

Republican-Majority Appellate Panels Increase Execution Rates for Capital Defendants

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Abstract: We use the quasi-random assignment of cases to three-judge panels on the US Courts of Appeals to assess the consistency of adjudication of death penalty appeals. We find clear evidence that panels apply different standards depending on whether a majority of the panel was appointed by Democratic or Republican presidents. Unlike previous work on panel effects in the US Courts of Appeals, we show that these effects persist to the end of the process of adjudication. Since the the early 1980s, the probability of ultimate execution has been increased for inmates when their first Court of Appeals case was assigned to a panel with a majority of Republican appointees.

Keywords: Judicial Politics; Death Penalty; US Courts of Appeals

Supplementary material for this article is available in an online appendix.

Replication files are available in the JOP Data Archive on Dataverse (<https://doi.org/10.7910/DVN/CMCZBR>).

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Capital punishment is the most punitive and irreversible form of judicial sanction. As a result, it is clear that its application must meet the very highest standards of fairness and justice. Indeed, it was out of a concern about unfair application of the death penalty that the US Supreme Court, in *Furman v. Georgia* (1972), struck down all death penalty statutes in the country. In a concurring opinion in that case, Justice Stewart wrote, “These death sentences are cruel and unusual in the same way that being struck by lightning is cruel and unusual. I simply conclude that the Eighth and Fourteenth Amendments cannot tolerate the infliction of a sentence of death under legal systems that permit this unique penalty to be so wantonly and so freakishly imposed.” When, just four years later, in *Gregg v. Georgia* (1976), the Supreme Court approved of newly written statutes governing sentencing procedures, the majority specifically argued that the new statutes addressed the problem of arbitrariness in death penalty sentencing.

There remains considerable disagreement as to whether these and subsequent reforms in capital sentencing indeed established a fair, even, and consistent application of the death penalty. As Baumgartner et al. (2018) argue, the spirit of the Court’s decisions validating new death penalty statutes in 1976 included the proposition that the imposition of the death penalty cannot be arbitrary or random. But recent research suggests juries and trials introduce wide variance in outcomes, including as a function of race (Anwar, Bayer, and Hjalmarsson 2012; Alesina and Ferrara 2014; Spurr 2002). Moreover, nearly all death penalty cases enter a long appeals process after the initial conviction and sentencing phase, which leads to a lengthy period of review (see Gelman et al. 2004). Of the 6,000 individuals sentenced to death between 1973 and 1995, only 5% had been executed by 1995. Most of the remaining 95% of the sentences were either overturned or under continued appellate review. This appellate process was frequently characterized by claims that were very unlikely to prevail, and as a consequence Congress passed, in 1996, the Antiterrorism and Effective Death Penalty Act (AEDPA), which contained procedural hurdles including a one-year statute of limitations period for seeking habeas corpus, severely restricting the ability to file a second or successive petition for a writ of habeas corpus, and making it more difficult to meet the

standards for a writ.

We examine one aspect of this capital sentence appellate process: the US Courts of Appeals, which, due to the limited number of cases heard by the US Supreme Court, is usually the court of last resort for those who face execution. Generally speaking, death penalty cases reach the Courts of Appeals after the convicted defendant has exhausted all state-level appeals. Appellate decisions are almost always made by a panel of three judges selected from the pool of judges in the circuit.¹ These courts have the opportunity to correct for error and inconsistency that has occurred previously, but also potentially to introduce inconsistency themselves. Previous work has demonstrated that the existence of three judge panels reduces inconsistency in death penalty cases versus a hypothetical alternative of single-judge decisions in the Courts of Appeals (Beim and Kastlelec 2014; Fischman 2015).² The deliberative process of collegial decision making on these courts may help promote a uniform standard across the cases heard in a circuit. However, even such influence is not necessarily sufficient to achieve a uniform standard across a circuit if judges' pre-deliberation standards vary widely, because randomly assigned three-judge panels will frequently group judges with similar views together. Although we have an understanding of the consequences of judges' individual ideology on criminal justice outcomes (see e.g. Huber and Gordon 2004; Cohen and Yang 2018) and on defendants' long-run outcomes (see e.g. Aizer and Doyle 2015); and a notion of panel effects influence how a case is ultimately resolved (Hall 2009), we do not have a firm understanding of the reduced-form, ultimate consequences of panel composition.

In this paper, we quantify inconsistency in the implementation of the death penalty due to panel composition—*both in case outcomes and in actual executions*. Consistent with a generation of research on the relationship between partisan appointment, judicial ideology and decision making, we show predictable variation in the standards judges apply to death

¹The US Courts of Appeals are arranged into 12 geographically-defined circuits. Active judges from the relevant circuit are the primary pool for any case; however, judges from US District Courts, from other circuits of the U.S. Courts of Appeals, and retired judges from the circuit sometimes sit “by designation” as one of the three judges.

²Though see also Sunstein et al. (2006), which argues that judges do not influence one another's decision-making in death penalty cases.

penalty cases. Panels with a majority of Democratic-appointed judges grant relief from a death sentence more often; panels with a majority of Republican-appointed judges grant relief less often. While the effects for execution are smaller than for relief (due to the remaining oversight mechanisms of en banc review and the Supreme Court) we nonetheless find evidence that panel composition predicts execution. This finding calls into question the extent to which the American legal system is meeting the standard that the Supreme Court has set out for the death penalty's conformity to the Eighth Amendment's prohibition on cruel and unusual punishment.

IDENTIFICATION, DATA AND METHODS

We estimate the consequences of panel composition on decisions and executions by comparing cases that were assigned to different judges. Like much previous research, our study relies on the assumption that as-if random assignment of cases to panels holds for these cases. Internal assignment rules vary by circuit, but while there is evidence that panels are not randomly constructed from the set of possible judges given the area of law (Hall 2010; Levy 2017), there is no evidence that cases are systematically assigned to judges in a way that relates to their relative merits within that area of law. It is not a problem for our identification strategy if panels themselves are not randomly constructed, so long as cases are assigned to those panels without respect to the facts of those cases. The most serious threat to our inferential strategy would be if cases are assigned in a way that causes some judges to get systematically weaker cases than other judges, because those judges would spuriously appear more conservative by virtue of more frequently denying relief. In the domain of death penalty cases, previous research has indicated that Republican and Democratic appointees are equally likely to see defendants who won at the district court level (Beim and Kastellec 2014).

As-if-random assignment only plausibly holds for the first case involving a given death-row inmate before the Courts of Appeals, and only when comparing such cases heard in the same year in the same circuit. Some inmates have multiple cases heard over a period

of years, but whether further cases are heard for the same inmate can be an outcome of the initial case, panel assignment is not independent across these cases, and the strength of such cases are unlikely to be independent either. Different circuits have different mixes of cases coming up from their constituent states and also different mixes of Democratic- and Republican-appointed judges. Within each circuit, the mix of cases and the mix of appointed judges potentially change over time. Further, the Supreme Court precedents that the Courts of Appeals apply are also changing over time. Taking these points together, among the set of first appearance cases considered at the same time by the same appellate court, the expected strength of the cases heard by any possible three-judge panel is the same. This means that we must analyze each Circuit separately and also adjust for the average “case strength” at any given moment in time in order to isolate the causal effect of the panel assignment.

Existing databases of death penalty appeals did not link all the information we required for our analysis. We performed an over-inclusive Westlaw search for all cases that could be a case from a death row inmate before any circuit in the US Court of Appeals following the procedures in Fischman (2015) and Beim and Kastellec (2014). This procedure yielded more than 20,000 cases between 1983 and 2012. Each case was then read, individually, by a member of our research team that comprised law students, graduate students in political science, and undergraduate students. Each case was assessed for whether it was a death penalty case. The vast majority of the cases in our data are initial federal habeas petitions. As such, these cases typically cover issues such as (a) prosecutorial misconduct; (b) improprieties related to the jury; (c) ineffective assistance of counsel; and (d) constitutional challenges to the death penalty, such as claims the defendant is ineligible for execution. We retained each case decided by a three-judge panel (i.e., we exclude cases decided *en banc*) and recorded a number of pieces of information, which we describe below.³ As a result, our data includes those individuals who appeared with death penalty-related cases for the first

³We then further examined all cases identified by a similar search decided between July 2, 1976 (when the Supreme Court decided *Gregg v. Georgia* 428 U.S. 153) and December 31, 1982, to see if any of our death penalty defendants had a case in the Court of Appeals prior to 1983. If they did, we excluded them from our data as their first appearance in our data set was not their first appearance at the Court of Appeals.

time before the Courts of Appeals between 1 January 1983 and 31 December 2012.⁴

Because some states sentence far more people to death than others and circuits are organized geographically, cases are very unevenly distributed across circuits. The D.C. Circuit, and the First, Second and Third circuits (covering the mid-Atlantic through New England) yield too few cases for us to study. Therefore, we focus on the eight circuits—the Fourth through Eleventh, inclusive—which handle nearly all death penalty cases. The resulting data include 1,991 initial death penalty cases decided in the Court of Appeals between 1983 and 2012. For each case, we recorded the judges on the panel, how each judge voted (whether to support any relief at all for the defendant), and whether the decision supported any relief at all for the defendant.

The first outcome of interest is the panel’s immediate decision to grant or deny relief to a death-row prisoner. This allows us to focus on the reduced-form effects of inconsistency, knowing that suppressed dissents, bargaining, and other inter-judge dynamics may influence the decisions that we observe. The second outcome of interest is whether the death-row prisoner is ultimately executed. We used the list of executions maintained by the Death Penalty Information Center (<https://deathpenaltyinfo.org/>) current as of 2017, which we verified by cross-referencing against states’ websites listing executions. We then matched each execution to an inmate appearing in our data. Inmates who were granted relief are not necessarily removed from death row, and inmates granted relief may still ultimately be executed. For example, “relief” may take the form of a remand but the outcome of the remand may once again be the death penalty. Nevertheless, whether an inmate is granted relief in their initial appeals court case and whether they are ultimately executed are highly correlated outcomes. In most circuits, the relief denial in the initial panel decision is associated with a roughly 30 percentage point difference in ultimate execution rates.⁵

⁴Our data exclude all cases in which a person was sentenced to death if that sentence was commuted prior to the appeal. Our data also exclude cases brought by next friends and cases in which the person on death row does not seek relief (such as cases about prison conditions).

⁵Appendix Table A1 reports the proportion of inmates ultimately executed in each circuit (as of 2017) divided into those who were granted relief at their first appeal before the Court of Appeals and those who were not. The baseline rates at which inmates are granted relief and the rates at which they are executed

Our analysis focuses on a reduced form identification of the causal effect of having different panel compositions on grants of relief y_j and ultimate execution z_j in cases j . This approach has the advantage of generating comparable estimates of the causal effect of panel composition on both the relief and the execution outcomes. The latter effect is likely to run partly, but not entirely, through the immediate decision of the panel on whether to grant relief. However this is not the only causal pathway because the same panel is more likely to hear subsequent appeals from that defendant and the identities of the assigned judges may influence the subsequent oversight process via en banc and/or Supreme Court review. It is the very complexity of this subsequent process that makes the reduced form analysis attractive: it allows us to estimate whether there is a causal effect of initial panel assignment on execution, regardless of the relative importance of the various causal pathways through which that effect could arise.

Our primary treatment variable is whether a panel has a majority of Democratic-appointed judges $T_j = 0$ or a majority of Republican-appointed judges $T_j = 1$. The key identifying assumption is that this treatment assignment is as-if-randomly assigned. As discussed above, this assumption is only plausible conditional on circuit and date. Therefore, we estimate regression models separately for each circuit with year fixed effects. Year fixed effects provide the most credible causal estimates, as they allow for potentially different baseline rates of grant denial and ultimate execution for cases that first reached the Courts of Appeals in each individual year. 89% of cases across all circuits are in a circuit-year where there is at least one Democratic majority case and at least one Republican majority case.

Our analysis only estimates variation in decision-making that is a function of the treatment variable. This means that we cannot detect inconsistency that is associated with “within-party” variation in the standards judges apply. The analysis also does not take a position on the mechanism creating partisan differences, although we note that past research has voluminously documented that Democratic and Republican appointees hold different views on the application of constitutional law, both in general and regarding the death

vary over the period we study within and across circuits (see Appendix Figure A1).

penalty. We are estimating an average treatment effect in each circuit, but the treatment effect may itself vary over the period under study. These limitations cannot lead us to overestimate inconsistency in decision-making. However, if judges vary within appointing party (which they surely do) or over time (which they likely do) that implies greater inconsistency in decisions than we estimate, albeit inconsistency that is not straightforwardly associated with whether there is a Democratic- or Republican-appointed majority on the panel.

In the online appendix, we present additional checks and alternative analyses. First, we report intercoder reliability statistics. Second, we show that the state from which each case arose is not systematically associated with having a Republican majority once we condition on circuit and year. Third, we show that the surname-predicted race of the inmates (Imai and Khanna 2016) do not predict receiving a Republican majority panel once we condition on circuit and year. Fourth, we show results of an analysis using four treatment levels, corresponding to 0, 1, 2, or 3 Republican-appointed judges on the panel, and find that differences in outcomes as a function of majority party (0 or 1 versus 2 or 3) are far larger than the differences between homogenous and heterogenous panels with the same majority party (0 versus 1 or 2 versus 3). Fifth, we split the analysis according to the party of appointment of the Chief Judge of the circuit in the year that the case was decided (the Chief Judge nominally administers the process of assigning cases to panels; Hettinger, Lindquist, and Martinek 2003) and find negligible differences. Sixth, we discuss in further detail why it is substantively implausible to think that there is a mechanism that systematically assigns stronger appeals to Democratic majority panels, particularly if it were really the case that there was no causal effect of panel majority on the decision-making.

RESULTS AND DISCUSSION

Table 1 shows the estimated effect of a change from a Democratic-appointed panel majority to a Republican-appointed panel majority on the probability of relief denial and execution in each of the circuits we consider. Republican-majority panels are associated with higher relief denial rates and higher probability of ultimate execution in most circuits. In the fixed

Outcome	Year FE	Circuit								Avg. Effect	Total Effect	95% CI
		4	5	6	7	8	9	10	11			
Relief Denial	No	0.14 (0.04)	0.07 (0.04)	0.30 (0.07)	0.23 (0.14)	0.12 (0.07)	-0.01 (0.08)	0.03 (0.07)	0.16 (0.04)	0.12 (0.02)	232 (41)	152-313
	Yes	0.18 (0.05)	0.09 (0.04)	0.31 (0.07)	0.19 (0.15)	0.17 (0.08)	0.00 (0.10)	0.04 (0.08)	0.08 (0.04)	0.12 (0.02)	235 (44)	149-322
Execution	No	0.08 (0.06)	0.12 (0.05)	0.12 (0.07)	0.13 (0.16)	0.10 (0.09)	0.00 (0.07)	0.07 (0.08)	0.04 (0.05)	0.09 (0.02)	169 (48)	76-263
	Yes	0.02 (0.07)	0.14 (0.05)	0.17 (0.07)	-0.07 (0.16)	0.12 (0.10)	-0.12 (0.09)	0.03 (0.09)	0.05 (0.05)	0.07 (0.03)	143 (52)	42-244
N		244	600	168	78	188	152	159	402	1991	1991	

Table 1: *Estimated treatment effects in each circuit for a change from a Democratic majority to a Republican majority panel, as differences in proportion of negative outcome for the inmate. The total effect is the implied difference in the total number of negative case outcomes over all circuits. Standard errors in parenthesis.*

effects model the circuit-level point estimates vary from a 0.00 to a 0.31 higher probability of a relief denial with a Republican-appointed majority versus a Democrat-appointed majority panel. For execution, the estimates range from -0.12 to 0.17. Each of these circuit-specific estimates are relatively imprecise, but they are independent from one another and so the collective evidence they provide about the full set of circuits is much stronger. If we weight the circuit-level estimates by the number of cases decided in those circuits, we can construct an estimate of the average treatment effect of going from a Democratic-appointed majority to a Republican-appointed majority, for all death penalty cases in our data set. For relief denial, the average treatment effect is 0.12; for execution, it is 0.07.⁶ These average treatment effects of 12 and 7 percentage points correspond to 235 additional relief denials (95%: 149-322) and 143 additional executions (95%: 42-244) out of 1991 total cases in a world where all panels had Republican majorities versus a world where all panels had Democratic majorities. The p-value for the null hypothesis that there was no average treatment effect on switching between Democratic and Republican majority panels across all circuits is 0.003 for executions and negligible for relief denial.

⁶In the online appendix, we show that the circuit-level estimates are largely consistent (given their estimation precision) with the average effect. Nonetheless, it is very likely that treatment effects are heterogenous across circuits and across time, and we know that appeals have faced different probabilities of having panels with Republican or Democratic majorities across circuits and across time.

Because the death penalty entails such a high degree of punitiveness and irreversibility, its exercise requires the highest level of legal scrutiny to ensure its application does not violate individual rights. In the US, the Supreme Court has held that the constitutional prohibition on cruel and unusual punishment requires the death penalty not be administered randomly or in a way affected by factors that are orthogonal to the legal merits of the crime and defendant. Our analysis demonstrates two findings that call into question the extent to which the death penalty is administered to this standard. First, we find that judges apply different standards which correlate with partisanship in deciding cases that are, in expectation, equally strong on the merits. Second, we show a causal effect of the partisan composition of Appeals Court panels persists for the ultimate fate of the litigants coming before the court. If the identity of the judges influenced how panels decided cases but the institutions of judicial oversight remedied this variation before execution, one could argue that such variation is of limited normative concern. What we find instead is that being randomly assigned to differently composed panels has a causal effect on whether or not an individual lives or dies.

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Online Appendix for Republican-Majority Appellate Panels Increase Execution Rates for Capital Defendants

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Intercoder Reliability

Of the total set of cases that we considered, the decision of the Appeals Court panel were coded twice for 1389 of 1991 cases, once by the authors for a previous version of the data set, and again by the research assistants as described in the main text. In the main analysis, we use the codings by the research assistants exclusively for consistency, but we can check the reliability of the coding by comparing to our previous coding. The table below shows the codings by the research assistants (rows) versus the codings by the authors (columns) on the question of whether some degree of relief was granted by the panel.

RA / Author	Relief	No Relief
Relief	377	30
No Relief	35	947

The overall agreement rate is 95% of cases. The cases where there was disagreement were spread across the years of the data as well as across circuits. Given the small number of cases on which there was disagreement, we do not see any plausible mechanism by which they could have biased the overall results.

State Balance Check

The only variable that we can easily measure which is potentially associated with case strength and therefore might provide a balance check is the origin state of each case. Because it is state law and criminal procedures that are typically the subject of the case being heard by the US Court of Appeals, any imbalance where Republican majorities were more or less likely to hear cases from certain states, holding constant circuit and year, would be problematic for our analysis. In order to test whether such an imbalance exists, we ran the same circuit-specific regressions used in our main analysis—with the Republican majority treatment variable and including or excluding year fixed effects—to predict the origin state of each case. This generates a set of 36 p-values on the treatment variable, one for each state that appears in our data. If there is no association between state and treatment, we would expect these to follow a uniform distribution.

Using the year fixed effects analysis that forms our main estimates, we cannot reject the null of a uniform distribution for the treatment variable p-values: the Kolmogorov-Smirnov test p-value is 0.32. Conditional on circuit and year, there is no consistent evidence that Republican majorities were more likely to hear cases from particular states. The analysis without the year fixed effects fails this test, with a p-value of 0.01, which demonstrates the importance of conditioning on year to credibly identify a causal effect. We obtain similar results using a one-sample t-test versus a mean of 0.5: the p-values from the year fixed effects models have mean of 0.43 with $p = 0.20$ versus the null of 0.5, and the p-values from the models without fixed effects have mean 0.36 with $p = 0.01$ versus the null of 0.5.

The only circuit where any state has a p-value less than 0.05 in this balance check is the 4th circuit, which consists of NC, VA, SC and MD (WV is in the 4th circuit, but does not use the death penalty). Even conditional on year, NC cases are less likely to have Republican majorities than cases from the other three states, and VA cases are more likely to. However the 4th circuit is not driving our main results. Our fixed effects estimate for the treatment effect of moving from a Democratic to Republican majority is somewhat higher in the 4th

circuit than the average circuit for relief denials (0.18 vs 0.12), but somewhat lower than average for execution (0.02 vs 0.07). The 4th circuit is only responsible for 12% of the cases in the data and the national average effects barely change if it is excluded. Adding state fixed effects in addition to year fixed effects to the main models has no consequential effect on either the 4th circuit estimates or the national estimates for either outcome.

Race Balance Check

We used the 2000 Census surname by race probabilities included in the R package “wru” (Imai and Khanna 2016) to calculate race probabilities for the all of the inmates applying for relief, and then to check whether there is any evidence that Republican majority panels were more or less likely to hear cases involving black or Hispanic defendants. If we pool all circuits, and run a regression predicting Republican panel majorities using the defendant race probabilities in a model with circuit by year fixed effects, we recover a coefficient estimate of -0.066 ($SE=0.065$) for probability black and 0.008 ($SE=0.050$) for probability Hispanic. The interpretation of the former coefficient is that Republican majority panels were slightly less likely to hear appeals from inmates with typically black surnames, holding constant circuit by year. However neither this coefficient nor the much smaller coefficient for probability Hispanic are close to statistically significant, so there is little evidence of imbalance. Explicitly controlling for the defendant race by surname probabilities makes no difference at all to the estimated effect of going from a Democratic to a Republican majority panel.

Analysis with Four Treatment Levels

If we disaggregate our binary treatment—Democratic majority versus Republican majority—into four treatment levels—DDD, DDR, DRR, RRR, we have very limited data for the DDD type in some circuits. The frequency of panels of each type in each circuit are as follows:

Circuit	DDD	DDR	DRR	RRR
4	14	77	107	46
5	6	129	265	200
6	16	58	70	24
7	1	7	33	37
8	8	31	93	56
9	24	67	49	12
10	11	47	68	33
11	46	151	161	44

We conduct the same set of regression analyses previously presented using a four-level indicator for these four possible panel compositions. We then aggregate the circuit estimates as before to formulate estimates of average causal effects over the period of study. The following table shows the effects in terms of proportions of adverse outcomes for the inmate versus a DDR panel baseline.

Model	DDD	DDR	DRR	RRR
Denial: No FE	0.01 (0.06)	0.00 (0.00)	0.09 (0.02)	0.14 (0.03)
Denial: Year FE	0.03 (0.07)	0.00 (0.00)	0.10 (0.02)	0.15 (0.03)
Execution: No FE	0.00 (0.07)	0.00 (0.00)	0.08 (0.03)	0.09 (0.03)
Execution: Year FE	0.01 (0.08)	0.00 (0.00)	0.07 (0.03)	0.06 (0.04)

We see in this table that there is little difference between DDD and DDR panels, though as noted above we lack a sufficient number of DDD panels to detect plausible differences between those panel types. We see some suggestion that the RRR panels are somewhat further from

the Democratic majority panels than DRR panels for denials, but not for executions. Thus, overall we see a clear difference between DDD/DDR panels and DRR/RRR panels—as a function of panel majority—but very weak evidence of differences between homogenous and non-homogenous panels with the same party majority.

Analysis Split by Chief Judge Party

The Chief Judge (at least nominally) oversees the process of assigning three-judge panels, “although the chief judge is typically not involved in the precise composition of individual panels or the calendaring of cases—a task delegated to the clerk’s office or other administrators” (Hettinger, Lindquist, and Martinek 2003). We coded whether the Chief Judge was a Democratic or Republican appointee for each circuit-year, and then estimated how the difference in outcomes from Democratic and Republican majority panels varied by the party of the Chief Judge. All circuits have variation on the party of the Chief Judge within the period of study except for the 7th circuit, which we must therefore omit from this analysis. The key results of this analysis are as follows. For denials, the case-weighted average coefficient for the interaction between Republican panel majority and Republican chief judge is -0.039, with a standard error of 0.043. For executions, it is -0.017 with a standard error of 0.050. These differences are not statistically significant and are not nearly as large as the baseline effects from the un-interacted model. The estimated differences between Republican majority panels and Democratic majority panels are in the same direction regardless of the party of the chief judge, with insignificant evidence of a larger magnitude under Democratic chiefs than Republican chiefs.

The Implausibility of Selection

The threat to causal inference is not that panels are non-random draws from the set of possible three judge panels, but that those draws might be related to features of particular death penalty appeals cases relative to other death penalty appeals cases. It is not a problem if the panels in death penalty appeals cases are systematically different than those in cases on other issues. For selection to explain our results, there would need to be some mechanism causing Republican judges to be more likely to appear on “weaker” appeals, and Democratic-appointed judges to be more likely to appear on “stronger” appeals, among cases decided in the same year in the same circuit, on average across all circuits in this period of US history.

The results of the four treatment level analysis in a preceding section of this appendix suggest that the big difference in outcomes is as a function of the party of the panel majority, not the simple count of Democratic versus Republican-appointed judges. This narrows the set of selection mechanisms that could explain the results to those which would particularly create such a difference as a function of appointing party majority. Mechanisms involving certain judges having expertise or experience that makes them more appropriate for assignment to “strong” versus “weak” appeals would not tend to create a step difference at majority control of the panel without also creating differences in 2-1 versus 3-0 panels. Indeed, it is difficult to generate this kind of pattern with a “natural” selection mechanism like relative seniority, rather one would need a strategic selection mechanism aimed specifically at controlling the panel majority in certain kinds of cases. While one could, in principle, imagine clerks or judges with seniority doing this if they controlled the assignment process, it would only make sense for them to do so on a case by case basis if there were actually a causal effect of controlling the panel majority of the type we aim to demonstrate. If there is no difference in the standards applied by Democratic- and Republican-appointed judges, there is no reason for anyone to try to manipulate the majority party as a function of the strength of the cases.

Such a selection mechanism would need to be large, on average, across circuits in order

to account for the size of the effects that we find. If we hypothesize that the causal effects of panel majority party are in fact zero for all cases, the required size of the selection mechanism is 15 percentage points on average across all circuits. That is, it would need to be the case that the appeals which were going to be denied (regardless of who heard them) were assigned to Republican majority panels at a rate 15 percentage points higher than the rate at which cases which were going to be granted relief (regardless of who heard them) were assigned to Republican majority panels. Given the difficulties of engaging in this kind of strategic selection, and lack of any incentive for anyone to do so if there were in fact no causal effect of the panel, it is far easier to see how a 10 percentage point effect on case outcomes as a function of the party majority would arise than a 15 percentage point effect on party majority as a function of case potential outcomes.

Additional Tables and Figures

Circuit	Execution Rate				Difference
	Granted Relief	n	Denied Relief	n	
4th	33%	27	64%	217	31%
5th	40%	110	72%	490	32%
6th	10%	60	43%	108	33%
7th	0%	13	29%	65	29%
8th	28%	40	58%	148	30%
9th	17%	71	37%	81	20%
10th	20%	41	76%	118	56%
11th	20%	117	48%	285	28%

Table A1: *Proportion of inmates executed in each circuit, according to whether they are granted any relief at the initial appeal.*

Circuit	State	# Petitioners	Relief Denied	Petitioner Executed
4	MD	10	10	2
4	NC	90	79	30
4	SC	33	29	24
4	VA	111	99	91
5	LA	46	32	24
5	MS	42	27	16
5	TX	512	431	356
6	KY	17	15	2
6	MI	1	1	0
6	OH	118	73	44
6	TN	32	19	6
7	IL	53	47	9
7	IN	25	18	10
8	AR	50	35	22
8	IA	3	3	0
8	MO	119	97	71
8	ND	1	1	0
8	NE	13	10	3
8	SD	2	2	1
9	AK	1	0	0
9	AZ	52	30	26
9	CA	66	37	10
9	ID	6	2	1
9	MT	5	4	1
9	NV	9	2	1
9	OR	3	1	0
9	WA	10	5	3
10	CO	3	3	1
10	KS	1	0	0
10	NM	2	2	1
10	OK	145	107	92
10	UT	6	5	3
10	WY	2	1	1
11	AL	75	61	46
11	FL	212	157	68
11	GA	115	67	46
All		1991	1512	1011

Table A2: *Distribution of cases and outcomes by circuit and state.*

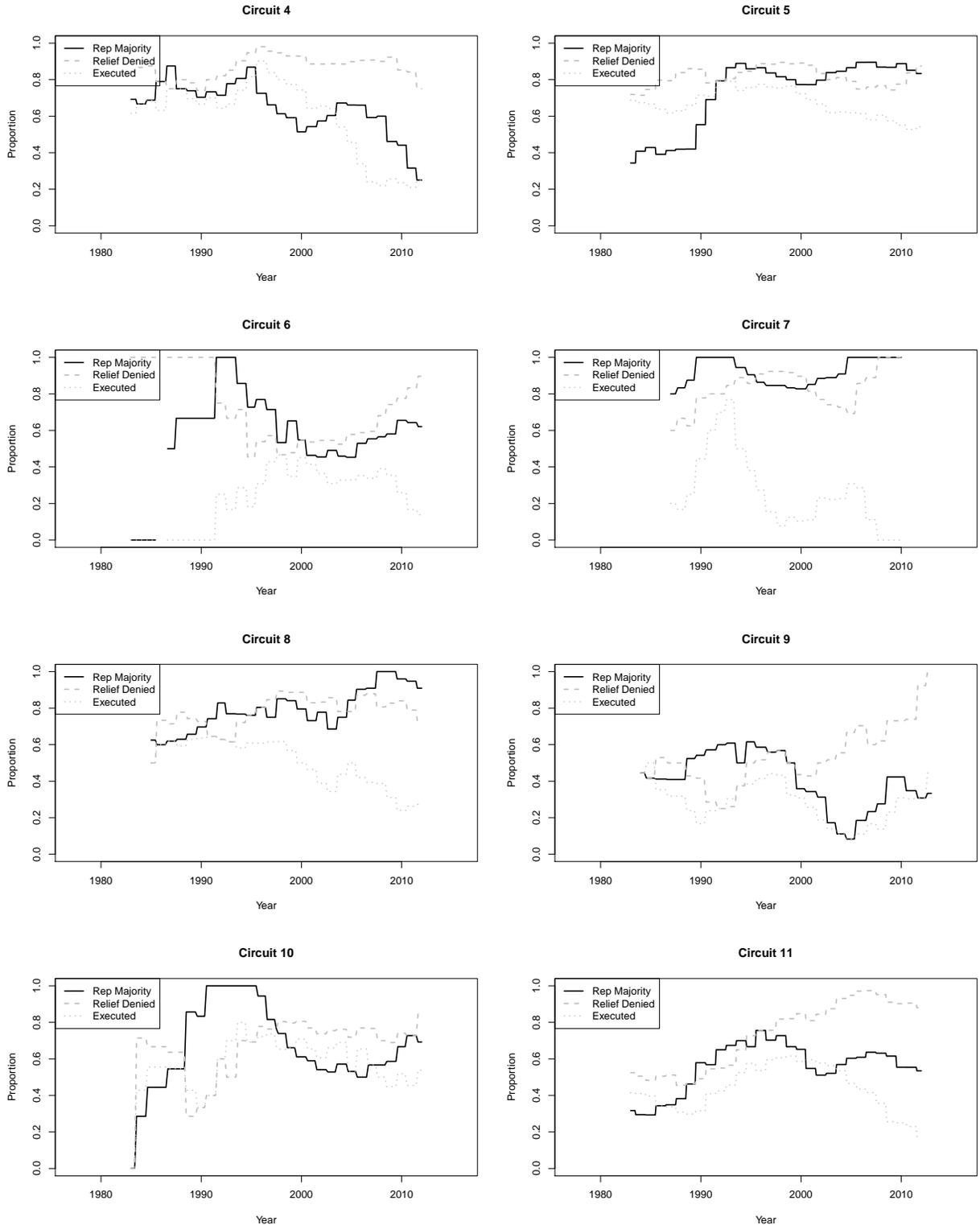


Figure A1: Five-year moving average for the rate at which death row inmates receive Republican majority panels, are denied relief by the Courts of Appeals, and are ultimately executed, by year of initial Courts of Appeals case decision.

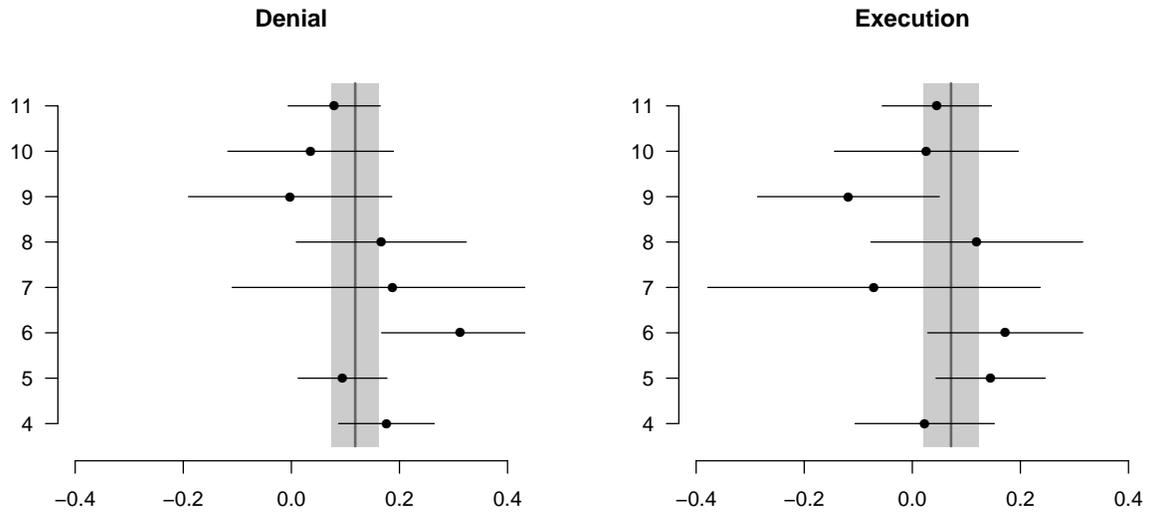


Figure A2: Estimated causal effects from time fixed effects models for relief denial (left) and execution (right) of going from a Democratic majority panel to a Republican majority panel, for each circuit and average across all circuits.

References

- Hettinger, Virginia A, Stefanie A Lindquist, and Wendy L Martinek (2003). “The role and impact of chief judges on the United States courts of appeals”. *Justice System Journal* 24.1, pp. 91–117.
- Imai, Kosuke and Kabir Khanna (2016). “Improving ecological inference by predicting individual ethnicity from voter registration records”. *Political Analysis* 24.2, pp. 263–272.