

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

Carlos Berdejó and Daniel L. Chen¹

September 2011

Abstract

We find field evidence for what experimental studies have documented regarding the contexts and characteristics that make individuals more susceptible to priming. Just before Presidential elections, unelected U.S. appellate judges double the rate at which they dissent and vote along partisan lines. Dissents occur for miscellaneous procedural, not substantive, issues, on less controversial cases, and by inexperienced judges, judges in ideologically polarized environments, and former federal prosecutors. Consistent with models of endogenous group identity, wartime and 9/11 suppressed dissents, again, especially by judges more prone to priming. We identify a channel for judicial partisanship that is neither legal nor conscious.

Keywords: Priming, Polarization, Normative Commitments, Ideology, Identity, Judges

JEL codes: D72, D83, H77, K00, K40, K42

¹ Carlos Berdejo, Loyola Law School, carlos.berdejo@lls.edu; Daniel Chen, Duke Law School, dchen@law.duke.edu. We thank numerous colleagues for helpful comments and research assistants for invaluable work. This project was conducted while Carlos Berdejo was a Terence Conside fellow at the Harvard Law School and while Daniel Chen received financial support from the Institute for Humane Studies, John M. Olin Foundation, and Ewing Marion Kauffman Foundation. We acknowledge joint financial support from the Program on the Legal Profession and the Center for American Political Studies at Harvard University.

1. Introduction

Group polarization is an important social problem (Esteban and Ray 1994, Duclos et. al. 2004, Montalvo and Reynal-Querol 2005), and since 9/11, has received increasing attention by social scientists. A growing body of experimental work documents a causal link between group identity and social preferences (Chen and Li 2009, Fong and Luttmer 2009), economic decisions (Benjamin, Choi, and Strickland 2010), charitable decisions (Benjamin, Choi, and Fisher 2010), and public opinion (Gerber, Huber, and Washington 2009, Huber and Lapinski 2006, Mendelberg 2001), particularly in expressing preferences that favor in-group members, and with the exception of Gerber, Huber, and Washington (2009), document these causal links in a laboratory setting. This paper examines the causal link between group identity and another outcome, more closely aligned with the concerns raised by 9/11, namely, ideology, and does so in the field. While market pressures may drive out other forms and sources of ideological bias, such as media slant (Gentzkow and Shapiro 2010, Mullainathan and Shleifer 2005, Groseclose and Milyo 2005, DellaVigna and Kaplan 2007), or drive identity effects in savings and risk preferences towards the rational model, behavioral anomalies in a context such as judicial decision-making can have more indelible consequences (Gennaioli and Shleifer 2007). Judges therefore provide an important environment to study ideological bias.

By and large, appellate judges believe there is no evidence for ideological bias (Edwards and Livermore 2009). Yet the view of judges as impartial and unbiased decision-makers has been questioned by studies showing that demographic and other background characteristics predict judicial decision-making in a range of legal issues (see e.g., Boyd, Epstein, and Martin 2010, Peresie 2005) and, in particular, that judges' voting behavior reflects partisan preferences (see e.g., Segal and Spaeth 2002, Ellman, Sunstein, and Schkade 2004, Abrams, Bertrand, and Mullainathan 2008, Shayo and Zussman 2010). Furthermore, laboratory studies document that

even when parties have the same information, they will come to different conclusions about what a fair judicial decision should be and they do so for self-serving reasons (Babcock, Loewenstein, Issacharoff, and Camerer 1995). However, how correlations between judges' decisions and their demographic characteristics should be interpreted, however, remains an open question. Even if judges vote along partisan lines, are judges simply following legal philosophy (i.e., different formal rules) instead of being per se biased?² Further, even if there is judicial bias, whether the bias is cognitive (i.e., unconscious) or motivational (i.e., conscious) is another important question. A finding of unconscious rather than conscious bias would suggest different remedies for judicial partisanship.³

This paper identifies a channel for judicial partisanship⁴ that is neither legal nor conscious. Using a dataset of over 18,686 randomly selected cases from 1925-2002, we present evidence that the presidential election cycle affects judicial decision-making by U.S. Circuit judges even though they (and their legal philosophies) should be immune from electoral incentives. We consider and rule out a number of incentive-based reasons for electoral cycles found in previous work⁵, such as re-election considerations⁶ (these judges are appointed for life), aspirations for higher office, getting out the vote, collegiality, reputational capital, and legacy considerations. We take our results as providing evidence of the priming⁷ of political ideology,

² Among the three main theories of judicial behavior—legal, attitudinal, and self-interested—the first posits that judges follow formal rules or legal philosophy (Kornhauser 1999). The latter two assume some form of bias, e.g. the attitudinal model posits that judges follow political preferences (Cameron 1993) and the self-interested model posits that judges maximize their utility (Posner 1973).

³ See Jolls, Sunstein, and Thaler (1998) for a discussion.

⁴ We define partisan as being about shared ideas with a particular group, that is, simply the fact that political party predicts judicial voting behavior.

⁵ For a review of the literature on the impact of judicial elections on judicial behavior, see Pozen (2008). For a study that examines the effect of judicial selection methods on dissents, see Choi et al. (2007).

⁶ A literature documents the relationship between proximity of a judge's retention election and a judge's sentencing behavior (see e.g., Gordon and Huber (2004, 2007), Lim (2008) and Berdejo and Yuchtman (2009)).

⁷ The priming effect of electoral cycles is more akin to that of implicit priming, since judges have a strong professional commitment to equal treatment and, when explicitly primed, control the influence of

leading to increasing levels of polarization and dissensus in judicial decisions. During the period preceding a presidential election, the media is saturated with political debate, so if individuals have partisan identities, priming may lead judges to view things in a different light.⁸ Our study is the first, as far as we know, to cleanly identify a non-incentive based reason for the effect of electoral cycles.⁹ This finding is important since if unelected appellate judges are susceptible to priming via the partisan nature of electoral cycles, they may also be susceptible to other forms of priming regardless of their professional commitments to be unbiased.

Our paper contributes to a burgeoning literature documenting how psychology matters in the field (Bertrand, Karlan, Mullainathan, Shafir, and Zinman 2010, Card and Dahl 2011). In general, it is difficult to measure priming effects in the field. Priming is a cognitive process, in which media information increases temporarily (primes) the accessibility of knowledge units in the memory of an individual, thus making it more likely that these knowledge units are used in the reception, interpretation and judgment of subsequent external information (Bargh and Chartrand 2000, Storms 1958, Higgins and Chaires 1980). The literature also refers to this phenomenon as passive, conceptual, or implicit priming. The key mechanism is that an activated concept becomes more likely than before to influence conscious judgments. Priming may be contrasted with cueing (Laibson 2001, Bernheim and Rangel 2004) insofar as a cue is necessary

unconscious identity-based bias (Rachlinski, Johnson, Wistrich, Guthrie 2009). Psychological factors can be expected to play a role in judicial behavior. A sizeable experimental literature on judges suggests that heuristics play a large role in judicial decision-making (see, e.g. Mussweiler and Strack 2000, English Mussweiler and Strack 2005, Guthrie, Rachlinski, Wistrich 2007, English and Soder 2009, Guthrie, Rachlinski, Wistrich 2006, Mussweiler and English 2005).

⁸ The quarter before a presidential election is a time period in which the news cycle is amplified along partisan lines as party conventions have already selected the major presidential candidates. See also Appendix Figure C, which shows the electoral cycle in the number of New York Times articles mentioning both “Republican” and “Democrat”. See generally, Druckman (2005) on the role of newspapers and television coverage of campaigns influencing the electorate.

⁹ Electoral cycles in other phenomena have also been documented, but their robustness has been questioned (e.g. electoral cycles in suicides are not robust to controlling for unemployment rates, Wasserman 1983). Other electoral cycles, such as those in stock prices, are difficult to attribute to priming since government gridlock can occur before elections and this can be good for business.

to the subsequent activity, whereas a prime is extraneous to the activity (Sudevan and Taylor 1987). Hence, priming may be expected to cause a more subtle response than cueing and may be more difficult to detect.

An additional difficulty in detecting priming effects in naturally occurring data comes from the varying length and intensity of primes as well as the different susceptibility to priming among different populations. For example, the longest laboratory study documents priming effects one week after the initial stimulus (Tulving, Schacter, and Stark 1982).¹⁰ The greater the concentration of primes or total number of primes, the stronger is the overall priming effect (Srull and Wyer 1979). However, conscious processing, directed by an individual's intentions and goals, can override the usual or habitual response to priming (Bargh and Chartrand 2000); indeed, experienced individuals are less prone to priming, while novices are more easily primed by news coverage (Krosnick and Kinder 1990). Furthermore, activation will only spread if there is an associative link that has been formed, and the stronger the association the more and faster the activation will spread (Bargh and Chartrand 2000).¹¹ This corresponds with findings that people with high levels of political knowledge are most likely to be primed by the national political climate (Kimball 2005); non-experimental studies of priming, however, have been criticized for a number of reasons, mostly for lacking adequate control (Lenz 2009).¹²

¹⁰ For other work on priming duration, see also Ostergaard (1994), Bock and Griffin (1998), Squire, Shimamura, and Graf (1987), Naito (1990), Althaus and Kim (2006), and Hassin, Ferguson, Shidlovski, and Gross (2007).

¹¹ This is known as sequential priming. The first experiment documenting sequential priming in a situational context was one where participants identified as likely to be sexual harassers or aggressors displayed the sequential priming effect of power on sexually related stimuli in a pronunciation task, and in a second experiment were more attracted to a female confederate (compared to other participants) if the concept of power had been primed (Bargh, Raymond, Pryor, & Strack, 1995).

¹² Lenz (2009) argues that prior results about the priming effects of elections (see Druckman 2004 for a summary) are more properly interpreted as learning from campaigns. Our results, however, provide strong evidence that elections cannot simply be about learning. In the first instance, judges are not supposed to be learning from elections nor are judges supposed to use what they learn about political parties as a basis for their decisions. Even if judges do learn from elections, then the increase in judicial dissents and voting along partisan lines should persist after elections, which we do not find.

To identify priming effects in the field, data is required on individual behavior tracked over time as well as on individual demographic characteristics before the prime to see if individuals from different groups diverge in behavior after the prime. Detailed behavior on U.S. appellate judges, which we compile from various sources, therefore provides an ideal context to study priming effects in the field. Because of the richness of the dataset, we can also observe whether the contexts and characteristics that make individuals more susceptible to priming in the lab are also found in the field.

Our evidence suggests several forms of electoral cycling in the expression of ideological commitments by appellate judges. Just before presidential elections, judges roughly double the rate at which they dissent and vote along partisan lines. Conditional on dissenting, judges dissent before elections on miscellaneous procedural issues, not the substantive procedural or merit issues, and they dissent on opinions that are less controversial, suggesting that judges may be finding more subtle reasons for dissent. Judges are 10% less likely to affirm and 10% more likely to reverse lower court decisions during this season. The shift in voting valence due to the electoral cycle is equivalent to a one-decile shift in judicial ideology score (based on a judge's lifetime of votes).

A number of related findings are consistent with a partisan priming characterization of the electoral cycle. In particular, we find field evidence for what experimental studies have documented regarding what makes individuals more susceptible to priming: contentious environments¹³, inexperience¹⁴, and greater political knowledge¹⁵. First, we observe stronger electoral cycles in dissents for those who are more subject to contention and polarization, whether through panel composition, general political climate, or ideological polarization. Not

¹³ Srull and Wyer (1979).

¹⁴ Krosnick and Kinder (1990).

¹⁵ Kimball (2005).

only are judges voting along partisan lines, it is the judges in the political minority (assigned to a 3-judge panel with no other judge affiliated with the same political party) and judges in the political majority with the more extreme ideology score who are increasing their dissent rate before elections. Judges sharing the same ideology score but from different political parties triple the rate at which they dissent before an election. Moreover, ideological polarization, measured through partisan voting or the degree to which judges time retirement contingent on the President's party, has increased sharply over time in tandem with electoral cycles.¹⁶ Landslide elections and wartime ameliorate electoral cycles while close elections exacerbate electoral cycles. Midterm elections also trigger voting along partisan lines. Electoral cycles are greatest in circuits with swing states. Second, the cycling behavior is more pronounced for inexperienced judges.¹⁷ Third, judges with greater political knowledge, such as those who previously served as U.S. Attorneys or Assistant U.S. Attorneys¹⁸, are more likely to display electoral cycles.

If U.S. presidential elections prime political group identities and lead to polarization and dissensus, do primes for national group identity have the opposite effect? During periods of national political reconciliation we observe a reduction in the rate of dissent: in the period after

¹⁶ Electoral cycles in dissenting behavior, in retirement, and in the partisan voting by judges along ideological lines all increased sharply around 1975. See generally, Pildes (2010) on the causes and consequences of increased ideological polarization.

¹⁷ Judicial experience may be related to the control of unconscious biases. We find that judges appointed to the court within the last 1 or 2 years are more uniform in their electoral cycles. Experimental research with judges, however, has *not* found that judicial experience mediates the role of psychology in judicial decision-making (see, e.g. Rachlinski, Guthrie, and Wistrich 2006, and English, Mussweiler, and Strack 2006). On the other hand, other experimental research have found that inexperience magnifies priming effects (Krosnick and Kinder 1990) and observational studies have found that new judges go through an acclimation period during which their policy preferences are unstable and may stumble more than their colleagues (Owen and Black (2010), Hettinger, Lindquist and Martinek (2003a)).

¹⁸ U.S. Attorneys and Assistant U.S. Attorneys are also known as federal prosecutors. These kinds of positions are uniquely both highly political and legal in the sense that they can choose to enforce or not enforce different aspects of federal law. Service in the roles frequently leads to higher office (Engstrom 1971, Gordon 2009).

an election¹⁹, in the three months after September 11, and during World War 2, the Korean War, and the Vietnam War. Notably, we find that the types of judges and panels that are particularly likely to be primed by elections for dissensus are also the ones that are more likely to be primed by wartime for consensus. Dissents are reduced during wartime particularly among divided panels and by less experienced judges. Lower court decisions are also less likely to be reversed and more likely to be affirmed.

Our study is most closely related to Abrams, Bertrand, and Mullainathan (2008) and Shayo and Zussman (2010). Both studies, like ours, rely on the random assignment of judges to cases to address omitted variables, but examine racial bias rather than ideological bias. Abrams et al. do not find prima facie evidence of ingroup bias nor use priming to ascertain a causal effect of group identity. Shayo and Zussman find that Jewish and Arab judges disproportionately find in favor for litigants of their own race, and this disproportionality increases after terrorist events. However, they do not rule out a possible shift in litigant strategy or the type of case appearing before the court in the year after a terrorist event. Our data has detailed information on litigant strategy and case type. The cases in Abrams et al. are felony cases from county judges and the cases in Shayo and Zussman are small claims cases (mostly traffic accidents), where judges may have less professional commitment to be unbiased. Other studies related to ours are ones that document the role of mood or arousal in decision-making, e.g. soccer outcomes and stock returns (Edman, Garcia, Norli 2007), cloudy weather and stock prices (Saunders 1993), weather and the college decision (Simonsohn 2010), and football outcomes and family violence (Card and Dahl 2011); these studies, however, are less about documenting priming than about cueing effects, nor do they examine individual-level field evidence for what experimental studies have found

¹⁹ The trough in dissents and partisan voting could reflect a political honeymoon period, when the new president is typically putting a new team together and each party waits awhile before conducting ideological attacks.

regarding the contexts and characteristics that make people more susceptible to priming.

The remainder of our paper is organized as follows. Section 2 presents the framework and data. Section 3 characterizes the electoral cycles in judicial behavior among unelected judges. Section 4 presents evidence explaining the electoral cycles. Section 5 documents the electoral cycles over time. Section 6 presents the data on electoral cycles in judicial retirements. Section 7 concludes.

2. Framework and Data

To motivate our empirical specifications, we build a simple model of group identity and latent ideology based on Akerlof and Kranton (2000) and Benjamin, Choi, and Strickland (2009) except that we also consider the effect of varying degrees of ideological polarization. Priming social categories, such as party affiliation, reveals the marginal effect of increasing the strength of affiliation with group identity on ideology. Consider latent ideology Q . A judge belongs to a social group C , such as Republican or Democrat, with strength s . Let Q_0 denote the judge's ideology without considerations of his/her identity, and let Q_C denote the ideology preferred by members of the social group that the judge belongs to, C . Each judge chooses Q to maximize $U = -(1-w(s))(Q - Q_0)^2 - w(s)(Q - Q_C)^2$, where $0 \leq w(s) \leq 1$ is the weight placed on social group C in the judge's utility function. Assume that $w(0) = 0$ and $w' > 0$. This assumption means 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.

Assume that s has a steady-state value s but can be temporarily perturbed away from s by a group identity prime $\varepsilon > 0$. Following the literature, we assume that $s(\varepsilon)$ and $s' > 0$. Experienced individuals would have smaller $s'(\varepsilon)$ because conscious processing can override the usual or habitual response to priming. Individuals with high levels of political knowledge would

have larger $s'(\varepsilon)$ because associative links have already been formed and the stronger the association, the greater and faster an activated concept will spread (Bargh and Chartrand 2000).

The first-order condition gives $x^*(s) = (1-w(s))Q_0 + w(s)Q_C$, a weighted average of the preferred action without identity considerations and the group norm. This implies that a group identity prime ε causes Q^* to move closer to Q_C . In particular, one can see that $\Delta Q = Q^*(s) - Q_0 = w(s)(Q_C - Q_0)$. Polarization is typically modeled as the presence of extreme value differences (Esteban and Ray 1994, Duclos et. al. 2004), then the more contentious or ideologically polarized the environment, the larger is $|Q_C^- - Q_C|$ and the bigger the effect of priming on Q^* .²⁰ More polarized environments, such as in divided panels, close elections, swing states, or recent years, should see a bigger effect due to priming.

Priming ideology, by shifting Q^* to be closer to Q_C , will cause voting to be more aligned along partisan lines. Our measure of vote valence therefore directly captures latent ideology Q^* moving further apart. Dissents increase with the distance between Q^* on divided panels. That is, the utility of dissent increases (cost of dissent falls) the farther apart are the latent ideology Q^* of the panel of judges. Finally, priming causes only the marginal dissents to increase. Assuming declining marginal utility of dissents based on ideological principles to dissents based on procedural quibbles (Appendix Figure 6²¹), then priming of latent ideology shifts the utility of dissent outwards or the cost of dissent downwards, either of which would lead to marginal dissents to increase.

As prima facie evidence of possible priming, the magnitude of the priming effect does not appear for the most ideologically extreme judges, but rather at two interior locations of the judge's ideology score (See Appendix Figure 4). If ideology goes from 1 (liberal) to 5

²⁰ For simplicity, here we assume there are two categories and the baseline of individuals Q_0 is in between Q_C^- and Q_C .

²¹ We thank our discussant Nolan McCarty for this figure and observation.

(conservative), those at the ends (1 and 5) are extremely ideological and are always more likely to disagree and dissent. The 2 and 4's are usually less ideological and willing to compromise. However, priming pushes them a bit afar, enough so that they may dissent more often. For example, the 5 and 2's would never agree, regardless of priming, nor would the 1 and 4's. However, 2 and 4 may be able to reach an agreement without priming, but when they are primed, they are shifted enough that they disagree more.

2.1 Basic aspects of the U.S. legal system

Understanding several aspects of the United States legal system is important to put our study in context. First, U.S. appellate courts handle the vast majority of cases deciding issues of new law and provide new interpretations or distinctions of pre-existing precedents or statutes.²² Unlike many European legal systems, the United States has a common law system where American judges not only apply the law but also make the law. This effective making of law occurs since a judge's decisions in current cases become precedent for use in decisions in future cases. Second, it is worth briefly discussing the significance of judicial dissents. Dissents are extraordinarily costly as they are not determinative of the final decision, but require dissenting judges to write an opinion. Dissents also corrode judicial collegiality and create crowding in the judicial docket.²³ Nevertheless, dissenting opinions can sometimes be cited as

²² There are three layers of courts in the United States. District courts are the general trial courts. When cases are appealed, they go to appellate courts, referred to as circuit courts, which typically decide issues when there are new fact patterns or look to see if the district court was in error. A small portion of these cases is appealed again to the Supreme Court. In our dataset, only 2% of appellate cases reach the Supreme Court.

²³ Roughly 8% of appellate cases have dissents but 20% of Supreme Court cases reviewing appellate court cases had an appellate dissent. In the sample of Ninth Circuit decisions analyzed by Berdejo (2010), decisions in which a dissent is filed are more likely to be reheard en banc (i.e., by the entire court, not just 3 judges) (4.9%) and be granted certiorari by the U.S. Supreme Court (i.e. heard) (1.4%) than decisions without a dissent. Crowding of the judicial docket decreases the attention that judges are able to spend on cases and can have significant consequences (Huang 2010). Among our sample of cases, the

persuasive authority when arguing that a court's holding should be limited or overturned. Third, appellate judges are randomly assigned²⁴ to cases, and each case typically receives three judges, which can be from either political party. Therefore, the panel can be politically divided or not. We will discuss randomization in further length in the following section. In the next sub-section, we describe our dataset and the construction of the variables used in our analyses.

2.2 *Variables used in our analyses*

We obtained case-level data from the Appeals Court Database Project.²⁵ This database includes information on opinion-specific variables (including the names of the judges sitting on each panel) for a random sample of cases from the period 1925 through 2002. 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.²⁶ We restrict our analysis to cases decided by panels composed of three judges and, for a few analyses, exclude cases where we were unable to obtain biographical information for the judges sitting in the panel.²⁷ Using these datasets, we construct the following variables:

- *Dissent*: Our main outcome variable of interest, *dissent*, equals 1 if one of the judges sitting in the panel dissented from the majority opinion.

cases with dissents before and after the presidential election are reheard en banc or granted certiorari by the U.S. Supreme Court at statistically similar rates.

²⁴ Some judges take a reduced caseload but all are randomly assigned by a computer and are typically not revealed to the litigating parties until after they file their briefs and sometimes only a few days before the hearing, if there is a hearing.

²⁵ Documentation and data available at <http://www.cas.sc.edu/poli/juri/appctdata.htm>.

²⁶ Documentation and data available at <http://www.cas.sc.edu/poli/juri/auburndata.htm>.

²⁷ The bulk of our analyses are unaffected by missing data. For a few of the individual, vote-level regressions, some judges are missing data regarding elevation and previous service as Assistant U.S. Attorney or U.S. Attorney. The regressions that use this data have a smaller 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 years of experience. These regressions have a 13% smaller sample size.

- *Reason for Dissent*: For cases decided in the period 1997-2002, we are able to obtain reasons for dissent (i.e., for procedural or merit reasons).²⁸ For the entire time period, we are able to obtain whether the majority opinion mentions procedural issues and what type of procedural issue.²⁹
- *Electoral Proximity Measures*: We construct both linear and nonlinear measures of “electoral proximity,” which is equal to the number of quarters remaining until the next presidential election (we calculate the quarters relative to November 1). This measure, *quartertoelect*, ranges from 16 to 1. In most specifications, we include a full set of indicator variables for each quarter.³⁰
- *Ideological Measures*: To measure the ideological division within a panel, we construct the variable *divided*, which equals 0 if the three judges assigned to the case belong to the same political party and equals 1 otherwise.³¹
- *Legal Issue*: Based on data from the Appeals Court Database Project, we classify cases into the following categories: criminal, civil rights, constitutional (i.e., First Amendment, due process and privacy cases), labor relations, economic activity and regulation and a miscellaneous category.³²

²⁸ The Appeals Court Database asks whether or not the author of a dissent used a similar “issue frame” as that employed in the majority. The idea is to capture whether the dissent has re-framed the case as presenting a different issue—other than that addressed in the majority opinion. The database codes whether the dissent addresses a procedural issue or addresses a merits issue. We acknowledge the likely difficulty the coders faced, but note that the measurement error is unlikely to be systematically different just before an election as opposed to just after an election.

²⁹ These procedural issues include standing, mootness, statute of limitation, government immunity, frivolousness, admissibility of evidence, and so on.

³⁰ In some specifications, we aggregate the electoral proximity measure to the three quarters preceding the presidential election. In robustness checks, we also use a linear measure for the number of quarters remaining until the next presidential election.

³¹ The database we use to obtain judge’s biographical information 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.

³² We also use finer categories of legal issues in our randomization check discussed below.

- *Opinion Length:* To measure the length of an opinion, we count the number of pages of the majority opinion in the corresponding federal reporter. This information was obtained from the Appeals Court Database Project database. To ensure that our results are not driven by outliers, we winsorize this variable at the 1% level, that is, we censor all observations outside of 1% to 1%.
- *Citations:* Using Lexis’s *Shephards* service, we count the number of times that an opinion has been cited in subsequent cases and how often it has been cited in subsequent dissents.³³ To ensure that our results are not driven by outliers, we winsorize these variables at the 1% level.
- *Judicial Background Characteristics:* Using the Multi-User Data Base on the Attributes of U.S. Appeals Court Judges, we obtain a number of judge background variables including gender, date of birth (which we use to calculate a judge’s age), professional experience (i.e., whether a judge was previously a U.S. Attorney or Assistant U.S. Attorney) and year of appointment (which we use to calculate a judge’s judicial experience), as well as the political affiliation of the judge.
- *Political Environment:* From the judicial background variables and data from CQ Voting and Elections Collection³⁴, we can identify whether a judge is affiliated with the same political party as the incumbent President at the time a case is decided; similarly, we can identify Presidents who won closely contested elections (i.e., with electoral counts less than 55%) or landslide elections (i.e., with electoral counts greater than 95%).

³³ We have citation data for 18,649 of our 18,686 observations. The citation data is current as of March 26, 2011. In addition, we calculate the total citation count for the judicial opinions authored by the judge and contained in the Appeals Court Database Project.

³⁴ 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, <http://library.cqpress.com/elections/g2c6e2-973-36489-1842592>. Document ID: g2c6e2-973-36489-1842592.

- *Decision Valence*: We use the Appeals Court Database measures of ideological valence of a decision, conservative, liberal, mixed, or unable to code. We code a liberal vote as 1, a conservative vote as -1, and a mixed or unable to code³⁵ vote as 0.³⁶
- *Treatment of Lower Court*: We use the Appeals Court Database measures of treatment of the lower court decision. We code *Affirm* (0 or 1) and we code *Reversed* (0 or 1, which includes (a) reversed and (b) reversed and remanded). The other treatments are (c) stay, petition, or motion granted, (d) vacated and remanded, (e) affirmed in part and reversed in part, (f) affirmed in part, reversed in part, and remanded, (g) vacated, (h) petition denied or appeal dismissed, (i) certification to another court, (j) not ascertained. These other treatments would all be coded as 0 for *Affirm* and *Reversed*.
- *Retirements and Resignations*: There are several reasons for which a judge may leave the bench, including retirement, resignation, death, elevation to the Supreme Court or demotion to the district court, among others. We examine the first two reasons, retirement and resignation, as decisions to voluntarily leave the bench.
- *Ideological Measures*: We code two measures of ideological commitment. First, we code whether judges are leaving when the president in power is from the same party that appointed the judge. Second, we code whether the judge is a Democrat appointee or a Republican appointee.³⁷

³⁵ Our results are robust to dropping the votes that are unable to be coded.

³⁶ The Appeals Court Database Project codes directionality as follows. 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". We acknowledge that these directionality measures are attempting to capture a moving target: what is liberal or conservative in one decade may not be the same as during another decade. However, the possible bias introduced by this measurement that the coders faced is unlikely to shift just before elections.

³⁷ Judges from third parties and leaving under a non-two party system are dropped from the analysis when our analysis stratifies along these dimensions.

Our final sample contains 18,686 decisions for the period 1925-2002.

2.3 *A note on randomization and timing*

Before presenting the results of our analyses, it is important to consider a key assumption that needs to be satisfied for our estimates of the effects of electoral proximity on judicial behavior to be unbiased: panel attributes and cases must be randomly assigned across the political cycle (conditional, of course, on the control variables that are included in the model). In practice, an important concern is that aspects of a case that we cannot observe (some characteristics of the appeal, of the parties, etc.), and that could affect case decisions, are changing over time in a way that is correlated with our measure of electoral proximity.

Although appellate judges in each circuit are randomly assigned to 3-judge panels, we must still consider the extent to which judges can control whether a case appears in our sample and where in the sample it appears (relative to the electoral cycle). For example, the database we employ in our analyses considers only published decisions, which are not necessarily a random sample of the entire universe of decisions rendered by the appellate courts. In fact, Berdejo (2010) finds that panel composition is related to the decision of whether or not to publish an opinion. Empirically, this bias could be problematic if the decision to publish is a strategic one.³⁸ Even within the universe of published decisions, one could still worry that judges may choose to decide certain cases in their docket at an earlier or later time.³⁹ If the most controversial cases were consistently decided during the months leading up to presidential

³⁸ For a discussion of the strategic use of publication rules by judges see Law (2005). For purposes of this paper, focusing on published decisions is advantageous as these cases are more likely to raise difficult issues and establish new precedent.

³⁹ For instance, Berdejo (2010) finds that published opinions take more than twice as long to be finalized after oral arguments take place than unpublished opinions (122.1 days vs. 50.4 days). Similarly, opinions in which a dissent is filed take longer to be finalized (139.4 days) than opinions in which there is no dissent (70.7 days).

elections our measure of electoral proximity could capture the effect of this change in the composition of cases.

Using data from the Administrative Office of the Courts (AOC) for 1991-2006, we verify that the decision to publish opinions does not appear to be correlated with the presidential election cycle.⁴⁰ Columns 4 and 5 of Appendix Table B show that cases per month and fraction of opinions published per month do not vary over the cycle. This suggests that whether a case appears in our sample is not varying over the cycle. We can also examine whether the time elapsed between docket filing and judgment, and time between hearing and judgment dates vary over the cycle. Interestingly, opinions published in the quarter before a presidential election take more than average time between hearing and judgment date⁴¹, consistent with the notion that judges are taking a little more time to write the dissents (Column 11). In addition, given that on average it takes two or three months after a hearing to get a decision (Column 9 of Appendix Table B), the decision to dissent could to occur up to three months before the actual publication date.⁴² This does not change the main inference that in the quarters leading up to a presidential election, judges are more likely to dissent.

Since we do not know exactly when a judge makes a decision to dissent (as we only have the date of publication in our main dataset), we cannot precisely pinpoint when the decision to

⁴⁰ We cannot, however, rule out the court choosing to publish more divisive cases before the election. This scenario sounds like priming but provides a slightly different interpretation. We also cannot rule out court management pushing along divisive cases to be published just before the election and delaying non-divisive cases to be published just after the election. Court management, however, is unlikely to be able to time the pipeline to such an extent so as to explain the lull in dissents during wartime.

⁴¹ This difference is not statistically significant, however.

⁴² When we examine the dissent rate on a month-by-month basis (see Appendix Figure B), the peak in dissents occurs three months before the presidential election and the trough in dissents occurs 5 or 8 months after the election (the graph displays coefficients on the month-to-election dummies from a regression that also controls for whether a panel is divided and circuit and year fixed effects, an analog to Column 1 of Table 1). Therefore, the timing between hearing and judgment is unlikely to cause the actual trough in dissents to move before the election. When we examine the partisan voting on a month-by-month basis, we also observe the peak in partisan voting in the third month before the presidential election when we use a limited sample of month dummies, such as a dummy for each month up to four months before and four months after the presidential election.

dissent occurs. However, if we use the estimate in Berdejo (2010) for the average length of time between hearing and judgment for opinions with dissents (140 days) vs. opinions without dissents (71 days)⁴³, we can backdate each opinion to its hearing date. Using these calculated hearing dates, we find no evidence of electoral cycling. We take this as tentative evidence that the decision to dissent or not may occur quite late in the opinion-writing process, which is consistent with the finding that judges appear to be dissenting on procedural issues, in particular on miscellaneous procedural issues (in results described further below), which may be expected to take less time to do. Moreover, in conversations with former appellate clerks, the conventional view of judges convening right after the hearing to decide who votes in which way and whether someone will write a dissent may be incomplete. It is frequently possible that the judges decide in the last minute to *not* dissent. Therefore, one interpretation of our findings is that electoral cycles prime judges to stick to their dissent.⁴⁴

As a standard check on randomization, we test whether case type, caseload, and composition of the 3-judge panels vary over the electoral cycle. Not surprisingly, the proportion of ideologically divided panels is evenly distributed across the political cycle, as shown in Column 1 of Appendix Table C. Case composition is also evenly distributed across the political cycle as shown in Columns 2-6 of Appendix Table C. Finally, confirming that the net outcome of the number of cases filed, the fraction of cases published, and the time it takes to become published leads to an even distribution across the political cycle, Column 7 shows that the

⁴³ Decisions rendered in the quarter immediately preceding a presidential election take longer to be drafted (after oral arguments) than decisions decided earlier in the electoral cycle (86.9 vs. 75.7 days). This difference is similar for cases in which a dissent was filed (150.4 vs. 138.6 days).

⁴⁴ Relatedly, it is possible that judges typically suppress dissents too much and the dissents during the quarter before the election are actually a good thing. Under this view, courts are not dissenting or reversing lower courts enough during the other 15 quarters of the electoral cycle. Our inferences regarding who is prone to priming and the causal effect of group identity on ideology nevertheless are unaffected.

number of cases is also evenly distributed across the political cycle in our sample of published opinions.

We then exhaustively explore whether any characteristic of the case and lawyer behavior as coded in our database varies over the electoral cycle.⁴⁵ In results available from the authors, we find no electoral cycle along substantive issues.⁴⁶ Along four procedural issues—issues in the “other” category⁴⁷—we find some evidence of electoral cycling, and this dimension of our analysis will be further explored in the next section.⁴⁸ These results reassure us that the dissent cycles we observe and analyze below are not the result of changes in the composition of cases due to the strategic behavior of litigants.⁴⁹

3. Characterizing the Electoral Cycles in Judicial Behavior Among Unelected Judges

3.1 Dissenting Behavior Across the presidential Election Cycle

At the appellate level where cases are decided by panels, judges may express their preference for a particular outcome by joining the majority opinion or filing a dissent. If a

⁴⁵ The database codes 106 characteristics.

⁴⁶ Substantive issues include whether there was an issue of constitutionality, whether the court engaged in statutory interpretation, how many appellants or respondents were persons, businesses, public interest groups, or government actors, 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, and so on.

⁴⁷ We find an increase in the mention and intensity of mention (i.e. discussed the question and answered it affirmatively vs. negatively vs. no mention at all) regarding (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. The last question is asked in the context of asking about discovery and evidentiary issues. About 1 to 6% of the cases mention these issues. What is remarkable about all four of these issues is that they are quite discretionary. For example, attorneys’ fees are only to be awarded to the appellant in exceptional circumstances (*Rolax v. Atlantic C. L. R. Co.* 186 F2d 473).

⁴⁸ Our results are robust to the inclusion of controls for the four procedural issues that appear to follow the presidential election cycle.

⁴⁹ The dissent rate cycle is more statistically significant in criminal, civil rights, and economic activity cases, for which we have more observations, and the point estimates are similar.

judge's political ideology plays an important role in how that judge votes in a case, then one could expect ideological differences among the members of a panel to be associated with higher rates of dissent.⁵⁰ In this section, we document another nuance in the relationship between judicial ideology and voting behavior.

Figure 1 presents the fluctuation of the average dissent rate across the presidential electoral cycle. As we can see, 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% (on average, a judge dissents from the majority opinion in 8% of the cases in our sample). To further examine this preliminary finding, we next control for factors that may explain the variation we observe in the rate of dissent across the political cycle. For instance, the ideological composition of the panel hearing a case could be one of the factors determining whether a dissenting opinion is written. That is, if judicial ideology plays a role in the adjudication process, then one could expect a conservative judge to be more likely to dissent when sitting on a panel with two liberal judges than when sitting in a panel with two other conservative judges.⁵¹ Thus, in our regressions we include an indicator variable equal to 1 if an ideologically divided panel heard the case. Since circuit courts may differ in characteristics that may affect the rate of dissent (such as culture or collegiality), we include a set of circuit court fixed effects. In addition, we include a set of legal issue fixed effects to control for differences in case composition as some legal issues may be in general more controversial than others. Finally, we include a set of year-specific fixed effects to capture shocks or trends affecting judicial

⁵⁰ The link between a judge's ideology and voting behavior is rather nuanced: a series of papers has documented "panel effects" – not only does the political ideology of a particular judge affect how that judge votes in a given case, but so does the ideology of the other judges sitting in the panel (see, e.g., Revesz (1997), Cross and Tiller (1998), Revesz (2001) and Sunstein et al. (2004)). To account for these "panel effects" scholars have proposed a number of theories where judges behave strategically to maximize their preferences in the long run by sacrificing (or moderating) their views in any given case (see e.g., Cross and Tiller (1998), Hettinger et al. (2004), Landes and Posner (2008), Epstein et al. (2010), Sunstein et al. (2004), and Kim (2009)).

⁵¹ In our sample, 70% of the cases are decided by ideologically divided panels.

decision-making that are common to all judges in a given year and, in certain specifications, we also include a set of fixed effects for each quarter of the year (e.g., January through March, April through June, etc.), in order to control for the effects of seasonality on judges' adjudication behavior.⁵²

The explanatory variable of interest is our electoral proximity measure *quartermtoelect*, an indicator variable representing the number of quarters left before the next presidential election. If the dissenting behavior of judges did not vary across the election cycle once we controlled for the factors discussed above, we would expect the coefficients on all the *quartermtoelect* indicators to be small and statistically insignificant. On the other hand, if judges are more likely to dissent as the next presidential election approaches, we would expect the coefficients on the indicators corresponding to the quarters closest to the election to be positive and statistically significant. With this in mind, consider the following linear specification for latent ideology:

$$Q_{ip} = \alpha_{0p}F(t) + \alpha_{1p}Proximity_i + \alpha_{2p}Z_i + \varepsilon_{ip} \quad (1)$$

where, Q_{ip} is ideology for judge i before and after the prime $p = 1$ or 2 ; $F(t)$ includes a set of year and quarter fixed effects; $Proximity_i$, our explanatory variable of interest, is the set of quarter-to-election fixed effects described above;⁵³ Z_i contains a dummy indicating whether the panel was divided, a set of case controls including fixed effects for different types of legal issues (criminal, civil rights, constitutional, labor relations, and economic activity) and a set of circuit fixed

⁵² For example, controlling for seasonality rules out the possibility that the observed dissent cycle is simply due to the holiday season reducing dissents. When we control for seasonal quarter effects, we will therefore be identifying the coefficient for 1 quarter-to-election by comparing October vs. November and December. Without seasonal quarter effects, our 1 quarter-to-election coefficient is identified from comparing August to October vs. November to January.

⁵³ In the regressions presented in the tables that follow, the omitted quarter is quarter 16, i.e., the quarter immediately following an election.

effects.⁵⁴ The subscript p on α_{ip} allows the effect of exogenous control variables, *Proximity* or case characteristics, to vary before and after the prime.

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

$$\Delta Q_i = (\alpha_{02} - \alpha_{01})F(t) + (\alpha_{12} - \alpha_{01})Proximity_i + (\alpha_{22} - \alpha_{21})Z_i + \omega_i \quad (2)$$

where ΔQ_i represents changes in ideology due to priming and $\omega_i = \varepsilon_{1p} - \varepsilon_{0p}$. Recall that our theoretical model suggests that $\Delta Q_i = x^*(s) - x_0 = w(s)(x_C - x_0)$. Since electoral *Proximity* _{i} perturbs s , this motivates the interaction specifications that follow below since $s'(\varepsilon)$ may be smaller or larger due to exogenous characteristics.

In practice ΔQ_i is unobserved. Instead, our primary observations are of voting valence and dissents. In the simplest model, the probability of dissent increases with the size of the prime though of course this depends on whether the panel is politically divided, whether the judge is in the minority party of the 3-judge panel, and so on. In particular, we first estimate the linear probability specification:

$$Y_i = F(t) + \beta_1 Proximity_i + \beta_2 Z_i + \varepsilon_i \quad (3)$$

where, Y_i , our outcome of interest, is an indicator variable equal to 1 if a dissent was filed in case i ; alternatively, Y_i is voting valence of liberal (1), conservative (-1), or mixed (0).⁵⁵ Because Y_i has the following ordered structure:

$$Y_i = \begin{cases} 0 & \text{if } \Delta Q_i \leq \mu \\ 1 & \text{if } \Delta Q_i \geq \mu \end{cases}$$

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

⁵⁴ Unless otherwise noted, in all calculations of statistical significance, we estimate case-level specifications allowing for clustering of the error term at the quarter-year level and estimate vote-level specifications allowing for clustering of the error term separately by quarter-year and case following the double-clustering algorithm outlined in Cameron, Gelbach, and Miller (2010) and Peterson (2008). The latter accounts for both serial correlation and the fact that dissents necessarily correspond with two non-dissenting votes at the case level.

⁵⁵ We study this alternative measure in more depth in Section 4.2.2.

primes ideology:

$$\Pr(\Delta Q_i = -1 | \mathbf{I}_i) = F_{\omega_i} (\mu - (\alpha_{02} - \alpha_{01}) F(t) - (\alpha_{12} - \alpha_{11}) Proximity_i - (\alpha_{12} - \alpha_{11})' \mathbf{Z}_i | \mathbf{I}_i)$$

$$\Pr(\Delta Q_i = 1 | \mathbf{I}_i) = 1 - F_{\omega_{ij}} (\mu - (\alpha_{02} - \alpha_{01}) F(t) - (\alpha_{12} - \alpha_{11}) Proximity_i - (\alpha_{12} - \alpha_{11})' \mathbf{Z}_i - \gamma_j | \mathbf{I}_i)$$

where $\mathbf{I}_i = (F(t), Z_i)$ and $F_{\omega_{ij}}(\cdot | \mathbf{I}_{ij})$ is normal. Equation (1) is the linear approximation of the probit specification. We will estimate linear probability and probit specifications.

The results are presented in Table 1. Not surprisingly, dissents are more likely to occur when a panel is divided along ideological lines. Holding all other factors constant, decisions reached by divided panels are roughly 20% more likely to have a judge dissenting from the majority opinion (a result significant at the 1% level).⁵⁶ More importantly, the coefficients on our electoral proximity measure indicate that 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.⁵⁷ Using our baseline specification (Table 1, Column 1), we find that when a case is decided in the quarter immediately preceding an election, the probability of there being a dissent increases by 6.3% percentage points when compared to the quarter following the election, an increase which represents over 75% of the average rate of dissent.⁵⁸ In fact, this is true if we compare the quarter immediately preceding an election to any other quarter in the two years immediately following an election.⁵⁹ More generally, one can note that all the coefficients for the last 8 quarters in the presidential electoral calendar are greater in

⁵⁶ The R-squares are low even with a full set of fixed effects since we have a binary outcome variable.

⁵⁷ The t-statistic on the quarter-before-election is 4.01. When we randomly assign each quarter to a different quarter-to-election and rerun our basic specification 200 times, the 95% interval for t-statistics is between positive and negative 2.62, indicating that our estimates are statistically significant even with a natural bootstrap.

⁵⁸ The average rate of dissent is 7.8% as indicated in Table A.

⁵⁹ This follows from the fact that the coefficients for quarter-to-elect-15 through quarter-to-elect-9 are not only statistically insignificant, but have negative sign in Columns 2 and 4. A similar inference can be made with respect to quarter-to-elect-8 through quarter-to-elect-4, as these coefficients are statistically undistinguishable from zero; however, since they have positive coefficient the magnitude of the difference in the probability of dissent is likely to be slightly smaller than 75%.

magnitude than those corresponding to the earlier half of the cycle. Adding controls for seasonality yields slightly smaller, but still large and significant coefficients for the quarters leading to an election (Column 2), leaving our inferences unchanged.⁶⁰ Columns 3 and 4 show the main results are unchanged when using a probit model.⁶¹ When we compare the dissent rate in the last quarter with all other quarters, the increase in 3.6% percentage points represents a 50% increase in the average rate of dissent.

Conditional on dissenting, the reason for dissent also follows the presidential electoral cycle.⁶² We code a dummy variable equal to 1 if the dissent mentioned a procedural reason but did not mention a merit reason.⁶³ On average, 9% of dissents mention procedural but not merit reasons for the dissent (Appendix Table A). In the quarter immediately before an election, this motivation increases by 8% percentage points when compared to all other quarters, representing a substantial effect (Column 4 of Table 2).⁶⁴ Perhaps judges are looking for easier reasons to dissent before election or perhaps procedural dissents are a less direct form of dissent so judges can avoid injecting themselves into the political process. In our investigation of whether any case characteristic is correlated with the electoral cycle, only four 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

⁶⁰ Appendix Table D displays several robustness checks, including a linear version of the quarters to election (Column 1), dropping one circuit at a time (Column 2), keeping one circuit at a time (Column 3), and parsimonious probit and logit models (Columns 4 and 5).

⁶¹ The OLS coefficients are similar in magnitude to the marginal effects of the probit specification.

⁶² However, we only have 227 dissents coded between 1997-2002 so it remains to be seen whether this pattern remains as the data is extended into the future.

⁶³ This suggests that judges are not simply trying to strategically draw attention to certain substantive issues on their political agenda before elections.

⁶⁴ Column (9) does not include quarter-to-election fixed effects because there are only 227 observations, however, the result is robust to the inclusion of these controls.

there was some “other issue” of civil law.⁶⁵ About 1 to 6% of the cases mention these issues. What is remarkable about all four of these issues is that they are quite discretionary. For example, attorneys’ fees are only to be awarded to the appellant in exceptional circumstances (Rolax v. Atlantic C. L. R. Co. 186 F2d 473).

3.2 *Length of Opinion and Subsequent Citations*

Next, we explore whether those opinions written right before an election differ on dimensions other than whether or not a judge dissented from the majority opinion. Of particular interest are those attributes that may be associated with the opinion’s potential contribution to the development of the law. Following Choi et al. (2008), 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 (Appendix Table A). To this end, we estimate the following specification,

$$Y_i = F(t) + \beta_1 LastQuarter_i * Dissent_i + \beta_2 Dissent_i + \beta_3 LastQuarter_i + \beta_4 Z_i + \varepsilon_i \quad (4)$$

and show, in column (1) of Table 2, that opinions written when there are dissents are over 40% longer than opinions without dissents. 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. Similarly, the results in Columns (2) and (3) suggest that cases decided when there are dissents are cited more often in subsequent opinions and dissents. The coefficients on the dissent indicator variables in column (2) represent over 30% of the average citation count for opinions in our data

⁶⁵ These procedural variables are coded for the entire time period of the dataset.

and in column (3) represent over 75% of the average count of citations from future dissents for opinions in our data. Citations by subsequent dissents suggest controversy or ambiguity in the law, perhaps occurring during the development of new law (Choi et al. (2008)).⁶⁶ Based on these results one could argue that opinions written in the quarter preceding an election are not of lower quality or of lower importance or precedential value.⁶⁷ However, the results in column (3) suggest that opinions written in the quarter preceding an election and in which a dissenting opinion is filed are cited less often by subsequent dissents than cases decided in other quarters (in which a dissent was filed).⁶⁸ One interpretation of this finding is that judges choose less controversial cases to dissent from in the quarter preceding an election.⁶⁹

3.3 *Substantive Impact*

To explore whether electoral cycles has a substantive impact on the law, we examine whether the appellate court affirmed or reversed the lower court. If the priming effect of electoral proximity causes variance in ideology to increase, then one may expect a decline in affirmations and an increase in reversal rates. 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 as can be seen in Columns (5) and (7) of Table 2. The baseline is 57% affirmation and 22% reversals as indicated in Table A summary statistics.

⁶⁶ This could occur, for example, 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.

⁶⁷ Rough estimates for the length of the dissent and whether the dissent was published (using the 1%-winsorized difference between the last page of the opinion and the last page of the case) also do not vary over the electoral cycle. A priori, however, dissenting for miscellaneous procedural issues could lead to longer or shorter dissents.

⁶⁸ The decrease in citations may also be explained by the quality of the judge being assigned to write the opinion in the quarters before an election. In any case, we are unable to definitely say whether lower citations reflects the impact of electoral cycles or the decline in importance of the cases being dissented from over the electoral cycle.

⁶⁹ An alternative explanation is that dissents before elections cause a reduction in future citations to the majority opinion by subsequent dissents.

However this effect is not robust to the inclusion of quarter fixed effects (included in Columns 6 and 8) suggesting that part of the shift is seasonal.

3.4 *Strategic Hypotheses*

Before presenting our examination of the partisan priming hypothesis for electoral cycles, we first rule out alternative explanations for the electoral cycles in dissents documented above. We consider several prominent strategic explanations for electoral cycling: re-election considerations, aspirations for higher office, getting out the vote, and several others. Some of our discussion touches on results explained in more detail in subsequent sections. However, since strategic considerations are the *prima facie* explanations for judicial behavior, we discuss strategic hypotheses here.

Because appellate judges are appointed for life, re-election considerations are immaterial to our analysis. However, one could argue that judges who dissent before elections may be signaling to raise the likelihood of their elevation to the Supreme Court. A priori, it is not clear whether or not this is a good strategy since a nominee needs to be confirmed by the Senate, which may be controlled⁷⁰ by the opposing political party. Our data suggests that judges are no more likely to dissent when their party wins the Presidency and controls the Senate after the election. Moreover, Column 1 of Table 3 indicates that judges that were elevated following an election were no more likely to have increased their rate of dissent before such elections.⁷¹ Even if judges are just trying to get noticed, and it is the lower quality judges who are trying to get

⁷⁰ A nomination can be derailed with as little as 41 of the 100 Senators choosing to filibuster.

⁷¹ Our regression in this column looks for the partial correlation between dissent and subsequent elevation. None of the judges who are elevated to the Supreme Court, in fact, dissent in the last quarter (in our sample). Therefore if we run a probit specification, the coefficient on elevation drops out. Still, one could worry that judges are unaware that dissenting will not lead to elevation and judges may still dissent hoping to be elevated. Even if that were the case, it is not clear why this unawareness would be accentuated in divided panels and more contentious elections.

noticed, one would not expect dissents to occur more on procedural, particularly miscellaneous procedural, cases before elections. Another possibility is that judges may dissent before elections in order to win votes or increase voter turnout for their party. That is, judges may present a unified party front on substantive issues in order to increase voter morale. However, this seems inconsistent with the finding that dissents are more likely to occur for procedural, not merit, issues. This get-out-the-vote explanation is also inconsistent with the fact that dissents occur more when the President who wins the election is from the opposing party.⁷²

We now consider a number of less standard explanations for electoral cycles that are still based on a strategic, conscious rationale: collegiality, reputational capital, and legacy considerations. First, judges who dissent before elections may be doing so in the final round of a repeated collegiality game vis-a-vis the other judges. However, judges who retire or resign in the year following an election are not significantly more likely to dissent in the quarters before the election (Column 2).⁷³ Second, judges may dissent in order to gain social prestige among their peers because a dissent before an election is particularly valuable (such as by signaling better faithfulness to the appointing president). However, dissenting on miscellaneous procedural, not merit, issues is inconsistent with maximizing reputational capital. Even if judges are dissenting for their peers, this hypothesis suggests that peers may be particularly attentive because they are primed or leaves open the question why a dissent before an election is valuable since it appears to have no instrumental consequences.⁷⁴ Third, as the election approaches, there

⁷² Dissenting also does not help the senatorial candidate win. One interpretation of the dissents by the losing party is that these judges are expressing frustration at the pending loss by their political party. These results are available from the authors.

⁷³ Perhaps judges who remain are dissenting precisely because their colleagues are about to leave so they no longer need to be concerned about the collegiality of those colleagues. However, this auxiliary hypothesis assumes that judges who stay have no repeated collegiality game rationales vis-a-vi their colleagues who remain on the bench after the election.

⁷⁴ On a somewhat related hypothesis, perhaps judges pick a different writer for the majority opinion just before an election, a writer whose writing is more likely to lead to a dissent. Our data is unable to distinguish at what stage priming occurs during the decision process.

is increasing certitude about the subsequent President, so judges may wish to leave a legacy by dissenting. Even though dissents have no precedential power, judges may try to influence the pool of arguments available for future courts to cite just before a new crop of judges enters the judicial pool. This hypothesis is inconsistent with the fact that dissents are greater in closer elections, where incertitude is likely to have been greater. In addition, this hypothesis predicts that judges would continue to dissent any time before the new President has a chance to appoint new judges, but the dissent rate falls after the election. In summary, we do not find evidence that judges benefit from dissenting before a presidential election.

4. Explaining the Electoral Cycles in Judicial Behavior Among Unelected Judges

4.1 Ideologically Divided Panels and the Electoral Cycles in Dissents

To begin our examination of the partisan priming hypothesis for electoral cycles, we consider whether the higher dissent rate we observe in the quarter immediately preceding an election is more pronounced for cases decided by ideologically divided panels. Such a pattern would suggest that the increase in the rate of dissent towards the end of the presidential electoral cycle is occurring more in contentious environments, the first prong of what experiments in the lab have documented as a factor that increases susceptibility to priming. The model in Section 2 also predicts that contentious environments will increase priming. To address this question, we regress the dissent indicator variable on an electoral proximity measure variable which equals one if a case was decided in the quarter immediately preceding an election, the ideological division indicator variable, as well as an interaction between these two measures:

$$Y_i = F(t) + \beta_1 LastQuarter_i * Divided_i + \beta_2 Divided_i + \beta_3 LastQuarter_i + \beta_4 Z_i + \varepsilon_i \quad (5)$$

A positive and significant coefficient on this interaction term would indicate that the dissenting behavior in the quarters preceding an election is more pronounced on ideologically divided panels.

Our results for this specification, which we show on Table 6 and present graphically in the first four rows of Figure 4, indicate that dissents in the last quarter are driven by cases heard by ideologically divided panels.⁷⁵ The coefficient for the *divided* indicator variable is slightly smaller in magnitude than in the baseline results shown in Table 1 (i.e., .015 vs. .013) and still highly significant. However, the coefficient on the *lastquarter* indicator decreases substantially in magnitude and becomes statistically indistinguishable from zero. Most importantly, the interaction term is large in magnitude and statistically significant (at the 5% level in Column 2 and at the 10% level in Column 3's probit specification). The magnitude is similar in size to the non-interacted *lastquarter* coefficient in Column 1.⁷⁶ In summary, these results suggest that the increase in the dissent rate that we observe during the quarter preceding an election is due in great part to the presence of ideologically divided panels, which are characterized by higher dissent rates in the quarter immediately preceding an election.

4.2 Judge Level Analyses

In this section we explore whether certain type of judges are more likely to exhibit cycles in their dissenting behavior and explore the political nature of these dissents. Our analyses in this section are at the judge-vote level, instead of the case level, which allows us to examine the

⁷⁵ To place these results in perspective, Column 1 repeats the test presented in Column 1 of Table 1, but rather than comparing the dissent rate in each quarter individually, it compares the dissent rate in the quarter immediately preceding an election to the dissent rate in all other quarters combined. As one can see, the coefficient for the *divided* indicator variable in these specifications is identical to those found in Table 1 and the coefficient for *lastquarter* is similar to the coefficient in Table 1 for *quartermoelect1*. Holding everything else constant, dissents are 67% more likely in the quarter preceding the election when compared to any other quarter.

⁷⁶ Figure 4 shows with the raw data that the exceptional increase in dissents during the last quarter is primarily observed in divided panels.

role of various judicial background characteristics in mediating electoral cycles in judicial behavior and in judge's propensity to dissent.

4.2.1 *The Role of Judicial Characteristics in Mediating the Dissent Rate Cycle*

We begin by testing whether those judges who are particularly susceptible to partisan priming are the ones more likely to dissent in the shadow of presidential elections. In particular, we estimate the following specification:

$$Y_i = F(t) + \beta_1 LastQuarter_i * X_i + \beta_2 X_i + \beta_3 LastQuarter_i + \beta_4 Z_i + \varepsilon_i \quad (6)$$

where X_i is a vector of judge characteristics that may include whether the judge is sitting on an ideologically divided panel, whether the judge is sitting on a panel with another judge from the same party, and whether the judge was previously a U.S. Attorney or Assistant U.S. Attorney. The estimates are displayed in Table 7.

To provide a basis of comparison to our earlier results, in columns 1 and 2 we repeat the same analysis as in Columns 1 and 2 of Table 2 but at the vote level instead of at the case level. Interestingly, the effect of whether a judge sits in a divided or unified panel is partly due to a judge belonging to the political minority (the coefficient on the non-interacted majority term in column 3 is negative and significant⁷⁷). This pattern can be observed in the raw data (see last four rows of Figure 4): minority judges almost double the rate at which they dissent in the quarter preceding a presidential election.⁷⁸ Judges in the political majority are also more likely to dissent. We return to this issue in sub-section 4.2.3.

Second, we consider judge characteristics that are not related to a judge's political affiliation, but which one may expect to affect the susceptibility of a judge to be primed during

⁷⁷ Berdejo (2010) also documents that judges in the political majority of a panel are *less* likely to file a dissenting opinion.

⁷⁸ It is worth noting that 76% of judicial votes in our sample come from judges who are assigned to a panel with another judge from the same party (Appendix Table A).

the presidential electoral campaign. First, we test whether judges who were previously U.S. Attorneys or Assistant U.S. Attorneys are more likely to dissent in the quarter before an election. U.S. Attorneys and Assistant U.S. Attorneys are also known as federal prosecutors. These kinds of positions are uniquely both highly political and legal in the sense that they can choose to enforce or not enforce different aspects of federal law and they frequently lead to higher office (Engstrom 1971, Gordon 2009). Although the magnitude of the coefficients suggests that judges with previous experience as federal prosecutors display greater electoral cycles (see Column 5), this effect is only statistically significant in a probit specification (see Column 6).⁷⁹ This is consistent with the second prong of factors that experimental lab research has documented as increasing the likelihood of being primed.

Next, we explore the role of judicial experience by estimating the specification for subsamples of judges grouped by the number of years they have served as appellate judges and present the results in Table 8. On average, if we look at the entire sample of judges, we observe a 1.3% point increase in dissenting votes, which represents 57% of the base-level 2.3% of votes cast by judges being dissents (Appendix Table A). For those judges with 1 or 2 years of experience, the magnitude of this effect is a considerably larger 3.3%.⁸⁰ The fact that inexperienced judges are more likely to dissent before a presidential election is consistent with a priming interpretation of the dissent rate cycle, to the extent that judges may take awhile to develop the collegiality norms or strong professional commitments that would otherwise control the influence of unconscious identity-based bias (Rachlinski, Johnson, Wistrich, Guthrie 2009)

⁷⁹ It is worth noting that the age or gender of a judge does not appear to be correlated with an increase in the rate of dissent in the quarters leading up to a presidential election. Furthermore, judges from both political parties display electoral cycles.

⁸⁰ In the quarter immediately preceding an election these relatively inexperienced judges dissent in 3.4% of the cases, substantially higher than their 2.2% rate of dissent in the other quarters. Although the other age groups do not exhibit a statistically significant cycling behavior, for the most part they do tend to dissent more often in the quarter preceding an election (see Appendix Figure A).

and is consistent with the third prong of factors that experimental lab research has documented as increasing the likelihood of being primed.⁸¹

4.2.2 *Partisan Voting over the presidential Election Cycle*

To further explore the partisan interpretation of the electoral cycle, we consider whether judicial votes in the quarter before an election are more likely to align with the party of the judge's appointment. In particular, Table 4 estimates:

$$Valence_i = F(t) + \beta_1 LastQuarter_i * Democrat_i + \beta_2 Democrat_i + \beta_3 LastQuarter_i + \beta_4 Z_i + \varepsilon_i \quad (7)$$

In general, Democratic appointees cast votes that are more liberal than those of Republican appointees. Notably, the ideological difference between Democratic appointees and Republican appointees doubles in magnitude in the quarter before an election (Column 2).⁸² Interestingly, this priming of partisan voting appears aligned to the party of the judge's appointing president rather than the judge's own political party (Column 1).⁸³ The raw correlation between voting valence and party of appointment for each individual quarter-to-election is displayed in Figure 2.⁸⁴ These effects, while statistically significant, are subtle in magnitude. For Democratic appointees, 48%, 16%, and 36% of votes are conservative, mixed, and liberal, respectively,

⁸¹ Note however, experimental evidence has *not* found that judicial experience mediates the role of psychology in judicial decision-making (see, e.g. Rachlinski, Guthrie, and Wistrich 2006, and Englich, Mussweiler, and Strack 2006). On the other hand, other experimental research have found that inexperience magnifies priming effects (Krosnick and Kinder 1990) and observational studies have found that new judges go through an acclimation period during which their policy preferences are unstable and may stumble more than their colleagues (Owen and Black (2010), Hettinger, Lindquist and Martinek (2003a)).

⁸² The OLS coefficient is similar in magnitude to the sum of the marginal effects going from conservative to neutral/liberal and going from conservative/neutral to liberal in the ordered probit specification in Columns (3). The same is true for the ordered probit version of Column (1).

⁸³ We have no strong reason for why this is the case. In *Bush v. Gore*, the Supreme Court judges divided along party of appointment. In general, party of the appointing President more strongly predicts the valence of the judge's decision than does the judge's political party.

⁸⁴ In results available on request, we conduct a more detailed analysis of the relationship between voting valence and party of appointment for each individual quarter-to election. The overall picture portrayed by this analysis is qualitatively similar to that of Figure 2.

outside the last quarter, but during the last quarter, the vote shares are 46%, 16%, and 38%. For Republican appointees, the vote shift is from 52%, 17%, and 32% outside the last quarter to 53%, 18%, and 29% during the last quarter.⁸⁵ Nevertheless, the increase in partisan voting during the last quarter is sizeable relative to the general degree of partisan voting. The correlation between party of appointment and voting valence increases by over 100% while the correlation between the judge's current party and voting valence increases by 64%. Figure 2 also displays an increase in dissensus around midterm elections, a time period when there are also more political news articles as shown in Appendix Figure 3.

Appendix Figure 7 quantifies the relationship in a different manner. The 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. We exclude quintile 5 in this back-of-the-envelope calculation since judges in the most conservative quintile according to the ideology score are actually making votes that are more difficult to code (voting valence = 0), which makes these judges appear more liberal than the judges in the second most conservative quintile. Despite this non-linearity, the electoral cycle still increases the proportion of conservative votes for the most conservative judges. If we interpret ideology score as a judge's action baseline x_0 in the model in Section 2, this figure suggests that the category action x_C for both liberal and conservative judges is more extreme than the action baseline of the most extreme judges.

⁸⁵ Since we control for whether the panel is divided, this addresses the possibility that retirement post-election causes a period of time before the election where unified panels are assigned more often because of the composition of judges available to be assigned and panel effects increase the correlation between party and vote valence.

4.2.3 *Dissents as Expressions of Ideological Commitments or General Disagreeableness?*

We next consider whether the dissents are expressions of ideological commitments or simply general disagreeableness. The previous sub-section showed that voting behavior preceding a presidential election expresses ideological commitment. This sub-section shows that dissents are generally expressions of ideological commitments and then provides further evidence that the dissents preceding a presidential election express ideological commitment.

First, we examine whether dissents are expressions of ideological commitments consistent with the judge's party of appointment. Table 5 estimates:

$$Valence_i = F(t) + \beta_1 Dissent_i * Democrat_i + \beta_2 Democrat_i + \beta_3 Dissent_i + \beta_4 Z_i + \varepsilon_i \quad (8)$$

As Figure 3 shows, these effects are more dramatic. Democratic Appointees who dissent are issuing a liberal vote 58% of the time, while Democratic Appointees who do not dissent are issuing a liberal vote 36% of the time. Dissenting Republican Appointees are issuing a liberal vote 25% of the time while non-dissenting Republican Appointees are issuing a liberal vote 32% of the time. It is important to note that Table 5 Columns 4-5 indicate that even if judges from the political majority are dissenting against their 'own' party, they are dissenting in a manner that expresses the ideological commitments of the President that appointed the judge and not of the minority party.⁸⁶ We take these results as validating an assumption of our model in Section 2.

A natural follow-up question to the finding in Table 7 that judges sitting in the majority are more likely to dissent before elections is which of the judges in the political majority of a panel is dissenting. If ideological preferences are playing a role in this particular increase in the dissent rate, then one would expect the majority judge who is siding with the judge in the

⁸⁶ Columns (1)-(2) corroborate the general finding that divided panels result in ideological leveling (Sunstein et al. 2004), which we can see from the negative coefficient on the interaction term. Column (3) is closest to the illustration in Figure 3, showing that dissents typically represent the ideological preferences of the President that appointed the judge.

political minority of a panel to represent the ideological middle of that panel.⁸⁷ To address this question, we use the Judicial Common Space scores from Epstein et al. (2007) to rank the judges in each panel according to their ideology score from most liberal to most conservative. We find that it is the most liberal Democrat (or most conservative Republican) in the political majority who is more likely to dissent in the quarters before an election. The first four rows of Appendix Figure 5 show that majority judges with the extreme ideology score almost double the rate at which they dissent.⁸⁸

Furthermore, in the subset of cases where the minority judge shares 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 (the bottom four rows of Appendix Figure 5). This suggests that the electoral cycle moves the ideology of the minority and majority judge to no longer be the same. Finally, if electoral cycles are simply causing general disagreeableness, then we should observe homogeneous panels dissenting more. That is, we should see everyone, regardless of ideology dissenting more. However the second and fourth rows in Figure 4 and plot in Appendix Figure 4 indicates that this is not the case. We therefore interpret these dissents as expressions of ideological commitments rather than general disagreeableness.

5. Dissenting Behavior Across Time

⁸⁷ In other words, we should expect that in a panel with two Democrats and one Republican, the dissenting Democrat should be the most liberal member of the panel. For example, in his study of Ninth Circuit Court decisions, Berdejo (2010) finds that those judges who are in the political majority of a panel and who are also in the ideological center of that panel are more likely to “defect” and side with the political minority.

⁸⁸ An additional question arises if ideology score is driving the decision to dissent: why don’t the more extreme judges in a non-divided panel also dissent more before the election? We think there are several possible answers. The distribution of scores may not be as dispersed in a randomly drawn non-divided panel. The threshold score for which the policy outcome tips may be in-between the two parties’ distribution of scores. In addition, the more extreme majority judge may have greater collegiality concerns vis-a-vi the judicial colleagues from the same party.

5.1 *Increasing Trends in Polarization and Electoral Cycles*

There is growing evidence that politics in the United States has become more polarized in recent decades and that this polarization may have an effect on the federal courts. For example, 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 next address the question of whether the results presented above are emblematic of the entire period covered by our data (1925-2002) or if they are being driven by this recent trend in polarization. In Figure 8, we display the 18-year moving average correlation between last quarter and dissent. Each year on the x-axis represents the center point for 4 elections. As can be seen, electoral cycles in dissents are actually quite small during the 1940s, but they increase around 1960. After 1975, the electoral cycle becomes statistically significant in these 4-election moving averages. Accordingly, we divide the cases in our dataset into two groups, those cases decided on or prior to December 31, 1975 and those decided afterwards and estimate two sets of specifications. A very similar pattern can be observed in Figure 9, where we display the 10-year moving average correlation between party of appointment and voting valence. Each year on the x-axis represents the mid-point of the decade.

In Table 11, we examine whether the fact that cases decided in the quarter immediately preceding an election are characterized by a greater number of dissents is a recent development. To make this determination, we regress the dissent dummy on an indicator variable which equals one if the case was decided in the more recent period, an indicator variable equal to one if the case was decided in the month preceding an election, and an interaction term between these two:

$$Y_i = F(t) + \beta_1 LastQuarter_i * Recent_i + \beta_2 Divided_i + \beta_3 Recent_i + \beta_4 Z_i + \varepsilon_i \quad (9)$$

The coefficient *lastquarter* will tell us whether our main results hold for the period 1925-1974, while the sum of this coefficient and the coefficient on the interaction term (*lastquarter*recent*)

will tell us whether the relationship holds in the recent period. Based on the results we present in Table 11, columns (1) – (2), one can make the argument that any “electoral effect” on the dissent rate is smaller though statistically significant in the earlier period, and larger by a factor of 133% and statistically significant in the more recent period. Judges are also twice as likely to vote along party lines after 1975 (Column 3). This provides evidence that the political polarization that has characterized the executive and legislative branches in recent years may also be impacting the judiciary. Indeed, when we restrict the data to years before 1975, neither type of electoral cycle is visually apparent in a replication of Figures 1 and 2.

As previously noted, cases decided by ideologically divided panels are characterized by higher rates of dissents. In light of the findings discussed in the last paragraph, one may ask whether this fact is also the result of the recent polarization of the courts. In column (2) we estimate specifications combining equations (4) and equation (5), using an indicator variable that equals one if the case was decided by an ideologically divided panel as our explanatory variable of interest (in addition to the *recent* indicator variable and an interaction term between them). The results suggest that cases decided by ideologically divided panels are much more likely to have a dissent in the more recent period, even after controlling for the fact that recent cases have on average a higher rate of dissent. This, we believe, provides further evidence that political ideology has played an increasing role in judicial decision making in recent years.

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. As column (4) shows, both birth cohort and time effects explain the increasing tendency for judges to vote along partisan lines, but as column (5) shows, the priming effect of *lastquarter* lies primarily on *recent* and not birth cohort. This suggests that the political primeability of judges is not due to some cohort-specific formative or educational experience.

Rather, all judges, regardless of birth, become more “primeable” in the recent time period. As further evidence that the ideological commitments of older judges have become stronger in recent years, in the next section we show a similar pattern among judges’ retirement decisions along the electoral cycle.

5.2 *The Effect of the Political Climate*

Next, we explore the role of the political climate in mediating the dissenting behavior of judges. For example, one can expect judge’s propensity to dissent to be attenuated in periods of national reconciliation and exacerbated in periods of deep political division. First, we test whether during periods of war (where the country tends to set political differences aside) judges are less likely to dissent. To investigate this hypothesis, we explore the effect of September 11, 2001, widely considered to have an ideologically unifying effect, as well as other periods of war, which have been found to have effects on judicial decision-making (Epstein, Ho, King, Segal 2005).⁸⁹ We estimate equations (1), (3), and (6) but replace *Prox_i* or *LastQuarter_i* with an indicator variable equal to 1 for those dates in which the country was at war. For Columns 2-10, we do not include year, quarter, or quarter-to-election fixed effects as that would make interpreting the coefficients on wartime more difficult, but we do include year as a linear term to control for any trends in the dissent rate (Figure 5). Column (1) of Table 10 indicates that judges in the first and third month after September 11 decreased their dissent rate.⁹⁰ Column (2) of Table 10 and Figure 6 indicate that a decrease in dissent rates occurs during World War 2, the

⁸⁹ For our analysis, we rely dates 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.

⁹⁰ Standard errors are clustered at the case level in Column (1) instead of quarter-year level since most of the treatments of interest are a subset of one quarter-year. The other columns in this table allow for clustering of the error term separately by quarter-year and case following the double-clustering algorithm outlined in Cameron, Gelbach, and Miller (2010) and Peterson (2008).

Korean War, and the Vietnam War (moreover, weakening of the electoral cycles displayed in Figure 8 also appears to coincide with wartime).⁹¹ Column (4) and Figure 7 indicate that the decrease in dissent rates during wartime is primarily observed in divided panels. Column (5) indicates that the decrease in dissent rates during wartime is more likely by less experienced judges.⁹² Importantly, these results are consistent with previous studies documenting the dimensions along which people are more prone to priming.⁹³ Judges who are less experienced and sitting on divided panels are both more likely to be primed to dissent before presidential elections and more likely to be primed for consensus during wartime.⁹⁴ Judges are also more (less) likely to affirm (reverse) lower court decisions (Columns 11 and 12).

Conversely, during periods in which the country is more divided along political lines, we may expect judges to be more influenced by their ideology. In Table 9 and Figure 5 we explore the role of contentious elections in exacerbating the electoral cycle.⁹⁵ As one can see, close elections in which the electoral count is less than 55% increase the rate of dissent during the contested period just before an election. Column 1 of Table 9 indicates the electoral cycle in dissents is roughly five times larger in contentious elections and Column 3 indicates that electoral cycle disappears in landslide elections. The remaining columns indicate that these effects are robust to the inclusion of controls for time period and wartime.

Finally, Appendix Table D shows that electoral cycles are larger and more statistically significant in Circuits 11, 3, 12, 6, and 9. These Circuits include the highly populous and traditionally swing states of Florida, Pennsylvania, California, Ohio, and Michigan. Circuit 12 is

⁹¹ Column (3) indicates that these effects are robust to the inclusion of a linear time trend.

⁹² We show results using inexperience defined as less than or equal to 10 years, but the finding does not depend critically on the particular threshold.

⁹³ We obtain similar results when conducting these analyses at the case-level (Columns 7-10).

⁹⁴ For judges with prior political experience, we do not find that wartime differentially primes judges.

⁹⁵ In these tests we define electoral proximity based on whether a case was heard in the three quarters preceding an election due to the limited number of contentious elections. We chose to aggregate these three quarters into the electoral proximity variable based on the results on Table 1, which indicate that each of the three quarters before the presidential election display an increase in dissent rates.

the District of Columbia, which is not a swing state but may be expected to have a more politicized environment.

6. Electoral Cycles in Judicial Retirements

6.1 The Decision to Retire or Resign Across the presidential Election Cycle

In this section, we examine whether there is a relationship between a judge's decision to voluntarily leave the bench and the proximity to the next presidential election. The retirement results are more easily explicable as manifestations of conscious, strategic choices. We document these electoral cycles, however, for two reasons. First, showing a positive correlation between the size of electoral cycles in retirement and the size of other electoral cycles provides further field evidence for experimental results: increasing polarization strengthens the priming effects of elections. Second, previous studies of the relationship between politics and judicial retirements (i.e., assume senior status) that have conducted the analysis at a yearly level and with less credible identification, have found mixed evidence of political factors influencing judicial turnover rates (see, e.g., Yoon (2006), Spriggs and Wahlbeck (1995), Stolzenberg and Lindgren (2010), Zuk et. al. (1993)).

Our analysis in this section accounts for judicial retirement decisions at monthly intervals, which allows us to measure more precisely the impact of politics (and, most importantly, of particular events such as presidential elections) on a judge's decision to retire.⁹⁶

⁹⁶ For our analysis in this section we use data from the Multi-User Data Base on the Attributes of U.S. Appeals Court Judges to create an unbalanced panel of all judges in all time periods based on whether they retire or not. This methodology follows Yoon (2006). Thus for judge j , variables are not defined (i.e., missing values) for the period of time preceding that judge's appointment and the period of time following his or her retirement judge's retirement. In defining a judge's retirement we consider two events: the assumption of senior status and the resignation from the bench. We focus on the former as it has greater strategic value and in general appears to be more under a judge's control; that is, by assuming senior status a judge allows for the President to appoint a new member to the court but can also remain in

As we can observe in Figure 10, 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.⁹⁷ This general pattern could explain why using yearly retirement data (which would combine these pre- and post-election periods) may lead to results that suggest that politics does not play a significant role in judges' retirement decisions.

To conduct a more in-depth analysis, we estimate the following specification:

$$Retire_i = F(t) + \beta_1 Proximity_i + \varepsilon_i \quad (10)$$

where the outcome variable $Retire_i$ is the number of judicial retirements for the month i ; the explanatory variable of interest, $Proximity_i$, is our electoral proximity measure $quartertoelect$, an indicator variable representing the number of quarters remaining before the next presidential election;⁹⁸ $F(t)$ includes a set of year-specific fixed effects, and, in certain specifications, may also include a set of fixed effects for each quarter of the year (e.g. January through March, April through June, etc.);⁹⁹¹⁰⁰ finally ε_i is a mean-zero stochastic error term.¹⁰¹

The results for our main specifications in this section are in Table 12. The coefficients on our electoral proximity measure indicate that judges are more likely to retire in the three quarters before an election, a result that is both statistically and economically significant. In the quarter

the court and continue to hear cases. We sum up the number of retirements per month to conduct the analyses that follow.

⁹⁷ 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 (Appendix Table A).

⁹⁸ As a robustness check we use an alternative linear measure of electoral proximity (see Appendix Table E).

⁹⁹ The set of year-specific fixed effects is intended to capture shocks or trends affecting judicial retirement that are common to all judges in a given year, while the quarterly fixed effects control for seasonal variation in judges' retirement decisions.

¹⁰⁰ As a robustness check we re-estimate the main specification dropping one circuit at a time. The results of this robustness check are displayed in the Appendix. As another robustness check, we disaggregate the data to obtain the number of retirements per month and by circuit, include circuit fixed effects, and cluster the standard errors at the circuit level. The results change little.

¹⁰¹ In all calculations of statistical significance in this section, robust standard errors are used.

immediately preceding an election, 0.11 fewer judges retire per month, a decrease that represents over 87% of the average rate of judges leaving voluntarily (Column 1). None of the other quarters before an election show a statistically significant effect and the magnitude of their point estimates are generally smaller. It is worth noting that at first glance, there appears to be no electoral cycling in resignations (Column 4).

6.2 *Ideological Commitments*

In order to investigate whether this retirement behavior is evidence of ideological commitments, we explore whether judges who may not share the same political views of the President whose term is about to expire drive the effect. If this were the case, one could argue that the decrease in the number of judges voluntarily leaving at the 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). To address this question, we separately regress the number of judges voluntarily leaving when the party of the President in power is the same as the party of the President who appointed the judge on the electoral proximity measure and then run a similar regression but using the number of judges voluntarily leaving when the party of the President in power is different from the party of the President who appointed the judge as the dependent variable.¹⁰²

The results for these specifications, which we present in Columns (2-3, 5-6) of Table 12, suggest that the decrease in the number of judges that retire in the quarters before an election is being driven by judges choosing not to retire when the sitting President belongs to a *different* party. As compared to the quarter immediately following an election, approximately 0.09 fewer judges retire each month during the three quarters preceding an election when the sitting

¹⁰² We use the unbalanced panel described in footnote 49 and sum up the monthly number of retirements or resignations with particular characteristics to conduct these analyses.

President belongs to a different political party (Column 3). These effects are statistically significant at the 5% or 1% level. The coefficients for the remaining quarters are not statistically significant.

We also uncover an electoral cycle in judicial resignations in Columns 5 and 6. In the four quarters *after* a presidential election, judicial resignations increase when the party of the President in power is the *same* as the party of President appointing the judge. Again, the coefficients are quite sizeable in magnitude, roughly equal to the baseline average number of resignations, 0.02 per month.¹⁰³

A related question is whether the lower rate of judges voluntarily leaving the bench in the quarters around an election is more pronounced for Democratic or Republican appointees. We separately regress the number of Democratic and Republican appointees voluntarily leaving the bench on the electoral proximity measure. The patterns we uncover in Table 12 are slightly more pronounced for Republican appointees, especially for resignations (Column 4 of Appendix Table F).

6.3 *Increasing Trends in Polarization*

We now investigate whether the results presented above hold for the entire period we analyze (1802-2004) or if our results are mostly driven by the recent trend in political polarization. To answer this question we divide our dataset into pre- and post-1975 periods. Using a regression discontinuity framework, we compare judicial retirements in the three quarters immediately following an election with the three quarters immediately preceding an

¹⁰³ 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.

election. We regress the number of voluntary judge retirements on a dummy that indicates whether the retirement occurred after an election (*after*), a dummy indicating whether the retirement occurred after 1975 (*recent*), and an interaction between these two indicators.

$$Retire_i = F(t) + \beta_1 After_i * Recent_i + \beta_2 After_i + \beta_3 Recent_i + \varepsilon_i \quad (11)$$

where $Retire_i$ and $F(t)$ are the same sets of variable described above; $After_i$ 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 coefficient $After$ will tell us whether the results discussed in Section 5.1 above hold for the period 1802-1974, while the sum of this coefficient and the coefficient on the interaction term ($After * Recent$) will tell us whether the relationship holds in the recent period. The results from this specification are shown in Table 11 (Columns 6 and 7). The higher rate of voluntary retirements following an election appears to be concentrated in the post-1980 period. The coefficient on the non-interacted term $after$ is in fact negative in sign, suggesting that the electoral cycles we observe in judicial retirement decisions may be entirely a recent phenomenon. This is corroborated by Figure 11, which displays the 20-year moving average correlation between retirement decision and whether it is after the election. In summary, these results, taken in combination with the results in Section 5, indicate a remarkable increase in ideological polarization and proneness to priming in tandem with the increase in polarization.

7. Conclusion

There is now a large political economy literature on how politicians' behavior changes as an election nears. That effect is nearly always attributed to the incentive effects of an upcoming election. In this paper, we find that even though they do not face electoral incentives, appellate judges alter their behavior in a manner that responds to the presidential electoral calendar—in the

three quarters immediately preceding an election, judges are more likely to dissent, vote along partisan lines, and time retirements according to whether the party of the President in power aligns with their own. We explore strategic and behavioral reasons for expressing ideological commitments before presidential elections. Our evidence is most consistent with the priming of ideology as dissents by judges in the quarter before presidential elections are accentuated for judges in contentious environments or who are more prone to priming. Judges appear to be creating reasons to dissent for less controversial cases and are less likely to affirm and more likely to reverse lower court decisions during this season. Dissents by judges before elections reverse the ideological convergence typically occurring in divided panels. These electoral cycles by unelected judges have accelerated in recent years.¹⁰⁴ We leave open the question as to why electoral cycles have increased.¹⁰⁵

Taken together, these results shed additional light into the causes and consequences of judicial polarization along political party lines, raise questions regarding the independence of the federal judiciary, and contribute to a literature on endogenous normative commitments.¹⁰⁶ Our paper builds on a large literature questioning the ability of judges to make impartial and unbiased decisions¹⁰⁷ and sheds light on one possible mechanism for judicial polarization along political

¹⁰⁴ With the exception of the impact on crowding of court dockets, we are unable to make a strong inference that electoral cycles are deleterious to welfare. If judges are generally suppressing dissents for collegiality reasons, the increase in dissents and partisan voting may simply indicate that judges are making decisions more reflective of the constituencies they indirectly represent.

¹⁰⁵ One possible reason may be that until the 1970s, many Supreme Court justices came from outside the judiciary. Another reason may be that after around 1970, the presidential candidates were selected through primary elections, which are likely to attract the more partisan voters. These primary elections may have created a more contentious public discourse, particularly in the third month before the presidential election, when candidates are incentivized to attract the partisan base rather than move to the center.

¹⁰⁶ See, e.g. Benabou and Ok (2001), Chen and Lind (2007), Chen (2010a) and (2010b).

¹⁰⁷ Babcock, Loewenstein, Issacharoff, and Camerer (1995); Abrams, Bertrand, and Mullainathan (2008); Shayo and Zussman (2010); Boyd, Epstein, and Martin (2010); Segal and Spaeth (2002); Ellman, Sunstein, and Schkade (2004).

party lines.¹⁰⁸ It is possible that electoral cycles may be self-reinforcing: appellate judges are less likely to retire in 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.¹⁰⁹ Judges are also more likely to resign in 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, which can, in turn, cause judges to wait to have their replacements selected by a President from the same party.

U.S. judges have a strong commitment to be unbiased yet display behavior indicating that judicial partisanship is not simply about differences in legal philosophy. The judges with more extreme ideology scores dissent more before a presidential election and the judges in the political minority who share the same ideology score as a judge in the political majority is also dissenting more. These facts combined with the fact that only judges at two interior points along the ideological spectrum are displaying the sharpest electoral cycles indicate that these highly trained professionals are unconsciously expressing their ideological commitments. In the course of our analysis, we find field evidence for what experimental studies have documented regarding the contexts and characteristics that make individuals more susceptible to priming as well as a causal link between group identity and ideological bias. Social scientists have long speculated whether people choose the same group because of a shared set of ideas or whether group identity imparts a set of ideas. Our causal framework isolates the second channel in a naturally occurring setting.

¹⁰⁸ We build on pioneering work by McCarty, Poole, and Rosenthal (2006), who find polarization in the U.S. House and Senate to have increased markedly in the 1970s, the same turning point as found judicial polarization and electoral cycles. However, we also find polarization to have been high in the late 1950s, which they do not find in the legislature.

¹⁰⁹ 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.

References

- Abrams, David, Marianne Bertrand, and Sendhil Mullainathan. Forthcoming. "Do Judges Vary in their Treatment of Race?" *Journal of Legal Studies*.
- Akerlof, George A., and Rachel E. Kranton. 2000. "Economics and Identity," *Quarterly Journal of Economics*, 115(3): 715-733.
- Althaus, Scott L., and Young Mie Kim. 2006. "Priming Effects in Complex Information Environments: Reassessing the Impact of News Discourse on presidential Approval." *The Journal of Politics*, 68(4): 960-976.
- Babcock, Linda, George Loewenstein, Samuel Issacharoff, and Colin Camerer. 1995. "Biased Judgments of Fairness in Bargaining." *The American Economic Review*, 85: 1337-1342.
- Bargh, John A., and Tanya L. Chartrand. 2000. "Studying the Mind in the Middle: A Practical Guide to Priming and Automaticity Research." In *Handbook of Research Methods in Social Psychology*, ed. H. Reis and C. Judd, 253-285. New York: Cambridge University Press.
- Bargh, J. A., P. Raymond, J. Pryor, and F. Strack. 1995. "The attractiveness of the underling: An automatic power association and its consequences for sexual harassment and aggression." *Journal of Personality and Social Psychology*, 68: 768-781.
- Benabou, Roland, and Efe Ok. 2001. "Social Mobility and the Demand for Redistribution: The POUM Hypothesis," *Quarterly Journal of Economics*, 116(2): 447-487.
- Benjamin, Daniel J., James J. Choi, and A. Joshua Strickland. 2010. "Social identity and preferences." *American Economic Review*, 100(4): 1913-1928.
- Benjamin, Daniel J., James J. Choi, and Geoffrey Fisher. 2010. "Religious Identity and Economic Behavior." NBER Working Paper 15925.
- Berdejo, Carlos. 2010. "It's the Journey not the Destination: Judicial Preferences and Decision-Making in the Ninth Circuit." Harvard University Working Paper.
- Berdejo, Carlos and Noam Yuchtman. 2009. "Crime, Punishment, and Politics: An Analysis of Political Cycles in Criminal Sentencing." Review of Economics and Statistics: Revise and resubmit.
- Bertrand, Marianne, and Sendhil Mullainathan. 2004. "Are Emily and Greg More Employable than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination." *The American Economic Review*, 94(4): 991-1013.
- Bertrand, Marianne, Dean Karlan, Sendhil Mullainathan, Eldar Shafir, and Jonathan Zinman. 2010. "What's Advertising Content Worth? Evidence from a Consumer Credit Marketing Field Experiment." *Quarterly Journal of Economics*, 125(1): 263-306.
- Brennan, Thomas, Lee Epstein, and Nancy Staudt. 2009. "Economic Trends and Judicial Outcomes: A Macro-Theory of the Court." *Duke Law Journal*, 58: 1191-1230.
- Bock, Kathryn, and Zenzi M. Griffin. 2000. "The Persistence of Structural Priming: Transient Activation of Implicit Learning?" *Journal of Experimental Psychology*, 129(2): 177-192.
- Boyd, Christina L., Lee Epstein and Andrew D. Martin. 2010. "Untangling the Causal Effect of Sex on Judging." *American Journal of Political Science*, 54(2): 389-411.
- Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller, Forthcoming, "Robust Inference with Multi- Way Clustering," *Journal of Business and Economic Statistics*.
- Cameron, Charles. 1993. "New Avenues for Modeling Judicial Politics." Conference on the Political Economy of Public Law.
- Card, David, and Gordon B. Dahl. 2011. "Family Violence and Football: The Effect of Unexpected Emotional Cues on Violent Behavior." *Quarterly Journal of Economics*, 126(1): 103-143.

- Chen, Daniel L. 2010. "Club Goods and Group Identity: Evidence from Islamic Resurgence during the Indonesian Financial Crisis." *Journal of Political Economy*, 118(2): 300-354.
- Chen, Daniel L. 2010. "Markets and Morality: How Does Competition Affect Moral Judgment?" Working Paper, University of Chicago.
- Chen, Daniel L., and Jo Lind. 2007. "Religion, Welfare Politics, and Church-State Separation." *Journal of Ecumenical Studies*, 42(1): 42-52.
- Chen, Yan, and Sherry Xin Li. 2009. "Group Identity and Social Preferences." *American Economic Review*, 99(March): 431-457.
- Choi, Stephen J., G. Mitu Gulati, and Eric A. Posner. 2010. "Professionals or Politicians: The Uncertain Empirical Case for an Elected Rather than Appointed Judiciary." *Journal of Law, Economics, and Organization*, 26(2): 290-336.
- Cross, Frank B. 2003. "Decision Making in the U.S. Courts of Appeals." *California Law Review*, 91: 1457.
- Cross, Frank B., and Emerson H. Tiller. 1998. "Judicial Partisanship and Obedience to Legal Doctrine: Whistle blowing on the Federal Court of Appeals." *Yale Law Journal*, 107: 2155-2176.
- Duclos, J., J. Esteban, and D. Ray, 2004. "Polarization: Concepts, Measurement, Estimation." *Econometrica*, 72(6): 1737-1772.
- Druckman, James N., 2004. "Priming the Vote: Campaign Effects in a U.S. Senate Election." *Political Psychology*, 25(4): 577-594.
- Druckman, James N., 2005. "Media Matter: How Newspapers and Television News Cover Campaigns and Influence Voters." *Political Communication*, 22(4): 463-481.
- Edmans, Alex, Diego Garcia, and Oyvind Norli. 2007. "Sports Sentiment and Stock Returns." *The Journal of Finance*, 62(4): 1967-1998.
- Englich, Birte, and Kirsten Soder, 2009. "Moody Experts— How Mood and Expertise Influence Judgmental Anchoring." *Judgment and Decision Making*, 4(1): 41–50.
- Englich, Birte, Thomas Mussweiler, and Fritz Strack. 2005. "The Last Word in Court - A Hidden Disadvantage for the Defense." *Law and Human Behavior*, 29: 705–722.
- Engstrom, Richard L. 1971. "Political Ambitions and the Prosecutorial Office." *The Journal of Politics*, 33(1): 190-194.
- Epstein, Lee, William M. Landes, and Richard A. Posner. 2011. "Why (and When) Judges Dissent: a Theoretical and Empirical Analysis." *Journal of Legal Analysis*, 3(1): 101-137.
- Epstein, Lee, Daniel E. Ho, Gary King and Jeffrey A. Segal. 2005. "The Supreme Court During Crisis: How War Affects only Non-War Cases." *New York University Law Review*, 80(1): 1-116.
- Epstein, Lee, Andrew D. Martin, Jeffrey A. Segal and Chad Westerland. "The Judicial Common Space." *The Journal of Law, Economics, and Organization*, 23(2007): 303-325.
- Esteban, J., and D. Ray. 1994. "On the Measurement of Polarization." *Econometrica*, 62(4): 819-851.
- Farhang, Sean, and Gregory Wawro. 2004. "Institutional Dynamics on the U.S. Court of Appeals: Minority Representation Under Panel Decision Making." *The Journal of Law, Economics and Organization*, 20: 299-330.
- Fong, Christina, and Erzo Luttmer. 2009. "What Determines Giving to Katrina Victims? Experimental Evidence on Racial Group Loyalty." *American Economic Journal: Applied Economics*, 1(2): 64-87.
- Gennaioli, Nicola, and Andrei Shleifer. 2007. "The Evolution of Common Law." *Journal of*

- Political Economy*, 115(1): 43-68.
- Gentzkow, Matthew, and Jesse M. Shapiro. 2010. "What Drives Media Slant? Evidence from U.S. Daily Newspapers." *Econometrica*, 78(1): 35-71.
- Gerber, Alan S., Gregory A. Huber, and Ebonya Washington. 2010. "Party Affiliation, Partisanship, and Political Beliefs: A Field Experiment." *American Political Science Review*, 104(4): 720-744.
- Giner-Sorolla, Roger, Shelly Chaiken, and Stacey Lutz. 2002. "Validity Beliefs and Ideology Can Influence Legal Case Judgments Differently." *Law and Human Behavior*, 26(5): 507-526.
- Gordon, Sanford C. 2009. "Assessing Partisan Bias in Federal Public Corruption Prosecutions." *American Political Science Review*, 103(4): 534-554.
- Gordon, Sanford C., and Gregory A. Huber. 2004. "Accountability and Coercion: Is Justice Blind when It Runs for Office?" *American Journal of Political Science*, 48: 247.
- Gordon, Sanford C., and Gregory A. Huber. 2007. "The Effect of Electoral Competitiveness on Incumbent Behavior." *Quarterly Journal of Political Science*, 2: 107.
- Graham, Sandra, and Brian S. Lowery. 2004. "Priming Unconscious Racial Stereotypes about Adolescent Offenders." *Law and Human Behavior*, 28(5): 483-504.
- Groseclose, T., and J. Milyo. 2005. "A Measure of Media Bias." *Quarterly Journal of Economics*, 122: 1187-1234.
- Guthrie, C., J.J. Rachlinski, and A.J. Wistrich. 2002. "Inside the Judicial Mind." *Cornell Law Review*, 87: 267-79.
- Guthrie, C., J.J. Rachlinski, and A.J. Wistrich. 2007. "Blinking on the Bench: How Judges Decide Cases" *Cornell University Law Review* 93: 1.
- Hassin, Ferguson, Shidlovski, and Gross. 2007. "Subliminal Exposure to National Flags Affects Political Thought and Behavior" *Proceedings of the National Academy of Sciences* 104 (50): 19757-19761.
- Hettinger, Virginia A., Stefanie A. Lindquist, and Wendy L. Martinek. 2003a. "Acclimation Effects on the United States Courts of Appeals." *Social Science Quarterly*, 84(4): 792-810.
- Hettinger, Virginia A., Stefanie A. Lindquist, and Wendy L. Martinek. 2003b. "Separate Opinion Writing on the United States Courts of Appeals." *American Politics Research*, 31(3): 215-50.
- Hettinger, Virginia A., Stefanie A. Lindquist, and Wendy L. Martinek. 2004. "Comparing Attitudinal and Strategic Accounts of Dissenting Behavior on the U.S. Courts of Appeals," *American Journal of Political Science*, 48: 123.
- Hettinger, Virginia A., Stefanie A. Lindquist, and Wendy L. Martinek. 2007. "Splitting the Difference: Modeling Appellate Court Decisions with Mixed Outcomes." *Law & Society Review*, 41(2): 429-456.
- Higgins, E.T., and W.M. Chaires. 1980. "Accessibility of Interrelational Constructs. Implications for Stimulus Encoding and Creativity." *Journal of Experimental Social Psychology*, 16: 348-361.
- Hong, Harrison, and Leonard Kostovetsky. 2008. "Red and Blue Investing: Political Values and Finance." Princeton University and University of Rochester Working Paper.
- Huang, Bert. Forthcoming. "Deference Drift? Evidence from a Surge in Federal Appeals." *Harvard Law Review*.
- Huber, Gregory A., and John S. Lapinski. 2006. "The "Race Card" Revisited: Assessing Racial Priming in Policy Contests." *American Journal of Political Science*, 50(2): 421-440.

- Jolls, Christine, and Cass R. Sunstein. 2006. "Debiasing through Law." *Journal of Legal Studies*, 35: 199-241.
- Kim, Pauline T. 2009. "Deliberation and Strategy on the U.S. Courts of Appeal: An Empirical Exploration of Panel Effects." *University of Pennsylvania Law Review*, 157: 1319-1381.
- Kornhauser, Lewis A. 1999. *Judicial Organization and Administration*. Encyclopedia of Law and Economics.
- Landes, William M., and Richard A. Posner. 2009. "Rational Judicial Behavior: A Statistical Study." Working Paper, University of Chicago.
- Law, David S. 2005. "Strategic Judicial Lawmaking: Ideology, Publication, and Asylum Law in the Ninth Circuit." *University of Cincinnati Law Review*, 73: 817-866.
- Lenz, Gabriel S., 2009. "Learning and Opinion Change, Not Priming: Reconsidering the Priming Hypothesis." *American Journal of Political Science*, 53:4: 821-837.
- Lindquist, Stefanie A. 2007. "Bureaucratization and Balkanization: The Origins and Effects of Decision-Making Norms in the Federal Appellate Courts." *University of Richmond Law Review*, 41: 659-706.
- Liptak, Adam. 2010. "In a Polarized Court, Getting the Last Word." *The New York Times* (March 9): A12.
- Mendelberg, Tali. 2001. *The Race Card: Campaign Strategy, Implicit Messages, and the Norm of Equality*. Princeton, NJ: Princeton University Press.
- Montalvo, Jose and Marta Reynal-Querol, "Ethnic Polarization, Potential Conflict and Civil Wars," *American Economic Review*, 2005, (June), 796-816.
- Mullainathan, Sendhil, and Andrei Shleifer. 2005. "The Market for News." *American Economic Review*, 95: 1031-1053.
- Mussweiler, Thomas, and English, Birte, 2005. "Subliminal Anchoring: Judgmental Consequences and Underlying Mechanisms." *Organizational Behavior and Human Decision Processes*, 98: 133-143.
- Mussweiler, Thomas, and Fritz Strack. 2000. "The Use of Category and Exemplar Knowledge in the Solution of Anchoring Tasks." *Journal of Personality and Social Psychology*, 78: 1038-1052.
- Naito, Mika. 1990. "Repetition Priming in Children and Adults: Age-Related Dissociation Between Implicit and Explicit Memory." *Journal of Experimental Child Psychology*, 50: 462-484.
- Ostergaard, Arne L. 1994. "Disassociations between Word Priming Effects in Normal Subjects and Patients with Memory Disorders: Multiple Memory Systems or Retrieval?" *The Quarterly Journal of Experimental Psychology, Section A*, 47(2): 331-364.
- Owens, Ryan, and Ryan Black. 2010. "Strategic Bargaining on the United States Courts of Appeals." Harvard University mimeo.
- Peresie, Jennifer L. 2005. "Female Judges Matter: Gender and Collegial Decision Making in the Federal Appellate Courts." *Yale Law Journal*, 114: 1759.
- Peterson, Mitchell A., 2008, "Estimating Standard Errors in Finance Panel Data Sets: Comparing Approaches," *Review of Financial Studies*, 22(1): 435-480.
- Pildes, Richard. 2010. "Ungovernable America: The Causes and Consequences of Polarized Democracy," The Jorde Lecture.
- Political Wheel of Fortune: Is Wall Street Tied to presidential Cycle? 2005. Knowledge at W.P. Carey School, Arizona State University. <http://knowledge.wpcarey.asu.edu/article.cfm?articleid=1042> (accessed October 18, 2010).

- Posner, Richard. 1973. "An Economic Approach to Legal Procedure and Judicial Administration." *Journal of Legal Studies*, 2(2), 399-458.
- Pozen, David E. 2008. "The Irony of Judicial Elections." *Columbia Law Review*, 108(2): 265.
- Rachlinski, Jeffrey J., Sheri Lynn Johnson, Andrew J. Wistrich, and Chris Guthrie. 2009. "Does Unconscious Bias Affect Trial Judges?" *Notre Dame Law Review*, 84: 1195.
- Rachlinski, Jeffrey J., Chris Guthrie, and Andrew J. Wistrich. 2006. "Inside the Bankruptcy Judge's Mind." *Boston University Law Review*, 86: 1227.
- Revesz, Richard. 1997. "Environmental Regulation, Ideology and the D.C. Circuit." *Virginia Law Review*, 83: 1717-1772.
- Revesz, Richard. 2001. "Congressional Influence on Judicial Behavior? An Empirical Examination of Challenges to Agency Action in the D.C. Circuit." *New York University Law Review*, 76: 1100-1141.
- Saunders, E. M. 1993. "Stock Prices and Wall Street Weather." *American Economic Review*, 83(5): 1337-1345.
- Segal, Jeffrey A., and Harold J. Spaeth. 2002. *The Supreme Court and the Attitudinal Model Revisited*. New York: Cambridge University Press.
- Shayo, Moses and Asaf Zussman. Forthcoming. "Judicial Ingroup Bias in the Shadow of Terrorism." Hebrew University, *Quarterly Journal of Economics*.
- Simonsohn, U. 2010. "Weather to Go to College." *Economic Journal*, 120(543): 270-280.
- Spriggs, James F., and Paul J. Wahlbeck. 1995. "Calling it Quits: Strategic Retirement on the Federal Courts of Appeals, 1893-1991." *Political Research Quarterly*, 48(September): 573-597.
- Squire, Larry R., Arthur P. Shimamura, and Peter Graf. 1987. "Strength and Duration of Priming Effects in Normal Subjects and Amnesic Patients." *Neuropsychologia*, 25(1B): 195-210.
- Srull, T.K., and R.S. Wyer, Jr. 1979. "The Role of Category Accessibility in the Interpretation of Information about Persons: Some Determinants and Implications." *Journal of Personality and Social Psychology*, 37: 1660-1672.
- Stolzenberg, Ross M. & James Lindren. 2010. "Retirement and Death in Office of U.S. Supreme Court Justices," *Demography*, 47(2): 269-298.
- Stone, Rebecca. 2008. "Following Precedent to Signal Ideological Neutrality." Working Paper, NYU Law School.
- Storms, L.H. 1958. "Apparent Backward Association: A Situation Effect." *Journal of Experimental Psychology*, 55: 390-395.
- Sudevan, P. and David Taylor. 1987. "The Cuing and Priming of Cognitive Operations." *Journal of Experimental Psychology*, 13: 89-103.
- Sunstein, Cass R., David Schkade, and Lisa M. Ellman. 2004. "Ideological Voting on Federal Courts of Appeals: A Preliminary Investigation." *Virginia Law Review*, 90: 301.
- Tulving, Endel, Daniel L. Schacter, and Heather A. Stark. 1982. "Priming Effects in Word-Fragment Completion Are Independent of Recognition Memory." *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 8(4): 336-342.
- Wasserman, Ira M. 1983. "Political Business Cycles, presidential Elections, and Suicide and Mortality Patterns." *American Sociological Review*, 48(5): 711-720.
- Yoon, Albert. 2006. "Pensions, Politics, and Judicial Tenure: An Empirical Study of Federal Judges, 1862-2002." *American Law and Economics Review*, 8: 143.
- Zuk, Gary, Gerard S. Gryski, and Deborah J. Barrow. 1993. "Partisan Transformation of the Federal Judiciary, 1869-1992." *American Politics Quarterly*, 21.

Figure 1: Dissent Rate across the Political Cycle

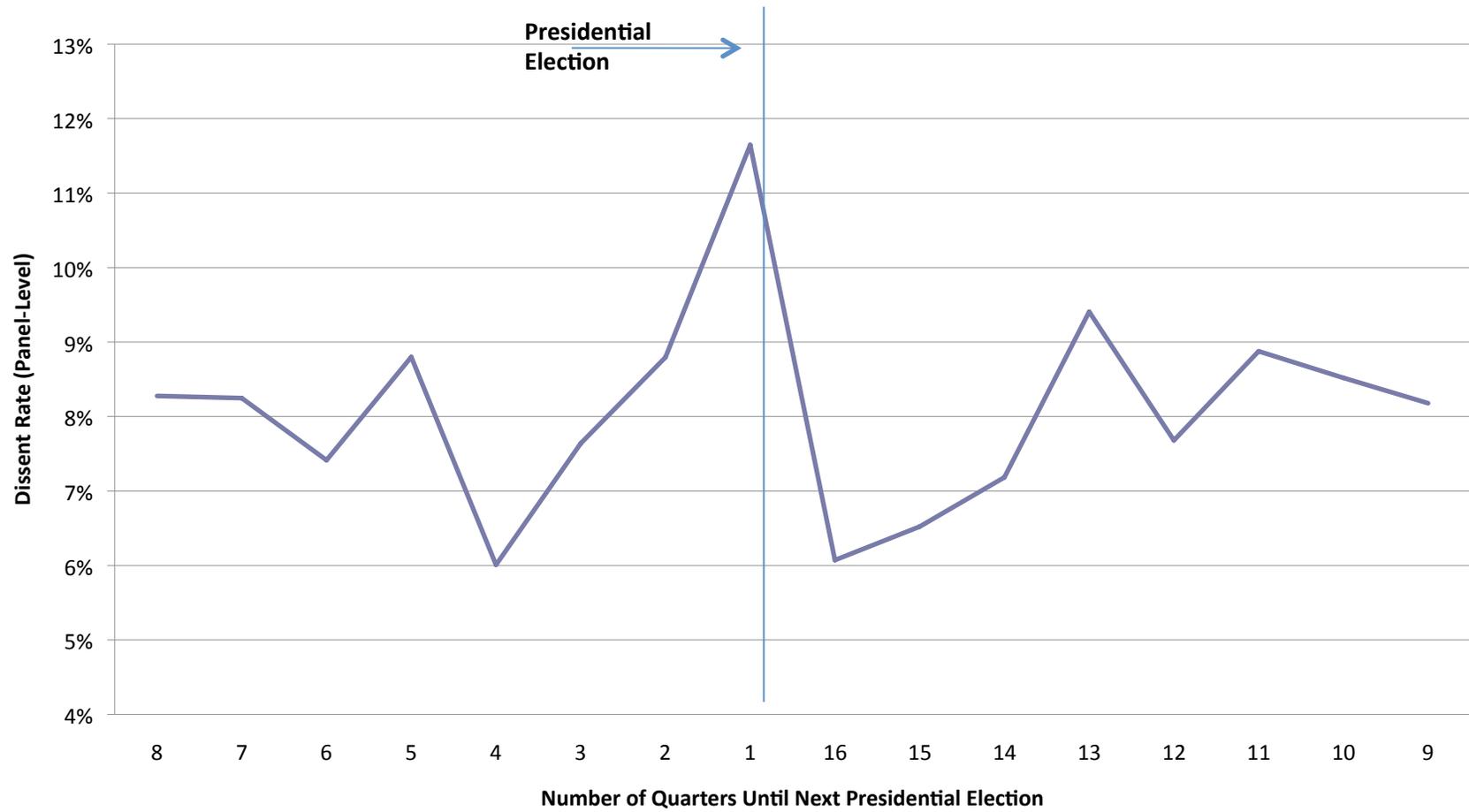


Figure 2: Correlation Between Party of Appointment and Ideological Commitment across Political Cycle

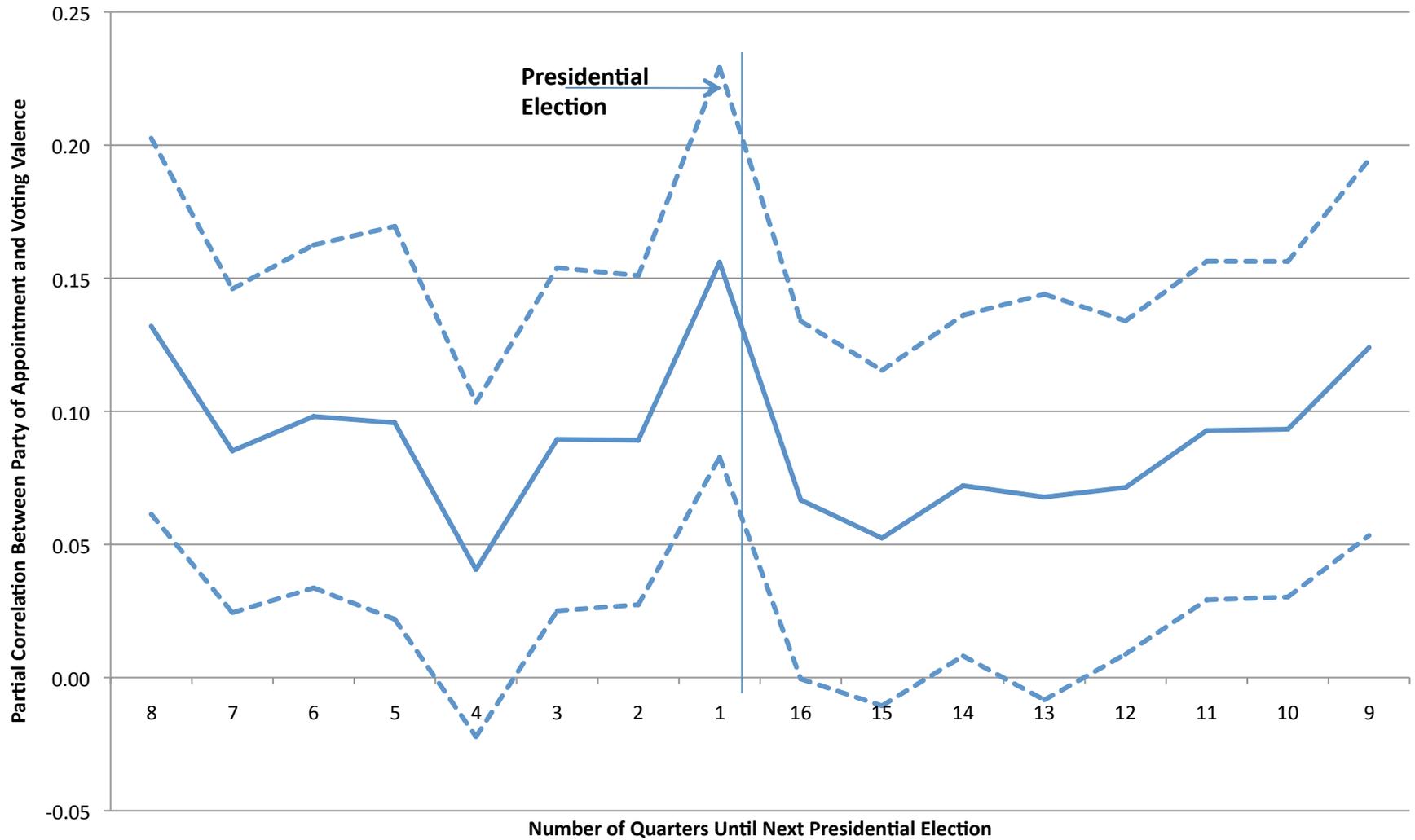


Figure 3: Dissents as Expressions of Ideological Commitments

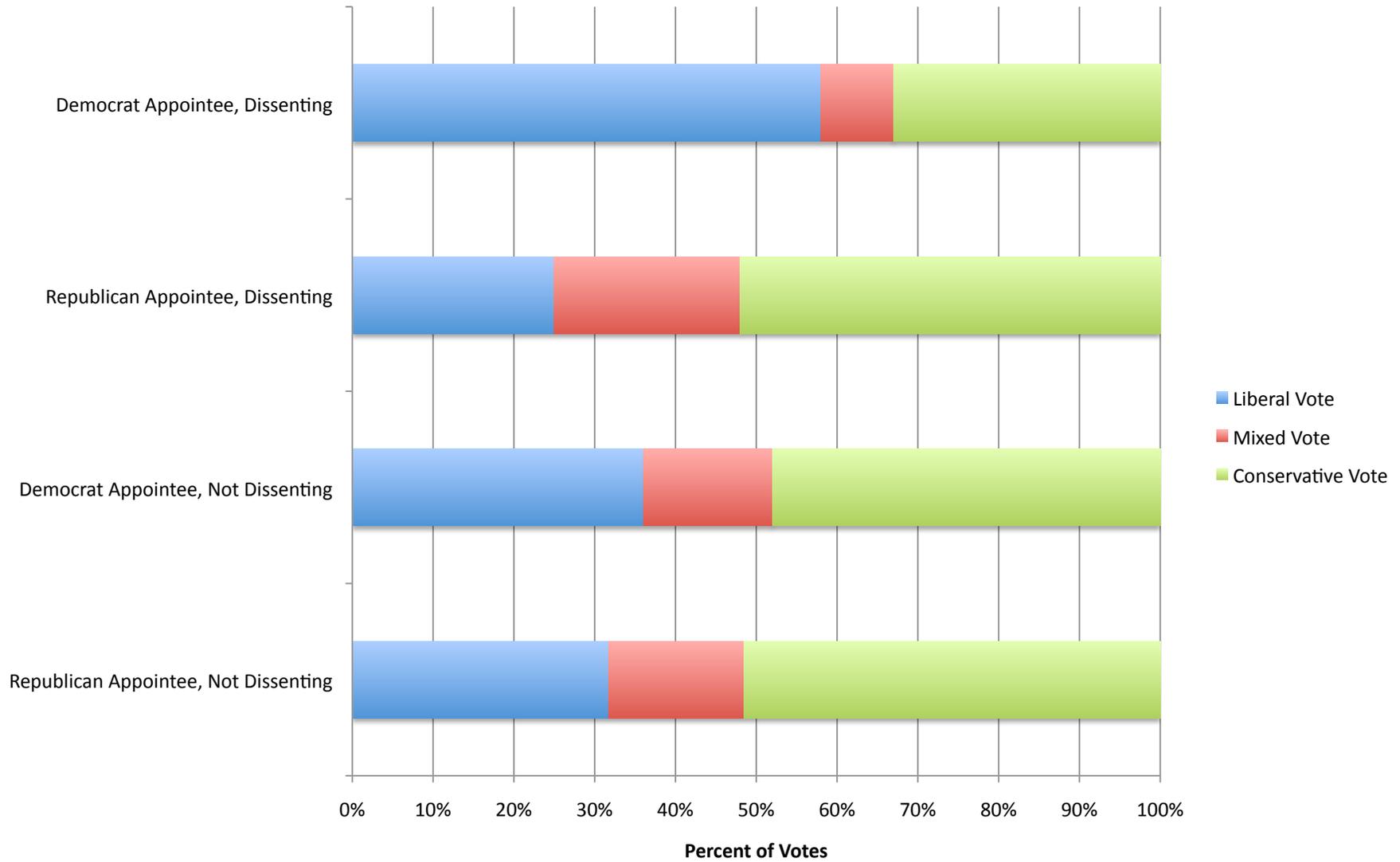


Figure 4: The Role of Judicial Characteristics in Mediating the Effect of Electoral Cycles on Dissents

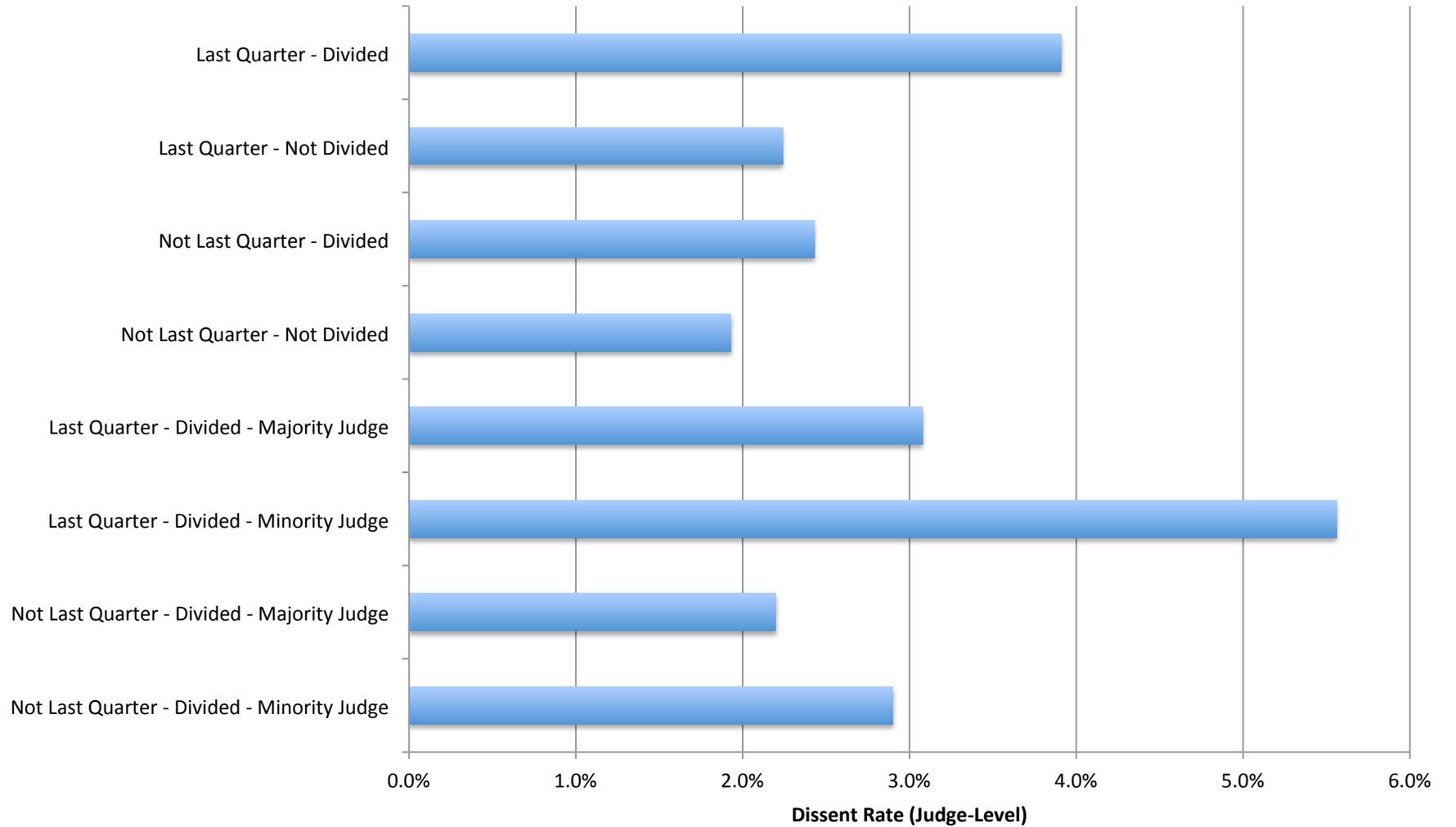


Figure 5: The Role of Contentious Elections in Mediating the Effect of Electoral Cycles on Dissents

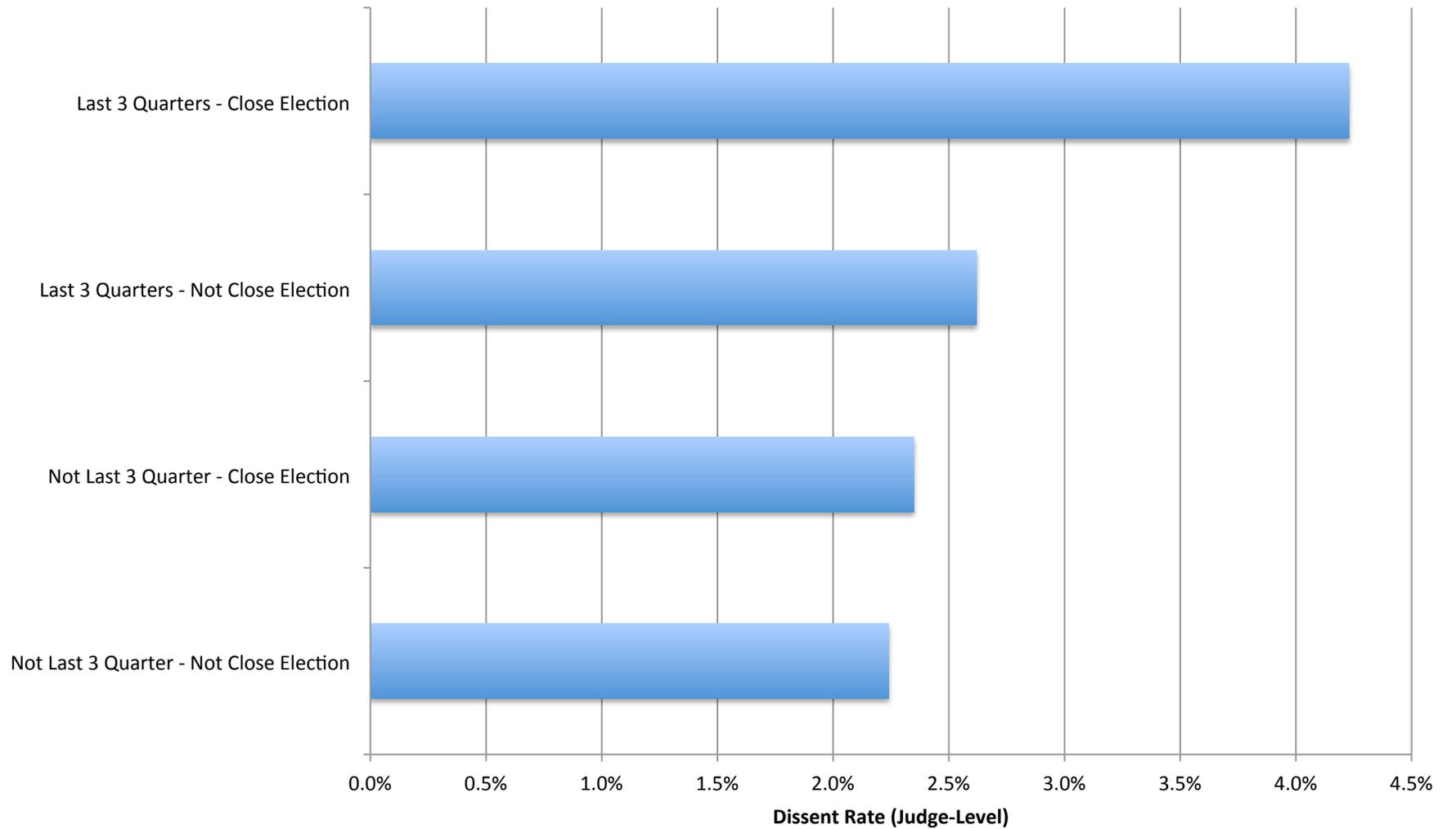


Figure 6: The Effect of War Time on Dissents



Figure 7: The Role of Panel Characteristics in Mediating the Effect of War Time on Dissent Rates

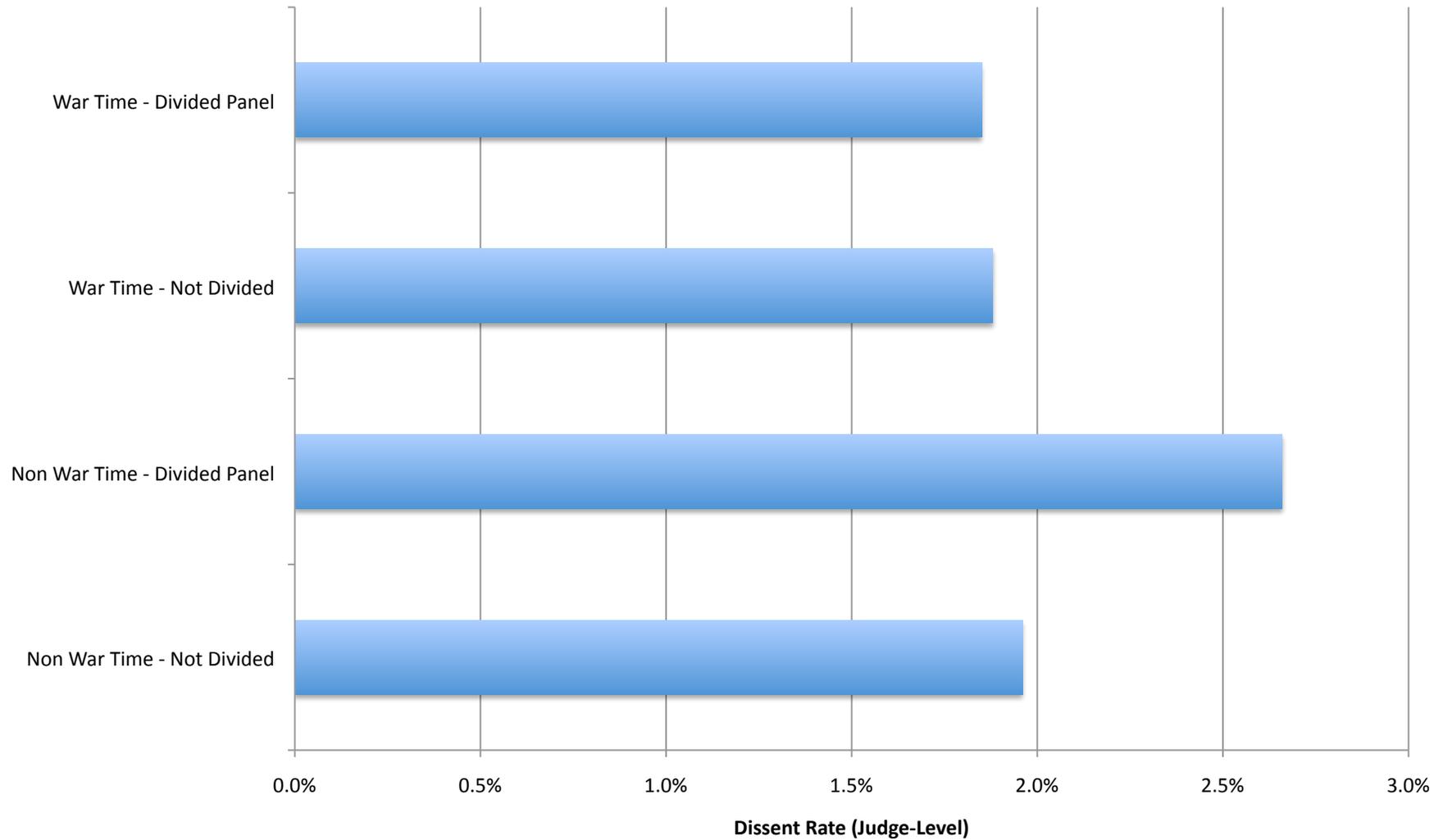


Figure 8: Increase in Electoral Cycles in Dissents over Time

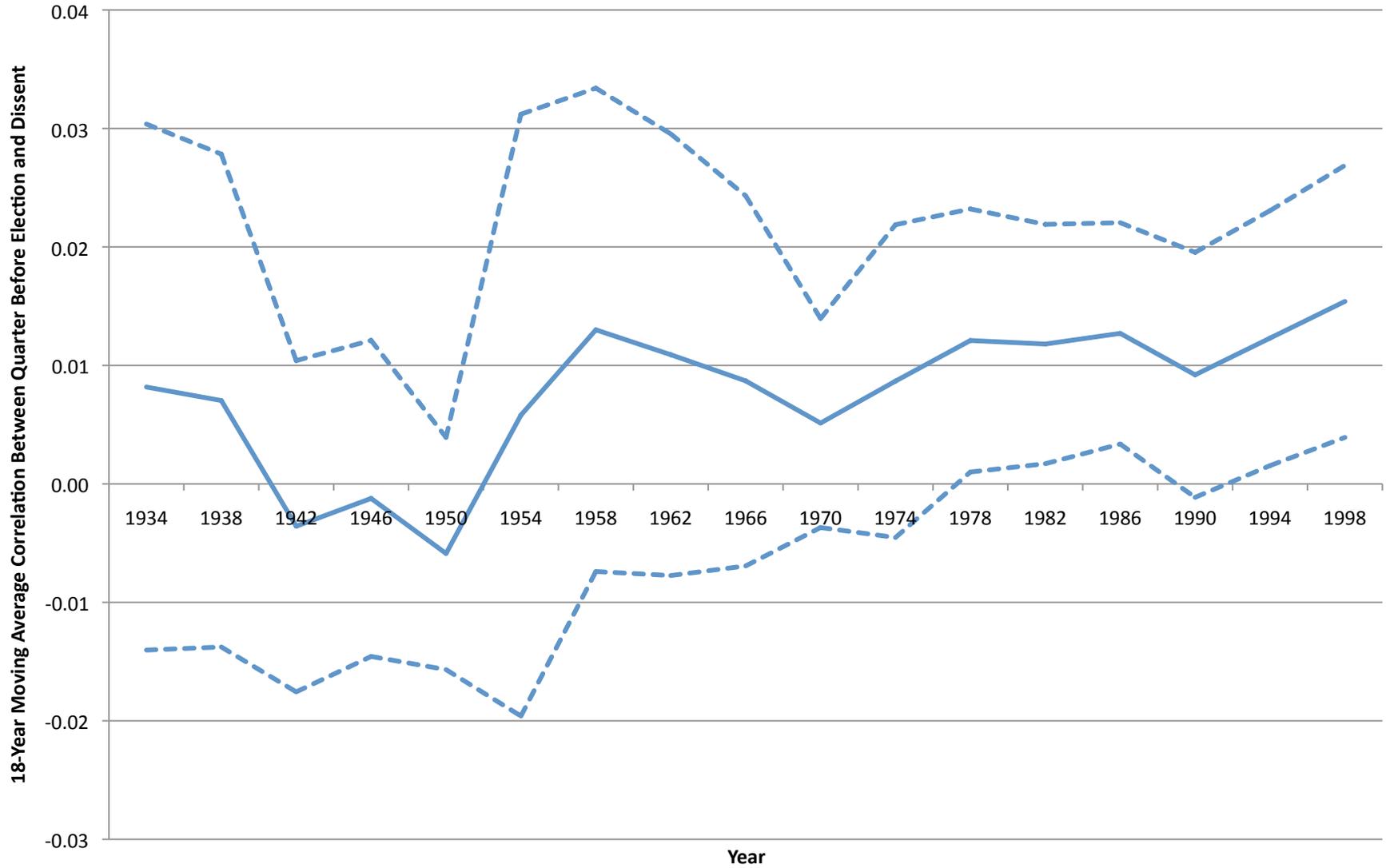


Figure 9: Increase in Ideological Voting Over Time

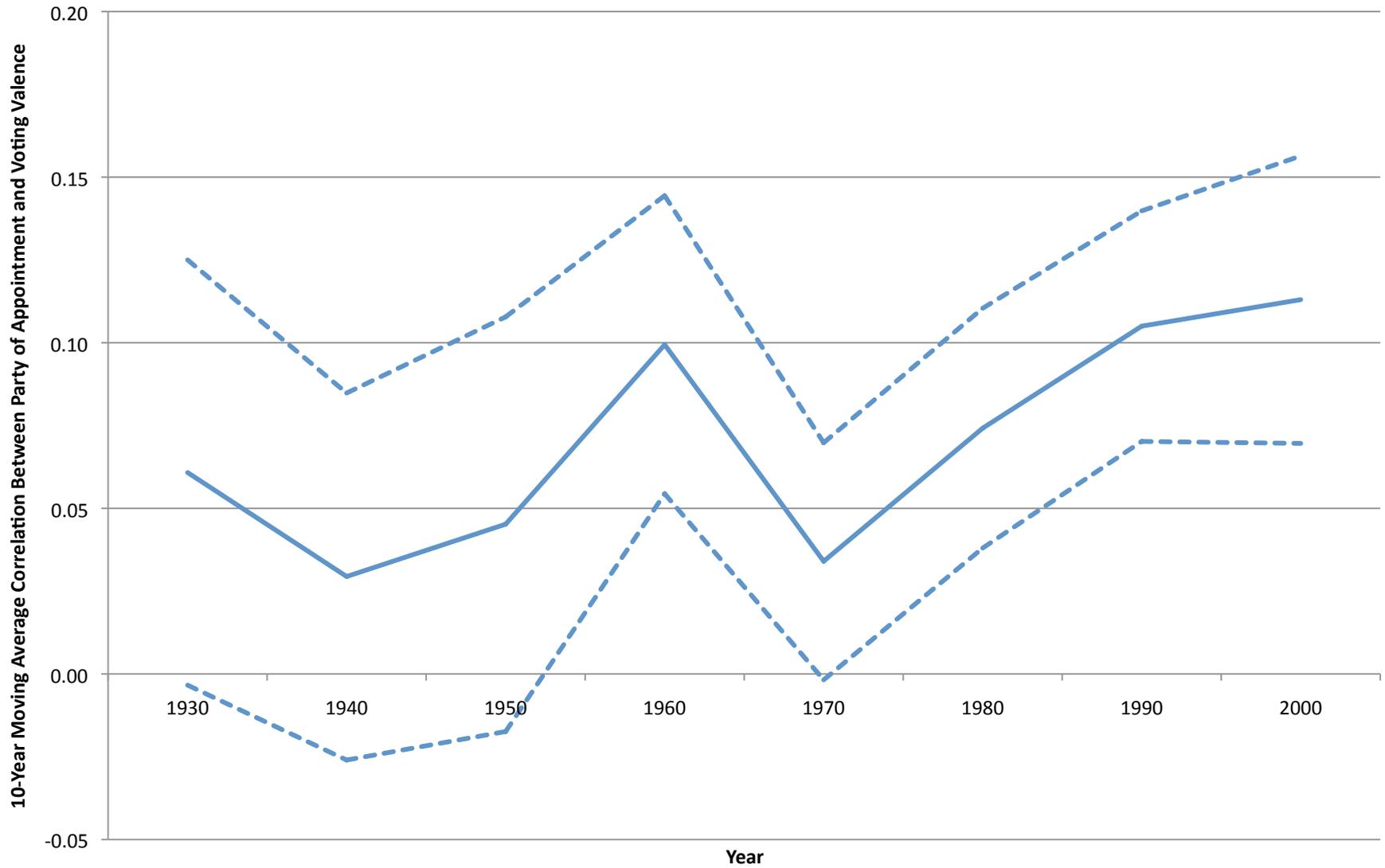


Figure 10: Judicial Exit across the Political Cycle

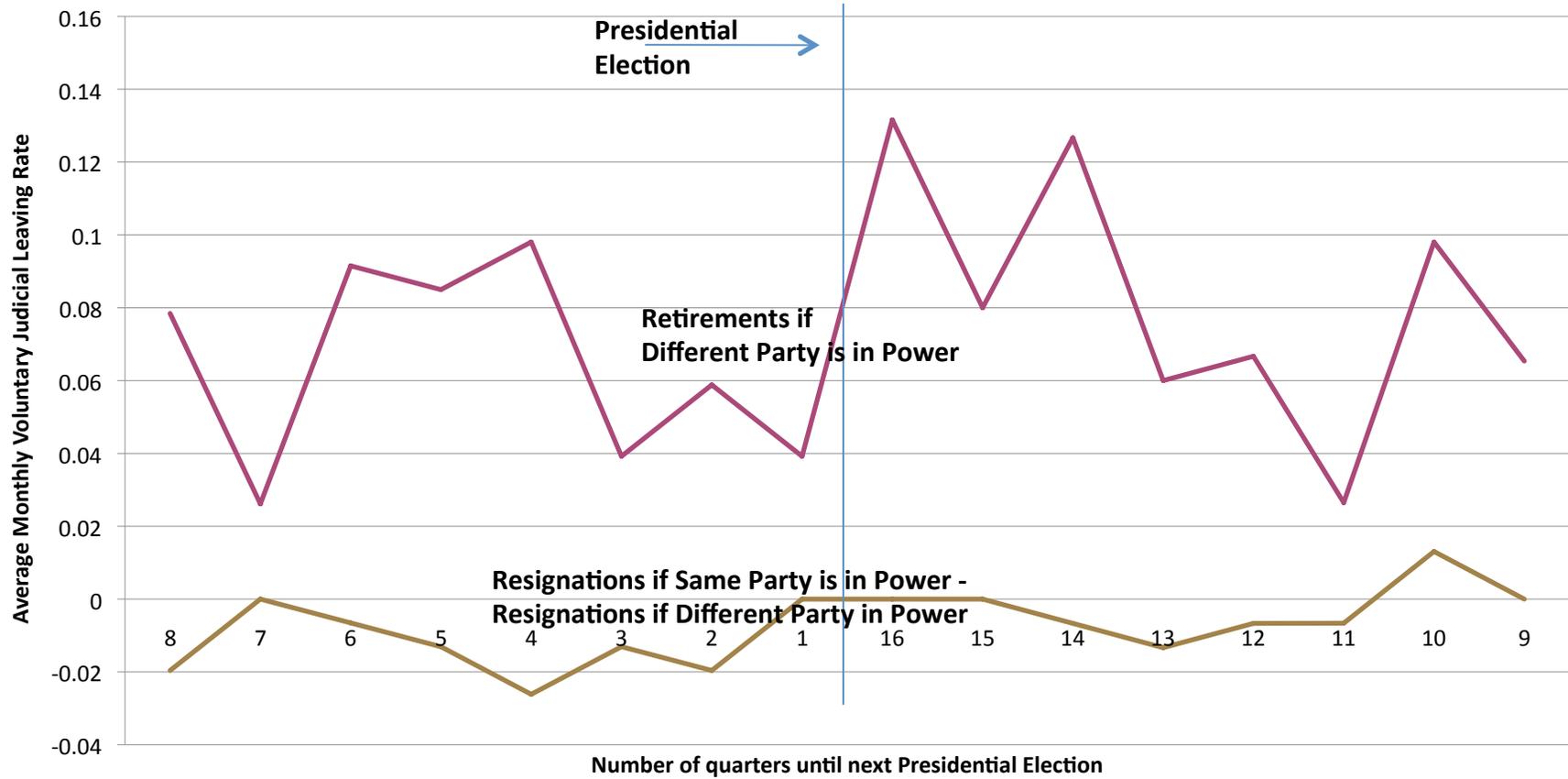


Figure 11: Increase in Electoral Cycles in Judicial Exits Over Time

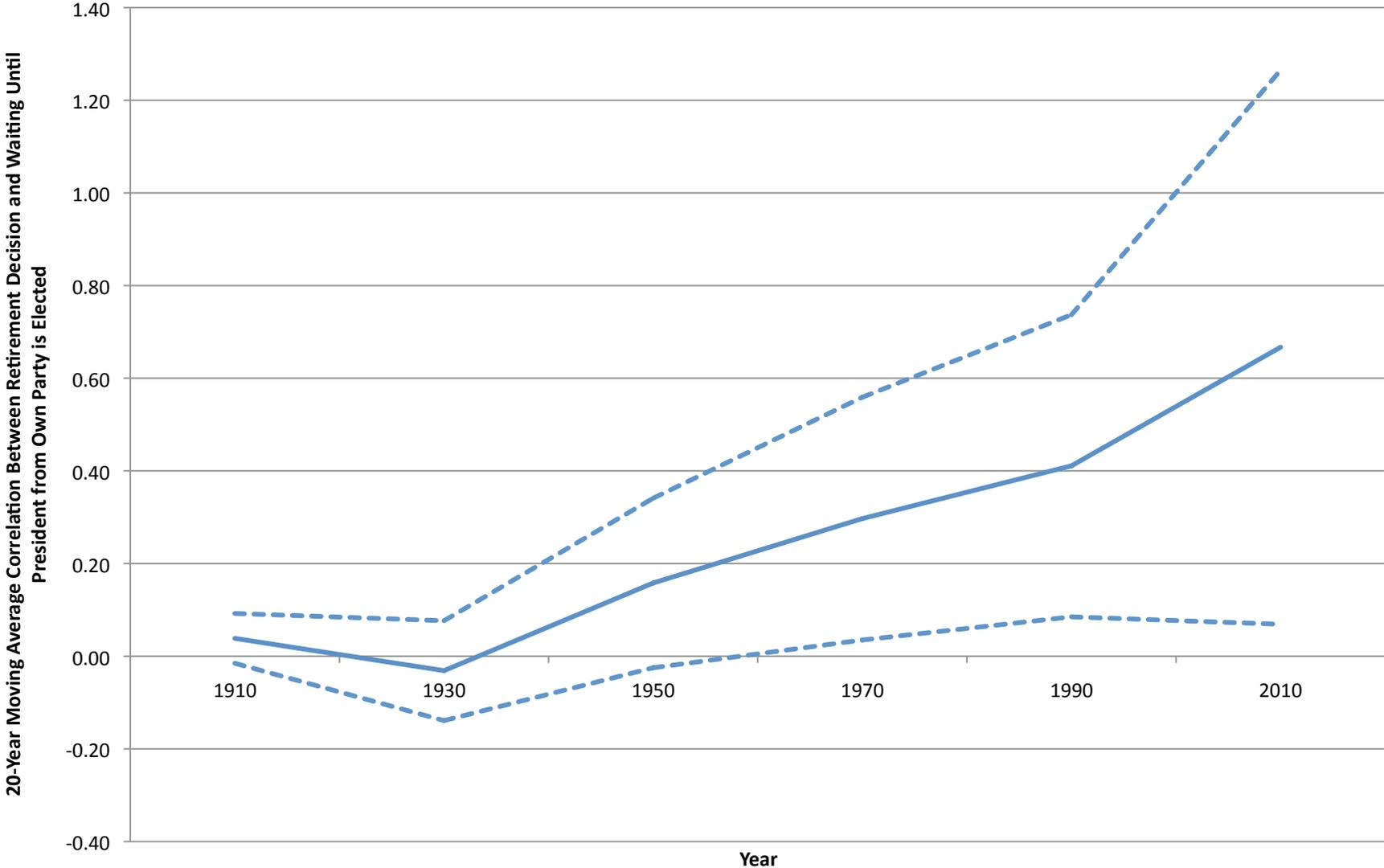


Table 1: Electoral Cycles in Dissents

	Ordinary Least Squares		Dissent	
	(1)	(2)	(3)	(4)
Divided	0.0157 [0.00452]***	0.0153 [0.00451]***	0.114 [0.0327]***	0.111 [0.0328]***
Quartermoelect = 1	0.0637 [0.0123]***	0.0527 [0.0132]***	0.448 [0.0857]***	0.377 [0.0936]***
Quartermoelect = 2	0.0347 [0.0121]***	0.0255 [0.0138]*	0.284 [0.0960]***	0.224 [0.105]**
Quartermoelect = 3	0.0325 [0.0123]***	0.0302 [0.0134]**	0.270 [0.0982]***	0.256 [0.103]**
Quartermoelect = 4	0.00581 [0.0111]	0.00578 [0.0111]	0.0444 [0.0963]	0.0481 [0.0962]
Quartermoelect = 5	0.0209 [0.0152]	0.0102 [0.0156]	0.170 [0.119]	0.101 [0.122]
Quartermoelect = 6	0.0120 [0.0141]	0.00302 [0.0155]	0.0970 [0.114]	0.0418 [0.118]
Quartermoelect = 7	0.0226 [0.0141]	0.0194 [0.0150]	0.178 [0.111]	0.159 [0.113]
Quartermoelect = 8	0.00772 [0.0141]	0.00859 [0.0141]	0.0521 [0.107]	0.0644 [0.106]
Quartermoelect = 9	-0.0115 [0.0155]	-0.0218 [0.0157]	-0.0717 [0.112]	-0.138 [0.114]
Quartermoelect = 10	-0.0114 [0.0160]	-0.0193 [0.0174]	-0.0779 [0.115]	-0.128 [0.122]
Quartermoelect = 11	0.000311 [0.0162]	-0.00142 [0.0171]	0.00509 [0.116]	-0.00295 [0.118]
Quartermoelect = 12	-0.0102 [0.0128]	-0.00912 [0.0129]	-0.0628 [0.0900]	-0.0521 [0.0903]
Quartermoelect = 13	0.00115 [0.0148]	-0.0101 [0.0148]	0.00433 [0.0961]	-0.0726 [0.0980]
Quartermoelect = 14	-0.0157 [0.0134]	-0.0243 [0.0151]	-0.105 [0.0940]	-0.157 [0.103]
Quartermoelect = 15	-0.0176 [0.0117]	-0.0194 [0.0127]	-0.121 [0.0788]	-0.131 [0.0832]
Controls	N	Y	N	Y
Observations	18686	18686	18686	18686
R-squared	0.019	0.022		

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 in columns (1)-(4) 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 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 year fixed effects and circuit fixed effects. The regressions in columns (2) and (4) also include legal issues fixed effects and seasonality controls (i.e. quarter fixed effects).

Table 2: The Impact of Electoral Cycles

	Length of Majority Opinion	Citations	Dissenting Citations	Conditional on Dissenting, Dissent for Procedural, not Merits Issue	Affirm		Reverse	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lastquarter	0.216 [0.227]	0.289 [0.536]	-0.0809 [0.0456]*	0.0785 [0.0289]**	-0.0515 [0.0240]**	-0.0342 [0.0263]	0.0349 [0.0183]*	0.0183 [0.0204]
Dissentdummy	1.902 [0.123]***	2.115 [0.262]***	0.413 [0.0363]***					
Lastquarter * Dissentdummy	0.387 [0.428]	-0.232 [1.206]	-0.292 [0.111]***					
Controls	Y	Y	Y	N	N	Y	N	Y
Observations	18686	18649	18649	227	18686	18686	18686	18686
R-squared	0.223	0.168	0.077	0.038	0.054	0.054	0.025	0.026

Notes: Robust standard errors, clustered at the quarter-year level, in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). The outcome variable in columns 1 is the length of the majority opinion in pages (winsorized at the 1% level). In columns 2 and 3 the outcome variable is the total number of times the case has been cited in subsequent opinions (winsorized at the 1% level), and in columns 5-6 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. In columns 5 and 6 the outcome variable is a dummy variable equal to 1 if the case affirmed the decision being reviewed; while the outcome variable in columns 7 and 8 is a dummy variable equal to 1 if the case reversed the decision being reviewed. 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, 6, and 8 also include quarter-to-election fixed effects, year fixed effects, quarter fixed effects, circuit fixed effects and legal issues fixed effects. Column 4 includes year fixed effects, quarter fixed effects, circuit fixed effects and legal issues fixed effects. Columns 5 and 7 includes quarter-to-election fixed effects, year fixed effects, circuit fixed effects and legal issues fixed effects.

Table 3: Evidence Inconsistent with Strategic Explanations for Electoral Cycles

	Dissent		
	Ordinary Least Squares		Probit
	(1)	(2)	(3)
Lastquarter	0.0116 [0.00396]***	0.0126 [0.00402]***	0.226 [0.0716]***
Elevated	-0.00207 [0.00510]		
Elevated * Lastquarter	-0.0304 [0.00624]***		
Retire or Resign Next Year		0.00126 [0.00416]	0.0179 [0.0734]
Retire or Resign Next Year * Lastquarter		0.00908 [0.0230]	0.110 [0.257]
Controls	Y	Y	Y
Observations	51460	56058	56058
R-squared	0.007	0.006	

Notes: Robust standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Standard errors are double-clustered at the case level and quarter-year level. The outcome variable in columns (1)-(3) is a dummy variable equal to 1 if the judge filed a dissenting opinion. 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 and its interactions with whether the judge is elevated to higher office or whether the judge leaves the bench next year. All regressions also include quarter-to-election fixed effects, year fixed effects, quarter fixed effects, circuit fixed effects, legal issues fixed effects, and a dummy variable indicating whether the panel was divided ideologically.

Table 4: Electoral Cycles in the Expression of Ideological Commitments

	Liberal Vote		
	Ordinary Least Squares		Ordered Probit
	(1)	(2)	(3)
Lastquarter	-0.0197 [0.0358]	-0.0337 [0.0348]	-0.0507 [0.0497]
Democrat	0.0663 [0.00796]***		
Democrat * Lastquarter	0.0425 [0.0369]		
Appointed by Democrat		0.0707 [0.00820]***	0.0988 [0.0115]***
Appointed by Democrat * Lastquarter		0.0707 [0.0367]*	0.0955 [0.0497]*
Controls	Y	Y	Y
Observations	56058	56058	56058
R-squared	0.087	0.087	

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 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 in column (1) 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 is self-identified as a Democrat and an interaction between these two variables. The explanatory variables of interest in columns (2) and (3) 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. All regressions include circuit fixed effects, legal issues fixed effects, year fixed effects, quarter fixed effects, and a dummy variable indicating whether the panel is divided.

Table 5: Dissents as Expressions of Ideological Commitments

Sample:	Liberal Vote				
	Ordinary Least Squares	Entire Sample Ordered Probit	Last Quarter, Majority Judges in Divided Panels		
			Ordinary Least Squares	Ordered Probit	
	(1)	(2)	(3)	(4)	(5)
Appointed by Democrat	0.136 [0.0236]***	0.186 [0.0327]***	0.0616 [0.00811]***	0.106 [0.0324]***	0.151 [0.0448]***
Divided Panel	0.0294 [0.0185]	0.0388 [0.0261]	-0.0112 [0.0140]		
Appointed by Democrat * Divided Panel	-0.0830 [0.0250]***	-0.112 [0.0346]***			
Dissenting Vote			-0.115 [0.0398]***	-0.186 [0.181]	-0.218 [0.291]
Appointed by Democrat * Dissenting Vote			0.520 [0.0552]***	0.570 [0.270]**	0.783 [0.413]*
Controls	Y	Y	Y	Y	Y
Observations	56058	56058	56058	1350	1350
R-squared	0.087		0.089	0.155	

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 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 in columns (1) and (2) are a dummy variable whether the case was heard by divided panel, a dummy variable indicating whether the judge is a Democratic appointee and the interaction between these two variables. The explanatory variables of interest in columns (3)-(5) are a dummy variable indicating whether the vote is a dissenting vote, a dummy variable indicating whether the judge is a Democratic appointee and the interaction between these two variables. All regressions include circuit fixed effects, legal issues fixed effects, year fixed effects, quarter fixed effects, and a dummy variable indicating whether the panel is divided.

Table 6: Evidence Suggesting the Role of Priming in Electoral Cycles -- Divided Panels

	Dissent		
	Ordinary Least Squares		Probit
	(1)	(2)	(3)
Divided	0.0153 [0.00451]***	0.0129 [0.00451]***	0.0953 [0.0334]***
Lastquarter	0.0527 [0.0132]***	0.0205 [0.0197]	0.186 [0.141]
Divided * Lastquarter		0.0461 [0.0226]**	0.256 [0.140]*
Controls	Y	Y	Y
Observations	18686	18686	18686
R-squared	0.022	0.022	

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 a dummy variable equal to 1 if there was a dissenting opinion in the case. 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 the panel deciding the case was divided along ideological lines and an interaction term between these two variables. All regressions include quarter-to-election fixed effects, year fixed effects, quarter fixed effects, circuit fixed effects and legal issues fixed effects.

Table 7: Evidence Suggesting the Role of Priming in Electoral Cycles -- Judicial Characteristics

	Dissent					
	Ordinary Least Squares				Probit	
	(1)	(2)	(3)	(4)	(5)	(6)
Divided	0.00545 [0.00139]***	0.00479 [0.00139]***	0.00245 [0.00145]*	0.00524 [0.00148]***	0.00323 [0.00151]**	0.0637 [0.0299]**
Lastquarter	0.0129 [0.00413]***	0.00381 [0.00595]	0.0216 [0.0147]	-0.00255 [0.00589]	0.0171 [0.0155]	0.165 [0.210]
Divided * Lastquarter		0.0130 [0.00680]*	0.00709 [0.00880]	0.0146 [0.00700]**	0.00821 [0.00947]	0.137 [0.158]
Majority			-0.00700 [0.00193]***		-0.00609 [0.00204]***	-0.101 [0.0331]***
Majority * Lastquarter			-0.0178 [0.0120]		-0.0197 [0.0125]	-0.185 [0.144]
Previous US Attorney or Asst US Attrny				-0.00249 [0.00188]	-0.00258 [0.00188]	-0.0430 [0.0357]
Previous US Attorney or Asst US Attrny * Lastquarter				0.0186 [0.0127]	0.0193 [0.0126]	0.230 [0.126]*
Controls	Y	Y	Y	Y	Y	Y
Observations	56058	56058	56058	51086	51086	51086
R-squared	0.006	0.006	0.007	0.007	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 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) also includes an indicator variable equal to 1 if another member of the panel was appointed by the same president as the judge, as well as the interaction of this variable with lastquarter. Column (4) also includes an indicator variable equal to 1 if the judge was previously a U.S. Attorney or Assistant U.S. Attorney, as well as the interaction of this variable with lastquarter. The regressions in columns (5) and (6) combine the explanatory variables contained in columns (1)-(4). All regressions include year fixed effects, quarter fixed effects, circuit fixed effects and legal issues fixed effects.

Table 8: Evidence Suggesting the Role of Priming in Electoral Cycles -- Judicial Inexperience

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

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.

Table 9: Evidence Suggesting the Role of Priming in Electoral Cycles -- Contentious Elections

	Dissent					
	Ordinary Least Squares				Probit	
	(1)	(2)	(3)	(4)	(5)	(6)
Last Three Quarters	0.00661 [0.00391]*	0.00621 [0.00400]	0.0105 [0.00461]**	0.00705 [0.00404]*	0.160 [0.0906]*	0.178 [0.0917]*
Close Election (Electoral Count is Less than 55%)	0.0182 [0.00784]**	-0.00528 [0.00752]			-0.0137 [0.122]	
Close Election (Electoral Count is Less than 55%) * Last Three Quarters	0.0325 [0.0132]**	0.0264 [0.0132]**			0.649 [0.271]**	
Landslide Election (Electoral Count is More than 95%)			0.0110 [0.00948]	0.00881 [0.00417]**		-0.0641 [0.141]
Landslide Election (Electoral Count is More than 95%) * Last Three Quarters			-0.0126 [0.00628]**	-0.00881 [0.00424]**		-0.153 [0.0806]*
Year > 1975		0.0233 [0.00676]***		0.00129 [0.00599]	0.573 [0.297]*	0.559 [0.299]*
Year > 1975 * Last Three Quarters		0.00667 [0.00475]		0.0104 [0.00540]*	0.0648 [0.104]	0.131 [0.113]
War		0.00183 [0.00377]		0.00182 [0.00377]	0.0246 [0.0729]	0.0245 [0.0729]
War * Last Three Quarters		-0.0118 [0.00534]**		-0.00919 [0.00532]*	-0.256 [0.109]**	-0.204 [0.113]*
Controls	Y	Y	Y	Y	Y	Y
Observations	56058	56058	56058	56058	56058	56058
R-squared	0.006	0.006	0.006	0.006		

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. In Columns 1, 2, and 5, the explanatory variables of interest are a dummy variable indicating whether a case was decided in the three quarters immediately preceding a presidential election, a dummy variable indicating whether the margin of victory in the electoral count was less than 10% and an interaction between these two variables; in Columns 3, 4, and 6, the explanatory variables of interest are a dummy variable indicating whether a case was decided in the three quarters immediately preceding a presidential election, a dummy variable indicating whether the margin of victory in the electoral count was more than 90%, and an interaction between these two variables. The regression also includes circuit fixed effects, legal issues fixed effects, quarter fixed effects, year fixed effects, and a dummy variable indicating whether the panel was divided ideologically.

Table 10: Evidence Suggesting the Role of Priming in Electoral Cycles -- The Effect of War Time on Dissents

	Dissent										Affirm	Reverse
	Vote-Level					Case-Level						
	Ordinary Least Squares					Probit	Ordinary Least Squares		Probit	Ordinary Least Squares		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Divided	0.00578 [0.00132]***	0.00578 [0.00135]***	0.00534 [0.00135]***	0.00682 [0.00153]***	0.00579 [0.00148]***	0.135 [0.0314]***	0.0169 [0.00442]***	0.0150 [0.00442]***	0.0198 [0.00499]***	0.142 [0.0360]***		
4 Months Before	0.0165											
September 11, 2001	[0.0263]											
3 Months Before	-0.0136											
September 11, 2001	[0.0121]											
2 Months Before	0.00948											
September 11, 2001	[0.0206]											
1 Month Before	-0.0112											
September 11, 2001	[0.0185]											
Month of	0.00560											
September 11, 2001	[0.0240]											
1 Month After	-0.0224											
September 11, 2001	[0.00870]**											
2 Months After	0.0675											
September 11, 2001	[0.0452]											
3 Months After	-0.0233											
September 11, 2001	[0.00707]***											
4 Months After	0.0438											
September 11, 2001	[0.0314]											
5 Months After	-0.00952											
September 11, 2001	[0.0178]											
6 Months After	-0.00350											
September 11, 2001	[0.0178]											
World War 2		-0.00616 [0.00370]*	-0.000960 [0.00371]				0.00280 [0.0131]	0.0240 [0.0132]*				
Korean War		-0.00928 [0.00283]***	-0.00575 [0.00284]**				-0.0331 [0.0102]***	-0.0187 [0.0101]*				
Vietnam War		-0.00556 [0.00171]***	-0.00488 [0.00168]***				-0.0153 [0.00573]***	-0.0126 [0.00565]**				
Gulf War		0.000711 [0.00776]	-0.00281 [0.00769]				0.0401 [0.0200]**	0.0257 [0.0197]				
Afghan War		0.00122 [0.00572]	-0.00445 [0.00549]				0.00740 [0.0240]	-0.0157 [0.0229]				
War				0.000949 [0.00241]	-0.0000581 [0.00308]	0.116 [0.0721]			0.00992 [0.00869]	0.0870 [0.0660]	0.0389 [0.0107]***	-0.0166 [0.00962]*
Divided * War				-0.00782 [0.00282]***		-0.147 [0.0680]**			-0.0263 [0.00972]***	-0.191 [0.0727]***		
Inexperience (<= 10 Years)					0.00189 [0.00163]	0.0336 [0.0284]						
Inexperience * War					-0.00744 [0.00367]**	-0.160 [0.0710]**						
Year, Quarter, Quarter to Elect F	Y	N	N	N	N	N	N	N	N	N	N	N
Year (linear time trend)	N	N	Y	Y	Y	Y	N	Y	Y	Y	Y	Y
Observations	56058	56058	56058	56058	49374	49374	18686	18686	18686	18686	18686	18686
R-squared	0.006	0.003	0.004	0.004	0.005		0.010	0.014	0.014		0.045	0.017

Notes: Robust standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). 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. Controls for time period are listed in table.

Table 11: Evidence Suggesting an Increase in Electoral Cycles over Time

	Dissent		Liberal Vote		Dissent	Number of Judicial Retirements	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lastquarter	0.0297	0.0300			0.0263		
	[0.0172]*	[0.0171]*			[0.0173]		
Year > 1975 * Lastquarter	0.0407	0.0412			0.0418		
	[0.0205]**	[0.0204]**			[0.0210]**		
Divided		0.00535					
		[0.00500]					
Year > 1975 * Divided		0.0218					
		[0.00897]**					
Democrat Appointee			0.0520	0.0509			
			[0.0106]***	[0.0110]***			
Democrat Appointee * Year > 1975			0.0466	0.0403			
			[0.0159]***	[0.0170]**			
Born on or after 1940				-0.0329	0.00270		
				[0.0200]	[0.00624]		
Born on or after 1940 * Democrat Appointee				0.0714			
				[0.0321]**			
Born on or after 1940 * Lastquarter					-0.00478		
					[0.0341]		
After Election						-0.0191	-0.0153
						[0.0436]	[0.0374]
After Election * Year > 1975						0.497	
						[0.223]**	
After Election * Year > 1900							0.145
							[0.0827]*
Controls	Y	Y	Y	Y	Y	Y	Y
Observations	18686	18686	56058	51460	51460	911	911
R-squared	0.021	0.022	0.087	0.088	0.022	0.328	0.313

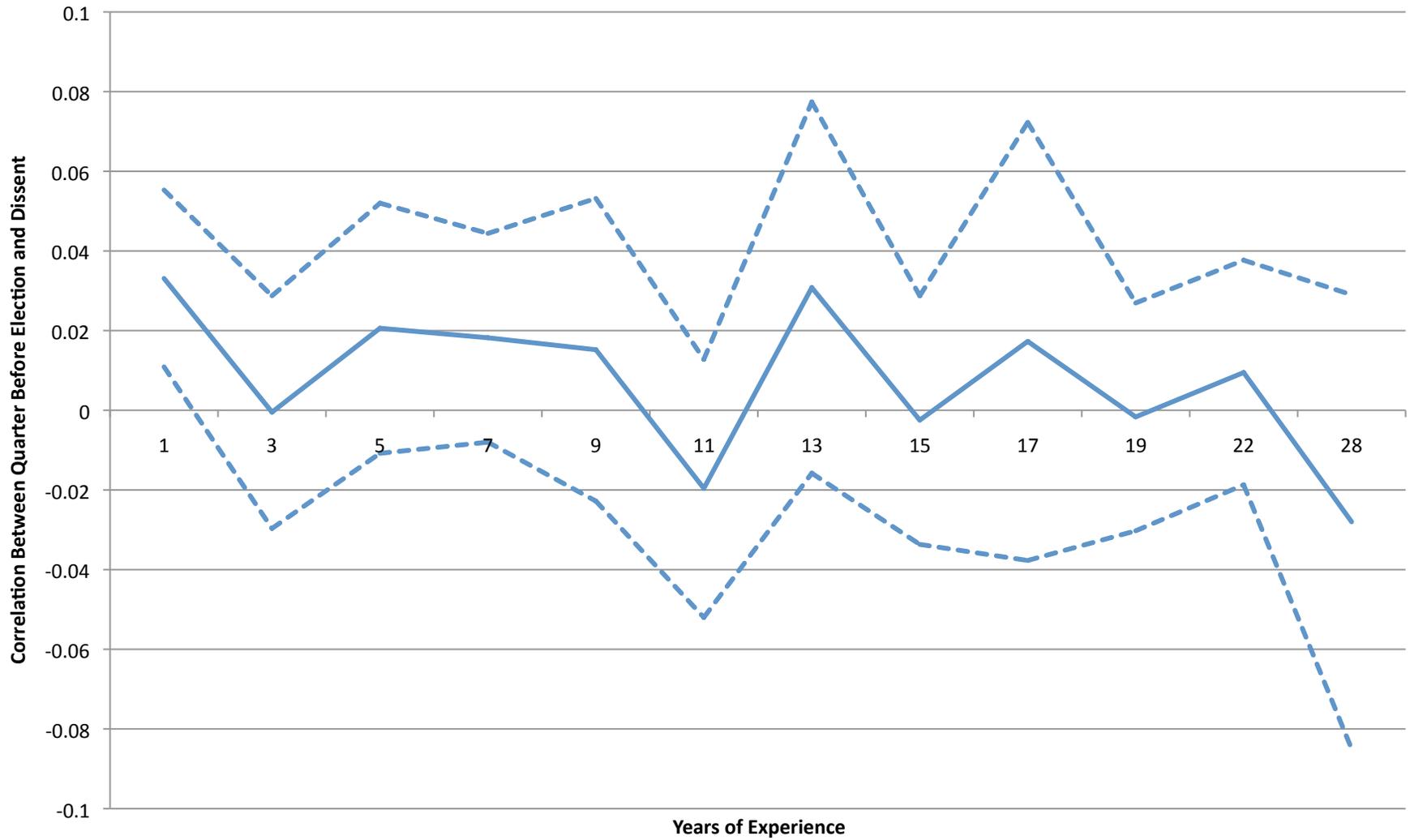
Notes: Robust standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Standard errors are double-clustered at the quarter-year and case level for the vote-level regressions, otherwise they are clustered at the quarter-year level. In Columns 1-5, regressions also include quarter to election fixed effects, year fixed effects (so the coefficient on the dummy indicator for whether it is after 1975 (or 1900) is not displayed), quarter fixed effects, circuit fixed effects and legal issues fixed effects. In Columns 6-7, the explanatory variables of interest are dummy variables indicating whether it is after an election or not (the first three quarters after an election count as after while the three quarters before an election count as before) and whether it is recent (before or after 1975; before or after 1900) and the regressions also include year fixed effects and seasonly quarter fixed effects.

Table 12: Electoral Cycles in Judicial Exits

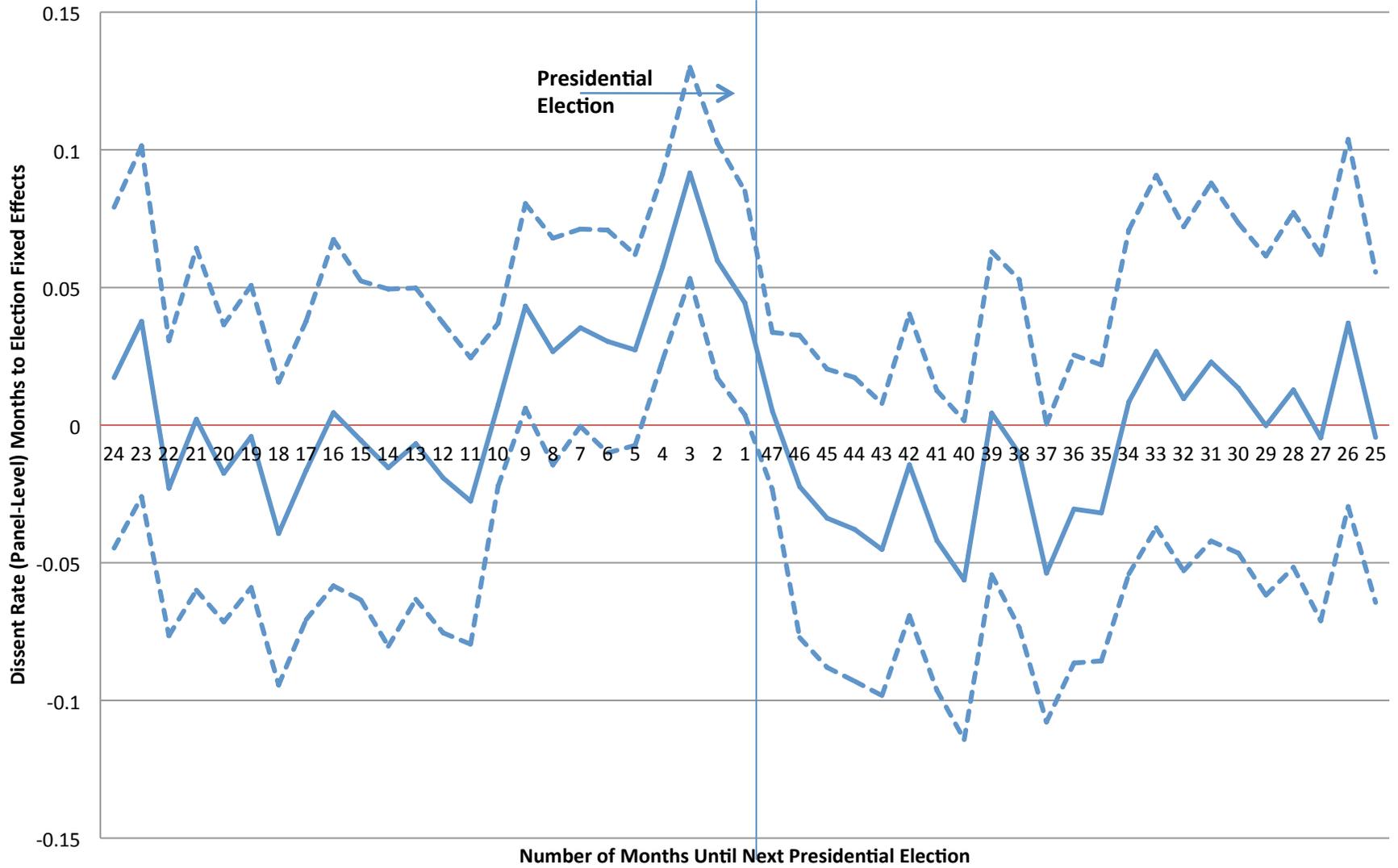
Sample:	Number of Retirements			Number of Resignations		
	All Judges (1)	Party in Power is Same (2)	Party in Power is Different (3)	All Judges (4)	Party in Power is Same (5)	Party in Power is Different (6)
Quartermoelect = 1	-0.0948 [0.0487]*	-0.00431 [0.0277]	-0.0905 [0.0384]**	-0.0188 [0.0135]	-0.0157 [0.0121]	-0.00306 [0.00517]
Quartermoelect = 2	-0.110 [0.0521]**	-0.0290 [0.0282]	-0.0813 [0.0400]**	0.0148 [0.0204]	0.00616 [0.0178]	0.00862 [0.00974]
Quartermoelect = 3	-0.141 [0.0518]**	-0.0382 [0.0276]	-0.103 [0.0396]**	0.0102 [0.0211]	-0.00267 [0.0183]	0.0129 [0.0103]
Quartermoelect = 4	0.0167 [0.0843]	0.0270 [0.0507]	-0.0102 [0.0532]	0.00180 [0.0243]	0.00673 [0.0242]	-0.00492 [0.00587]
Quartermoelect = 5	-0.0192 [0.0917]	-0.00347 [0.0538]	-0.0157 [0.0616]	-0.000635 [0.0323]	0.000813 [0.0282]	-0.00145 [0.0116]
Quartermoelect = 6	0.0437 [0.0935]	0.0633 [0.0569]	-0.0196 [0.0621]	-0.00630 [0.0296]	-0.00999 [0.0273]	0.00370 [0.0117]
Quartermoelect = 7	-0.0655 [0.0880]	0.0215 [0.0528]	-0.0870 [0.0585]	-0.0370 [0.0271]	-0.0319 [0.0252]	-0.00513 [0.0100]
Quartermoelect = 8	-0.00572 [0.0847]	0.0344 [0.0478]	-0.0401 [0.0582]	0.0134 [0.0273]	0.0233 [0.0252]	-0.00984 [0.0106]
Quartermoelect = 9	-0.0547 [0.0859]	0.0104 [0.0492]	-0.0652 [0.0613]	0.0175 [0.0284]	0.0239 [0.0245]	-0.00637 [0.0138]
Quartermoelect = 10	-0.00492 [0.0940]	0.0380 [0.0548]	-0.0429 [0.0662]	0.0315 [0.0302]	0.0196 [0.0236]	0.0118 [0.0185]
Quartermoelect = 11	-0.115 [0.0828]	0.00250 [0.0462]	-0.118 [0.0622]*	0.0419 [0.0305]	0.0322 [0.0241]	0.00972 [0.0184]
Quartermoelect = 12	-0.108 [0.0730]	-0.0378 [0.0408]	-0.0700 [0.0558]	0.00766 [0.0206]	0.0227 [0.0134]*	-0.0150 [0.0152]
Quartermoelect = 13	-0.127 [0.0787]	-0.0454 [0.0483]	-0.0821 [0.0588]	0.0253 [0.0235]	0.0368 [0.0148]**	-0.0114 [0.0178]
Quartermoelect = 14	-0.0897 [0.0841]	-0.0639 [0.0475]	-0.0258 [0.0639]	0.0329 [0.0259]	0.0325 [0.0151]**	0.000377 [0.0205]
Quartermoelect = 15	-0.148 [0.0804]*	-0.0732 [0.0429]*	-0.0745 [0.0635]	0.0284 [0.0259]	0.0237 [0.0136]*	0.00476 [0.0220]
Controls	Y	Y	Y	Y	Y	Y
Observations	2433	2433	2433	2433	2433	2433
R-squared	0.273	0.198	0.282	0.104	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)-(3) is the number judges that retire in a particular month. The outcome variable in columns (4)-(6) 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.

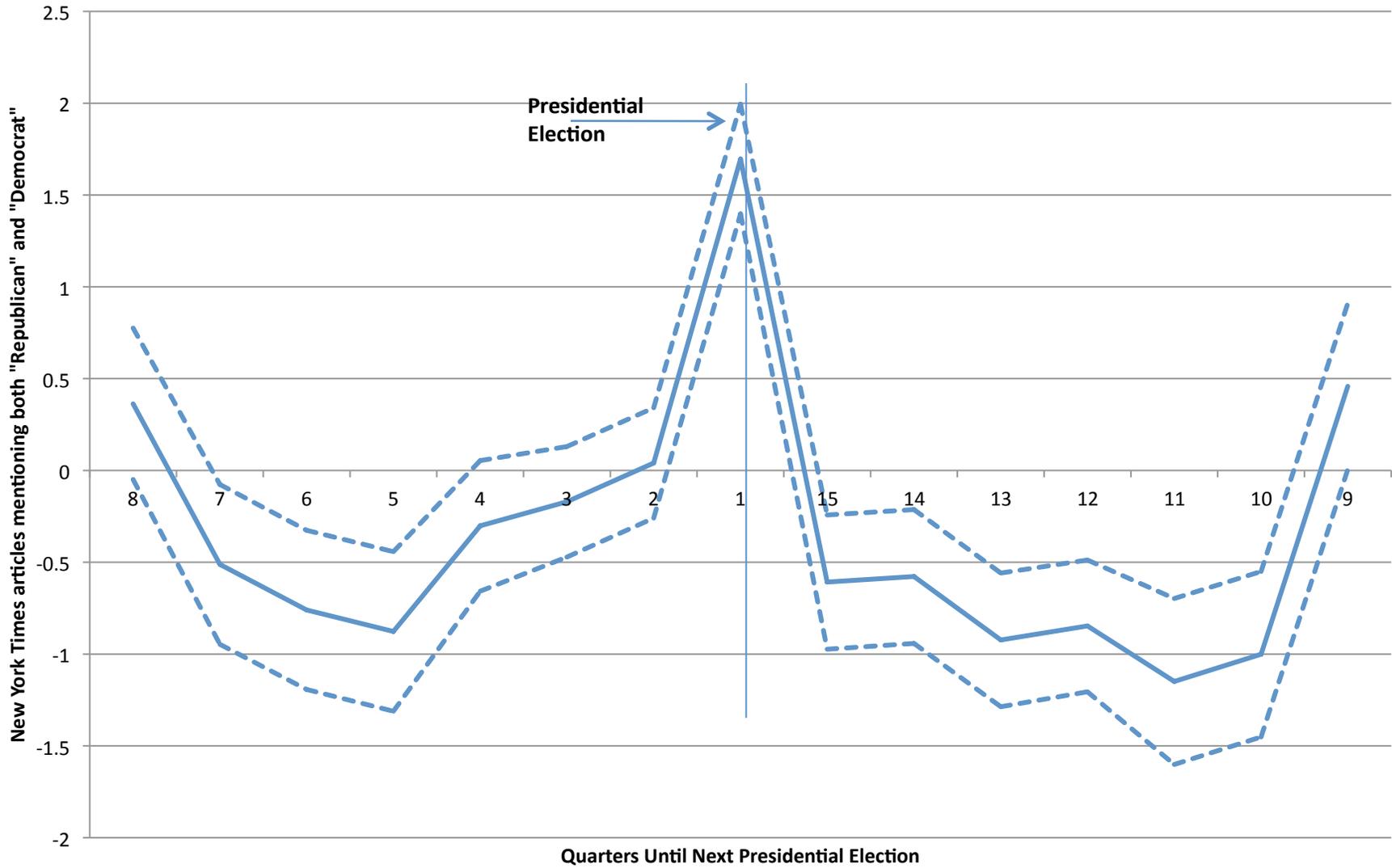
**Appendix Figure A:
Years of Judicial Experience and the Electoral Cycle**



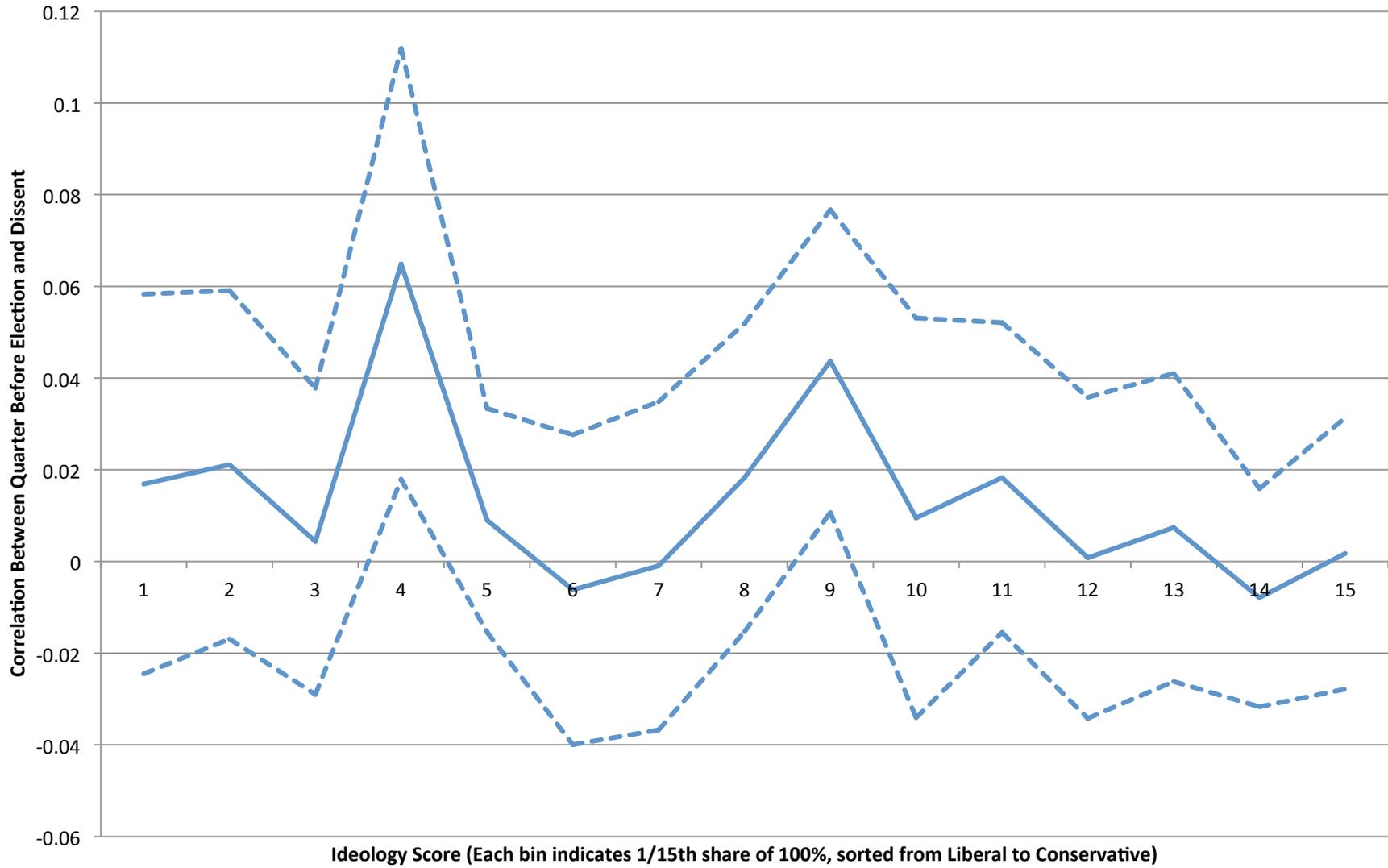
Appendix Figure B: Dissent Rate across the Political Cycle



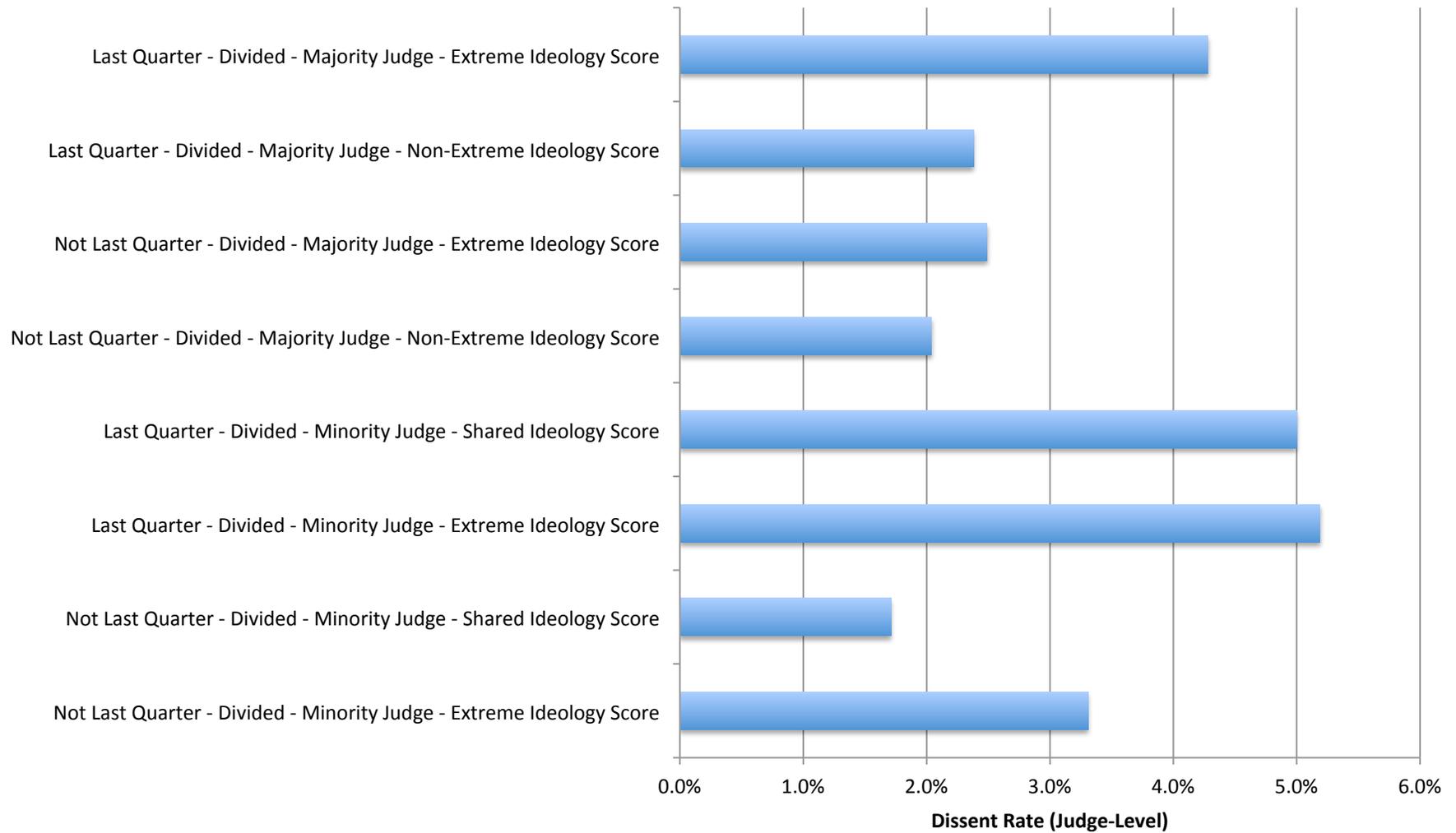
Appendix Figure 3: Political News Articles across Electoral Cycle



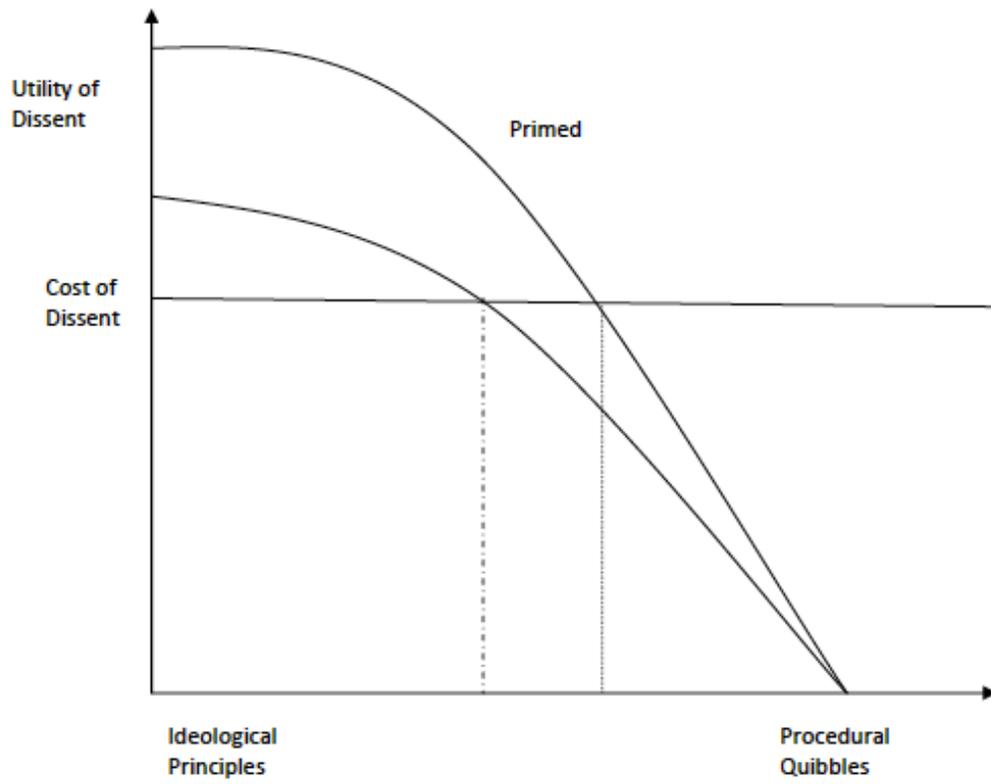
Appendix Figure 4: Ideology Score and Electoral Cycle



Appendix Figure 5: The Role of Ideology in the Effect of Electoral Cycles on Dissents



Appendix Figure 6: The Role of Ideology in Dissents



Appendix Figure 7: Electoral Cycles in Voting Valence by Ideology Score



Appendix Table A: Summary Statistics

Case Level		Judge-Vote Level		Judge Level	
Dissent	0.0786 [0.00197]	Dissent	0.0234 [0.000638]	Resignation	0.023 [0.00310]
Divided	0.697 [0.00336]	Majority	0.768 [0.00178]	Retirement	0.124 [0.00870]
Criminal	0.269 [0.00324]	Previous US Attorney or Assistant US Attorney	0.154 [0.00160]	Resignation, when Same Party in Power	0.0152 [0.00248]
Civil Rights	0.0859 [0.00205]	Democrat	0.510 [0.00211]	Resignation, when Different Party in Power	0.00781 [0.00178]
First Amendment, Due Process, Privacy	0.0281 [0.00121]	Appointed by Democrat	0.491 [0.00211]	Retirement, when Same Party in Power	0.0506 [0.00521]
Labor Relations	0.0723 [0.00189]	Opposing Party Wins Election	0.504 [0.00211]	Retirement, when Different Party in Power	0.0732 [0.00611]
Economic Activity	0.509 [0.00366]	Close Election	0.048 [0.00090]	N	2433
Dissent for Procedural, not Merit Reasons	0.0925 [0.0193]	Electoral Count < 55%	62.19 [0.0393]	Dissent Valence if Democratic Appointee	
Length of Majority Opinion (in pages)	4.565 [0.0245]	Age	10.33 [0.0335]	Conservative	31%
Citations	6.060 [0.0690]	Experience	0.600 [0.00221]	Liberal	59%
Dissenting Citations	0.547 [0.0079]	Inexperience	0.0161 [0.000555]	Mixed	8%
Affirm	0.568 [0.00362]	Experience <= 10 years	0.0309 [0.000731]	Could not be determined	2%
Reverse	0.221 [0.00303]	Elevated	56147	N	651
N	18686	Retire Next Year		Voting Valence if Democratic Appointee	
Note: Coefficients shown are from an OLS regression on a constant.		N		Conservative	47%
		Dissent Valence if Republican Appointee		Liberal	36%
		Conservative	58%	Mixed	6%
		Liberal	29%	Could not be determined	11%
		Mixed	10%	N	27550
		Could not be determined	3%	Voting Valence if Republican Appointee	
		N	657	Conservative	52%
				Liberal	32%
				Mixed	7%
				Could not be determined	9%
				N	28597

Appendix Table B: Falsification -- Electoral Cycles in Workload, Fraction of Opinions Published, and Time Spent per Case

Quarters to Election from Judgement	Number of Months in sample for Corresponding 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 C: Falsification -- Electoral Cycles in Panel Composition, Case Type, and Case Load

	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)
Quartermoelect = 1	-0.0193 [0.0191]	0.000959 [0.0216]	-0.00626 [0.0151]	-0.0141 [0.00987]	-0.00179 [0.0117]	-0.0120 [0.0314]	-0.0356 [0.0973]
Quartermoelect = 2	0.0227 [0.0220]	0.00380 [0.0211]	-0.00820 [0.0144]	0.000377 [0.00930]	-0.0133 [0.0129]	0.00868 [0.0270]	-0.0159 [0.101]
Quartermoelect = 3	0.0252 [0.0221]	-0.00355 [0.0189]	-0.0133 [0.0134]	-0.00814 [0.00820]	0.00968 [0.0122]	0.00820 [0.0240]	-0.00575 [0.0943]
Quartermoelect = 4	0.00376 [0.0214]	0.00901 [0.0214]	-0.0161 [0.0147]	0.00653 [0.00871]	0.00698 [0.0114]	-0.0214 [0.0262]	0.122 [0.108]
Quartermoelect = 5	-0.0393 [0.0303]	-0.00654 [0.0286]	-0.00261 [0.0172]	-0.00933 [0.0106]	-0.0202 [0.0145]	0.00645 [0.0354]	-0.0516 [0.136]
Quartermoelect = 6	-0.00114 [0.0292]	-0.0106 [0.0272]	-0.00242 [0.0180]	-0.00599 [0.00992]	-0.00163 [0.0154]	0.0108 [0.0318]	0.0940 [0.140]
Quartermoelect = 7	0.0240 [0.0297]	-0.00843 [0.0257]	0.00551 [0.0179]	-0.00306 [0.00928]	0.00810 [0.0162]	0.00177 [0.0298]	0.0806 [0.138]
Quartermoelect = 8	0.0266 [0.0266]	-0.0222 [0.0241]	0.000457 [0.0162]	-0.00704 [0.00853]	-0.00398 [0.0131]	0.0220 [0.0263]	0.115 [0.126]
Quartermoelect = 9	-0.00273 [0.0304]	0.00741 [0.0303]	-0.0159 [0.0197]	-0.0131 [0.0109]	-0.0216 [0.0152]	0.0324 [0.0309]	-0.0647 [0.143]
Quartermoelect = 10	0.0161 [0.0307]	-0.00125 [0.0299]	0.00727 [0.0187]	-0.0164 [0.01000]	-0.0322 [0.0149]**	0.0344 [0.0326]	-0.0118 [0.148]
Quartermoelect = 11	0.0133 [0.0295]	-0.0302 [0.0300]	0.000967 [0.0162]	-0.0171 [0.00947]*	-0.0126 [0.0147]	0.0546 [0.0320]*	-0.0537 [0.143]
Quartermoelect = 12	-0.00647 [0.0265]	0.0123 [0.0219]	-0.0126 [0.0145]	-0.0109 [0.00699]	-0.0162 [0.0110]	0.0313 [0.0216]	0.223 [0.119]*
Quartermoelect = 13	-0.0347 [0.0308]	0.0264 [0.0240]	-0.0102 [0.0170]	-0.00574 [0.00856]	-0.0252 [0.0111]**	0.0138 [0.0242]	-0.170 [0.112]
Quartermoelect = 14	0.00176 [0.0316]	0.0180 [0.0237]	0.00296 [0.0164]	-0.0104 [0.00860]	-0.0148 [0.0120]	-0.00979 [0.0240]	0.0505 [0.117]
Quartermoelect = 15	0.0180 [0.0323]	0.00532 [0.0237]	-0.00417 [0.0170]	-0.0172 [0.00776]**	-0.0167 [0.0118]	0.0359 [0.0250]	0.0240 [0.111]
Controls	Y	Y	Y	Y	Y	Y	Y
Observations	18686	18686	18686	18686	18686	18686	8016
R-squared	0.048	0.050	0.053	0.022	0.021	0.099	0.090

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). All regressions include year fixed effects, quarter fixed effects, and circuit court fixed effects.

Appendix Table D: Electoral Cycles in Dissents - Robustness Checks

	Dissent				
	(1)	(2)	(3)	(4)	(5)
	Each coefficient represents a separate regression			Probit	Logit
		Drop 1 Circuit at a time	Keep 1 Circuit at a time		
Quarters to Election	-0.00242 [0.000700]***				
Lastquarter (Entire Sample)		0.0527 [0.0132]***		0.0405 [0.0113]***	0.0405 [0.0113]***
Lastquarter Circuit 1		0.0532 [0.0142]***	0.0545 [0.0398]		
Lastquarter Circuit 2		0.0548 [0.0139]***	0.0383 [0.0589]		
Lastquarter Circuit 3		0.0444 [0.0134]***	0.164 [0.0651]**		
Lastquarter Circuit 4		0.0573 [0.0136]***	0.0102 [0.0552]		
Lastquarter Circuit 5		0.0586 [0.0137]***	0.0128 [0.0286]		
Lastquarter Circuit 6		0.0492 [0.0125]***	0.102 [0.0500]**		
Lastquarter Circuit 7		0.0523 [0.0139]***	0.0567 [0.0378]		
Lastquarter Circuit 8		0.0568 [0.0140]***	0.00822 [0.0453]		
Lastquarter Circuit 9		0.0521 [0.0130]***	0.0669 [0.0371]*		
Lastquarter Circuit 10		0.0578 [0.0147]***	0.00819 [0.0320]		
Lastquarter Circuit 11		0.0503 [0.0136]***	0.192 [0.0590]***		
Lastquarter Circuit 12		0.0471 [0.0145]***	0.135 [0.0614]**		

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 variable indicating whether it is the last quarter before an election or a continuous variable for quarters to election. The regression also includes quarter to election fixed effects, 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 for the last quarter only in Columns (4) and (5).

Appendix Table E: Electoral Cycles in Judicial Exits - Robustness Checks

	Number of Retirements	
	(1)	(2)
	Each coefficient represents a separate regression Drop 1 Circuit at a time	
Quarters to Election	0.00563 [0.00286]**	
After Election (Entire Sample)		0.0741 [0.0425]*
After Election (Drop Circuit 1)		0.0686 [0.0401]*
After Election (Drop Circuit 2)		0.0784 [0.0426]*
After Election (Drop Circuit 3)		0.0828 [0.0427]*
After Election (Drop Circuit 4)		0.0828 [0.0427]*
After Election (Drop Circuit 5)		0.0414 [0.0375]
After Election (Drop Circuit 6)		0.0828 [0.0423]*
After Election (Drop Circuit 7)		0.0752 [0.0415]*
After Election (Drop Circuit 8)		0.0686 [0.0415]*
After Election (Drop Circuit 9)		0.0501 [0.0333]
After Election (Drop Circuit 10)		0.0686 [0.0400]*
After Election (Drop Circuit 11)		0.0643 [0.0426]
After Election (Drop Circuit 12)		0.0512 [0.0397]

Notes: Robust standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Voluntary judicial leavings are the number of judges that retire or resign in a particular month. The explanatory variables of interest are dummy variables indicating whether it is after an election or not (the first three quarters after an election count as after while the three quarters before an election count as before). Each coefficient represents a separate regression. The regression also includes year fixed effects and seasonly quarter fixed effects (only in Column (1)).

Appendix Table F: Who Does Electoral Cycles in Judicial Exit -- Party of Appointment

	Number of Judicial Retirements		Number of Judicial Resignations	
	of Democratic Judges	of Republican Judges	of Democratic Judges	of Republican Judges
	(1)	(2)	(3)	(4)
Quartertolect = 1	-0.0367 [0.0307]	-0.0456 [0.0315]	-0.0141 [0.0105]	-0.00219 [0.00772]
Quartertolect = 2	-0.0390 [0.0319]	-0.0578 [0.0350]*	-0.00943 [0.0127]	0.0291 [0.0155]*
Quartertolect = 3	-0.0599 [0.0295]**	-0.0683 [0.0354]*	-0.00556 [0.0142]	0.0203 [0.0151]
Quartertolect = 4	0.00730 [0.0427]	0.0217 [0.0573]	-0.0000577 [0.0177]	0.00739 [0.0170]
Quartertolect = 5	-0.0359 [0.0491]	0.0316 [0.0636]	-0.00110 [0.0212]	0.00847 [0.0211]
Quartertolect = 6	-0.00556 [0.0522]	0.0652 [0.0645]	-0.0160 [0.0190]	0.0202 [0.0217]
Quartertolect = 7	-0.0265 [0.0511]	-0.0238 [0.0591]	-0.0187 [0.0184]	-0.00828 [0.0183]
Quartertolect = 8	0.0146 [0.0483]	-0.0253 [0.0575]	-0.000115 [0.0183]	0.0246 [0.0182]
Quartertolect = 9	-0.0351 [0.0517]	-0.0349 [0.0605]	0.00211 [0.0190]	0.0289 [0.0179]
Quartertolect = 10	0.00174 [0.0552]	-0.00791 [0.0680]	0.0133 [0.0211]	0.0341 [0.0175]*
Quartertolect = 11	-0.0590 [0.0502]	-0.0578 [0.0619]	-0.00243 [0.0184]	0.0518 [0.0195]***
Quartertolect = 12	-0.0375 [0.0432]	-0.0727 [0.0556]	-0.0102 [0.0170]	0.0122 [0.0106]
Quartertolect = 13	-0.0381 [0.0455]	-0.0892 [0.0630]	0.00554 [0.0200]	0.0167 [0.0117]
Quartertolect = 14	-0.00707 [0.0498]	-0.0814 [0.0647]	-0.00298 [0.0208]	0.0352 [0.0145]**
Quartertolect = 15	-0.0152 [0.0464]	-0.132 [0.0631]**	-0.00565 [0.0214]	0.0330 [0.0142]**
Controls	Y	Y	Y	Y
Observations	2433	2433	2433	2433
R-squared	0.185	0.226	0.088	0.115

Notes: Robust standard errors in brackets (* significant at 10%; ** significant at 5%; *** significant at 1%). Outcome variables are the number of judges that retire or 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 seasonly quarter fixed effects.