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**Judges as Party Animals:
Retirement Timing by Federal Judges and
Party Control of Judicial Appointments**

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Abstract

9
10 Longstanding debate over the Politicized Departure Hypothesis (PDH) asserts that federal judges
11 tend to arrange to retire under presidents of the same political party as the president who first
12 appointed them, thereby giving that party the right to nominate their successor. By timing their
13 departures politically, judges both contribute to the long-term political party orientation of courts
14 and express party agency, even though judges receive no consequent personal benefit. PDH studies
15 inevitably suffer from an absence of data on known and unknown determinants of retirement
16 timing. To avoid these and other problems, we apply 11 sharp regression discontinuity (SRD)
17 analyses to voluntary judicial departures before and after five elections that replace Republican
18 presidents with Democrats, and six that replace Democrats with Republicans, 1920 to 2018. For
19 10 of 11 analyses, the results of difference tests, difference-in difference tests, and others are as
20 predicted by PDH for pre-election and post-inauguration observation periods of 270 days. These
21 political effects appear to be stronger for Republican appointees than for Democratic appointees.
22 We also offer a novel explanation of politicized departure based on normative reciprocity rather
23 than simple ideology. The implications of the results from this pseudo-experimental design are
24 considered.
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26 **Introduction.** Much debated hypotheses claim that, as judges appointed under Article III of
27 the U.S. Constitution end their courtroom careers, they seek replacement by others who share their
28 political party orientation as Republicans or Democrats (Yoon 2017; Stolzenberg and Lindgren
29 2010; Yoon 2006; Zorn and Van Winkle 2000; Spriggs and Wahlbeck 1995).ⁱ Delivery of
30 appointment rights is straightforward, but sometimes difficult to execute: If judges retire, resign, or
31 accede to senior status early in the four-year term of an incumbent president, or whenever the sitting
32 president’s party controls the Senate, that president has the right to nominate a successor judge, and
33 time for confirmation by a cooperative Senate. By fine-tuning their dates of departure, judges can
34 control assignment of rights to appoint their replacements, barring their sudden death, poor
35 prediction of presidential election outcomes, or unforeseen personal exigencies (Chabot 2019;
36 Erikson and Wlezien 2008; Campbell 2008).

37 In one version of this politicized departure hypothesis (hereafter, PDH), presidents seek to
38 mold legal decisions by nominating judges who share their political ideology, values, attitudes and
39 opinions. Conversely, when judges leave full time court service, they seek to have their
40 replacements named by presidents who share their judicial ideology, values, attitudes or, at least,
41 opinions. Presidents choose among many candidate judges, but judges choose only between
42 departure under a Democratic or a Republican president, and can only trust that presidents’
43 ideologies, attitudes, values and opinions correlate with their political party affiliation (e.g., Chabot
44 2019; Stolzenberg and Lindgren 2010). We call this hypothesis the “enduring ideology” version of
45 PDH, because it relies upon judges’ maintenance of the political and legal attitudes and values that
46 led to their first appointment to the federal bench.ⁱⁱ

47 We propose a second version of PDH in which the norm of reciprocity, not enduring
48 ideology, calls on judges to return rights of appointment to presidents of the same party as the
49 presidents who first appointed them. The reciprocity norm is a mainstay of culture-focused social
50 science (see Gouldner 1960 and Molm, Collett, and Schaefer 2007 in sociology; Lubell and Scholz

51 2001 in political science; Whatley et al. 1999 in social psychology; Kloppenberg 2016 in history;
52 Graeber 2001 in anthropology; Fehr and Schmidt 2006 and Malmendier et al. 2014 in economics).
53 We call this variant of PDH the “reciprocity norm” version.

54 Both versions of PDH assert that when age, health, family matters, occupational fatigue or
55 anything else induces judges to end full-time judicial service, they tend to delay or accelerate their
56 separations, thereby delivering rights to nominate their successors to presidents of the same party as
57 the presidents who first appointed them. Neither version of PDH excludes the other.

58 If true, PDH is important for its far-reaching implications. Although party politics may be
59 unavoidable for judicial aspirants, PDH suggests that judges themselves act politically, without
60 financial or career advancement incentives, as they end their courtroom careers. If objective data
61 indicates that judges tend to act politically at career end, then it also provides evidence that party
62 politics also tends to influence their behavior throughout their judicial careers, and when evidence
63 of influence is less available.

64 More abstractly, PDH is important because it describes a self-replicating system shaped by
65 societal norms, supported by judges’ values, attitudes and behavior; facilitated by judicial,
66 presidential and Senatorial organizational structure, practices and procedures; replete with influence
67 of previous judges on the nominations of current jurists, and, barring unforeseen changes in judicial
68 selection, full of promise that current judges will have opportunities to choose the party whose
69 president will nominate their future replacements. If it is indeed institutionalized, as just described,
70 then politicized departure is likely to be durable, with diffuse effects extending beyond the careers
71 and decisions of individual judges, and past the tenures of individual presidents.

72 Finally, PDH is important because it implies that party politics influence American social
73 stratification through the courts as well as through the elected officials of the legislative and
74 executive branches of government. Competition for market advantage, indicia of social status, and
75 political power (i.e., the entire Weberian stratification paradigm) in the United States is governed by

76 federal laws, and refereed by federal judges. When disputes over resources, privileges and
77 competition reach those courts, “Article III judges” interpret laws and admissibility of facts, instruct
78 juries, decide sentences, make monetary awards, sometimes reach verdicts themselves, and issue
79 injunctions to halt prohibited behaviors. Thus, judges are arbiters of competition and dispute
80 involving labor and product markets, public accommodations, schools, housing, voting rights, civil
81 rights, and intragovernmental conflict. If correct, PDH provides a concise explanation of previous
82 findings of correlation between the political parties of presidents and the decisions of judges they
83 appoint (see, e.g. Sunstein et al. 2006; Shepherd 2009; Kang and Shepherd 2011; Kastellec 2011;
84 Spitzer and Talley 2016). Thus, a proper empirical test of PDH is broadly important for theoretical
85 and policy-related understanding of significant social, political, economic and legal issues.

86 Previous studies use judicial career data to consider PDH (see Stolzenberg and Lindgren
87 2010 for a review through 2010; and Yoon 2017, 2006; Bailey and Yoon 2011; Zorn and Van
88 Winkle 2000; Spriggs and Wahlbeck 1995; Barrow and Zuk 1990; Choi 2013; Van Tassel 1993
89 Hansford, Savchak, and Songer 2010; and Nixon and Haskin 2011). Those studies focus on subsets
90 that constitute a minority of Article III judges (usually Supreme Court justices; occasionally Circuit
91 Courts of Appeals judges), and therefore heighten current interest in testing PDH in the entire
92 Article III judiciary.

93 New PDH tests are also motivated by labor force studies that suggest the need to control for
94 health, family circumstances, work attitudes, long term career plans and other career characteristics
95 that are difficult to ascertain for living judges and simply unavailable for most or all of the dead (see
96 Munnell, Sanzbacher and Rutledge 2018 on the importance of these measures in the general
97 population; and Greenhouse 1984 regarding difficulties in obtaining such information from judges).
98 These data difficulties are analogous to problems found in studies of class size effects on school
99 learning, and minimum wage legislation effects on demand for labor (Angrist and Lavy 1999; Card
100 and Krueger 1994), and we propose that the same methods used to cope with those problems in

101 school and employment data can be applied to tests of PDH. Those methods include regression
102 discontinuity methods and sharp regression discontinuity (hereafter SRD) methods. Application of
103 SRD to PDH is novel, but SRD is both old and now widely applied social science data (see e.g.,
104 Thistlewaite and Campbell 1960; Holland 1986; Wasserman 2003:251; Morgan and Winship 2014;
105 Cunningham 2021: Chapter 6).

106 In short, this paper describes SRD tests of PDH for judges who were appointed under
107 Article III of the U.S. Constitution, and who terminated full-time judicial service from 1919, when
108 employment terms of these judges first approximated their current form, to 2018, when we began
109 this research. To further identify unanswered questions, necessary methods and appropriate data, the
110 next section reviews previous PDH studies. Then, we describe the SRD method that we apply to
111 those data. Finally, we describe findings and draw conclusions.

112 **Previous Research.** This section considers previous PDH research. Stolzenberg and
113 Lindgren (2010: Table 1) list and briefly describe some 20 previous analyses of departures from the
114 Supreme Court of the United States (hereafter, SCOTUS). Some of these studies examine only the
115 statistical distribution of SCOTUS vacancies (Wallis 1936; Callen and Leidecker 1971; Ulmer
116 1982). Some other judicial career research is not probative of PDH. For example, King (1987) and
117 Hagel (1993) combine death-in-office with retirement, resignation and partial retirement (“senior
118 status”), although death-in-office is an involuntary biological consequence of failure to leave the
119 bench before death, while senior status accession, resignation and retirement are voluntary actions
120 reserved for the living.

121 Labor force-wide studies find that the probability of voluntary employment termination
122 varies inversely with workers’ health or “vitality” (French 2005; Bound 1991; Dwyer and Mitchell
123 1999; Parsons 1982). Virtually all previous historical narrative studies of SCOTUS voluntary
124 terminations consider the retirement effects of declining vitality, or, in Garrow’s (2000) sensational
125 wording, “decrepitude.” In statistical analyses, Squire (1988) includes a measure of poor health

126 which is criticized by Hagle (1993: 35) and Zorn and van Winkle (2000: 162). For dead justices,
127 Stolzenberg and Lindgren (2010) use years-left-to-live at a time before death to indicate health at
128 that time. However, remaining lifetime is more reliable for measuring population average health
129 than individual health. Zorn and van Winkle (2000) use justices' written opinion production to
130 measure physical health, but the many determinants of productivity raises questions about the
131 validity of this measure (see Green and Baker 1991). Finally, we suggest that judges' career and
132 employment decisions seem likely to be less affected by actual health and future longevity than by
133 judges' unobservable perceptions of those things: Sick judges may refuse retirement if they think
134 themselves healthy; healthy people may be more likely to retire if they think themselves ill.
135 Moreover, Hagle (1993:46) asserts that SCOTUS justices are flagrantly dishonest and willfully
136 misleading about their health. Thus, controlling for health in judicial career studies requires
137 methods that do not require direct health measurement or candid self-reporting by judges. We return
138 to this issue below, after describing previous efforts to distinguish effects of political party identity,
139 which would be central to the reciprocity norm version of PDH, from political ideology effects,
140 which would be central to the enduring ideology version of PDH.

141 “Conceptually, differences between ideology and party are stark, because parties are
142 organizations of people, and ideologies are complexes of values, attitudes, ideas and perceptions
143 (see J.L Martin 2015). Conceptual differences notwithstanding, empirical observations of ideologies
144 and party affiliations of individuals can be correlated empirically, even to the point that effects of
145 one are difficult or impossible to distinguish from effects of the other. In the general population,
146 party identification and ideology of individuals are regularly measured by survey questions. For
147 judges, party identification is conveniently defined and observed as the party of the president who
148 first appointed them to the Article III bench. But judicial custom and ethics makes measurement of
149 ideology has been more involved. Pinello (1999: 219) reviews and exhaustively meta-analyzes 84
150 prior studies, then concludes, “party is a dependable yardstick for ideology.” Thus, Pinello implies

151 that ideology and reciprocity versions of PDH are empirically indistinguishable, even if their
152 conceptual dissimilarity suggests otherwise.

153 Judicial ideology measurement has grown considerably since Pinello (1999). Martin and
154 Quinn (2002) (hereafter MQ) show that ideology can be measured without reference to party, by
155 Item Response Theory (IRT) scaling of SCOTUS justices' votes in court decisions. In a
156 computational tour de force, MQ calculate annual ideal point IRT ideology scores for SCOTUS
157 justices, starting in 1937ⁱⁱⁱ, based on voting in case decisions. Whatever their advantages, MQ
158 methods cannot be applied to judges of district courts, because they do not cast votes on panels, as
159 do SCOTUS and appellate court judges. Judicial Common Space (JCS) scales combine MQ scores
160 with other data for SCOTUS justices. For judges of Circuit Courts of Appeals, the JCS scale
161 confounds party and ideology, which is inferred from the political parties of the appointing
162 president and senators from the judge's home state. In a recent, novel, indirect measurement
163 strategy applied to judges of all Article III courts, Bonica et al (2019) use political donations of
164 money by law clerks of Article III judges to indicate political ideologies of the judges for whom
165 they work.^{iv}

166 In short, techniques for measuring judicial ideology have developed considerably since
167 Pinello's analysis. So one can no longer rely on the implication of his study that empirical measures
168 of party identity and judicial ideology are not different enough to allow separate tests of the
169 ideological endurance and reciprocity norm versions of PDH. To make the necessary update, we
170 examine the empirical congruence of party and ideology measures by principal components factor
171 analysis of data on the 31 SCOTUS justices who served at any time from 1960 to 2018. We focus
172 on SCOTUS justices because they are the only judges for whom there exist ideology measures that
173 are not at least partially based on party identity – i.e. MQ scores. We focus on 1960 to 2018 to
174 include other ideology scores that are available only after 1960. We end observations in 2018,
175 because that is the year we began research reported here. Factor analyzed variables include lifetime

176 averages of Bailey, MQ and JCS scores, plus Rep (= 1 for justices first appointed the Article III
177 judiciary by a Republican president; = 0 else). Data and analysis details are given in an appendix.

178 Using data just described, principal components factor analysis finds only one factor with an
179 eigenvalue greater than 1, and it explains 99.37 percent of the variance among Rep, Bailey, MQ and
180 JCS scores. Factor loadings are all above .65 and average .90. Although the small N for the
181 analysis, and its restriction to SCOTUS justices from 1960 to 1988, calls for restraint, findings are
182 bolstered by their consistency with Pinello's summary of previous studies. In short, in spite of new
183 methods and resurgent interest in distinguishing ideology effects on PDH from party identity
184 effects, the factor analysis suggests that for SCOTUS justices, party identity and the ideology scales
185 analyzed here are all indicators of the same underlying factor.^{v vi}

186 For the present purpose of testing PDH in the entire Article III judiciary, the implications of
187 past research, and the factor analysis results just presented can be summarized briefly in three
188 points:

189 **First**, a disproportionate share of prior PDH research focuses on SCOTUS justices, who are
190 a small segment of the Article III judiciary. Thus, testing PDH in the entire Article III judiciary
191 remains an important task, and is the focus of analyses presented here.

192 **Second**, PDH can be hypothesized both as an effect of judges' ideology and as an effect of
193 their political party identity. Conceptual differences between political ideologies and political
194 parties are clear, but factor analysis finds that SCOTUS ideology and party measures are indicators
195 of the same underlying latent factor. For lower court judges, the ideology measures are computed
196 from party identity variables. Consequently, our analyses of politicized departure focus on the role
197 of party identity, which is readily and reliably available for all judges. However, without controlling
198 for ideology, effects of party identity on politicized departure would be a mixture of both ideology
199 and political identity effects.

200 Third, consideration of career and retirement studies in the general labor force suggests that
201 unobservable personal characteristics and circumstances of judges affect their ability to adjust the
202 timing of their retirements and resignations from full time judicial service. The next section
203 describes a strategy to hold unobservable characteristics of judges constant, and to test PDH for the
204 entire Article III judiciary.

205 **Analytic Strategy.** We re-state the PDH as follows: When judges are ready to end their
206 full-time federal judicial service, those who were first appointed by a Republican president are more
207 likely to end full-time service when the incumbent president is a Republican than when the
208 president is a Democrat, all else equal. Similarly, when Democratic appointees decide to end their
209 full-time judicial service, they are more likely to do so when the incumbent president is a Democrat,
210 all else equal. *PDH is an hypothesis that when judges retire or resign, they tend to adjust the timing*
211 *of their retirements so that the president who nominates their successors is of the same party as the*
212 *president who first appointed them to the Article III bench.*

213 Our strategy is to test these hypotheses by selecting pairs of time periods in which all
214 determinants of termination probability, except the political party of the incumbent president, may
215 be regarded as identical, or nearly so, for every judge who terminates full-time service in either
216 period. If pre-election and post-inauguration periods are adjacent and sufficiently short, judges'
217 attitudes, values, health, family characteristics, finances and other retirement-related characteristics
218 can be considered to be the same in both periods, leaving the political party of the sitting president
219 as the only retirement-related characteristic that changes with the inauguration of a new president.
220 Consequently, any difference between the termination probability after inauguration and the
221 probability before the election is attributed to the change in presidential party.

222 Circumstances just described occur naturally but irregularly, shortly before "regime-
223 changing" elections (here defined as elections and inaugurations that replace Democratic presidents
224 with Republicans, or vice versa) and after the inaugurations that follow them. For example, consider

225 the 270 days (about 9 months) before the presidential election of 2008 and the equal period after the
226 inauguration in 2009. The 2008 pre-election president was Republican; the 2009 post-inauguration
227 president was Democratic. We assume that retirement-related characteristics of judges do not
228 differ meaningfully between adjacent pre-election and post-inauguration periods. If this assumption
229 is tenable, then the average treatment effect of a Democratic president on departures from full time
230 judicial service of Democratically-appointed judges is the difference between the proportion of
231 Democratically-appointed judges who retire in the 2009 post-inauguration period and the proportion
232 of Democratically appointed judges who retire in the 2008 pre-election period. The PDH hypothesis
233 can be expressed as a positive after-before difference in the number of terminations, a positive after-
234 before difference in the rate of terminations, an after/before ratio greater than one, or an after/before
235 odds-ratio greater than one, depending on statistical preferences.

236 Regime changing elections and inaugurations occur 11 times from 1920 to 2017 (i.e.
237 elections of 1920, 1932, 1952, 1960, 1968, 1976, 1980, 1992, 2000, 2008, and 2016). By starting
238 these analyses in 1920, we evade statistical consequences of a small judiciary in earlier years (the
239 entire Article III judiciary does not exceed 200 active duty judges consistently until 1919), and
240 escape problems of comparing terminations of full-time judicial service before and after the 1919
241 modifications of Article III judicial employment regulations, which create the option of Senior
242 Service for long-serving, sub-SCOTUS judges. Accession to Senior Service facilitates terminations
243 from full time service by permitting judges a reduced caseload, or no case load at all, without loss
244 of honorific status, income or other perquisites.

245 As an additional control for confounding and spuriousness due to unobserved variables, we
246 also calculate the same after-before voluntary termination probability difference for judges first
247 appointed by a president of the same party as the presidential election loser, and subtract it from the
248 difference obtained from judges appointed by presidents of the same party as the election winner.
249 This is the “difference-in-differences” (“diff in diff” or, hereafter, DiD) statistic. Again, depending

250 on statistical preferences, DiD can be expressed as a difference between rates, a ratio or an odds
251 ratio. PDH predicts a positive value for DiD based on differences between rates, or ratios greater
252 than unity, if DiD is based on ratios and odds ratios.

253 To observe and control effects of historical peculiarities such as time elapsed between
254 regime-changing elections, or the political balance of the Senate, we replicate analyses at each of
255 the 11 regime-changing presidential elections from 1920 to 2016. For example, Eisenhower's 1952
256 election was the first regime-changing election after 1932. Perhaps World War II, the Great
257 Depression, or the unusually long, 20-year interval between these regime changes altered career
258 dynamics for politically-influenced federal judges during F.D. Roosevelt's presidential tenure.
259 Similarly, to control for possible confounding by political party of the incumbent president, we
260 stratify analyses by the party of the winner of the regime-changing election – six Republican and
261 five Democratic regime-changing victories from 1920 to 2016.

262 We perform all analyses separately for pre-election and post-enumeration periods of 180,
263 270, 365, 547 and 730 days, or approximately 6, 9, 12, 18 and 24 months before the regime-
264 changing election, and after the subsequent inauguration.^{vii} Thus, we stratify analyses by length of
265 the pre-election and post-inauguration enumeration periods, to determine if the treatment effect is
266 strongest at the beginnings of presidential terms in office, when incumbent presidents tend to be
267 most popular, have their greatest Senate support, and the maximum time available to negotiate
268 Senate confirmation of nominees.

269 Finally, we emphasize that the hypothesized presidential party effect on judicial full-time
270 service departures is probabilistic and incomplete (thus neither necessary nor sufficient). For
271 example, judges' voluntary terminations from full time judicial employment may coincide
272 randomly with White House occupancy by presidents of the same party as the presidents who first
273 appointed them to the federal bench, or fail to coincide despite effort by judges to arrange the
274 contrary. Also, judges' desires to comply with norms of reciprocity and enduring ideology may be

275 overwhelmed by their inaccurate predictions of future presidential election outcomes, or by
276 unexpected personal exigencies. As Justice Ginsburg illustrates, inaccurate election predictions and
277 personal exigencies can defeat intentions for politicized departure, thereby reducing the number of
278 politicized departures, biasing Diff and other measures downward, and thereby making tests of PDH
279 more stringent than their significance levels imply. Good luck and accurate predictions neither
280 compel nor motivate politicized departure, and so do not affect tests of it described here.

281 **Research Design And Data.** The process just described appears to be a previously un-
282 noticed, naturally occurring example of the sharp regression discontinuity (SRD) research design,
283 with 11 replications (Cattaneo and Vazquez-Bare 2016; Lee and Lemieux 2010; Imbens and
284 Lemieux 2008: 217-19; Thistlewaite and Campbell 1960). The hallmark of SRD is abrupt,
285 exogenous change in the state or value of a treatment.^{viii} We now describe the design of this
286 research in the language of experimentation, focusing on subjects, outcomes and treatments.

287 *Subjects.* The units of analysis – the subjects – in analyses presented here are persons who
288 were employed full-time as Article III federal judges for at least 730 days (about two years) prior to
289 a regime-changing presidential election between 1920 and 2016.^{ix} For brevity, we call retirements,
290 resignations and accessions to senior status “**trigger actions**,” because they trigger new presidential
291 nominations to the bench. Prior service of at least 730 days excludes judges who lack a minimal
292 claim to a federal judicial career, rather than a recent posting to a new job. Requiring a year of
293 post-inaugural life avoids the need to distinguish those who take a trigger action in that period from
294 those who might have done so, had they endured. Judges are, or would be, excluded from analysis if
295 they leave office involuntarily due to death, abolition of their appointed court, or Congressional
296 impeachment and conviction.

297 *Treatment.* Treatment occurs during enumeration periods shortly before regime changing
298 elections, and shortly after inaugurations that follow them. For each judge, treatment consists of
299 changing the party of the incumbent president from different from, to the same as, the political party

300 of the president who first appointed them to the federal judiciary. Characteristics of judges are
301 assumed to not change meaningfully from the start of the pre-election enumeration period to the end
302 of the post-inauguration period. These characteristics include judges' perceptions of their own
303 health, personal finances, job satisfaction, desire to retire, and similar.

304 *Outcomes.* For any of the 11 regime-changing elections considered here, three outcomes are
305 possible: Judges can take no trigger action; they can take a trigger action in the pre-election period;
306 or they can take a trigger action in the post-inauguration period.

307 *Effect Measures.* PDH predicts that, if treated judges terminate full-time service about the
308 time of a regime changing election, they are more likely to do so post-inauguration than pre-
309 election. Thus, for any particular regime changing election, the treatment effect is the difference
310 between the number of treated judges who terminate full-time service in the post-inauguration and
311 the number of treated judges who terminate full-time service in the pre-election period. Growth of
312 the federal judiciary from 1920 to 2018 would affect these numbers, so results are also expressed as
313 proportions, odds and odds ratios, per common statistical practice (Agresti 1990). Counts and
314 proportions can be recovered from n's, odds and odds ratios.

315 *An Example.* To illustrate and clarify the analysis plan, Figure 1 provides a schematic
316 diagram of the analysis design, the hypotheses it tests, and treatment effect measures for a single
317 election-inauguration (2008-2009; won by the Democratic candidate) and enumeration periods of
318 270 days before election and 270 days after inauguration. Symbols and terms are defined in Table 1.

319 --Insert Figure 1 about here --

320 --Insert Table 1 about here--

321 Row labels on the left side of Figure 1 distinguish untreated (Republican) appointees in the bottom
322 row from treated (Democratic) appointees above them. Across the top, column labels distinguish
323 pre-election periods on the left from post-inauguration periods to the right. Judges in Group A were
324 first appointed by Democratic presidents. After the election, those same Democratic appointee

325 judges appear in “Group B.” Hypothesis 1 asserts that the number of trigger actions by judges in
326 Group B after the election ($\sum^d Y_{aRD}$) exceeds the number of trigger actions by those very same
327 judges before the election ($\sum^d Y_{bRD}$) when they constitute Group A. Without loss of information, the
328 numbers of triggers in Group A and Group B can be divided by the number of Democratic
329 appointee judges N_d to obtain proportions, and the hypothesis becomes $H_A: (\sum^d Y_{aRD}/ N_d) -$
330 $(\sum^d Y_{bRD})/ N_d) > 0$. Re-scaling proportions to odds and comparing them by division instead of
331 subtraction yields the odds ratio, $\text{Diff} = {}^d O_{aRD}/{}^d O_{bRD}$, and the hypothesis becomes $H_A: \text{Diff} > 1$,
332 where super- and sub-scripts retain their meaning as earlier defined, O replaces Y to indicate the
333 odds of a trigger action rather than a count of trigger actions, and Diff is defined as written here.

334 We also compute Difference-in-Differences (hereafter, DiD) which is the ratio of Diff for
335 judges appointed by presidents of the same party as the winner of the most recent presidential
336 election to the same ratio for judges appointed by presidents of the same party as the *loser* of the
337 most recent presidential election. DiD controls for the possibility that some unrecognized agent has
338 appeared in the form of a secular trend or a random shock to increase trigger actions after
339 inauguration by all judges, regardless of the party of the president who first appointed them to the
340 federal bench.

341 Further, we consider a measure we call Directional Diff in Diff (hereafter, DDD), which
342 compares Diff to the end-of-term odds ratio measure of the effect of the pre-election president’s
343 political party on terminations by judges first appointed by presidents of that party. DDD is useful
344 in addressing the secondary hypothesis that political influence on trigger action timing declines as
345 the presidential term of office approaches expiration.

346 For the 2008 election and 2009 inauguration shown in Figure 1, there are 755 judges
347 appointed by Republican presidents and 499 appointed by Democrats. Thirteen Republican
348 appointees and 12 Democratic appointees take trigger actions in the 270 days preceding the 2008
349 presidential election. In the 270 days following the 2009 inauguration, 15 Republican appointees

350 and 26 Democratic appointees take trigger actions. Odds and odds ratios are computed with the
351 usual continuity correction of 0.5 (Agresti 1990:68), yielding the following results:

352 1. The odds ratio, Diff, equals 2.18, indicating that,

353 *as the political influence hypothesis predicts, the odds that Democratic appointees take a trigger*
354 *action in the post-inauguration period are more than twice the odds that they do so in the pre-*
355 *election period.*

356 2. The value of DID, the ratio of Diff for Democratic appointees to the same odds ratio for
357 Republican-appointed judges in the same period, is 1.90, indicating that,

358 *even if a secular trend or aberrant influence increased post-inauguration departures from full-time*
359 *judging, the increased odds ratio for Democratic appointees predicted by the political influence*
360 *hypothesis remains almost twice (1.90 times) the size of the odds ratio for Republican appointees.*

361 3. Finally, we obtain a value of 2.51 for DDD, indicating that

362 *the boost in odds of trigger actions by Democratic appointees during the first 270 days of this*
363 *regime-changing Democratic presidency is about two and one half times as large as the disparity*
364 *between Republican appointee odds of trigger action during the last 270 days before the election,*
365 *when the president was Republican, and Republican appointee odds of trigger action during the*
366 *270 days after the inauguration. This result for DDD is consistent with the hypothesis that political*
367 *influence effects decline as the end of the presidential term in office approaches.*

368 Identification of effect measures in these analyses is explicated formally by Hahn et al
369 (2001); see also Imbens and Lemieux (2009: 217-19); Lee and Lemieux (2010); Cattaneo and
370 Escanciano (2017); and Cattaneo et al. (2017). Informally, identification is apparent from several
371 design features of this research.. First, there is no self-selection for treatment: assignment to control
372 and treatment groups is determined by the outcome of a presidential election, and therefore beyond
373 control by any individual judge.^x Second, temporal ordering and close conjunction of treatment and
374 outcome are assured by strictly-defined periods in which the outcome is measured and the treatment

375 is either entirely present or completely absent. If unspecified individual characteristics of subjects
376 affect outcomes, their effects are cancelled by division in calculation of Diff. And, third, effects are
377 measured by comparisons of treated individuals to themselves when not treated, thereby permitting
378 an assumption that unobserved characteristics of treated and untreated subjects do not differ.
379 Formally, this last comparison is stratification on retirement/resignation/accesion to senior status
380 (retirement): everyone in the analysis is leaving full-time judging during an interval that straddles
381 an election and inauguration. The estimand of interest compares the is the ratio of the probability of
382 actual departure during the term of the outgoing president . As described famously by Frangakis
383 and Rubin (2002), this stratification on retirement renders retirement invariant in the analyses and
384 therefore without effect on the estimand (DIFF), obviating any need to specify an instrument for
385 retirement. For a comparison to instrumental variables estimation, see

386 *Replication and Stratification.* We apply the regression discontinuity design method just
387 explicated to federal judicial trigger actions immediately before and after each of the 11 regime-
388 changing presidential elections between from 1920 to 2016, using data from 1919 through 2018,
389 when we began preparation of data for these and other analyses. Because 6 of those regime-
390 changing elections were won by Republicans, and 5 were won by Democrats, the replication also
391 stratifies the analysis by the party of the presidential election winner.

392 *Significance tests.* We perform separate, disjoint tests of PDH, one for each regime
393 changing presidential election from 1920 to 2018. Absent any PDH effect, and other things equal,
394 probabilities of retirement before and after the election would be equal so that ${}^dY_{aRD} = {}^dY_{bRD}$.
395 Following Agresti (1990: 352), the null hypothesis of no presidential party effect on voluntary
396 terminations is

397
$$H_0: \text{difference} = {}^dY_{aRD} - {}^dY_{bRD} = 0,$$

398 and ${}^dY_{aRD} - {}^dY_{bRD} > 0$ is distributed as Bernoulli (binomial) trials with $p=0.5$ and $n=11$. The
399 probability of 8 or more successes is 0.113, which is the test significance level. For 9, 10 or 11

400 successes, significance levels are .033, .006 and .0005 respectively. In 6 analyses of Republican
401 appointees, probabilities of 5 or more, or 4 or more successes are .109 and .344 respectively. For
402 $n=5$ analyses of Democratic appointees, the probability of 4 or more successes is .188, and the
403 probability of 3 or more is .500. These tests do not address compound null hypotheses.

404 *Data*

405 Primary data examined here were produced by extensive checks, corrections and re-codes of
406 data downloaded from the Federal Judicial Center (n.d.a.) on April 28, 2018. Most corrections are
407 based on consistency checking and comparison with records and on-line biographies from the the
408 Federal Judicial Center (n.d.b.), resulting in a file of 86,316 judge-year records for all 3,516
409 individuals who were nominated by presidents to Article III judicial positions, confirmed by the
410 Senate and commissioned in office, from 1789 to April 2018.

411 *11 Results for Diff in 9-Month Observation Periods.* Table 2 reports values of Diff in
412 column (3) for analyses in which pre-election and post-inauguration periods are both 270 days, for
413 all regime-changing elections from 1920 through 2016.

414 --Insert Table 2 about here --

415 Per Column (3), Diff exceeds one in 10 of 11 analyses, and is consistent with the first hypothesis at
416 a significance level of 0.0059. Consistent with PDH, the mean of Diff is 3.23: on average, the odds
417 of trigger action in the post-inaugural period is 3.23 times the odds of a trigger action in the
418 immediately preceding pre-election period.

419 --Insert Figure 2 about here --

420 Figure 2 plots Diff for 270-day enumeration periods, from 1919 to 2018, with a line fitted by
421 Cleveland's (1979) "robust locally weighted regression" method. The main finding, by inspection
422 of the solid line in Figure 3, as from Column (3) of Table 2, is that temporal variation in Diff
423 reflects atypically large values at the elections of 1952 and 1960, and is consistent with PDH.

424 **DiD Results for 9-month observation periods.** Consistent with PDH, Column (5) of Table 2
425 shows the mean of DiD as 4.19. So, on average, Diff is 4.19 times as large for judges first appointed
426 by presidents of the same party as the newly-inaugurated president (concordant party judges) as for
427 those first appointed by presidents of the other party (discordant party judges). Consistent with
428 PDH, DiD exceeds one in 10 of 11 analyses, for a binomial test significance level of .00059.

429 **DDD results for 9-month observation periods.** Directional Diff in Diff (DDD) compares
430 beginning-of-presidential term PDH effects to end-of-term PDH effects. The mean of DDD in
431 column (7) of Table 2 is 3.30, indicating that the impact of party concordance is more than three
432 times as large at the start of a president's term as at the end. DDD exceeds unity in 8 of 11 election-
433 inauguration sequences, with a significance level of 0.113.

434 --Insert Table 3 about here --

435 **Party Differences.** Rows 2 and 3 of Table 3 compare values of Diff, DiD and DDD for all
436 11 regime-changing presidential election-inauguration sequences from 1920 to 2018, separately for
437 the 6 elections won by Republicans, and the 5 elections won by Democrats. At every observation
438 period length, Diff is larger on average when Republicans win than when Democrats win. Indeed,
439 for 14 of these 15 comparisons of row (2) to row (3) of Table 3, the average values of Diff, DiD and
440 DDD obtained under Republican presidents exceeds the average value obtained under Democrats.
441 These results are consistent with the claim that exit timing of Republican appointees is more
442 influenced by the political party of the newly-elected president than exit timing of Democratic
443 appointees. We know of no previously-published hypotheses of party differences in PDH effects, so
444 we only note them, and wait for future research to properly test for and explain their existence.

445 **Enumeration Period Length Effects.** Results presented so far pertain to 270-day
446 observation periods (about 9 months) before regime-changing elections and after regime-changing
447 inaugurations. Row 1 of Table 3 summarizes results for periods of 180, 270, 365, 547 and 730 days
448 – about 6, 9, 12, 18, and 24 months – for Diff, DiD, and DDD.^{xi} As observation periods lengthen,

449 Table 3 shows that average values of Diff and DDD decline strictly monotonically. DiD declines
450 similarly, although its value in 1-year observation periods is larger than for the 9-month periods.
451 These patterns are consistent with the assertion that judges who wish to leave full-time service
452 honor principles of enduring ideology or party reciprocity, but only up to a point. That point seems
453 to be based on the time they must linger in full time jobs they wish to leave.

454 ***Amalgamated Results.*** Table 4 re-tabulates voluntary terminations in 270-day enumeration
455 periods, by concordance of the party of the presidential election winner with the party of the
456 appointing president, for all 11 regime-changing election-inauguration periods from 1920 to 2017.

457 --Insert Table 4 about here --

458 Although not a proper test of PDH, Table 4 is consistent with it: 225 judges appointed by presidents
459 of the same party as the recently elected president resigned or took senior status in these
460 enumeration periods, triggering new presidential appointments. Of these, 36.0 percent did so in the
461 pre-inauguration period, and, consistent with PDH, 1.8 times as many (64.0 percent) did so in the
462 post-election interval – a difference of 28.0 percent. For judges appointed by presidents of the
463 election-losing party, the corresponding difference is -7.6 percent, and the difference between these
464 differences is 35.6 percent, which is all consistent with PDH.

465 **Discussion and Conclusion.** This paper considers the politicized departure hypothesis, a
466 venerable but still controversial assertion that as Article III judges approach the ends of their
467 careers, they tend to adjust the timing of their departures so that the rights to name their
468 replacements are given to presidents of the same political party as the president who first appointed
469 them to the federal bench. Previous research on politicized departure is abundant, but questions
470 remain, we think, because, first, previous research gives little attention to judges of courts below
471 the Circuit Courts of Appeal, and, second, because judicial ethics and custom discourage judges
472 from providing information about their health, family circumstances, job attitudes, work

473 satisfaction, and similar things that have been shown to affect voluntary job termination and
474 retirement in the general population.

475 To escape the problems of unmeasured and unknown variables, and to expand coverage to
476 all Article III judges, we apply sharp regression discontinuity methods, with and without the
477 “difference in differences” estimator to the entire Article III judiciary. To apply SRD, we seek
478 situations in which the political party of the sitting U.S. president changes abruptly over a span of
479 time that is too short for retirement-related characteristics of judges to change much, if at all. We
480 observe that such situations occur repeatedly, shortly before regime changing presidential elections
481 and shortly after the presidential inaugurations that follow them. Our application of regression
482 discontinuity methods to the politicized departure hypothesis appears to be novel, but neither
483 regression discontinuity research nor potential outcomes methods are new (see Thistlethwaite and
484 Campbell, 1960; Holland 1986; Haavelmo 1943, 1944). As we compare periods just before regime
485 changing elections to periods of equal length immediately after those elections, we find, consistent
486 the politicized departure hypothesis, that Article III judges are more likely to retire when their
487 party’s candidate wins the election and sits in the White House, than in the pre-election period,
488 when the president is of the other party.

489 SRD, like other potential outcomes research designs, gains much of its power by a strategy
490 that is characteristic of scientific experiments, and very uncharacteristic of survey research: it
491 focuses on times and conditions in which treatment effects are apparent –even if those
492 circumstances are atypical – and ignores other circumstances altogether. When regime changing
493 presidential elections occur, the politicized departure hypothesis predicts more retirements in the
494 post-inauguration period than in the pre-election period before it, for judges who were first
495 appointed by presidents of the same party as the recently elected president. We report that
496 difference as Diff, as well as a difference-in-differences (DiD) estimator, and related quantities.
497 This SRD pseudo experiment is replicated 11 times between 1920 and 2018. For pre-election and

498 post-inauguration observation periods of 270 days, we find values of Diff and DiD that are
499 consistent with PDH in 10 of these 11 replications. Treating these 11 analyses as binomial trials
500 leads to rejection of the null hypothesis of no PDH effects. Less formally, results lend credence to
501 the PDH.

502 The clarity of SRD is valuable, but not costless. In particular, in the 98 years from 1920 to
503 2018, there have been 25 elections, of which less than half (11) are regime-changing and suitable
504 for the regression discontinuity method that we apply. Similarly, data and method used here do not
505 allow much partitioning of judges into subsets based on organizational, demographic or political
506 characteristics, so little can be said about, for example, differences or nondifferences between
507 SCOTUS justices, judges of the Circuit Courts of Appeals, and judges of district courts. Potential
508 outcomes analyses specific to the SCOTUS and the Circuit Courts of Appeals would require
509 methods more suited to small n's than those we apply here. It appears that some invention would be
510 needed to create those methods.

511 Although we did not hypothesize party differences before undertaking this research, we
512 observe stronger average gross PDH effects for Republican appointees than for Democratic
513 appointees. These effects and differences are gross, rather than adjusted, insofar as results for
514 Republican and Democratic appointees are measured at different times, and therefore, perhaps
515 under different conditions. Like any results not hypothesized in advance of their detection, these
516 differences are harder to distinguish from statistical noise than if they were predicted *a priori*. To
517 wit, one could as easily conjure a post hoc expectation of this finding as its opposite, or a finding of
518 no difference at all. For that reason, examination of party differences might require a different
519 method or different data than we use here. For example, it may be worthwhile for future research to
520 consider the hypothesis that Republican presidents are more likely than Democrats to appoint party
521 stalwarts, such as those who have run for public office as party candidates. Or one might
522 hypothesize that party differences in this judicial behavior are the result of party differences in

523 grooming and systematic persuasion after judges take office. To wit, Teles (2008: chapter 1) offers
524 a model of judicial influence in which presidential nomination is a mere first step in a diffuse,
525 ongoing, career-long and fully institutionalized pattern of effort by ideologues and commercial
526 interests to influence the perceptions and decisions of federal judges.

527 Finally, it seems important to revisit the fundamental question that motivates tests of PDH:
528 Are Article III judges influenced by politics while in office? The politicized beginnings of Article
529 III judicial careers are apparent from the nominations of these judges by the politicians who serve as
530 elected presidents, and their confirmations by politicians who serve as elected Senators. But PDH
531 suggests that judges themselves tend to behave politically at the final moment of their full-time
532 courtroom careers, without discernible incentives, financial or otherwise, long after their
533 confirmation hearings. If it is apparent that judicial careers are politically vetted at their start, and if
534 sharp regression discontinuity analysis of objective data indicates that judges tend to act politically
535 at career end, then we think there is reason to believe that politics has been an active influence on
536 many of these judges in the interim.

537

538 **Figure 1 –Simplified Nonparametric Regression Discontinuity Design for Analyses of Judicial Trigger Actions, when**
 539 **Republican President is incumbent before election and Democrat is inaugurated after election**
 540

Subjects	Time Period Before Election and After Inauguration (2008 Election, 2009 Inauguration, 270 day Enumeration Period)			Tests of Hypotheses
	Pre-Election Year (270 days before Election) <i>Republican Incumbent</i>	Election-to- Inauguration Interval	Post-Inaugural Year (270 days after Inauguration) <i>Democratic President Elect</i>	
Treated Judges Democratic Appointees (i.e. Nominated by Democratic Presidents)	Group A $\sum^d Y_{bRD}$ = Trigger Actions by Democratic appointees in 270 days <i>before election</i> , when president before election is Republican and election winner is Democrat	Time → 75 days	Group B $\sum^d Y_{aRD}$ = Trigger Actions by Democratic appointees in 270 days <i>after inauguration</i> , when president before election is Republican and election winner is Democrat	Hypothesis 1 “Difference Hypothesis” Group B Compared to Group A $(\sum^d Y_{aRD} - \sum^d Y_{bRD}) / N_d > 0$
Not Treated Judges Republican Appointees (i.e. Nominated by Republican Presidents)	Group C Not Treated (Hypothesis 2) Treated (Hypothesis 3) $\sum^r Y_{bRD}$ = Trigger Actions by Republican appointees in 270 days <i>before election</i> , when president before election is Republican and election winner is Democrat		Group D Treated (Hypothesis 2) Not Treated (Hypothesis 3) $\sum^r Y_{aRD}$ = Trigger Actions by Republican appointees in 270 days <i>after election</i> , when president before election is Republican and election winner is Democrat	Hypothesis 2 “Difference in Differences” Hypothesis” $(\sum^d Y_{aRD} - \sum^d Y_{bRD}) / N_d$ $-(\sum^r Y_{bRD} - \sum^r Y_{aRD}) / N_r > 0$ Hypothesis 3 “Directional Diff in Diff” $(\sum^d Y_{aRD} - \sum^d Y_{bRD}) / N_d$ $-(\sum^r Y_{aRD} - \sum^r Y_{bRD}) / N_r > 0$

541
 542 Notes:

- 543 1 Outcomes are “Trigger Actions:” retirements, resignations, and senior service accessions that
 544 trigger new appointments
 545 2 Excluded are a) any who die before taking a trigger action before the end of the 270 days post
 546 inauguration period; b) any who are removed from office for “bad behavior,” or whose
 547 departure occurs after the election but before inauguration of the new president.
 548 3 This research design illustrates the case when party of president prior to election is
 549 Republican, and the Democratic candidate wins election.
 550 4. If the incumbent before the election is Democratic and the election winner is Republican, then
 551 the top row of this table would pertain to Republican appointees, rather than Democratic
 552 appointees shown, and the bottom row would pertain to Democratic appointees, rather than
 553 Republican appointees as shown.
 554

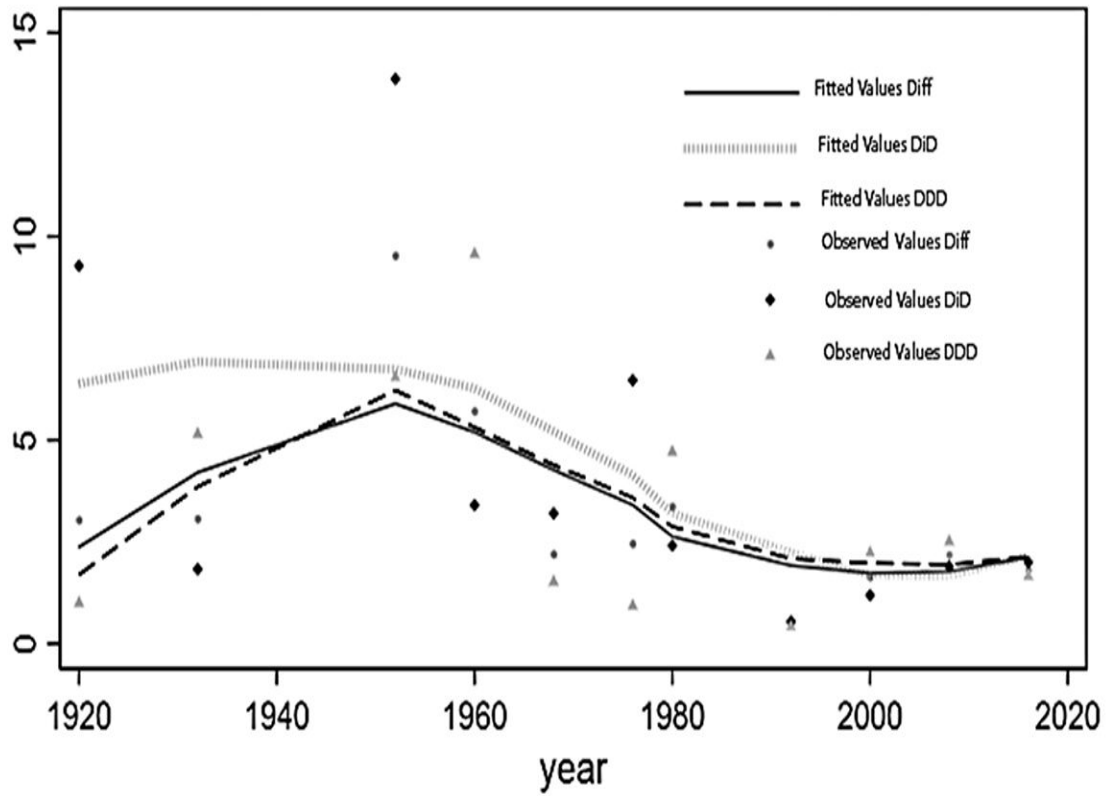


Figure 2 – Diff, DiD and DDD, by Election Year, for 270 Day Enumeration Periods, with Values Smoothed by Cleveland’s Robust Locally Weighted Regression (Bandwidth=0.9)

Table 1 – Symbols and Definitions

Symbol	Definition
Y_i	The outcome for the i^{th} subject. $Y_i = 1$ if the subject takes a trigger action, and $Y_i = 0$ else
${}^d Y_{bRD}$	Count of outcomes for subjects in a specified subgroup of subjects. Pre-superscript (d or r) indicates party (Democratic or Republican, respectively) of president who nominated judge to the federal bench. First post-subscript indicates if Y is measured before (b) or after (a) election. Post-subscripts R and D indicate parties of presidents before election and after election, in that order: RD indicates Republican president before election and Democratic president after election. DR indicates Democratic president before election, followed by Republican. Regression discontinuity occurs only when a presidential election changes the party of the incumbent president, so RR and DD would not occur in analyzed data.
τ	The estimand for testing the first hypothesis is the average treatment effect at the point of discontinuity: $\tau = E[Y_i(1) - Y_i(0) \mid X_i = c]$ For Democratic appointees before and after a Democratic victory in a regime-changing election $\tau = E[({}^d Y_{aRD} - {}^d Y_{bRD})]$ For Republican appointees before and after a Republican victory in a regime-changing election $\tau = E[({}^r Y_{aDR} - {}^r Y_{bDR})]$
where	
$Y_i(1)$	The outcome Y for the i^{th} subject, when treated (1)
$Y_i(0)$	The outcome Y for the i^{th} subject, when not treated (0)
E	The expectation operator, so that $E[Y(1)]$ is the expected value of Y for the treatment group and $E[Y(0)]$ is the expected value of Y for the control group.
X_i	A covariate that determines if subject is assigned to treatment or control. $X_i = 1$ indicates treatment. $X_i = 0$ indicates control.
c	The value of X that determines membership in the control or treatment group
N_d	The number of subjects appointed by Democratic presidents
N_r	The number of subjects appointed by Republican presidents
${}^d O_{aRD}$	The odds of a trigger action, with super- and sub-scripts as defined above.

Table 2 – Analyses of Trigger Actions 270 days before 11 Regime-changing Presidential Elections and 270 days after Subsequent Inaugurations

Election Year	Presidential Election Winner	DIFF		DiD		DDD		N Republican Appointees	N Democratic Appointees
		Winner's Odds Post/ Winner's Odds Pre	DIFF Odds Ratio >1	DIFF in DIFF Winner Diff / Loser Diff	DiD Odds Ratio >1	Directional DIFF in DIFF winner DIFF / (1/loser DIFF)	DDD Odds Ratio >1		
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1920	Republican	3.04	+	9.28	+	1.00		69	60
1932	Democratic	3.07	+	1.83	+	5.15	+	174	41
1952	Republican	9.53	+	13.87	+	6.54	+	72	246
1960	Democratic	5.71	+	3.40	+	9.57	+	160	205
1968	Republican	2.20	+	3.20	+	1.52	+	152	346
1976	Democratic	2.46	+	6.47	+	0.93		330	304
1980	Republican	3.37	+	2.41	+	4.71	+	331	314
1992	Democratic	0.47		0.54		0.42		620	371
2000	Republican	1.63	+	1.19	+	2.23	+	626	539
2008	Democratic	2.18	+	1.90	+	2.51	+	755	499
2016	Republican	1.82	+	2.00	+	1.66	+	667	676
	Mean	3.23		4.19		3.30		359.6	327.4
	Count > 1		10		10		8		

Source: Computed by author from data downloaded from the Federal Judicial Center (n.d.a.) on April 28, 2018, and subsequently corrected by author.

Notes: In columns (4), (6) and (8), “+” indicates that the odds ratio is greater than 1; a blank indicates that the relevant odds ratio does not exceed 1. In Columns (3) (5) and (7), “Winner’s Odds” are the odds of a trigger action by judges first appointed by a president of the same party as the presidential election winner.

Table 3 – Results of 55 Election- and Inauguration-Specific Analyses of Trigger Actions, by Parties of Appointing President and Election Winner

Election/Inauguration Years and Aggregation Method	Diff Odds Ratio by Length of Pre-Election and Post- Inauguration Periods: Mean of Diff and (Number of Analyses for Which Diff > 1)					Diff in Diff (DiD) Odds Ratio by Length of Pre-Election and Post- Inauguration Periods: Mean of DiD and (Number of Analyses for Which DiD > 1)					Directional Diff in Diff (DDD) Odds Ratio by Length of Pre-Election and Post- Inauguration Periods: Mean of DDD and (Number of Analyses for Which DDD > 1)				
	180	270	365	547	730	180	270	365	547	730	180	270	365	547	730
	<u>days</u>	<u>days</u>	<u>days</u>	<u>days*</u>	<u>days*</u>	<u>days</u>	<u>days</u>	<u>days</u>	<u>days*</u>	<u>days*</u>	<u>days</u>	<u>days</u>	<u>days</u>	<u>days*</u>	<u>days*</u>
(1) 11 Presidential Regime-changing Elections/Inaugurations 1920-2016	4.19 (8)	3.23 (10)	3.12 (9)	2.12 (8)	1.91 (8)	4.68 (9)	4.19 (10)	4.37 (8)	3.94 (9)	2.66 (10)	4.35 (8)	3.30 (8)	2.58 (7)	1.54 (5)	1.49 (7)
(2) 6 Presidential Regime-changing Elections/Inaugurations won by Republicans 1920-2016:	5.88 (5)	3.60 (6)	3.52 (6)	2.34 (5)	2.14 (5)	6.41 (5)	5.33 (6)	5.11 (5)	4.93 (6)	2.89 (6)	5.97 (5)	2.94 (5)	2.98 (5)	1.72 (3)	1.75 (5)
(3) 5 Presidential Regime-changing Elections/Inaugurations won by Democrats 1920-2016	2.16 (3)	2.78 (4)	2.64 (3)	1.84 (2)	1.64 (3)	2.61 (4)	2.83 (4)	3.48 (3)	2.75 (3)	2.39 (4)	2.40 (3)	3.72 (3)	2.12 (2)	1.32 (2)	1.19 (2)

Note: 547 and 730 Day Post-Inauguration Enumeration Periods for 2016 Election are Truncated to 536 days

Table 4 – Consolidated Counts of Trigger Actions, 1920-2018, by Concordance of Party of Appointing President and Party of Election Winner, 270 days before and after 11 regime changing elections

Party of Election Winner and Party of Appointing President		When Trigger Action is Taken		Total
		Pre-election (a)	Post-inauguration (b)	
(1) Judges Appointed by president of same party as election victor	%	36.0%	64.0%	100.0%
	n	81	144	225
(2) Judges Appointed by president of same party as election loser	%	53.8%	46.2%	100.0%
	n	78	67	145
Total	n	159	211	370

Notes: The number of judges varies from 1920 to 2017.

See text for explanation of use of counts in this table rather than odds, probabilities and proportions.

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Appendix

Factor Analysis of Ideology and Political Measures for 31 Supreme Court justices, 1960-2018

I. Analysis Results

Table A1 -- Pearson and Point Biserial Correlations Among Ideology Scores and Party of Appointing President

Measure	Correlations			11 Democratic Appointees		20 Republican Appointees	
	Bailey Score	JCS Score	MQ Score	mean	s.d.	mean	s.d.
Bailey Score	1	.979	.963	-.784	.758	.337	.850
JCS Score	.979	1	.970	-.254	.347	.245	.412
MQ Score	.963	.970	1	-1.350	1.550	.639	1.644
Republican Appointee*	.656	.611	.636	0	n/a	1	n/a

Note:

* Point biserial correlations in this row.

Table A2 – Principal Components Factor Analysis (two factors retained)

Factor	Eigenvalue	Difference	Proportion
Factor1	3.324	3.268	0.994
Factor2	0.056	0.064	0.017

Table A3 -- Factor loadings

Variable	Loading on	
	Factor 1	Factor 2
Bailey2020	0.988	0.015
JCS2020	0.983	-0.123
MQ2019	0.976	-0.026
Republican Appointee	0.654	0.200

Table A4 – Lifetime Average Ideology Scores and Party of First Appointing President of Supreme Court Justices, 1960-2018

<u>Last Name</u>	<u>Bailey Score</u>	<u>JCS Score</u>	<u>MQ Score</u>	<u>Republican Appointee</u>
Alito	1.13	0.58	1.82	1
Black	-1.61	-0.42	-1.76	0
Blackmun	-0.07	0.07	-0.03	1
Brennan	-1.13	-0.44	-1.78	1
Breyer	-0.65	-0.33	-1.23	0
Burger	0.94	0.59	1.89	1
Clark	0.23	0.26	0.46	0
Douglas	-1.90	-0.71	-4.72	0
Fortas	-1.09	-0.37	-1.33	0
Frankfurter	0.34	0.26	0.52	0
Ginsburg	-0.93	-0.43	-1.73	0
Goldberg	-0.97	-0.29	-1.08	0
Gorsuch	1.02	0.42	0.98	1
Harlan	0.77	0.53	1.62	1
Kagan	-0.72	-0.43	-1.58	0
Kavanaugh	0.77	0.30	0.54	1
Kennedy	0.42	0.33	0.68	1
Marshall	-1.47	-0.58	-2.83	0
O'Connor	0.56	0.41	1.01	1
Powell	0.49	0.42	0.97	1
Rehnquist	1.37	0.68	2.97	1
Roberts	0.65	0.39	0.93	1
Scalia	1.18	0.66	2.51	1
Sotomayor	-1.17	-0.61	-2.68	1
Souter	-0.52	-0.17	-0.77	1
Stevens	-1.02	-0.39	-1.81	1
Stewart	0.15	0.25	0.40	1
Thomas	1.45	0.75	3.60	1
Warren	-0.85	-0.34	-1.26	1
White	0.16	0.25	0.44	0
Whittaker	0.59	0.46	1.17	1

Data Sources:

JCS Scores: Epstein. Lee. 2021. “The Judicial Common Space.” Data downloaded Sept.20, 2021. <https://www.epstein.wustl.edu/jcs>

Bailey Scores: Bailey, Michael A. 2021. “Bridge Ideal Points.” Data downloaded Sept. 20, 2021. <https://michaelbailey.georgetown.domains/bridge-ideal-points-2020/>

MQ Scores: University of Michigan. 2021. “Martin-Quinn Scores: Measures.” Data downloaded Sept. 20, 2021. <https://mqscores.lsa.umich.edu/measures.php>.

Biographical information, dates of service, and party of first appointing president: Data downloaded Sept. 20, 2021

https://www.supremecourt.gov/about/members_text.aspx

NOTES

ⁱ “There are currently four Article III courts: The Supreme Court of the United States, the U.S. courts of appeals, the U.S. district courts and the U.S. Court of International Trade. Congress has abolished, combined or reorganized several other Article III courts over time.” Notable exclusions are Article I courts, including military courts martial tribunals and administrative law courts. Judges of Article I courts lack life tenure and compensation protections of Article III judges. (<https://www.fjc.gov/history/courts/courts-brief-overview>)

ⁱⁱ In using the word, “ideology,” we follow prior, related judicial and political research (e.g. Pinello 1999, Bonica et al. 2019; Segal and Cover 1989; Wetstein et al. 2009; Epstein et al. 2007). This usage of “ideology” neither requires nor prohibits a fully-elaborated, institutionalized political ideology as described in sociological treatments, e.g. Wright (1997), Althusser (2014), and Bell (1960).

ⁱⁱⁱ Ideal point measures differ from commonplace dominance scale measures, e.g. Likert scale measures, that range monotonically from low to high scale values (e.g. from “not at all liberal” to “very liberal”) (see Tay and Ng 2018). In contrast, ideal point measures recognize that respondents may prefer moderation (e.g. slightly liberal or slightly conservative) to extremes (e.g. staunchly conservative or extremely liberal), just as they might react with equal distaste to overcooked and undercooked food. Ideal point and dominance scale measures of the same phenomenon can, but need not, be negatively correlated for certain ranges of scale values.

^{iv} Related analyses include Chabot’s (2019) JCS-based examination of Supreme Court departures, Zigerell’s (2013) use of Bailey’s ideological scores, Bonica and Sen (2017), Segal et al. (2011), Farnsworth (2007) and Spruk and Kovac (2019). Some effort has been made to apply

MQ methods to judges beyond the Supreme Court (Spruk and Kovac 2019), but judicial ideology measures are generally unavailable for judges of federal district courts, who comprise the bulk of Article III judges (but see Bonica et al 2017).

^v A reader of this paper insightfully suggest that party effects might differ noticeably from ideology effects (and therefore become visible) when political parties move suddenly and significantly to the right or left. We think that those effects also might be revealed if individual judges change their ideologies after appointment. But parties do not appear to shift their ideologies often, and ideological shifts by individual lower court judges are as yet unmeasured. Analysis of changes in political party ideologies and attitudes of individual judges would be useful, if they could be done well, but they would also be a diversion from present concerns with PDH. More generally, it appears that small ideological differences between individuals might have measurable effects on their chances of presidential nominations to the judiciary, because presidents have many individuals to choose among. However, when judges consider retirement and resignation, they can, at most, choose only between a Republican or a Democratic president, and making minor adjustments in party ideology seems unlikely to have much effect on the party identification of judges.

^{vi} In passing, this paper neither accepts nor rejects any claim of causal relationships between party and ideology.

^{vii} By law since 1845, elections occur on the Tuesday between November 2 and November 8, inclusive (*An Act to Establish a Uniform Time for Holding Elections 1845*). In 1921, 1929 and 1933 inaugurations occurred on March 4, following elections by about four months (e.g. 122 days in 1922). After 1933 (starting in 1953) inaugurations occur between 73

and 78 days later, on January 20, per the 20th Amendment to the U.S. Constitution, passed in 1933.

^{viii} A thoughtful reader asks if the method used here is transformed from a sharp to a soft regression discontinuity model when the outcomes of some future elections are prognosticated in advance. We respond that “sharpness” here is produced by the transfer of power to nominate judges, which occurs definitively as the newly elected president completes recitation of the oath of office. Well-known examples of unsuccessful presidential aspirants who were widely predicted to win include Thomas E. Dewey, Albert Gore, and Hilary Clinton. More generally, the popular presidential election vote is often close, and therefore seemingly difficult to predict. To wit, in the 11 regime changing elections considered here, 5 were won by a candidate who received less than 50 percent of the popular vote, 7 were won by a candidate who received less than 51 percent of the popular vote, and only Harding, Roosevelt and Eisenhower took more than 55 percent of the popular vote (source: <https://www.archives.gov/electoral-college/results> accessed 10 24 2021).

^{ix} Two judges were excluded from analyses reported here because they served less than 730 days before terminating service: Lindley Beckworth resigned in 1968 after 546 days in office, and George Mitchell resigned in 1980 after 224 days in office. Both were first appointed by Democratic presidents. Mitchell resigned his judicial post for an appointment to fill a vacant seat in the U.S. Senate, where he served until 1994. More generally, from 1790 through 2016, 42 judges terminated judicial service with less than 730 days of service. Of these, 14 terminated due to abolition of the court on which they served, one retired and 27 resigned. After 1919, one judge retired and 11 resigned after less than 730 days of service.

^x The Supreme Court decision of *Bush v. Gore* in December, 2000 might be an exception to this claim, except that no Supreme Court justices left office just before or after this case came to the Supreme Court.

^{xi} Because data processing for this research began in 2018, 536 days after the 2017 inauguration, the 547 and 730 day post-inauguration enumeration periods for the 2016 election are truncated to 536 days, and results for them are not comparable to results for the same enumeration periods in earlier elections.