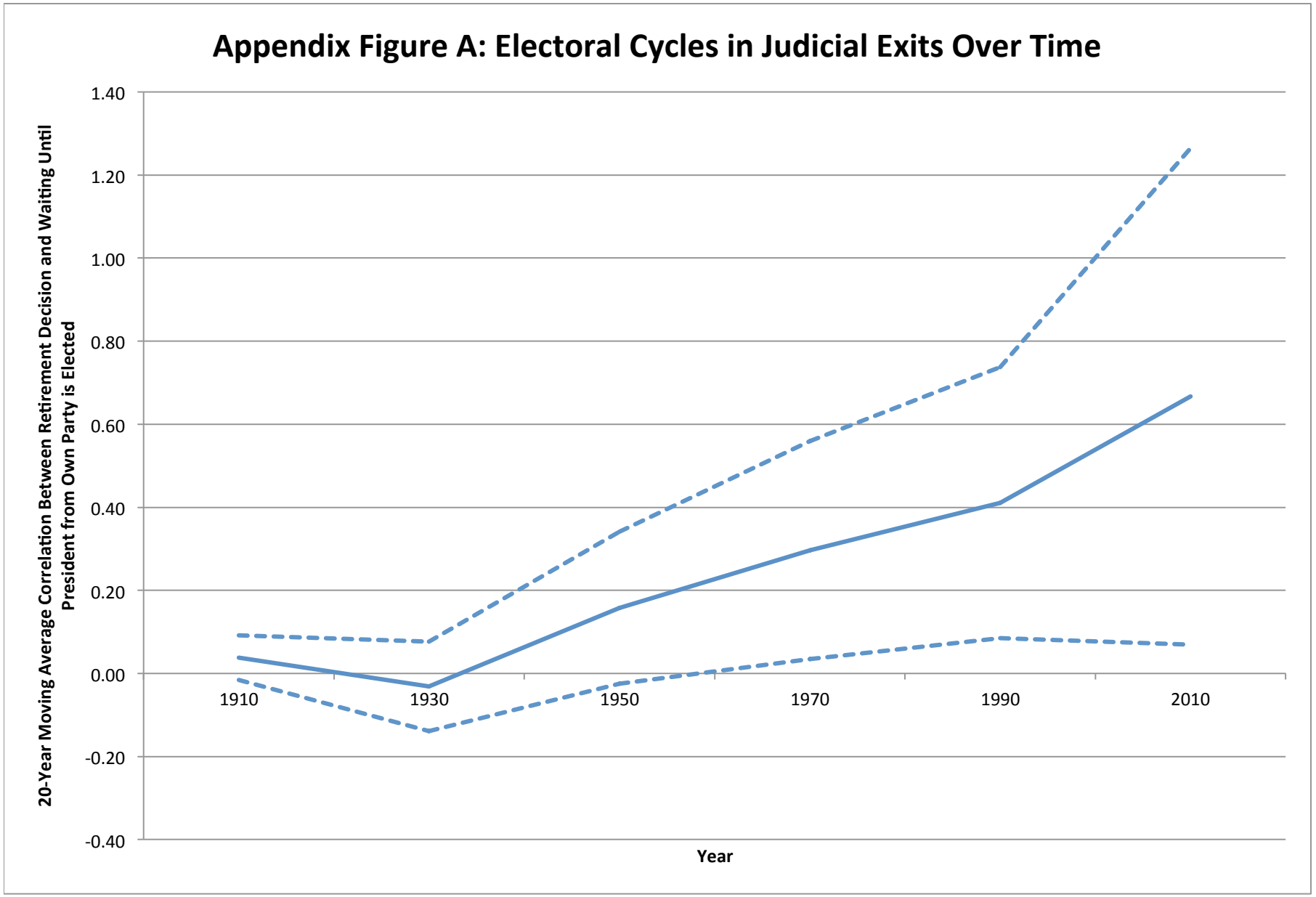
	Dissent										
	1-Digit Case Category							2-Digit Case Category			
	Criminal	Civil Rights	First Amendment	Due Process	Labor Relations	Economic Activity	Miscellaneous	Criminal Federal	Civil Rights Discrimination	Economic Commercial	Economic Property
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Divided	0.0248	0.0389	-0.0157	0.123	0.00771	0.00692	0.0152	0.0278	0.00266	0.00636	0.0248
	[0.00867]***	[0.0157]**	[0.0692]	[0.0639]*	[0.0183]	[0.00560]	[0.0297]	[0.00946]***	[0.0333]	[0.0102]	[0.0229]
Quartertoelect = 1	0.0545	0.0844	0.260	0.0113	0.0265	0.0537	-0.110	0.0478	0.198	0.0975	0.185
	[0.0231]**	[0.0493]*	[0.246]	[0.199]	[0.0666]	[0.0167]***	[0.136]	[0.0255]*	[0.0992]**	[0.0348]***	[0.0675]***
Quartertoelect = 2	0.0379	0.0488	0.277	-0.0661	-0.0790	0.0289	-0.156	0.0290	0.0642	0.0193	0.0701
	[0.0243]	[0.0434]	[0.183]	[0.135]	[0.0594]	[0.0171]*	[0.122]	[0.0253]	[0.0781]	[0.0274]	[0.0602]
Quartertoelect = 3	0.0348	0.106	0.0794	-0.0640	-0.0302	0.0249	-0.158	0.0251	0.139	0.0111	0.0830
	[0.0242]	[0.0521]**	[0.149]	[0.150]	[0.0522]	[0.0138]*	[0.108]	[0.0256]	[0.0822]*	[0.0240]	[0.0533]
Quartertoelect = 4	0.0321	0.0303	0.0199	0.122	-0.0135	-0.00481	-0.174	0.0347	-0.0647	0.0109	0.0316
	[0.0242]	[0.0542]	[0.128]	[0.146]	[0.0526]	[0.0139]	[0.0984]*	[0.0274]	[0.0794]	[0.0242]	[0.0460]
Quartertoelect = 5	0.0201	0.0420	0.130	0.111	-0.0394	0.00629	-0.0564	0.0517	-0.207	0.0346	0.00676
	[0.0348]	[0.0665]	[0.185]	[0.283]	[0.0730]	[0.0195]	[0.119]	[0.0376]	[0.138]	[0.0369]	[0.0545]
Quartertoelect = 6	-0.0122	0.0563	-0.0553	-0.0203	-0.103	0.0243	-0.127	-0.00247	-0.112	0.0699	0.0333
	[0.0316]	[0.0731]	[0.198]	[0.199]	[0.0693]	[0.0209]	[0.120]	[0.0335]	[0.141]	[0.0383]*	[0.0736]
Quartertoelect = 7	0.0284	0.0708	0.162	0.0839	-0.0748	0.0167	-0.129	0.0509	-0.197	0.0445	0.105
	[0.0309]	[0.0722]	[0.234]	[0.210]	[0.0683]	[0.0174]	[0.127]	[0.0319]	[0.145]	[0.0332]	[0.0775]
Quartertoelect = 8	0.0164	-0.0144	-0.315	0.258	-0.0345	0.0178	-0.145	0.0235	-0.152	0.0778	0.0689
	[0.0258]	[0.0717]	[0.183]*	[0.193]	[0.0640]	[0.0163]	[0.112]	[0.0262]	[0.136]	[0.0341]**	[0.0604]
Quartertoelect = 9	-0.00171	-0.0607	-0.436	0.000377	-0.0198	-0.00883	-0.224	0.0244	-0.188	0.0427	-0.0272
	[0.0290]	[0.0785]	[0.219]**	[0.245]	[0.0786]	[0.0210]	[0.134]*	[0.0312]	[0.156]	[0.0398]	[0.0696]
Quartertoelect = 10	0.00562	-0.0162	-0.384	0.149	-0.0259	-0.0178	-0.255	0.00758	-0.273	-0.0189	0.0696
	[0.0296]	[0.0891]	[0.210]*	[0.203]	[0.0806]	[0.0206]	[0.134]*	[0.0309]	[0.159]*	[0.0403]	[0.0755]
Quartertoelect = 11	0.0251	-0.0799	-0.412	-0.0186	-0.0138	0.00193	-0.110	0.0370	-0.295	0.0196	-0.00860
	[0.0306]	[0.0856]	[0.205]**	[0.237]	[0.0833]	[0.0217]	[0.140]	[0.0336]	[0.157]*	[0.0372]	[0.0698]
Quartertoelect = 12	-0.0100	-0.0704	-0.441	0.0334	0.0467	0.00373	-0.181	-0.0194	-0.183	0.00117	0.0449
	[0.0216]	[0.0784]	[0.148]***	[0.133]	[0.0540]	[0.0157]	[0.119]	[0.0212]	[0.144]	[0.0325]	[0.0525]
Quartertoelect = 13	0.0210	-0.0987	-0.268	0.206	0.00391	-0.00736	-0.178	0.0175	-0.186	0.0359	-0.0266
	[0.0259]	[0.0821]	[0.170]	[0.160]	[0.0565]	[0.0194]	[0.122]	[0.0262]	[0.160]	[0.0396]	[0.0632]
Quartertoelect = 14	-0.0140	-0.175	-0.431	0.0922	-0.0499	0.0121	-0.126	-0.0146	-0.314	0.0378	0.109
	[0.0253]	[0.0843]**	[0.171]**	[0.129]	[0.0529]	[0.0207]	[0.144]	[0.0258]	[0.161]*	[0.0432]	[0.0714]
Quartertoelect = 15	-0.00629	-0.149	-0.0869	0.00975	-0.0459	0.00409	-0.116	-0.00534	-0.224	0.0162	0.0936
	[0.0218]	[0.0958]	[0.216]	[0.0974]	[0.0559]	[0.0195]	[0.150]	[0.0228]	[0.182]	[0.0422]	[0.0626]
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	5023	1605	275	209	1351	9509	518	4224	572	2543	610
R-squared	0.036	0.064	0.316	0.364	0.084	0.024	0.187	0.037	0.188	0.058	0.207

Notes: Robust standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Standard errors are clustered at the quarter-year level. The outcome variable is whether the case had a dissent. The explanatory variables of interest are dummy variables indicating the number of quarters remaining before the upcoming presidential election (16 quarters to the election is the omitted dummy variable) and a dummy variable equal to 1 if the panel deciding the case was divided along ideological lines. All regressions include fixed effects for year, circuit, legal issues, and quarter of season.

Appendix Table F: Electoral Cycles in Judicial Exits

	Number of Retirements		Number of Resignations	
Party in Power:	Same	Different	Same	Different
	(1)	(2)	(3)	(4)
Quartertoelect = 1	-0.00431	-0.0905	-0.0157	-0.00306
	[0.0277]	[0.0384]**	[0.0121]	[0.00517]
Quartertoelect = 2	-0.0290	-0.0813	0.00616	0.00862
	[0.0282]	[0.0400]**	[0.0178]	[0.00974]
Quartertoelect = 3	-0.0382	-0.103	-0.00267	0.0129
	[0.0276]	[0.0396]***	[0.0183]	[0.0103]
Quartertoelect = 4	0.0270	-0.0102	0.00673	-0.00492
	[0.0507]	[0.0532]	[0.0242]	[0.00587]
Quartertoelect = 5	-0.00347	-0.0157	0.000813	-0.00145
	[0.0538]	[0.0616]	[0.0282]	[0.0116]
Quartertoelect = 6	0.0633	-0.0196	-0.00999	0.00370
	[0.0569]	[0.0621]	[0.0273]	[0.0117]
Quartertoelect = 7	0.0215	-0.0870	-0.0319	-0.00513
	[0.0528]	[0.0585]	[0.0252]	[0.0100]
Quartertoelect = 8	0.0344	-0.0401	0.0233	-0.00984
	[0.0478]	[0.0582]	[0.0252]	[0.0106]
Quartertoelect = 9	0.0104	-0.0652	0.0239	-0.00637
	[0.0492]	[0.0613]	[0.0245]	[0.0138]
Quartertoelect = 10	0.0380	-0.0429	0.0196	0.0118
	[0.0548]	[0.0662]	[0.0236]	[0.0185]
Quartertoelect = 11	0.00250	-0.118	0.0322	0.00972
	[0.0462]	[0.0622]*	[0.0241]	[0.0184]
Quartertoelect = 12	-0.0378	-0.0700	0.0227	-0.0150
	[0.0408]	[0.0558]	[0.0134]*	[0.0152]
Quartertoelect = 13	-0.0454	-0.0821	0.0368	-0.0114
	[0.0483]	[0.0588]	[0.0148]**	[0.0178]
Quartertoelect = 14	-0.0639	-0.0258	0.0325	0.000377
	[0.0475]	[0.0639]	[0.0151]**	[0.0205]
Quartertoelect = 15	-0.0732	-0.0745	0.0237	0.00476
	[0.0429]*	[0.0635]	[0.0136]*	[0.0220]
Controls	Y	Y	Y	Y
Observations	2433	2433	2433	2433
R-squared	0.198	0.282	0.099	0.091

Notes: Robust standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). The outcome variable in columns (1)-(2) is the number judges that retire in a particular month. The outcome variable in columns (3)-(4) is the number judges that resign in a particular month. The explanatory variables of interest are dummy variables indicating the number of quarters remaining before the upcoming presidential election (16 quarters to the election is the omitted dummy variable). The regression also includes year fixed effects and quarter fixed effects.


Appendix Figure A: Electoral Cycles in Judicial Exits Over Time
20-Year Moving Average Correlation Between Retirement Decision and Waiting Until President from Own Party is Elected
1.40
1.20
1.00
0.80
0.60
0.40
0.20
0.00
-0.20
-0.40
1910
1930
1950
1970
1990
2010
Year