TWO ESSAYS IN EMPIRICAL LAW AND ECONOMICS

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A Dissertation

Presented to

The Faculty of the Department

of Economics

University of Houston

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In Partial Fulfillment

Of the Requirements for the Degree of

Doctor of Philosophy

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By

Jason Best

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Jason Best

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Abstract

Federal judicial vacancies are an ever increasing issue in the public sphere with 14% of federal judgeship positions currently unfilled concurrently with increases in aggregate levels of caseloads. Despite concerns as to the adverse consequences of judicial vacancies, research about their effects has remained scant due to the difficulties of specifying the causal mechanism between vacancies and judicial decision-making. Consequently, courts are left with a lower supply of the necessary tools to handle the increased demand for services.

In my first chapter, I examine the effect of judicial vacancies on sentencing outcomes in district courts in the United States of America. I make use of an instrumental variables strategy in order to estimate a causal relationship. Specifically, the first instrument which is used in this paper uses judge deaths while the second instrument makes use of a judge's eligibility for senior status in order to explain the vacancy rate. My findings show that for a district with ten allocated judgeship positions, a vacancy present throughout the length of a case will result in sentence lengths that are 2 months longer on average due to a 4% decrease in downward departures and a 4% increase in sentences at the Guideline minimum. Moreover, for defendant characteristics which predict recidivism, here age, gender and criminal history, the effect is even larger. These effects highlight the problems associated with persistent vacancies at the district level.

To assess one of the policies adopted in order to handle vacancies which might have especially deleterious effects on federal district courts, I evaluate the effect of designating a vacancy as an "emergency" on durational and judge quality outcomes in my second chapter. Here I make use of a policy rule to find causal estimates with a regression discontinuity strategy. Estimating the duration of vacancies slightly above and below the cutoff rule I find no effects of the cutoff on durational outcomes. Furthermore, there are effects on judge quality measures in that judges above

the cutoff have slightly worse law school rankings on average while maintaining marginally higher American Bar Association qualification scores. These results suggest that the policy is not having the intended effects of reducing the duration of vacancies.

Acknowledgements

Ryan Ruddy is someone I'm extremely fortunate to have come into my life both professionally and personally. Whenever I talk about him, he's always my "fishing buddy," but in reality he's my inspirational companion. In a sea of people who are struggling begrudgingly through their strenuous graduate programs, Ryan struggles with a smile on his face. Whenever I would have a tough day of straining to see the light at the end of my graduate tunnel, Ryan would psych me up with engaging descriptions of the research he just read about or was currently undertaking. But, probably most importantly, I don't know how I would have survived this program without a buddy who was willing to drop everything, load up some kayaks and fishing gear and drive hours to spend all day washing away our collective worries on a body of water.

It's truly the case that it isn't a person's opinion is not shaped by the number of years you've known someone but the way they have used that time. Professor Lehmann came to affect my path late in my program but nevertheless had such a significant impact that I can certainly say I would be much the worse in her absence. As a mother of two, she clearly cares for her children and often has to spend the day outside the office for that reason. However, don't for one second think she's skipping out on her professorial duties as on numerous of these occasions she would send me feedback on my research at 9, 10 or even 11 pm. Her dedication to both her work and the work of a mentee in the program is something I'm extremely grateful for.

I have no dictionary wide enough to describe my good friend and mentor Sean McCall. Somehow, if you ever meet the man, you will know the hidden words; you can read them in his eyes. There you will see a man that embodies the description "determined." If you truly believe Rohn's message, the first person you might choose to befriend would be Sean. As a companion, he has been there through the tough times when I wasn't sure about my path in undertaking this program. As a mentor, he has been there to guide me to the road of success. But, most of all, he has been there to inspire and push me. If I can be half the man Sean is by the time I reach his age I will

consider myself to have accomplished something grand. That said, right now I'm contenting myself

with the goal of becoming his equal in skills with a Frisbee.

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Chapter 1

Vacancy in Justice: Analyzing the Impact of

Overburdened Judges on Sentencing Decisions

1.1 Introduction

A dramatic rise in the number of federal judicial vacancies since 2009 has led scholars and policymakers to reiterate the long-standing argument that persistent vacancies in federal judgeships create great challenges to the work of the American federal court system. As of October 2013, there were 92 vacancies out of a total of 677 judgeships – a vacancy rate of almost 14 percent.[1] Judicial vacancies are currently at the highest level since the early 1990s, an increase that many attribute to high levels of partisanship (Wheeler 2013). Those concerned about high levels of judicial vacancies argue that vacancies impose undue strain on the courts by overburdening active status judges with more cases than intended. Scholars have suggested that excess caseloads and shortages of judicial resources due to high vacancy rates can have a significant effect on the process and nature of judicial decision-making. Specifically, high caseloads and vacancies in federal courts may motivate judges to take shortcuts in legal proceedings and decision-making, compromising not only the defendants' overall experience in the court system, but also impacting the type and the length of punishment they receive (Nardulli 1979). These concerns are further supported by recent research in psychology, cognitive science, and economics demonstrating that decisions made in high-pressured environments are significantly different than those made under normal circumstances (Starcke and Brand 2012, Mather and Lighthall 2009).

Existing studies examining the impact of vacancies on court decisions do not account for the possibility that variations in vacancies and workloads may be correlated with unobserved judge and court characteristics, these themselves impacting case decisions. In other words, existing studies do not account for the endogenous nature of vacancies. Thus, a simple examination of the observed relationship between case outcomes and a court's vacancies will likely lead to biased estimates of the impact of vacancies on court decisions. To counter this, we use a novel instrumental variables (IV)

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[1] http://www.uscourts.gov/JudgesAndJudgeships/JudicialVacancies.aspx   Accessed October 31, 2013.

strategy to estimate the causal impact of federal vacancies on sentencing decisions. We employ two instruments for federal vacancies. First, as one instrumental variable, we exploit the plausibly exogenous variation in vacancies created by judges becoming "senior-status-eligible," an eligibility that one reaches when a judge is at least 65 years old and the sum of his/her age and time on the federal bench is 80.[2] Judges who are senior status eligible are not required to take this status, but if they do, they remain on the bench largely choosing their own type and amount of work,[3] while retaining the same salary levels of a full time judge. Second, as another instrumental variable, we use the variation in vacancies generated by deaths of judges, which create unexpected shocks in the system. Examining the effect of these two sources of vacancies on case outcomes allows us to test whether the effects of vacancies differ between these two sources of judicial vacancies, specifically those that are generally anticipated and those that are unexpected. Because vacancies that are unanticipated due to a sudden death, resignation, or elevation result in the loss of a judge's entire share of work versus a partial reduction in work caused from assuming senior-status, unexpected vacancies may have a greater effect on court decisions. While both sources of vacancies lead to larger caseloads for the remaining judges in the court, it is an empirical question whether they have the same impact.

We apply our instrumental variables or IV strategy to a dataset containing the vast majority of all criminal sentences in United States' district courts from 1999 to 2006. Our two-stage least squares (2SLS) estimates show that while vacancies induced by senior status eligibility have no statistically significant effect on sentencing decisions in criminal cases, vacancies induced by judges' deaths are

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[2] As of 1984, under the "Rule of 80," judges are deemed eligible for senior status from the age of 65 if the sum of their age and the length of their service equals 80 (i.e. judge is at least 65 years of age and has been on the federal bench for at least 15 years summing up to a minimum total of 80).

[3] Senior Status judges are required to complete about one-fourth of the work of active status judges each year to maintain their senior status. Specifically, The United States Code requires senior status judges to be certified by the Chief Justice of their district and show that they "have carried in the preceding calendar year a caseload involving courtroom participation which an average judge in active service would perform in three months." 28 USC 371(e)(1)(A).

associated with harsher criminal sentences. Specifically, a one-percent increase in vacancy rates resulting from judges' deaths increases sentence length by 0.2 months and reduces downward departures from the sentencing guidelines by 0.4 percentage points. These effects are statistically significant at the 1% level. We also find evidence that the effect of vacancies on court decisions varies across defendants' attributes such as gender, age, and criminal history of the defendant.

Our findings demonstrate that judicial vacancies do not simply overburden active judges, but they have a measurable and significant impact on actual court decisions, especially in those courts where strains from extra caseloads cannot be partially eased by senior status judges. Our results have important policy implications for the federal judicial confirmation process and provide a weighty justification for a more efficient process to fill judicial vacancies. More generally, in relation to other studies in economics and political science, our findings provide evidence that workloads and associated stress levels have a consequential impact on critical decisions in a non-experimental environment.

The paper proceeds in nine sections. Section 2 discusses the background of judicial vacancies and federal sentencing. Section 3 summarizes the literature on vacancies, caseload burdens, and the impact of stress in decision-making. Section 4 provides our conceptual framework for analyzing vacancies, and Section 5 outlines our empirical strategy. After describing our data in Section 6, we present our main results in Section 7. Section 8 presents results for tests of heterogeneity in the effect of vacancies on court decisions. Section 9 provides some conclusions.

1.2 Institutional Setting: Judicial Vacancies and Federal Sentencing

Often perceived to be one of the greatest challenges faced by the federal judiciary, vacancies are quite prevalent among federal district courts nationwide (Wheeler and Binder 2006; Wheeler 2013; Ashcroft 2002; Roberts 2010). Between 1997 and 2012, authorized district court judgeships

increased from 632 to 675. In the same period, vacancies in district courts ranged from 16 in 2004 to a high of 87 in 2010, constituting 2% to 13% of active district court judgeships per year. However, this yearly data mask the fact that monthly vacancies in judicial districts could be significantly higher and may be more prevalent in some districts than others. Vacancies also tend to persist for long periods. For example, under the Obama presidency, district court vacancies have persisted for 610 days on average (Hardin 2012), but there are also extreme outliers such as a vacancy lasting 2,655-days in the Eastern District of North Carolina and several vacancies lasting over 1,000 days as of February 2013.[4] The increasing lengths of judicial vacancies have often been associated with the progressively more contentious political process of filling vacancies (see Wheeler and Binder 2006; Wheeler 2013).

Although judicial vacancies could affect the adjudication of both civil and criminal cases, in this paper we focus on criminal cases under the United States Sentencing Guidelines. We focus on criminal cases as they pose the basis for many of the assumptions about the effect of vacancies on the judicial system. Further, criminal punishment is an important concern for politicians and citizens alike who gauge how well the legal system is operating by the punishment it renders and security it provides.

The 94 federal district courts in the United States sentence criminal defendants in over 70,000 cases per year. On average, each district court has 7 allocated judgeship positions, with each judge handling on average almost 500 combined criminal and civil cases each year. These numbers imply that one judgeship vacancy will increase the average workload in the court by around 14% or by an extra 70 cases per year.

In the district courts, punishment for federal crimes is prescribed by both the Federal Criminal Code and the United States Sentencing Guidelines. Although the Guidelines limit district judges'

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[4] An extreme outlier is found in the U.S. Court of Appeals for the Ninth Circuit which has had an unfilled judgeship lasting 3,200 days due to a dispute between senators from California and Idaho concerning a nominee's state of origin.

discretion, they also limit the ability of circuit courts to review district court sentences. Over 90% of

sentences in federal district courts are resolved by plea bargains[5] and, in most cases, defendants who

agree to be sentenced within proscribed Guideline ranges, waive their rights to appeal to a higher

court as part of the plea bargain. However, unlike state court judges, federal judges can and do

refuse to enforce plea bargains, forcing prosecutors and defense attorneys to reach more acceptable

agreements or go to trial, the latter being infrequent (see Guideline §6B1.3).

In determining sentences, lower district court judges use the Guideline's sentencing table with a

horizontal axis dividing a defendant's criminal history into six levels and a vertical axis classifying the

severity of a defendant's offense into forty-three levels. The Sentencing Guidelines also allow judges

to exercise their discretion and to depart from the Guideline ranges under specific circumstances.

According to the Guidelines, district court judges may "depart" above or below the fixed sentencing

Guideline ranges in response to "specific offender characteristics," including their age, education,

and socio-economic background (Guideline §§5H1.1 to 5H1.12) as well as aiding the prosecutor

(Guideline §5K1.1). However, according to the United States Sentencing Commission (USSC),

these factors "are not ordinarily relevant to the determination of whether a sentence should be

outside the applicable guideline range." From 1999 to 2006, judges are observed to depart

downward from the Guideline ranges in about 40% of the cases and upward in about 2% of the

cases.[6] As a result, almost all cases with departures involve *downward* departures, which result in

lower sentences than proscribed by the Guideline ranges. Sentences that are rendered outside of the

prescribed Guideline ranges require extra justification as they provide grounds for a defendants'

appeal. As such, departures require extra time and effort on the part of the judge due to the

[5] The USSC's sourcebook indicated that in 1996, 91% of the sentences were reached by plea agreement and in FY 2006, 95.7% (USSC fiscal year sourcebooks available at http://www.ussc.gov/Research_and_Statistics/index.cfm).
[6] The USSC's sourcebook indicated that in 1996, downward departures constituted 29.5% of all cases and upward departures 0.9%. In FY 2006, downward departures constituted 36.6% of all cases and upward departures 1.6% of all cases. (USSC fiscal year sourcebooks were accessed at http://www.ussc.gov/Research_and_Statistics/index.cfm).

increased probability that the departure will be appealed. Due to the discrepancy in effort to justify a departure from the Guideline ranges, it seems plausible that vacancies, which increase workloads and require judges to sentence more defendants in a shorter amount of time, may ultimately affect the length of sentence and decision to depart.

1.3 Related Literature

Although many policymakers and scholars acknowledge the potential threat of vacancies on the workings of the federal courts (Wheeler and Binder 2006; Wheeler 2013), to our knowledge Dayton (1993) is the only existing study that empirically assesses the direct impact of vacancies on courts. Her research focuses on the impact of vacancies on the delay of federal civil cases heard at the district court level. Using three measures of delay, she finds that there is no relationship between civil case delays and vacancies. While her analysis attempts to assess the direct impact of vacancies, Dayton measures vacancies at a yearly-level and is unable to account for the potentially important variations in vacancy levels throughout a calendar year. Moreover, Dayton's study is limited to examining the impact of vacancies on delays in civil case proceedings. More importantly, her empirical strategy does not address the potential identification problem that vacancies and case outcomes may be endogenously determined by unobserved judge or court characteristics. In contrast, our study is the first to our knowledge to use an estimation strategy that enables us to obtain the causal impact of vacancies. Furthermore, we broaden our research beyond assessing the impact of vacancies on civil case delays by studying the impact of vacancies on criminal cases and testing whether different types of vacancies have varying impacts on court decisions.

While there are scant studies on the impact of judicial vacancies, there are several studies dealing with how the byproducts of vacancies in the form of high caseloads and low staffing affect the manner in which judges make decisions, case outcomes, or other court outputs. Resource

constraints appear to make courts more bureaucratic with judges delegating more authority to other legal actors and spending more time on administrative tasks (Hydebrand and Seron 1990). This in turn results in "depersonalizing" the judiciary (Cohen 2002; Posner 1985). High caseloads and insufficient staffing also increase inefficiencies and delays in court proceedings (Buscaglia and Dakolias 1999) and result in judges taking less individual responsibility for their case decisions (Fiss 1983).

Caseload burdens are also found to be associated with case outcomes (See Habel and Scott 2014 for a review). Heavier caseloads make guilty pleas more likely (Dash 1951, Newman 1956; Alshuler 1968; but see Nardulli 1979), but effects on punishment are mixed, with some scholars finding punishment to be less likely (Smith 1921), and others finding no effect on the decision to punish or on the length of the sentence (Nardulli 1979, Ulmer and Johnson 2004; Esptein, Landes, and Posner 2013). Finally, a few existing studies indicate that strains on bureaucratic capacity affect case outcomes by compromising the ability of agents of the federal government to carry out the intent of legislators (Huber and McCarty 2004; McCubbins, Noll, and Weingast 1995; Braniff 1993).

Scholars also analyze staffing and caseloads in conjunction with court output or the number of resolved cases. Buscaglia and Ullen (1997) and Murrell (2001) claim that high caseloads cause congestion in the courts and lower court output. Beenstock and Hastovsky (2004) caution, however, that adding judges to courts may have the perverse incentive of reducing productivity, but increased caseloads may motivate judges to resolve more cases (see also Luskin and Luskin 1986; and Dimitrova-Grajzl et al. 2012).

Of all these studies estimating the impact of caseloads, the Dimitrova-Grajzl et al. (2012) study, using data on lower courts in Slovenia, is the only one to use an estimation strategy that recognizes

the potential endogeneity problems in the identification of the impact of staffing.[7] Using lagged

values of caseload measures as their instruments for contemporaneous caseload measures, they find

that an increase in caseloads leads to more case resolutions. While Dimitrova-Grajzl et al. attempt

to address the endogeneity concerns, their analysis is limited to studying the effects of staffing and

caseloads on the number of resolved cases in lower courts in Slovenia, thereby, limiting its

applicability to the U.S. courts.[8] Our study is the first to our knowledge to estimate the effect of

vacancies, not their byproducts related to caseloads and staffing, on case decisions using plausibly

exogenous variation in vacancies in the federal district courts.

   Aside from the aforementioned studies in legal scholarship, this study is also related to the

impact of stress in decision-making, which we believe is exacerbated by vacancies. In the legal and

other scholarly literature, there is indication that busy or stressed individuals may take shortcuts

which affect case outcomes (Klein and Hume 2003; Mann and Tan 1993; Epstein Landes and

Posner 2013). Additional studies in the field of psychology report findings from laboratory

experiments in which the decisions of subjects given a stress-inducing stimulus are compared to

those of control subjects. Mather et al. (2009) use cold water as a mechanism of triggering stress in

individuals before testing their actions in a driving simulator game and find that older individuals

tend to reduce risk-taking behavior when under stress. Porcelli and Delgado (2009) also use cold

pressure, but in the context of financial decision-making. They find that stress regulates risk-taking.

Additionally, they report that when under stress, people may rely more heavily on automated risk

biases, reinforcing preexisting preferences in decision-making. Although these findings from

laboratory studies provide some credible evidence that individuals' decision-making process may be

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[7] The specific concern of Dimitrova-Grajzl et al. is that resolving more cases in Slovenia courts may incentivize litigants to file more cases. In contrast to U.S. federal district courts where jurisdictional requirements are more rigid, Slovenian lower courts have more flexible jurisdictional requirements which allow litigants to choose where they file cases in some circumstances.

[8] Additionally, the validity of Dimitrova-Grajzl's exclusion restrictions might be a concern, as staffing and caseloads are likely to be highly persistent over time, such that even lagged values are likely correlated with unobserved characteristics of the court.

impacted by stress, some may question the external validity of these experimental findings (Levitt & List 2007). Our study provides an important contribution to the literature on stress and decision-making by demonstrating that stress and heavy workloads have consequential impact on real decision-making.

1.4 Conceptual Framework

1.4.1 Vacancies and Sentencing Outcomes

Judges can employ a number of strategies when overburdened by high vacancy levels in their court. First, judges could simply dismiss cases in order to lower their workload. While this is feasible, the lack of punishment would run afoul of elected politicians' law and order policies, public opinion, and more than likely, a judge's personal moral compass. Second, judges could take a longer time to decide cases, because they are overburdened. Although this is a realistic strategy to use in civil cases, such a strategy has less applicability for criminal cases where delay has immense ramifications for defendants' constitutional rights and the safeguarding of evidence.

A third strategy that judges could employ is to rely on shortcuts in sentencing decisions (Posner 1996, Wright 1974, Klein and Hume 2003). One short cut may involve judges' reliance on a minimum amount of information concerning the case rather than all of the information available to them. In support of such an argument, Mann and Tan (1993) indicate that time pressure "motivates a reduction in information search and reliance on simple decision rules" (p. 206). While Mann and Tan's study applies to management decisions in business, in the context of courts, Klein and Hume (2003) suggest that busy lower federal court judges may limit the amount of time they use "to weigh every detail of each case" and "limit their attention to key facts or patterns" (p. 602). With respect to district court judges' application of the federal Sentencing Guidelines, decisional short cuts may focus judges' attention on Guideline minimums (Tiede 2009) or maximums. Alternatively, as

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suggested theoretically by Epstein, Landes, and Posner (2013), when judges have heavy caseloads they take time saving short cuts such as choosing *not* to depart from the Guidelines in certain cases that may warrant departures in order to save time. Epstein, Landes, and Posner indicate that a judge who departs "has to spend more time justifying the sentences" (p. 246).[9] By departing less and agreeing to sentence within the Guideline range, judges save time by not having to justify their Guideline departures. Another short cut entails judges focusing on general aspects of the case, employing methods of statistical discrimination in making their decisions when burdened by high caseloads.

These considerations suggest that vacancies can decrease (e.g., by reducing upward departures from the Guidelines), increase (e.g., by reducing downward departures from the Guidelines) or leave unchanged sentences. As a result, the direction of the vacancies' effects on sentencing remains an empirical question to explore.

1.4.2 Differential Effects by Vacancy Classification

The effect of a judicial vacancy is likely to differ by the manner in which it occurs. Vacancies may occur due to judges' resignation, retirement, death, impeachment, or their assumption of senior status. Our empirical analysis examines whether the effect of vacancies due to assumption of senior status differs from the effect of vacancies due to other causes. We focus on the senior/non-senior vacancy distinction, because unlike the other causes of vacancies, when judges assume senior status, they do not leave the court but continue working with either a reduced or full caseload that meets a statutory minimum amount.

Since 2002, senior judges have constituted nearly 40% of the federal judiciary (Yoon 2005) and cause 76% of the total vacancies in our sample. For senior status judges, the decision to work

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[9] Author interviews in the district courts within California in 2007 and 2008 confirmed this (Tiede 2009).

beyond the statutory minimum is largely determined by the individual judges themselves. While the workloads of individual senior status judges vary widely, many of these judges carry significant caseloads (author interviews and survey 2007, 2008) that increase when their courts face particularly heavy caseloads or vacancies. U.S. Supreme Court Justice John Roberts stated in his 2010 Year End Report of the Federal Judiciary that "[w]e would be in dire straits without their [senior status judges'] service" (Roberts 2010).[10]

Given that senior judges continue to handle caseloads ranging from the required minimum to a full load, vacancies due to senior status lead to the loss of something less than one full-time judge, which is what the other sources of vacancies generate. There are other potential differences between senior and non-senior vacancies as well. For example, because senior status eligibility is determined by the Rule of 80, courts can predict when a senior vacancy will arise and can plan case allocations among the courts' judges in a way that eases the transition caused by accommodating a vacancy. These considerations lead us to predict that senior vacancies will have less pronounced effects on case outcomes than other types of vacancies. Although a number of studies have examined what drives judges to take senior status (see Yoon 2005, 2006; Choi, Gulati, and Posner 2011; Hansford, Savchak and Songer 2010; Boylan 2004), this study attempts to discern the impact of senior vacancies compared to vacancies caused by other reasons on case outcomes specifically.

1.5 Empirical Strategy

To estimate the causal impact of vacancies on case outcomes, it would be erroneous to simply take the difference in means between districts with and without vacancies, because unobserved court attributes could be correlated with both vacancies and case outcomes. As we show in the summary

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[10] Author interviews of several senior status district court judges in California reported that they handled about 500 sentencing cases a year (interviews held in 2007 and 2008)(Tiede 2009). An author-conducted national survey for the same period, with many senior status respondents, confirmed that many senior status judges have high sentencing caseloads.

statistics in our data section below, case outcomes and various case and district attributes differ depending on whether a vacancy persisted in a district where the case was being heard. Therefore, the estimated difference in court outcomes would capture both the true effect of vacancies as well as the selection bias.

One possible strategy to address this selection bias problem is to control for additional variables that may account for variations in court outcomes with a district court and year fixed effects models. Controlling for district fixed-effects allows us to account for all time-invariant court-level characteristics and including year-fixed effects controls to account for common time shocks experienced by all courts. In summary, this fixed effects model is specified as follows:

(1) $y_{ijt} = \alpha + \beta_1 \text{Vacancy}_{ijt} + \pi x_{ijt} + \gamma w_{jt} + \lambda_t + \phi_j + \varepsilon_{ijt}$

where y_{ijt} is the case sentencing outcome measure for case i in district j at time t. The main explanatory variable of interest is measured as the percentage of judges missing in a district over the time span the case is adjudicated, denoted by Vacancy_{ijt}. The x_{ijt} is a vector of case specific characteristics, w_{jt} is a vector of time-varying district specific characteristics and λ_t and ϕ_j refer to time and district fixed effects respectively. Specific descriptions of all variables are provided below in the section on the data.

As discussed previously, the effect of vacancies on judicial decisions is likely to differ by whether the vacancies arise from judges taking senior status or from other reasons. Modifying Equation 1 to allow for the differential effects by vacancy type, we estimate the following regression model:

(2) $y_{ijt} = \alpha_2 + \beta_2 \text{Senior Vacancy}_{ijt} + \beta_3 \text{Non-Senior Vacancy}_{ijt} + \pi_2 x_{ijt} + \gamma_2 w_{jt} + \lambda_{2t} + \phi_{2j} + \varepsilon_{ijt}$

where $\text{Senior Vacancy}_{ijt}$ ($\text{Non-Senior Vacancy}_{ijt}$), is the percentage of missing allocated judgeships that have arisen through each of these avenues.

Although estimating a district-fixed effects model allows us to account for any time-invariant district unobservables that may be correlated with both vacancies and case outcomes, there still may

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be time-varying district characteristics that may bias our estimates. For example, even within a particular court, judges may time their vacancy decisions based on certain unobserved events, which also may be correlated with sentencing decisions. Unobserved shocks that may burden judges could also lead to vacancies, biasing our estimates of the impact of vacancies on punishment severity upward. Moreover, judges may time their decisions to vacate the bench based on their concerns regarding the observed level of court efficiency. Specifically, judges may time their exits to coincide with periods when the courts are capable of handling a loss in judgeship. Thus, positive shocks to court efficacy could lead to vacancies, which would bias our estimates toward zero. Because vacancy measurements are likely endogenous in Equations 1 and 2, the OLS estimation is unlikely to lead to consistent estimates of the causal impact of vacancies. As a result, we use an alternative instrumental variables estimation strategy to address such endogeneity concerns and to obtain consistent estimates of the causal impact of vacancies. As discussed in the introduction, we rely on two instruments for vacancies: a judge's death and a judge's eligibility for senior status.

Our first instrumental variable for judicial vacancies involves the deaths of judges serving on the bench. Specifically, this IV is a dummy variable measuring whether a death occurred while a particular case is being adjudicated. In the time period examined in this paper, 13 districts experienced the plausibly exogenous shock of a judge's death.[11] Deaths arise mainly from heart attacks and various forms of cancer, which we expect to be uncorrelated with judges' decision-making attributes.[12] Although older age and sickness make death more likely, we note that the exact timing of death is hard to predict. Even if it were true that district courts could anticipate the deaths of some judges, we would expect muted effects of a deceased judge biasing our coefficients towards zero.

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[11] The Eastern District of Pennsylvania experiences two judge deaths in this period. As such, there are 14 judges that pass away in our sample

[12] The average age that judges pass away is 62. Deaths are generally due to cancer, some heart attacks, car accidents and other causes.

One possible critique of using judges' deaths as an instrument is that the sample of judges who pass away are different from the population of all judges since the number of deaths is relatively small in our overall sample. This critique, however, does not invalidate our method because the IV estimate provides the *local* average treatment effect (LATE) of the endogenous variable (i.e. vacancies) induced by the instrument (i.e. deaths). To determine whether the sample of judges who die is consistent with our population of judges on a whole, we compare the political affiliations of the judges who died in our dataset to those of active judges in Table 1. We use both party of the nominating president and the Giles, Hettinger, Peppers' (2001) scores to determine judges' ideology. While it appears that judges who die in our data set are less likely to be nominated by a Democratic president (i.e. 28.6% to 40.2%), these numbers are not statistically different, and judges' ideology in the two groups is strikingly similar. Giles-Hettinger-Peppers' common space scores (GHP scores)[13] show that judges in our data set who die are not significantly different when using this more precise measure of ideology (i.e. 0.163 to 0.126).

Another potential critique is that judges' deaths do not satisfy the exclusion restriction, because death may affect sentencing outcomes not only through vacancies, but also potentially through the emotional state of other judges in the same court. Such a concern is unlikely in the context of lower district court judges who sit alone and do not decide cases collegially limiting regular or intimate contact among other judges in their district.[14]

Our second instrumental variable for judicial vacancies is a measure of the eligibility of judges for senior status. As of 1984, under the "Rule of 80," judges are deemed eligible for senior status from the age of 65 if the sum of their age and the length of their service equals 80. Using data on

[13] GHP scores are calculated as a continuous measure from -1 to 1 using the common space scores of the current president and senators in the judge's district. If the president's party doesn't align with either senator in the judge's district, the president's common space score is used. If one senator aligns, their score is used and otherwise, an average of both senators' scores is given as the GHP score for the judge.
[14] Author interviews indicated that district court judges' rarely have time to socialize with other judges in their district let alone ask for advice on cases (Tiede 2009).

judges' tenure and birthdates, we construct a senior status eligibility instrument equal to one if the case was adjudicated at the same time a judge becomes eligible and zero otherwise.[15] As noted before, judges do not need to take senior status just because they become eligible.

As with judges who die, we also determine whether judges who become senior status eligible are somehow different than the population of district court judges. Table 2 shows that eligible judges tend to be more conservative than the average judge. In our empirical analysis, we control for this difference by using GHP score averages along with measures of the percent of judges in each judicial district nominated by presidents from the Democratic party for each month. As we argued for the death instrument above, the fact that eligible judges differ from ineligible ones does not change the fact that IV provides a consistent estimate of the causal impact of vacancies. As usual, the IV estimate provides the LATE. In our data set, 166 judges assume senior status and 74 do so within one year of becoming eligible, so senior vacancies induced by eligibility are common. As such, the eligibility for senior status judges' IV estimate provides plausible applicability for the more general effect of senior vacancies.

To summarize, the associated first stage regression to the structural relationship of interest for Equation 1 is

(3) $\quad \text{Vacancy}_{ijt} = \alpha_3 + \beta_4 Z_{ijt} + \pi_3 x_{ijt} + \gamma_3 w_{jt} + \lambda_{3t} + \phi_{3j} + \varepsilon_{3ijt}$

where Z_{ijt} is the identifying instrument (either *Death*, *Eligible* or both).[16] Similarly, where the structural relationship of interest is Equation 3, then the associated first stage regressions are:

(4) $\quad \text{Senior Vacancy}_{ijt} = \alpha_4 + \beta_5 Z_{2ijt} + \pi_4 x_{ijt} + \gamma_4 w_{jt} + \lambda_{4t} + \phi_{4j} + \varepsilon_{4ijt}$

(5) $\quad \text{Non-Senior Vacancy}_{ijt} = \alpha_5 + \beta_6 Z_{3ijt} + \pi_5 x_{ijt} + \gamma_5 w_{jt} + \lambda_{5t} + \phi_{5j} + \varepsilon_{5ijt}$

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[15] We define 'eligible' to equal the month a judge becomes eligible plus the average time a vacancy lasts in our sample, 13 months.

[16] Note although technically Equation 1 is over-identified (there are two potential instruments and one endogenous regressor), it is not desirable to do a test of over-identification because a priori we believe these instruments capture distinct sources of variation in vacancies and we hypothesized that senior vacancies caused by the assumption of senior status are likely to have different impacts than other vacancies.

where Z_{2ijt} is the death instrument and Z_{3ijt} is the instrument for senior status eligible.

It is important to note that the effects of vacancies will likely differ between estimates relying on these two instrumental variables above. A priori, one would expect the local average treatment effect for a judge dying to be greater and possibly different in sign than the effect of a judge becoming eligible for senior status. Judges' deaths are shocks to the district courts which are oftentimes not anticipated and result in a complete loss of a judge compared to senior status vacancies in which senior judges may stay on the court sometimes taking on as many cases as the active judges.

1.6 Data

To analyze the impact of vacancies, we construct a unique dataset that combines data on criminal sentencing cases from the United States Sentencing Commission (USSC) with district level attributes from the U.S. Courts and the Federal Judicial Center (FJC). The data from the USSC include information on 95% to 99% of all federal convictions reported from the district courts to the Administrative Office of the Court for all crimes sentenced between the years of 1999 and 2006 (USSC 2004, 2005, 2006).[17] After excluding cases from the four U.S. territories[18] and cases for crime classifications in which there were less than 15,000 occurrences, we are left with 424,465 sentencing cases. Each sentencing case provides our unit of analysis[19] and our panel data contain information on each district court over multiple periods. From this data, we derive three dependent variables for sentencing outcomes including: 1) Sentence length in months in prison; 2) Decision to depart

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[17] The United States Sentencing Commission Annual Reports for 2004, 2005, and 2006 indicate that for each fiscal year they received 99% of judgment and commitment reports for each case and 95% of presentence reports and statements of reasons all of which are used to construct the USSC data bases.

[18] Complete case data for Guam, the Northern Marianas Islands, Puerto Rico and the Virgin Islands are not available. In 1999, cases from these four territories only account for less than 1% of the total cases handled by the federal district courts.
[19] Due to issues of confidentiality in the agreement with the Administrative Office of the Courts, the USSC cannot include the identity of the individual judge deciding each case. Hence, we will be unable to control for judge attributes or explore which judges are most sensitive to vacancies.

downward; and 3) Decision to cluster at the Guideline minimum. The control variables for each

case include the seriousness of the offense, the crime type, the Guideline minimums and maximums,

the defendants' criminal history, gender, race and age, all described in Table 3 with supporting

literature justifying the variables included in our analysis.

We combine this case-level data from the USSC with information about the judicial districts

collected from two sources. First, the U.S. Court's website (UScourts.gov) provides data on every

judicial vacancy by exact date it occurred and district location as well as on district caseloads.

Second, the Federal Judicial Center provides information about judges' birthdates as well as

identification of the nominating president, dates of nomination, resignation, retirement, elevation,

death, impeachment, and assumption of senior status. From 1999 to 2006, there were 219 new

vacancies.[20] Of these, 14 occurred due to judges' deaths, 166 due to senior status, 15 due to

elevation, and 24 due to other reasons (such as retirement, resignation or impeachment).

Ideally, for each case (i.e. our unit of analysis), we would like to measure its exposure to

vacancies over its entire duration in the federal district court. A practical impediment is that the

USSC sentencing data set only provides the case termination or sentencing date and provides no

information about when the case starts or its duration. Therefore, we assign to each case a duration

that is equal to the mean duration observed for criminal cases for the same year as observed in the

U.S. Courts data. For the eight years analyzed, the case duration ranges from 6.5 to 7.6 months.

Using this measurement of case length, we form the case's exposure to vacancy, creating our

meausrement, *Vacancy,* as follows:

$$Vacancy_{ijt} = \frac{1}{t}\sum_{1}^{t}\frac{\text{\# of vacant judgeships}_{ijt}}{\text{allocated judgeships}_{ij}}$$

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[20] There are also 39 vacancies that we account for which originated prior to our sample but carried over into the sample.

where t is the time period between when a case is filed and when it concludes with a final sentence using statistics on case averages, j is the district of interest, and i is the individual case. Thus, *Vacancy* is simply the share of a court's judgeships that are vacant over the expected lifetime of the case. For example, if a single vacancy was present for the months of March through May (i.e. 3 months) in a district with 10 allocated judgeship positions and the average case length is 7 months, a case that ends in May will be recorded as having a vacancy of three months divided by seven months multiplied by 10% for the one missing judge out of ten in the district or 4.29%.

In our sample period there are 16 districts that experience increases in allocated judgeship positions and two that see decreases over this period. In our main specification, we do not change the denominator, judgeship positions, as this number is potentially endogenous.[21] We do, however, include robustness checks that allow the number of allocated judgeships to vary. We form the Senior Status Vacancy and Non-Senior Status Vacancy variables using the same methodology, but with the numerator changed to number of senior status vacancies and remaining vacancies, respectively.

We create two IVs, as mentioned in our empirical strategy section, for judges' senior status eligibility and death. Our instrumental variable for a judge's death takes the value of one if the case was adjudicated within the "effective period" of the death. In constructing this measure we take into consideration the average length of vacancy in our sample, 13 months, as an exogenous measure for duration. Accordingly, if the duration of the case falls within a 13 months period after a death, the variable *Death* is coded as one, otherwise it is zero. Using this definition, the variable *Death* is equal to one for 22,243 of the analyzed cases. Similarly, we define our IV for *Eligible* as a one if the any part of the case being adjudicated falls in the 13 month period after a judge becomes eligible for senior status. There are 231,850 cases for which the variable *Eligible* is coded as a 1.

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[21] The results do not change when actual judgeship positions, rather than initial number, are used to form the vacancy measure, as we show in a robustness check in our Online Appendix.

In our analysis, we also include controls for district level attributes that may affect sentencing. These include a measure for caseloads composed of weighted criminal and civil filings; the percent the district was appointed by Democratic presidents, the median GHP scores for the district, the percentage of judges who were on the bench prior to the introduction of the Guideline system, the number of magistrate judges, year and district binary variables, and a time trend. Table 3 includes a description and the source of all variables used in the analysis. Table 4, provides summary statistics of our key variables for our dataset divided by our measure *Vacancy*. This table shows there are differences in characteristics between district-time observations with vacancies and those without. *Vacancy* is shown to be correlated with lower Guideline levels, indicative of a different criminal population. Additionally, dissimilar allocations of race and distributions of caseloads are indications of differences between these types of district-time observations.

1.7 Results

In general our results support our hypotheses that vacancies affect the severity of punishment as measured by sentence length and downward departures. Vacancies also affect the distribution of sentences causing judges to cluster their sentences at the Guideline minimum. This along with departing less frequently provides some evidence that when judges are faced with vacancies, they may avoid downward departures and opt instead to lower sentences by using Guideline minimums. The results also show that the type of vacancy matters with non-senior status vacancies having a significant effect on decision-making. Table 5 reports our first stage estimates for all case outcome dependent variables for both a judge's death and eligibility for senior status. The first stage is extremely significant for both judges' passing away and becoming senior status eligible with t-statistics of 3.24 and 5.71 along with F-statistics of 10.99 & 23.54 respectively. Higher estimates are

apparent after the measures of vacancy are split among senior and non-senior status vacancies (t-stats of 4.74 and 6.88 and F-stats of 10.99 and 38.31 respectively).

Table 6 reports estimates of the effect of vacancies on our first dependent variable for case outcomes which is the sentence length in months in prison. Columns 1 and 2 report estimates from the district- and year-fixed effects model and columns 3 through 5 report our 2SLS estimates. Column 1, which pools both types of vacancies together, reveals that vacancy intensity has no significant impact on sentencing decisions. However, when separately identifying the impact of vacancies by their source, column 2 reports that although vacancies caused by senior status judges do not affect the lengths of sentences, vacancies from judges' deaths lead to longer sentences in district courts. Specifically, a percentage point increase in vacancy leads to a 0.1 month increase in sentence length. This means, for example, that a court with the mean allocated judgeships of 7, a vacancy spanning the entire length of the case will result in an increase in sentence length by 1.1 months.

Our IV results in columns 3 and 5 show that the estimates from the district and year-fixed effects model in column 2 are biased downwards. In column 3, we find that a one percentage point increase in vacancy rate is associated with a 0.2 months increase in sentence length. This means that a court with a 10 percent vacancy would result in an additional 2 months for each sentence rendered. In columns 4 and 5, we distinguish between vacancies caused by a judge taking senior status (*Senior vacancy*) and all other types of vacancies (*Non-senior vacancy*). Our 2SLS estimates show that the lengthening of sentences is only associated with vacancies arising from non-senior eligibility status of judges. As shown in column 4, we cannot reject that sentence totals are not affected by senior status vacancies in the courts. As argued previously, senior status vacancies appear to have minimal impact on court outcomes compared to complete departures from the bench. The point estimates in column 5 are different by a factor of greater than six.

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In order to better identify the potential mechanisms behind vacancies' impact on sentence length, we examine the impact of vacancies on our second dependent variable which is a judges' propensity to deviate downward from the federal sentencing Guidelines. We focus on downward rather than upward departures because downward departures constitute 98% of all departures outside the proscribed Guideline ranges appearing in 40% of all cases. Recall that each judge has the ability to sentence outside the proscribed sentencing Guidelines for statutory reasons specified in the Guidelines such as assisting the prosecutor or for certain offender characteristics such as youthfulness or lack of education. Such departures, however, can be reviewed by the courts of appeals. These institutional rules mean that departures from Guidelines require judges to take more time to document the specific reasons for the departure and to prepare statements to counter arguments which may appear in appeals of their decisions (Epstein, Landes and Posner 2012; Tiede 2009; author interviews). As such, increasing vacancies may motivate judges to depart less frequently from the Guideline ranges in order to save time.

Table 7 provides results from our analysis examining vacancies' impact on our second dependent variable, judges' propensity to depart downward from the Guideline ranges. *Downward departure* equals one if the judge departed downward from the Guideline ranges and zero otherwise. In our fixed effects regression, we find an insignificant effect of vacancies on downward departures. However, estimates in column 2 suggest that the results may differ across the two types of vacancies. A negative and significant point estimate on non-senior vacancies suggests that non-senior vacancies result in fewer departures from the given Guideline range. We implement our judge's death instrument in column 3 and find that the fixed effect estimate is biased downwards. The point estimate on vacancy shows that judges are about 0.4 percentage points less likely to depart in districts for each one percentage point increase in vacancy level or 4% for a court with 10% vacancies. In column 5, results show that non-senior vacancies result in fewer departures while

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senior status vacancies have little to no effect on the decision to depart from the Guidelines. Our Chi-square test of difference of coefficients on Senior Vacancy and Non-Senior Vacancy is significant at the 10%-level.

Table 8 examines our third dependent variable for sentencing outcomes which is judges' propensity to cluster the sentencing decision at the absolute minimum of the proscribed Guideline range. *Guideline Clustering* is a variable equal to one if the sentence falls at the lowest possible value *within* the Guideline ranges and zero otherwise. We posit that judges who are employing the time-saving tactic of departing less frequently might cluster their decisions at the lower bound of the given Guideline to still allow for a lower sentence without the extra work associated with justifying it. Here, comparing the fixed effects specifications 1 and 2, illustrates that there might be differential effects among the types of vacancy, but we do not find significant point estimates even after dichotomizing the types of vacancies. However, column 3 reports a significant IV estimate of 0.394 at the p<.001 level. This point estimate, combined with our previous estimate on departure rates reveals that an average district with 7 judgeships will experience slightly over a 4% decrease in downward departures for every 10 percent of vacancies, which is now found in the clustering of sentences at the Guideline minimum. Strikingly, these two point estimates differ by only .02 points. This supports a theory that judges are using time saving techniques in sentences when they are overburdened by vacancies. Judges, who in the presence of an adequate amount of time, would typically depart from the relevant Guideline, are instead sentencing as low as they can within the Guideline range without creating extra work. Column 4 reports a negative point estimate on senior status vacancies, which is not significantly different from zero. The test of the difference between the two estimates is significant at the 5% level, which bolsters our conclusion that the judicial decision-making is affected more by non-senior vacancies than by the assumption of senior status.

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We provide additional information in an Online Appendix. The full results for Table 5 are found in the Online Appendix A1. Alternative specifications for our main models that allow the denominator of judgeships to vary rather than be fixed at their allocated level in 1999 are found in Online Appendix A2. We also provide some alternative results for a slightly different measurement of *Vacancy* in Online Appendix A3.[22] Under these alternative specifications, the sign and the significance of variables remain the same and magnitudes of the effect increase slightly. Online Appendix A4 provides the results analyzing the effect of vacancies for specific types of crime types on our dependent variables of Departures and Clustering at the Guideline minimum.

1.8 Heterogeneity

Another potential time saving mechanism available to judges is to employ a decision making rule based on focusing on general, as opposed to specific, observables of the case. For instance, judges might focus on character traits of the defendant which are easily revealed rather than distinguishing the factual intricacies of a case. We use three defendant characteristics to test for such heterogeneity among vacancies including gender, age, and criminal history. Gendraeu et al. (1996) finds that gender, age and criminal history play an important factor in predicting recidivism with criminal history playing the largest role among the four. Presumably, judges know of this fact as they often see repeat offenders in their courtroom.

In order to test whether vacancies cause judges to focus on simple observables, we interact each of the three defendant attributes (i.e. gender, criminal history and age) with our measure of vacancy in separate regressions. In these, we focus only on non-senior vacancy, because this was the category of vacancies shown to have significant impact on sentencing outcomes in our previous

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[22] Sentence length is 0 when defendants receive an alternative punishment to incarceration and constitute 14% of the cases in our dataset. In two separate regressions, we windsorize zeros to be 1 month for all such observations, then regress all observations of sentence level above 1 month.

results. Judges might reduce their risk taking behavior along the gender margin as they do not want to risk under-sentencing a defendant who might become a repeat offender. Because of stark statistical gender differences in recidivism rates, judges may treat defendants differently based on their gender when they are overburdened by caseloads. Results in Column 1 in Table 9 show no evidence of dissimilar effects of vacancies across genders for sentence lengths. However, we do find significantly different results for *Departure* and *Guideline Clustering* on non-senior vacancies in columns 4 and 7. These coefficients expose judges' tendencies in overburdened districts to depart downward less and to cluster at the Guideline minimum more for males compared to females.

Similarly, we test whether criminal history[23] and age play a role. For our interaction terms of vacancy with criminal history, we find patterns similar to our findings on gender. Judges employed in districts with higher levels of vacancies do not sentence defendants to significantly different sentences (column 2), but they are less likely to depart downward from Guidelines (column 5). As far as age, we find that age exhibits seemingly opposing effects. On one hand, sentence lengths are decreasing with age in the presence of vacancies, which appears to be evidence of judges' preference for lower sentences for older individuals who are less likely to offend repeatedly. However, judges' rates of downward departure also decrease with age (column 5) and Guideline clustering increases with age (column 9). We speculate that several forces could be at play here in addition to judges' reliance on statistical discrimination such as their natural tendency to be more merciful on younger defendants. Additionally, nonlinearities in the effect of vacancies across age groups may exist, but we are limited in testing this due to power constraints.

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[23] There are six categories of criminal history each relating to a certain number of attained points. Each sentence gives at minimum one criminal history point (this for sentences less than six months) and at maximum six criminal history points. We define a high criminal history as being in at least the third category. The second category of criminal history refers to a defendant who has acquired 2-3 points and is the highest category someone can be in while still having only one prior conviction.

Overall, these results provide some credible evidence to support the hypothesis that judges may not simply reduce the time spent on each case in response to an increased workload, but that they are concurrently relying on general, easily accessible information about defendants in their sentencing decisions and statistically discriminating against defendants from populations with higher rates of recidivism (individuals with prior criminal records, men, and young offenders). In turn, federal vacancies may disproportionately affect certain subsections of the population more than others, perhaps exacerbating inequalities already present in the criminal justice system.

1.9 Implications and Conclusion

Judicial vacancies are thought to be one of the largest challenges facing the federal judiciary. While most scholars and policymakers concede that vacancies cause large strains on the judiciary, few if any quantify how these strains affect the adjudication of cases. Using unique data and an instrumental variables strategy, we estimate the casual effect of vacancies on sentences. In contrast to conventional wisdom which suggests that vacancies may cause judges to take short cuts that lessen the severity of the punishment, we find that cases in districts facing vacancies result in judges giving higher sentences by assigning more months in prison or departing downward from federal Guidelines *less* frequently. We also find evidence that in taking shortcuts to determine appropriate sentences, judges may rely more on general characteristics of defendants than they do in settings in which they are less strained by caseloads.

Sources of judicial vacancies also matter when considering their effect on court decisions. Officially, a vacancy arises when there are fewer active judges than the number authorized by Congress. However, not all vacancies are created equally. Most vacancies arise due to judges assuming senior status in which they do not exit the court, but continue working in the court, sometimes with reduced caseloads. Vacancies not associated with the assumption of senior status

result in the complete exit of a judge from service in the district court. Our results on the effect of vacancies on punishment show varying results that depend on how the vacancies were created. While vacancies resulting from judges taking senior status have little effect on punishment, vacancies caused by other reasons have significant effects on sentences.

The results suggest that judges use different decision-making strategies in assigning punishment when they are faced with a heavier workload resulting from vacancies in their district. Our results suggest that judges use short cuts to save time in sentencing when facing understaffed courts. These short cuts generally cause judges to depart downward from federal Guidelines less frequently. As suggested by Esptein et al. (2013) and Tiede (2009), departing less from Guidelines requires less paperwork and less time. Rather than downward departures, judges tend to cluster their sentences at the lower end of the Guideline range thus avoiding a decision outside the Guideline ranges which would require more time to justify.

We also find evidence that judges may rely more on general and easily accessible defendant characteristics in determining their sentences when there are more vacancies in their district. Testing for heterogeneous effects of vacancies across three common defendant characteristics related to recidivism (i.e. gender, age and criminal history), we find substantially different effects in how vacancies affect sentencing outcomes. The courts are less inclined to be lenient on repeat offenders, which is a simple cue on which judges can focus. These results are consistent with findings that show people under stress are more likely to focus their attention on simple straight forward cues rather than the myriad of complexities inherent in any case scenario.

Findings from this study provide evidence that concern about judicial vacancies is warranted and that they do have a significant impact on courts and their workings. However, in contrast to much of the literature which suggests that vacancies result in lax procedures and lower punishments, we find that vacancies result in more severe sentences. Further, the impact is only apparent from

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vacancies unrelated to judges assuming senior status. These results in turn support Chief Justice

Roberts' and others' belief that we cannot underestimate the role of senior status judges in our lower

courts. The challenge now is for policymakers to respond by making the nomination and

appointment process for federal judgeships more efficient and swift especially when the vacancies

are created by reasons other than senior status assumption.

Table 1.1: Judges' Ideology: Deceased vs All Others

	Deceased Judges	Active Judges	T-test
Democrat	0.286	0.402	0.88
	(0.125)	(0.017)	
GHP scores	0.163	0.126	0.33
	(0.093)	(0.015)	
Observations	14	831	

Notes: Democrat is a binary variable taking the value of one if the nominating president was a Democrat. GHP score is the Giles, Hettinger, Pepper (2001) scores of district court judges between -1 and 1. Only judges that are active at any point during the sample period are included. Standard errors are in parentheses. ***p<0.01, **p<0.05, * p<0.10.

Table 1.2: Judges' Ideology: Senior Status Eligible vs All Others

	Eligible Judges	Active Judges	T-test
Democrat	0.235	0.440	4.91***
	(0.033)	(0.019)	
GHP scores	0.241	0.099	3.91***
	(0.028)	(0.017)	
Observations	166	689	

Notes: Democrat is a binary variable taking the value of one if the nominating president was a Democrat. GHP score is the Giles, Hettinger, Pepper (2001) scores of district court judges between -1 and 1. Only judges that are active at any point during the sample period are included. Standard errors are in parentheses. ***p<0.01, **p<0.05, * p<0.10.

Table 1.3:   Variable Descriptions and Sources

Variable name	Description
	DEPENDENT VARIABLES
Sentence length	Sentence length is measured as the number of months in prison a defendant is sentenced after conviction. Source: United States Sentencing Commission data files 1999 to 2006 found at http://www.ussc.gov/Research_and_Statistics/Datafiles/index.cfm (USSC data files).
Downward departure	Downward departure is measured as a 1 if the judge departed downwards from the appropriate Guideline ranges and 0 otherwise. Source: USSC data files.
Clustering at the Guideline minimum	Clustering at the Guideline Minimum receives a 1 if the judge gave a sentence in a case that was at the Guideline range minimum and 0 otherwise. Source: USSC data files.
	INDEPENDENT VARIABLES
	Case level
Crime seriousness or offense level	The United States Sentencing Commission (USSC) under the Guidelines classifies each crime as to its seriousness ranging from 1 the lowest level of seriousness to 43 the highest level. We include fixed effects for each level.[24] Source: USSC data files.
Crime	The USSC also classifies each crime as to subject area. Our analysis includes the following crime types: murder and sexual crimes, theft, drug crimes, immigration offenses, harassment/domestic violence, public safety offenses, and conspiracy.[25] Source: USSC data files
Guideline minimums and maximums	We include continuous variables for both the Guideline minimum and maximum for each crime sentenced. Source: USSC data files.
Criminal history	Criminal history is a categorical variable in the data ranging from 1-6 based on the defendant's prior criminal record. In level one, a defendant has 0 to 1 prior convictions and at level 6 a defendant has at least 5 prior convictions. Fixed effects are included for each of these levels. Source: USSC data files.
Female	Female is coded 1 if the defendant is female and 0 otherwise. Source: USSC data files.
Race	Race is a categorical variable of defendants' race and includes six categories.[26] Source: USSC data files.

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[24] Scholars have shown that the facts of a case, such as crime seriousness, type of crime, and criminal history as well as other facts, affect sentencing (Boylan 2004; Fischman and Shanzenbach 2012; Anderson, Kling and Stith 1999; Waldfogel 1998; Ulmer and Light 2010, 2011; Scott 2010).

[25] Focusing on these crimes, removes 32,468 observations from our sample.

[26] Scholars show that the race of the defendant causes sentences to vary (Mustard 2001; Fischman and Schanzenbach 2012; Free 1997; Pasko 2002; Schanzenbach and Tiller 2006; Freeborn and Hartmann 2009; Kautt 2002; Kautt and Spohn 2002; Steffensmeier and Demuth 2000).

Age	Age refers to the age of defendant at the time of sentencing. Source: USSC data files.

District level

Vacancy	Vacancy is calculated as follows:

$$Vacancy_{ijt} = \ \frac{1}{t}\sum\nolimits_1^t \frac{\text{\# of vacant judgeships ijt}}{\text{allocated judgeships ij}}$$

Were *t* is the time period between when a case is filed and when it concludes with a final sentence using statistics on case averages, *j* is the district of interest, and *i* is the individual case. Source: Author construction with data from the U.S. Courts website at http://www.uscourts.gov/JudgesAndJudgeships/FederalJudgeships.aspx. (U.S. Courts)

Vacancy IV (death)	A binary instrumental variable taking the value of 1 if the case was adjudicated within the "effective period" of the district court judge's death and 0 otherwise. Effective period is defined as the average length of a vacancy in the sample, 13 months, plus the yearly average across all districts of the time it takes to adjudicate a case.[27] Source: Author construction with data from Federal Judicial Center (FJC) Biographical Directory of Federal Judges (http://www.fjc.gov/history/home.nsf/page/judges.html.)
Vacancy IV (eligible)	A binary instrumental variable for senior status eligibility equals one in the month a judge becomes eligible through the effective period and zero otherwise. Source: Author construction with data from the FJC Biographical Directory.
Weighted Criminal and civil filings per judge	Case filings per judge are fiscal year averages of case filings divided by the allocated number of judgeships for each district. Judgeships are indexed to the 1999 level. We dichotomize this measure into the averages for criminal and civil case filings for the year a case concluded. Source: Author construction with data from U.S. Courts.
Percent Democrat per district	Similar to Randazzo (2010), we construct a measure for the percent of the active district court judges who are affiliated with the Democratic party at the time the case concludes (using yearly averages for filing times). The political affiliation is determined by the party of the nominating president.[28] Source: Author construction with data from the FJC Biographical Directory.
Median Giles-Hettinger-Peppers'	We use updated common space scores to calculate a district's median Giles-Hettinger- Peppers' or GHP score are based on the judges' ideology in the

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[27] For example, if a death occurred in January 2004 with an average case length of 7 months, the variable 'Death' would be coded as 1 from January to August 2005.

[28] Scholars have shown that sentencing is affected by the party or ideology of the judges' nominating president where individual judges' names are listed in the data (Cross and Tiller, 1998; Stidham, 1996; George, 2001; Brudney, Schiavoni, and Merrit, 1999; Gotschall, 1986; 1986: Carp, Manning, and Stidham, 2009; Rowland and Carp 1996) as well as aggregated at the district level (Randazzo 2010).

scores per district (GHP scores)	district. This measure incorporates measures for the political leaning of the nominating president and the two senators in the state of the nominated judge. In our regression, we include a control variable for the average GHP score among the active judges in the each district each month.[29] Source: Giles, Hettinger and Peppers (2001) and author update.
Judge prior to 1989	Judge prior to 1989 refers to the percentage of the active judges in the district prior to the Supreme Court's decision in *Mistretta v. United States* (1989) as to the Guideline's constitutionally affirmed in January 1989. This variable is calculated in relation to the date a case concludes. Boylan (2004) concludes that the enactment of the Guidelines had an effect on judges' decisions to take senior status. Thus, we might expect this as a potential source of endogeneity as judges who were on the bench prior to the introduction of the Guidelines might sentence defendants differentially than their colleagues. Source: FJC Biographical Directory and author construction.
Magistrate judges	We include the number of magistrate judges by district per year. Source: Federal Magistrates Association.
Year and district	We include dummies for sentence year in our fixed effects analysis.[30] Source: USSC data files.
District	We include dummies for each judicial district in our fixed effects analysis. Source: USSC data files
Trend	A monthly trend variable captures systematic changes over time. Source: USSC data files.

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[29] Judges' ideology, taking into account the unique nomination and senatorial courtesy procedure, is an oft cited determinant of judicial decision-making (Giles, Hettinger and Peppers 2001). Since there are no senators in the District of Columbia, we use the common space scores for the nominating president for those DC judges.

[30] Location of the district court also affects sentencing (Bibas 2005; Braniff 1993).

Table 1.4: Summary Statistics

Variable name	Non-vacancy	Vacancy	Significant difference t-test (p-value)
	Dependent variables		
Sentence Length	52.98	48.93	18.67
(months)	(.15)	(0.16)	p=0.00
Downward Departure	0.39	0.42	-23.21
	(0.00)	(0.00)	p=0.00
Clustering at the	0.37	0.36	4.91
Guideline minimum	(0.00)	(0.00)	p=0.00
	Independent variables		
Vacancy	0	0.11	
	(0.00)	(0.00)	
Senior Vacancy	0	0.09	
	(0.00)	(0.0002)	
Non-Senior Vacancy	0	0.02	
	(0.00)	(0.0001)	
	Case level controls		
Guideline minimum	96.13	88.74	4.13
	(1.24	(1.25)	p=0.00
Guideline maximum	184.52	159.7	8.02
	(2.18)	(2.12	p=0.00
Age	33.65	34.18	-16.43
	(0.02)	(0.02)	p=0.00
Race: White	0.70	0.67	18.45

	(0.001)	(0.001)	p=0.00
Race: Black	0.26	0.29	-17.71
	(0.001)	(0.001)	0.00
Race: Other	0.02	0.03	-25.15
	(0.0002)	(0.0003)	(0.00)
District level controls			
Weighted criminal filings	186.62	152.62	72.01
	(0.34)	(0.33)	p=0.00
Weighted civil filings	390.93	428.20	-70.89
	(0.27)	(0.47	(0.00
Percent Democrat per district	0.48	0.49	-13.56
	(0.005)	(0.0005)	p=0.00
Median GHP scores	0.11	0.05	55.20
	(0.0007)	(0.001)	P=0.001
Judge prior to 1989	0.22	0.22	-3.92
	(0.0005)	(0.0006)	p=0.0001
Magistrate judges	7.43	9.33	-0.0014
	(0.009)	(0.0097)	p=0.00
Observations	232,547	195.354	

Notes: The table contains the means and standard deviations in parentheses for our main variable of interest. Those in the *Vacancy* column are statistics decided in districts which have a *Vacancy* measure greater than zero. Sentence length is the length of sentence in months in prison. *Downward departure* is a dummy variable taking a 1 if the judge departed below the crime's Guideline minimum and 0 otherwise. *Clustering at the Guideline Minimum* is a binary variable taking a 1 if the sentence is at the lowest value of the Guideline range for a particular crime and 0 otherwise.

Table 1.5: First Stage Estimates

	Vacancy		Non-Senior	Senior
	(1)	(2)	(3)	(4)
Death	0.073		0.071***	0.001
	(0.023)		(0.015)	(0.010)
Eligible		0.039***	0.001	0.038***
		(0.007)	(0.003)	(0.005)
F-stat	10.99	38.31	18.33	23.54
Observations	424,465	424,465	424,465	424,465

Notes: Vacancy is measured in percentage points. Each panel of each column reports the results of a separate regression. In addition to the reported variables, each regression controls for case attributes (crime seriousness, crime, Guideline minimums and maximums, defendants' criminal history, gender, race, and age), district level attributes (weighted criminal and civil filings per judge, percent district democrat, median GHP scores, percent judges appointed prior to Guideline enactment, and number of magistrate judges) as well as controls for year, district effects and a time trend. The reported F-statistic is associated with the test of the joint significance of the instruments in columns 3 and 4. Standard errors are clustered at the district level (90 clusters). ***$p<0.01$, **$p<0.05$, * $p<0.10$.

Table 1.6: Sentence Length (months in prison)

	OLS		2SLS		
	(1)	(2)	(3)	(4)	(5)
Vacancy	2.455		20.07**	4.566	
	(1.949)		(8.955)	(7.630)	
Non-senior vacancy		10.80**			
		(4.255)			20.57**
Senior vacancy		0.519			(8.233)
		(2.083)			3.882
					(7.910)
Death instrument	No	No	Yes	No	Yes
Eligibility instrument	No	No	No	Yes	Yes
Observations	424,465	424,465	424,465	424,465	424,465

Notes: *Sentence length* is in months in prison. *Vacancy* is measured in percentage points. Each panel of each column reports the results of a separate regression. In addition to the reported variables, each regression controls for case attributes (crime seriousness, crime, Guideline minimums and maximums, defendants' criminal history, gender, race, and age), district level attributes (weighted criminal and civil filings per judge, percent district democrat, median GHP scores, percent judges appointed prior to Guideline enactment, and number of magistrate judges) as well as controls for year, district effects and a time trend. Standard errors are clustered at the district level (90 clusters). **p<0.01, **p<0.05, *p<0.10.

Table 1.7: Downward Departures (i.e. sentences below the Guideline minimum)

	OLS		2SLS		
	(1)	(2)	(3)	(4)	(5)
Vacancy	0.00567		-0.399***	0.0647	
	(0.0401)		(0.148)	(0.152)	
Non-senior vacancy		-0.175*			-0.414***
		(0.0937)			(0.138)
Senior vacancy		0.0475			0.0852
		(0.0396)			(0.166)
Death instrument	No	No	Yes	No	Yes
Eligibility instrument	No	No	No	Yes	Yes
Observations	424,465	424,465	424,465	424,465	424,465

Notes: *Downward departure* is coded as 1 if the sentence was below the Guideline range and 0 otherwise. *Vacancy* is measured in percentage points. Each panel of each column reports the results of a separate regression. In addition to the reported variables, each regression controls for case attributes (crime seriousness, crime, Guideline minimums and maximums, defendants' criminal history, gender, race, and age), district level attributes (weighted criminal and civil filings per judge, percent district democrat, median GHP scores, percent judges appointed prior to Guideline enactment, and number of magistrate judges) as well as controls for year, district effects and a time trend. Standard errors are clustered at the district level (90 clusters). **p<0.01, **p<0.05, *p<0.10.

Table 1.8: Clustering at the Guideline Minimum

	OLS		2SLS		
	(1)	(2)	(3)	(4)	(5)
Vacancy	-0.014		0.394**	-0.151	
	(0.042)		(0.172)	(0.130)	
Non-senior vacancy		0.094			0.412***
		(0.079)			(0.154)
Senior vacancy		-0.039			-0.175
		(0.045)			(0.144)
Death instrument	No	No	Yes	No	Yes
Eligibility instrument	No	No	No	Yes	Yes
Observations	424,465	424,465	424,465	424,465	424,465

Notes: *Clustering at the Guideline minimum* is coded as 1 if the sentence was at the lowest point of the *within* Guideline range and 0 otherwise. *Vacancy* is measured in percentage points. Each panel of each column reports the results of a separate regression. In addition to the reported variables, each regression controls for case attributes (crime seriousness, crime, Guideline minimums and maximums, defendants' criminal history, gender, race, and age), district level attributes (weighted criminal and civil filings per judge, percent district democrat, median GHP scores, percent judges appointed prior to Guideline enactment, and number of magistrate judges) as well as controls for year, district effects and a time trend. Standard errors are clustered at the district level (90 clusters). **p<0.01, **p<0.05, *p<0.10.

Table 1.9: Heterogeneity by Defendant Characteristics

	Sentence length			Downward departures			Guideline clustering		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Non-senior vacancy	20.81	13.69	43.44***	0.154	-0.379	0.204	-0.109	-0.10	-0.633
	(18.76)	(15.73)	(12.25)	(0.241)	(0.164)	(0.166)	(0.210)	(0.142)	(0.130)
NSVacancy*Male	-0.160			-0.659**					0.601*
	(18.88)			(0.261)					(0.314)
NSVacancy*History		15.89			-.853**			0.950*	
		(28.41)			(0.412)			(0.507)	
NSVacancy*Age			-28.83***			-0.598***			0.597***
			(11.13)			(0.182)			(0.228)
Observations	424,465	424,465	424,465	424,465	424,465	424,465	424,465	424,465	424,465

Note: Each panel of each column reports the results of a separate regression. Male, History, and Age are binary variables for male, having a criminal history over 2 points and being over the age of 25 respectively. Standard errors are clustered at the district level (90 clusters). ***p<0.01, **p<0.05, *p<0.10.

Appendix 1.1

For the main regressions I define vacancy holding the number of judgeships fixed to their allocated level in 1999. Below I allow the denominator, congressionally allocated judgeships, to vary in our calculation of percent vacant in each district each month. For these regressions I observe qualitatively the same results. Quantitatively, the effect of a vacancy in the court increases in terms of increased sentence totals, probability of not departing and probability of sentencing at the Guideline minimum.

Table A1.1.1: Sentence Totals Allowing Judgeships to Change (2SLS)

	(1)	(2)	(3)
Vacancy	29.05**	5.14	
	(13.39)	(9.29)	
Senior Vacancy			5.27
			(9.24)
Non-Senior Vacancy			29.16**
			(12.06)
Death Instrument	Y	N	Y
Eligible Instrument	N	Y	Y
Observations	424,465	424,465	424,465

Notes: Vacancy is measured as a percent of total allocated judgeships for that district in the particular year. Standard errors are clustered at the district level (90 clusters). *** p<0.01, ** p<0.05, * p<0.1

Table A1.1.2 : Downward Departures w/ Changing Judgeships (2SLS)

	(1)	(2)	(3)
Vacancy	-0.561**	-0.0323	
	(0.237)	(0.174)	
Senior Vacancy			-0.0286
			(0.177)
Non-Senior Vacancy			-0.562***
			(0.214)
Death Instrument	Y	N	Y
Eligible Instrument	N	Y	Y
Observations	424,465	424,465	424,465

Notes: Standard errors are clustered at the district level (90 clusters). *** p<0.01, ** p<0.05, * p<0.1

Table A1.1.3 : Guideline Clustering w/ Changing Judgeships (2SLS)

	(1)	(2)	(3)
Vacancy	0.563**	-0.113	
	(0.232)	(0.146)	
Senior Vacancy			-0.109
			(0.148)
Non-Senior Vacancy			0.565***
			(0.218)
Death Instrument	Y	N	Y
Eligible Instrument	N	Y	Y
Observations	424,465	424,465	424,465

Notes: Standard errors are clustered at the district level (90 clusters). *** p<0.01, ** p<0.05, * p<0.1

Appendix 1.2

Below I allow effects of vacancy to differ by vacancy type. Here I split up the types of crime into the following categories and regress departures and guideline clustering individually for each category: murder, theft, drugs, public safety, immigration and conspiracy crimes. I find similar coefficients for most regressions, but immigration cases stand out with both large coefficients and significance levels. Senior vacancies also return mild significance levels for guideline clustering on drug crimes and departures in cases of public safety.

Table A1.2.1 : Departures and Guideline Clustering in Murder and Theft Cases (2SLS)

	murder		*theft*	
	departure (1)	guideline (2)	departure (3)	guideline (4)
Senior Vacancy	0.121	-0.380	-0.0676	0.0701
	(0.386)	(0.355)	(0.126)	(0.119)
Non-Senior Vacancy	-0.411	0.317	-0.357*	-0.0870
	(0.306)	(0.294)	(0.210)	(0.188)
Observations	16,526	16,526	61,626	61,626

Notes: The first two columns refer to crimes in which the highest offense given was under the category of murder whereas the second two refer to crimes of theft. Standard errors are clustered at the district level (90 clusters). *** p<0.01, ** p<0.05, * p<0.1

Table A1.2.2 : Departures and Guideline Clustering in Drug and Public Safety (2SLS)

	drugs		public safety	
	departure (5)	guideline (6)	departure (7)	guideline (8)
Senior Vacancy	-0.218	0.207*	0.356*	-0.267
	(0.175)	(0.120)	(0.196)	(0.212)
Non-Senior Vacancy	-0.246	0.266*	-0.365**	0.0353**
	(0.164)	(0.158)	(0.151)	(0.158)
Observations	187,422	187,422	40,013	40,013

Notes: The first two columns refer to crimes in which the highest offense given was under the category of a drug offense whereas the second two refer to crimes of public safety. Standard errors are clustered at the district level (90 clusters). *** p<0.01, ** p<0.05, * p<0.1

Table A1.2.3 : Departures and Guideline Clustering in Immigration and Conspiracy (2SLS)

	immigration		*conspiracy*	
	departure (9)	guideline (10)	departure (11)	guideline (12)
Senior Vacancy	-0.668	-0.581	-0.0595	0.395
	(0.523)	(0.482)	(0.354)	(0.277)
Non-Senior Vacancy	0.608***	0.563***	-0.491	0.567
	(0.208)	(0.207)	(0.432)	(0.418)
Observations	87,201	87,201	16,312	16,312

Notes: The first two columns refer to crimes in which the highest offense given was under the category of immigration whereas the second two refer to crimes of conspiracy. Standard errors are clustered at the district level (90 clusters). *** p<0.01, ** p<0.05, * p<0.1

Appendix 1.3

In our analysis I check for vacancy's effect on a judge's decision to prosecute a trial and, conditional on that, their decision to convict the defendants. I use yearly data on conviction and prosecution rates by district, thus I aggregate our data on vacancies to a yearly level. Using OLS and 2SLS I find no statistically significant evidence of judges changing their decisions to convict or prosecute based on the presence of vacancies in the judicature. Next I estimate whether judges are changing their timeline to decision based on the vacancy rate. Using median times to conviction, I estimate the time a judge spends on each case. Here I find no evidence for criminal cases but some indication that judges are speeding up their decision time for civil cases.

Table A1.3.1 Conviction and Prosecution (percent of total cases)

	Conviction		Prosecution	
	(1)	(2)	(3)	(4)
		A. Reduced Form – OLS Coefficient		
Vacancy	-5.82		-2.00	
	(4.90)		(6.85)	
Senior Vacancy		-5.96		-1.77
		(5.50)		(8.00)
Non-senior Vacancy		-4.92		-3.51
		(9.87)		(20.18)
		B. 2SLS – Instrumenting for Vacancy with a Death		
1st stage coefficient	0.07***		0.07***	
	(0.02)		(0.02)	
2nd stage - Vacancy	10.55		7.39	
	(30.50)		(35.13)	
Observations	720	720	720	720

Notes: Conviction and prosecution are variables that capture the percentage of cases a judge is presented with that result in conviction and prosecution respectively. In this model we control for weighted measures of criminal and civil filings, the giles rating of the judges, the percent of the district which is democrat, and number of senior status judges. Each control variable is specific to a district at a particular year. Standard errors are clustered at the district level (90 clusters). *** p<0.01, ** p<0.05, * p<0.1

Table A1.3.1 Adjudication Time (months)

	Criminal		Civil	
	(1)	(2)	(3)	(4)
	A. Reduced Form – OLS Coefficient			
Vacancy	-0.33		0.60	
	(1.44)		(0.98)	
Senior Vacancy		-0.87		-0.83
		(1.52)		(1.05)
Non-senior Vacancy		-4.46		-1.45
		(4.19)		(3.74)
	B. 2SLS – Instrumenting for Vacancy with a Death			
1st stage coefficient	0.06***		0.06***	
	(0.02)		(0.02)	
2nd stage - Vacancy	6.28		-18.15*	
	(6.54)		(9.96)	
Observations	720	720	720	720

Notes: In this model we control for weighted measures of criminal and civil filings present in the district for each year. Standard errors clustered at the district level (90 clusters) *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix 1.4

I also windsorize the variable for sentence length. A sizeable portion (i.e. 12%) of adjudicated cases is coded as having a sentence of 0 months. In reality, these defendants have pled down to community service, fines, and parole lengths as punishment rather than time in jail. As a result, I amend the variable sentence length to be 1 month in the case that it is initially coded as 0 months. The regression table below shows that my results do not change qualitatively or otherwise.

Table A1.4.1   Sentence Totals Windsorized

	(1)	(2)	(3)
Vacancy	20.99**	4.45	
	(9.08)	(7.98)	
Senior Vacancy			3.49
			(8.27)
Non-Senior Vacancy			21.36***
			(8.20)
Death Instrument	Y	N	Y
Eligible Instrument	N	Y	Y
Observations	424,465	424,465	424,465

Notes: Standard errors clustered at the district level (90 clusters) *** p<0.01, ** p<0.05, * p<0.1

Appendix 1.5

Below I estimate the effect of vacancies on the designated criminal offense levels of the defendant. I find no effects, suggesting that the type of crime defendants are tried for isn't systematically changing based on vacancy rates.

Table A1.5.1. Results for Criminal Offense Levels (2SLS)

	(1)	(2)	(3)
Vacancy	0.44	-1.75	
	(1.93)	(1.57)	
Senior Vacancy			-1.847
			(1.63)
Non-Senior Vacancy			0.50
			(1.91)
Death Instrument	Y	N	Y
Eligible Instrument	N	Y	Y
Observations	424,465	424,465	424,465
R-squared	0.401	0.401	0.401

Notes: Offense level is a variable which ranges from 0 to 53 depending on the severity of the crime. Standard errors clustered at the district level (90 clusters) *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix 1.6

The following is the first stage for Table 8 in our regular analysis.

Table A1.6.1 First Stage of Interaction

	Non-Senior			Non-Senior Interaction			Senior		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Death	0.071***	0.066***	0.075***	-0.005	-0.001	-0.009	0.000	0.000	0.000
	(0.015)	(0.014)	(0.016)	(0.004)	(0.002)	(0.003)	(0.011)	(0.010)	(0.010)
Eligible	0.001	0.001	0.001	0.001	0.001	0.001	0.038***	0.038***	0.038***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.001)	(0.002)	(0.006)	(0.006)	(0.006)
Death*Male	0.000			0.077***			0.002		
	(0.002)			(0.015)			(0.001)		
Death*History		0.013***			0.085***			0.002	
		(0.005)			(0.017)			(0.004)	
Death*Age			-0.005***			0.081***			0.000
			(0.001)			(0.015)			(0.002)
F-stat	22.52	23.18	22.61	24.81	24.19	28.1	46.85	46.86	46.85
Observations	424,465	424,465	424,465	424,465	424,465	424,465	424,465	424,465	424,465

Notes: Columns 4-6 have dependent variables that are interactions with Non-Senior Vacancy and the characteristic of the defendant specified (Column 4-Male, 5-History, 6-Age). Standard errors are clustered at the district level (90 clusters). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Chapter 2

Does the Federal Judicial Emergency Policy Reduce Judicial Vacancies? Evidence from a Regression-Discontinuity Design

2.1. Introduction

Judicial vacancies are growing in prevalence among federal district courts nationwide (Ashcroft 2002; Roberts 2010) and the duration of these vacancies have been increased dramatically. For the period between 2000 and 2014, the number of vacancies in federal courts ranged from 28 in 2004 to a local peak of 96 as of February 2014, accounting for 4 to 15% of the active district court judgeship positions per year. Moreover, yearly data masks the fact that monthly vacancies in judicial districts could be significantly higher and more prevalent in some districts than others. Coupled with the rise in the number of judicial vacancies, there has been a well-documented increase in the length of time vacancies remain unfilled. For example, under the Obama presidency, district court vacancies have persisted for 610 days (Hardin 2012) on average, far outpacing Clinton at 447 and George W. Bush at 420. There are also extreme outliers such as a vacancy lasting 2,923-days in the Eastern District of North Carolina and three vacancies lasting over 1,700 days as of February 2013.[31] The lengths of vacancies have often been associated with the increasingly contentious political environment impeding the nomination and approval process of filling judicial seats (Wheeler and Binder 2011).

In an effort to expedite the appointment process in districts that are deemed especially in need of a speedy confirmation, the "emergency" distinction was created at the Judicial Conference[32] in March 1988[33] and revised in March 1999[34]. This "emergency" label was aimed at filling the role as a red flag to the President and the Senate to highlight need for their expedited treatment over other vacancies. The creation of the emergency designation was justified by the reported "deleterious

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[31] In the Courts of a Appeals an extreme outlier is found in the U.S. Court of Appeals for the Ninth Circuit which has had an unfilled judgeship lasting 3,200 days due to a dispute between senators from California and Idaho concerning a nominee's state of origin

[32] In 1922, at the urging of Chief Justice Taft, Congress created the Judicial Conference with the original design to oversee and make recommendations to Congress. In this mandate they were called upon to "prepare plans for the assignment of judges to or from courts of appeals or district courts..." (See United States Code §331.28).

[33] http://www.uscourts.gov/FederalCourts/JudicialConference/Proceedings/Proceedings.aspx?doc=/uscourts/Federal Courts/judconf/proceedings/1988-03.pdf

[34] http://www.uscourts.gov/FederalCourts/JudicialConference/Proceedings/Proceedings.aspx?doc=/uscourts/Federal Courts/judconf/proceedings/1999-03.pdf

effects such vacancies have on the administration of justice" (JCUS-MAR 88, p.31). However, the effect of this policy has been murky at best. Chief Justice John Roberts tried to bring light to the issue in his 2012 year-end report on the federal judiciary, "At the close of 2012, twenty-seven of the existing judicial vacancies are designated as presenting judicial emergencies. I urge the Executive and Legislative Branches to act diligently in nominating and confirming highly qualified candidates to fill those vacancies." While this is an issue with clear political ramifications, no previous research has investigated whether these emergency distinctions serve their intended goal of expediting the process of judicial nomination and approval.

In this paper, I identify the causal effect of an emergency designation on both the duration of the vacancies and the prima facie characteristics of confirmed judges by exploiting the quasi-experimental variation in emergency designation exposure generated by policy rules governing the use of the emergency label in district courts.[35] As of 1999, federal judicial vacancies are designated as an emergency if they meet any one of the following three criteria: 1) a vacancy in a district court in which its weighted filings[36] were in excess of 600 per judgeship in the previous calendar year, 2) a vacancy in existence for more than 18 months where the weighted filings in the court were between 430 and 600 per judgeship, or 3) a vacancy in any court with more than one authorized judgeship and only one active judge.[37] If the vacancy meets none of these three rules, the emergency distinction should not be applied.

The presence of these cutoffs in vacancy lengths and number of filings in the court system suggests the use of a regression discontinuity (RD) design to estimate the effect of declaring an

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[35] The emergency designation is also present in the Court of Appeals, which we do not examine here.

[36] In calculating this number, the Judicial Conference implements a measure of weighted filings per allocated judgeship position as a means of accounting for differences in the time required for judges to resolve certain types of civil and criminal cases where weights are applied based on the nature of the case. While this measure is adjusted to account for the number of judgeship positions, it might be important to note that it accounts for neither senior status nor magistrate judges in the district.

[37] http://www.uscourts.gov/JudgesAndJudgeships/JudicialVacancies/JudicialEmergencies.aspx

emergency status to a judgeship vacancy -- by comparing the vacancy outcomes in districts just above the 600 weighted filings per judgeship (henceforth WFPJ) cutoff (and therefore more likely to be designated an emergency) to those districts just below the cutoff. I focus solely on this first definition of an emergency as it is the most widely implemented definition and it goes into effect immediately upon the vacancy being announced as opposed to 18 months later. For example, of the 93 vacancies in March of 2014, 18 were defined as emergencies for exceeding the 600 WFPJ cutoff, 8 were emergencies that had exceeded 400 WFPJ past the 18 month threshold and there were no emergencies with only one judge present.[38] I therefore implement the RD strategy using panel data on judicial vacancies in districts near the 600 WFPJ cutoff as defined by the policy rule. I discuss this empirical strategy in more detail in Section III.

My findings show that the emergency distinction has little to no effect on the length of the judicial vacancies in district courts. Specifically, I find no change in the total time to commission of a new judge, or when I split this process into the time to first nomination, confirmation or first hearing date. However, I find some impact of the emergency distinction on the characteristics of the judges who eventually fill those vacancies. Judges who take up office in emergency vacancy positions tend to have graduated from lower ranked law schools but attained marginally higher American Bar Association scores.[39] These results are robust to alternative specifications controlling for quadratic smoothers and non-linear normalization of the duration variables.

2.2 Background and Related Literature

2.2.1 Judicial Vacancies and the Appointment Process

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[38] There were also 11 Circuit Court emergencies

[39] The Standing Committee on the Federal Judiciary, created by the ABA, is made up of 15 members who serve to evaluate candidates for the federal judiciary. After nomination, they score the nominated judge in their qualification for the nominated position.

On March 15, 1988, at its semiannual meeting, the Judicial Conference of the United States noted the adverse effect judicial vacancies of more than 18 months had on courts and litigants and deemed that all such vacancies created judicial emergencies. These types of vacancies initially began being officially documented as emergencies in April 1992 with the goal of limiting vacancy durations in these districts.[40] For this study, I limit my analysis to the study of the first type of emergency, defined by the 600 WFPJ cutoff. Figure 1 graphs the evolution of vacancies and all types of emergencies over my sample period of interest from 2002 to 2014 and demonstrates that emergency vacancies reached their peaks in the past three years (Bannon 2013). Although the period from 2003 to 2009 had relatively few vacancies and emergencies, both have risen dramatically in the following five years. It is also important to note that while the substantial amount of vacancies and emergencies in 2002 and 2003 could be attributed to a large number of newly created positions, none of the vacancies in the last eight years were positions that were newly formed by Congress. Hence, the recent surge in the total count of judicial vacancies and openings designated as emergencies underscore the potential need for a reform in the judicial nomination and appointment process.

On its surface, the process of commissioning a new judge is straightforward, consisting of three stages. In the first stage, the president nominates a candidate for the vacant position, having been advised by the Senate and the Judiciary Committee. At this juncture, senators of the state in which the vacancy resides wield significant power and influence due to their ability to provide a negative blue slip[41] for the nominee in the confirmation proceedings. Post nomination, each senator is presented a blue slip on which they can write down their opinions on the candidate, and the Senate Judiciary Committee historically takes these opinions into consideration. For this reason, the

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[40] http://www.uscourts.gov/JudgesAndJudgeships/JudicialVacancies/ArchiveOfJudicialVacancies.aspx
[41] The practice of blue slips dates back to 1917, but it is of debate how much power the blue slip actually holds. The final say is with the Chair of the Judiciary Committee on whether to put any stock into the message contained on the blue slip.

president consults closely with the two home Senators in choosing a nominee who will be likely to pass the confirmation stage.

After the president has nominated a candidate, that candidate is then vetted at the Senate level. At this leg in the process, the candidate completes a comprehensive questionnaire and faces hearings where the candidate is asked questions inquiring into their legal thought process and the manner with which they would likely fill the role of a judge on the federal bench. In conjunction with the Senate inquiry, the American Bar Association independently evaluates these nominees and presents an opinion on their qualification for the federal position. As alluded to earlier, the home state senators wield significant authority to speed up or hinder the senate approval process. By approving the candidate, senators may be able to expedite the process of confirmation (Bell 2002, Binder and Maltzman 2002). Finally, if the Senate Judiciary Committee moves to a vote and finds a majority of Senate votes[42] in favor, the judge will take the bench. If the vote fails, the process starts over from square one.

While it is typically the case that the judges who make it to the voting stage are easily confirmed, many judges are blocked, filibustered or in some other way hampered in their goal to reach a vote. Filibusters are a major means of blocking a candidate with the goal of ending the bid for confirmation since overcoming filibusters require a Senate vote of cloture of 60 votes. As of 2012, President Barrack Obama had a 76% confirmation rate of federal judicial nominations, with Presidents George W. Bush and Bill Clinton exceeding his confirmation rate at 92% and 81%, respectively. The low confirmation rates during the Obama administration can partially explain the recent increases in vacancies and emergencies.

While the nomination and confirmation process has not changed recently on the surface, the rules of the game in the political arena have dramatically shifted. The tool of the filibuster has only

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[42] The current practice typically requires a de facto 60 votes to pass confirmation stages because of the minority's filibuster ability

recently become a highly used strategy for an opposition party who dislikes a candidate.[43] As a result, the process of nomination and confirmation has greatly lengthened, further adding to the burden of the recent meteoric rise in vacancies and emergencies have had on the judicial system.

Confirmation processes that endure for long durations, even if resulting in eventual confirmation, can dissuade potential candidates from even accepting the nomination. Currently, filibusters on nominations are at an all-time high, forcing 20 clotures to be filed during President Obama's term in office. Prior to Obama's tenure, only 3 clotures had ever been filed. An ABA press release in February 27, 2012 reads, "Washington's partisan gridlock has stymied not just the policy process but also the responsibility of the Senate to give advice and consent in the nomination process. Longstanding vacancies on courts with staggering caseloads impede access to the courts." Partially a result of this increase in partisan politics, in the realm of emergencies, while there were only 13 upon the induction of President Obama, there has been nearly a three-fold increase to 37 as of March, 2014.

2.2.2   Literature Review

Most of the existing research on judicial vacancies has focused on investigating the effects of vacancies on court proceedings. However, only a few studies have examined factors determining the lengths of these judicial vacancies, and there has been no rigorous empirical study assessing the effect of the emergency designation on the confirmation process. However, in light of the growing emergency vacancies, there is a pressing need such a study.

Wheeler and Binder (2011) provide a descriptive summary of the difference in the average duration of emergency vacancies versus non-emergency vacancies and find no significant differences

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[43] While others have given up their bid due to threats of filibuster, the first candidate to be successfully filibustered was Miguel Estrada in 2003 and closely following this nine other candidates were filibustered until George Bush famously threatened the "nuclear option" for judicial confirmation.

in both the Appellate and District Courts. They further show anecdotal evidence that "neither the (Judicial) Conference nor its Administrative Office highlights these (emergency) vacancies…." However, Wheeler and Binder's study is unable to identify the causal effect of emergency designation on the duration of vacancies, because they are unable to fully account for unobserved factors that are both correlated with the likelihood of an emergency designation and lengths of vacancies.

Other related studies have focused on uncovering the factors that significantly affect the process of nomination and confirmation. Many studies note that delays in confirmation will be longer when there is a divided control between the Senate and White House (Bell, 2000; Nixon and Gloss, 2001; Binder and Maltzman, 2002; Binder and Maltzman 2002). Moreover, Bell (2000) finds that each link in chain leading to confirmation – the majority party in the Senate, the President's party, the party of the two Senators in the state of the vacancy, and the Judicial Committee – is essential to the appointment process. If Senate support for the president is low, the majority party in the Senate is small, the judge is an "orphan" nominee[44], or those nominees without a supporting Judicial Committee member,[45] the vacancy duration will be greater than otherwise. In examining the confirmation process in the federal courts, Stratmann and Garner (2004) find that vacancies that occur in states in which their Senators had agenda control or were senior Senators had shorter duration lengths. Nixon and Gloss (2001), in addition to showing a 700% increase in vacancy duration in the Appellate Courts, point out that vacancy durations are abbreviated when there is a filibuster-proof senate, a judge that becomes elevated, or increases in district workload. Their study also argues that females and minorities take longer to confirm, along with vacancies that arise abruptly due to deaths.

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[44] An orphan nominee is one in which the home state Senators are of the opposing party as the nominating President.
[45] Those nominees without a Judicial Committee member from their home state.

The political nature of the nomination and confirmation has also been studied along several lines. Dancey et al. (2012) investigate the types of questions asked during judicial confirmation hearings. They argue that hearings revolve less around the nominees themselves and more around political and ideological battles between the parties. Moreover, Sen (2012) points out that while ABA ratings are extremely predictive of a speedy confirmation process along with confirmation, they are not at all predictive of any outcome measures after confirmation. Lott (2005) finds that higher-quality judges, measured ex post by their output once on the bench, take much longer to get confirmed. These findings might inform my results as I study the impact on judge quality.

I contribute to the literature on vacancies by employing a new identification strategy to estimate the causal effect of the emergency designations. Furthermore, this is the first research study of its kind that analyzes the effect of emergencies on vacancy durations in an empirically rigorous method. Using a novel regression discontinuity strategy, I find causal estimates on the effect of vacancy emergencies on vacancy durations and the characteristics of confirmed judges. My analysis uncovers evidence that the emergency designation has no beneficial effects to outcomes associated with the judicial nomination and confirmation process.

2.3 Empirical Strategy

2.3.1 Conceptual Framework

The direction of the impact of an emergency label on the duration vacancies and the characteristics of the appointed judges is theoretically ambiguous. The Executive and Legislative branches of government may have several strategies when faced with an emergency vacancy they need to fill. Perhaps more importantly, they may have vastly different goals in mind, especially if the branches are held by separate parties. In this scenario the preferred candidate and timeline to confirmation are likely to differ greatly. For example, a Democratic president with a Republican led

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Congress might face significant delays in the confirmation process. In such a situation, an emergency designation may convey differing messages to various parties involved in the nomination and confirmation process, perhaps depending on the distribution of bargaining power across the various agents. An opposition controlled Congress might wish to reduce the probability of final confirmation by drawing out the confirmation of a candidate in a vacancy position which has been marked as important or highlighted as needy. However, a Congress that is in alignment might wish to fast-track the process for the exact opposite reason. Alternatively, the political desires of the major players could vastly outweigh any considerations on the part of the judiciary, thus rendering the emergency distinction a moot point.

Additionally, the main power in the nomination and confirmation process lies in the hands of a very short list. In this process the most obvious is the President, but the Senate and, within the Senate, the senators in the vacancy district also wield a large amount of power. Thus it is typical of the President to consult with the Senate, paying special attention to the two senators with blue slip power. Furthermore, the chair of the Judicial Committee also controls some sway in this decision. When these players are in alignment politically, the process is likely to go much more smoothly, especially in times of high political polarization such as the period in question. As a result, though this paper is unable to tackle the issue due to sample size considerations, it might be important to test the effects of an emergency both in the state of the world where these players are aligned, and where they are not.

Separately, while durational outcomes are the primary goals of an emergency designation, there may be numerous other potential outcomes of distinguishing certain vacancies. Of these, most notably is probably the observed quality of confirmed judges. Those judges who enter into districts with a caseload above 600 WFPJ can expect to be thrust into a strenuous and demanding work environment. Additionally, insofar as the political actors in control of this process care about

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agenda and political ideology of judges, we might expect those positions which appear a priori to be presiding over the conclusion of a larger body of cases to be more searched and thought out. Thus, if the President or Congress believes their best response to an emergency is to allocate potentially more time to finding the best candidate to fill such a demanding position, we might expect no effect on duration (or perhaps a positive shock) but instead an increase in a confirmed judge's observable ability. However, if these same players feel rushed in their attempt to nominate and confirm a judge, judges with worse characteristics may be confirmed.

2.2.3   Identification Strategy

Because the direction of the impact of emergency designations on the time to and quality of the judicial appointment is undetermined, we require a valid empirical approach to estimate the overall causal impact of these emergency distinctions. One simple approach is to estimate the following equation:

(1) $y_{idt} = \alpha + \beta Emergency_{idt} + \pi X_{idt} + \varepsilon_{idt}$

for vacancy i in district d at time t. y is some outcome measure such as vacancy length, *Emergency* is an indicator for the emergency label, X is a set of vacancy specific, district and time characteristics and ε is the error term.

The parameter of interest is β, representing the impact of *Emergency* designation on the outcome of interest. However, in the above specification, there might be omitted variables that would bias the ordinary least square (OLS) estimate of β. Specifically, the emergency label is not randomly assigned and is determined in such a manner as to leave us to believe that emergency vacancies are objectively different than other vacancies. For example, vacancies are more likely to be designated

as emergencies in districts with a higher caseload[46] and these districts tend to be larger and characterized by more political connectedness or exposure. Districts which hold more political connectedness presumably could use this power to obtain swift nominations and confirmations. Consequently, the presence of these confounding factors would more than likely bias the OLS estimates toward finding a negative effect emergency designation on the duration of vacancies. On the other hand, high caseloads may be associated with the district's inability to effectively petition Congress for new positions and curtail the increase in caseload pressures.[47] This scenario would likely bias my estimates in a positive direction, as emergency vacancies would be longer in duration than counterpart vacancies. As a result, even the direction of the bias in the OLS estimates of emergency designation is indeterminate.

To address this issue, I make use of a regression-discontinuity design that exploits the cutoff given by the firm definition of an emergency. As previously outlined, the Judicial Conference defines a vacancy as an emergency in three possible situations but most pertinent to this paper is any vacancy in a district where weighted filings equal to or exceed 600 per judgeship.

Making use of this first and most widely applied definition, I will employ a strategy that compares vacancies that occur in districts with marginally more than 599 WFPJ to those with marginally less. It is unlikely that districts slightly below the defined cutoff WFPJ differ in any meaningful way from districts slightly above the cutoff for WFPJ other than the fact that, due to the policy rule, the latter districts receive the emergency designation while the former do not. However, for practical purposes, restricting my sample to just the observations in the immediate proximity of the cutoff would result in an extremely small sample size and large standard errors from any estimation. In such a world I would be unlikely to detect any effects of the policy, regardless of the

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[46] The average WFPJ in emergency districts in my sample is 689 while non-emergency districts are at 456.
[47] While the number of WFPJ in emergency districts vastly outpaces non-emergency districts, emergency districts have on average only one more allocated judgeship position in my sample, 12.7 to 11.4.

presence of an effect, thus increasing the chance of a Type 1 error. Hence, the actual analysis will expand the bandwidth to the neighborhood encircling the cutoff to include a larger number of observations while maintaining the integrity of the RD design. Of my sample, there are 51 vacancies that are within a window of 100 WFPJ above the cutoff and 89 that are in the same window below. Expanding the bandwidth, however, opens the possibility that there are unobservable factors that are correlated with differences in the number of weighted filings and the outcomes measures of interest. Therefore, we control for a function of the WFPJ count in all of my specifications.

In order to demonstrate that the above 600 WFPJ threshold rule is strictly followed in designating a vacancy as an emergency, I first estimate the following equation to study the impact of exceeding the cutoff:

(2) $\text{Emergency}_{idt} = \alpha_1 + \delta_{1idt} \text{Above600}_{dt} + f(\text{WFJCount}_{dt}) + \pi_1 X_{1idt} + \varepsilon_{1idt}$

for vacancy i in judicial district d occurring at time t. *Emergency* is an indicator for the designation as an 'emergency' vacancy, *Above600* an indicator for WFJCount being greater than or equal to 600, f(WFJCount) is a continuous linear function of WFJ count, and X_1 is a set of vacancy, district and time characteristics.

Table 3 shows that δ_1 is positive and statistically significant, indicating that the "emergency" designation does indeed result largely from the cutoff rule. However, the emergency designation is not in perfect compliance with the 600 WFPJ count cutoff rule, owing mainly to the two other definitions of emergency. As such, from here I implement what is often referred to as a fuzzy regression discontinuity design (Imbens and Lemieux 2008, Lee and Lemieux 2010). I instrument for the potentially endogenous regressor in Equation 1, *Emergency*, an indicator for the WFJ being above 600 in order to obtain a causal estimate of β. Then my reduced-form equation is estimated as:

(3) $y_{idt} = \alpha_2 + \delta_2 \text{Above600}_{dt} + f(\text{WFPJ\_Count}_{dt}) + \pi_2 X_{2idt} + \varepsilon_{2idt}$

The reduced-form effect of being just above the 600-WFPJ cutoff, δ_2, indicates the effect of increasing the likelihood of a vacancy being an emergency; it is not a direct measurement of the effect of an emergency on outcomes. Therefore, in order to obtain the full measure of the effect of an emergency on various outcomes, it is important to rescale the reduced form estimate by δ_{1idt} in order to gain a more accurate understanding of various effects.

My fuzzy RD strategy identifies the local average treatment effect (LATE) for judicial districts close to the 600-WFPJ cutoff. One might exercise some caution when trying to apply the results below to draw broader conclusions for the larger body of emergencies. This cluster of districts is just slightly right of the mean in terms of average judge workload.[48] Additionally pertinent, these districts are somewhat larger in terms of number of judgeship positions[49] and thus might be more extensive and exhibit more political connectedness. Despite these caveats, this study is the first to estimate the causal effect of the emergency designation on judicial vacancies, albeit for a selected group of districts. Furthermore, the LATE that I estimate might be more interesting than other possible cutoffs since it is estimated amongst those districts which have objectively been defined as having the greatest need. Therefore, the policy ramifications are potentially higher for the group I am estimating upon. Thus, I feel that evidence present here is useful even in the presence of a LATE.

2.4 Data

In implementing my empirical strategy, I use publicly available data acquired from UScourts.gov on district court vacancies and other court characteristics. This data set provides me with the exact

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[48] Mean for those in the window specification above the cutoff is 530 vs. 507 for the full sample.
[49] The mean number of judgeship positions for those in the higher window is 14.75 vs. 11.72 in our full sample.

day in which a vacancy occurs along with the day of the first nomination for a new judge, the final confirmation date and the day in which the judge takes up commission. This site also gives me information on whether that vacancy was defined as an emergency upon its creation. Additionally, I match this data with information obtained electronically from US Courts providing us with the total measure of WFPJ reported for the previous year. District courts tend to rely on weighted filings as opposed to aggregate numbers of case filings as it gives a better measure of each case's workload. More important to my study, the rule that defines emergencies makes use of this calculated measure of WFPJ. Since accurate measures of my running variable are available only from 2001 forward, I have to restrict the sample to vacancies occurring after the start of the 2002 calendar year.[50] I further restrict my sample to vacancies occurring prior to 2012 as there are a large number of vacancies originating in that year that are still unfilled, yet there are only six unfilled vacancies originating prior to 2012. Using data from vacancies occurring after 2012 would complicate and potentially hinder my analysis; I would either be forced to top code a large portion of vacancies or introduce a selection bias as I only make use of vacancies which have been filled and exclude a large group of vacancies emanating in 2012.

I further restrict my analysis to district courts. District court judges account for the vast majority of judgeship positions (677 compared to 179 in Court of Appeals and just 9 in both the Supreme Court and Court of International Trade)[51] and hence have a larger share of the vacancy pool.[52] Also, while the same on paper, it is not entirely true that the vacancy process is exactly equivalent between the two courts as the Court of Appeals has a significantly lower confirmation rate than the District Court (74% to 84% over the past 3 presidents). Moreover, the rules governing the allocation of emergency status are different between district and circuit courts; the running variable is adjusted

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[50] Recall that emergencies are defined by the previous year's WFPJ.
[51] http://www.uscourts.gov/Statistics/FederalCourtManagementStatistics.aspx
[52] This fact will be important since I am working with a relatively small sample size.

filings[53] and the threshold count is higher. Finally, since the Court of Appeals wields more power and creates more precedents, we might expect the political actors involved to behave differently with respect to these nominees. Due to the restricted sample, the results presented below pertain to the effect of emergency provision among district courts inside the United States and might not have external validity in other courts. However, considering that the district court system is a huge player in the United States federal structure, presiding over more cases than the combined other courts, understanding the provision of emergencies here has important policy relevancy.

To assess whether the policy is binding, I must examine whether the probability of being in an emergency at the outset increases just above the 600 cutoff. In my empirical analysis below, I use a count of the WFPJ in the district the year prior to a seat being vacated for determining emergency provision. It is important to note that emergency status is not a temporary factor in a vacancy. In other words, if an emergency is declared in 2007, but in 2008 the WFPJ drops below the threshold, that vacancy retains its emergency label. Additionally, vacancies can pick up the designation of emergency anywhere along in their tenure. In the vast majority of situations this occurs at the 18 month mark via the second definition of emergency. For my sample of ten years, 43 emergencies arise in this manner at some point after the vacancy has existed 18 months in duration. However, I do not examine the effect of these emergencies and instead loop them in with vacancies that never get labeled emergencies. This might lead to attenuation bias, but I suspect this attenuation effect will be minimal if not imperceptible. Firstly, the average vacancy in my sample of districts above 400 WFPJ does not even last to the 18 month cutoff[54] which means that a large portion of examined vacancies won't be eligible. More importantly however, if it were true that emergencies had

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[53] Adjusted filings assumes all cases have an equal effect on judge workload except for cases in which one or both parties are not represented by a lawyer—these being documented with 1/3 the weight of all other cases.
[54] In my sample, the mean length of time for districts above 400 WFPJ is around 14 months with a median of 385 days.

differential effects on outcomes, I posit that there would be a noticeable difference between districts which held the designation for 18 consecutive months prior to their counterpart emergency vacancies.

Because the policy rule specifies a 600 WFPJ cutoff, I restrict the data to vacancies which upon creation are near this cutoff. In my main specification, I use observations with between 500 and 700 WFPJ. It is important to note that many districts switch along the border and thus might have emergency and non-emergency vacancies in my sample.[55] This in itself is partial evidence of a lack of control on the part of districts when it comes to labeling vacancies as emergencies. Specifically, 18 districts experience both vacancies that fall above the cutoff and those that fall below.

My outcomes of interest are most notably various measures of duration in the vacancy filling process. Most obvious to such a study would be *CommissionLength* which is a simple subtraction of the date a judicial seat is vacated with the date a new judge takes commission. Additionally, I dichotomize this variable into its two most important components, the first of those being *NominationLength*. This variable is calculated as the difference between the date the President first nominates a judge for a position and the initial vacancy date. The second part, *ConfirmationLength*, is generated as the difference between a judge being nominated and when a candidate is finally confirmed by the Senate.[56] It is important to note that if a candidate fails the confirmation process, the time spanning the length between first nomination, each subsequent nomination and final confirmation is what I label *ConfirmationLength*. As mentioned earlier, 43 emergencies arise after the 18 month duration due to the second definition of an emergency. Thus, I have reason to believe

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[55] WFPJ, even within district, is highly variable with districts sometimes experiencing extreme upturns and downturns.

[56] There are some observations in my sample for which the President has nominated a candidate that has yet to be confirmed. In this case, the value for *NominationLength* will be present in my analysis but there will be no analysis on *CommissionLength* for these observations.

that my estimates of confirmation length might be somewhat complicated owing to this factor. As a result I also tabulate the length between the date of nomination and the date in which the Senate holds its first hearing to question the nominee. This part is in this paper termed *HearingLength* and is slightly shorter than the time to confirmation, thus offers me less potential bias with the second definition of emergency. There is a final part, *CommissionLength* which is less descriptive; it is simply the time it takes for a judge to take office after being confirmed. I include estimations on the time that lapses between confirmation and commission in Appendix Table 1 as it is extremely abbreviated in duration (slightly shy of two weeks on average) while also being extremely variable. Moreover, this is not a part of the process which either the Executive or Legislative bodies has control of so is more than likely a measure of the time it takes to vacate an old office and move into their new one. Nevertheless, the results repeat what is reported in my main duration measures.

Furthermore, I wish to study another set of possible outcomes related to the quality of the approved judge. As such, I have two measures of judicial quality, the American Bar Association score[57] (*ABAScore*) and the ranking of the attended law school[58] (*Ranking*) for the confirmed judge. Theory might say that the President and Senate could react to the declaration of an 'emergency' vacancy not by seeking to fill the position more hastily but instead filling the position with someone who is more qualified to handle the caseload pressures of such a district. As such, I wish to test alternative channels to solving the perceived increase in need.

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[57] The ABA rates nominated judges as Well Qualified, Qualified, or Not Qualified and I generate a variable that ranges from 1-10 based on the combination of the majority and minority rankings of each judge. Additional variation in this score comes from whether the majority rating was substantial or marginal. This rating is based on a vote of ten to thirteen members depending on the number of recusals.

[58] Law school ranking comes from US News & World Report from 2013. Rankings range from 1 to 149 with unranked schools taking the value of 150. They employ a weighted average of 12 measures of law school quality to generate an aggregate measure for each school inside the top 149.

I also draw from several other sources in order to find potential covariates for testing the validity of this study. In the realm of politics and its intersection with the judicial climate, our best political measures are given by Dynamic Weighted Nominate (henceforth DWN) scores.[59] I use DWN measures to calculate a range of variables. Since it has been determined that the distance between Congress and the President might be of importance, I create a variable, *PresidentDifference* which is the absolute value of the difference in the median DWN for the Senate and the President. Additionally, the difference between the DWN score in the district and the Judiciary Committee Chair might be relevant to this duration study, so I generate *ChairDifference* as the absolute value of the difference between the DWN score in the district[60] and the Judiciary Committee Chair. Finally, in order to gain a measure of the Senate's political views relative to the district, I create a variable that subtracts the median DWN value of the Senate from the score of the district and label this *SenateDifference*. As other controls I use in the regression analysis, I create a variable for the number of vacancies for which a judge has not been nominated (*NumberInNomination*), a variable for the number of vacancies for which a judge has been nominated but not confirmed (*NumberInConfirmation*) and a variable for the difference in time between a vacancy arising and when, prior to the vacancy, it was initially reported by the retiring judge (*LengthKnown*). There are a number of other covariates I test which are more straightforward such as the number of judgeships in a district and whether the vacancy is of the type senior status. I discuss this more in the Results Section.

2.3 Results

2.4.1 OLS Estimates of the Effect of Emergency Status on Vacancy Outcomes

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[59] DWN is a multidimensional scaling method developed by Keith T. Poole and Howard Rosenthal to analyze preferential and choice data.
[60] This score is calculated as a piecewise function. If both Senators are of the same party as the president, it equals their mean DWN whereas if only one is of the same party, their DWN is used and the President's DWN is used in absence of a politically aligned Senator.

In Table 2, I present the OLS results from estimating equation 1. I find a significant coefficient on the *Emergency* regressor in both the Commission and Hearing Length regressions (columns 1 and 7 respectively). Additionally, the coefficients are negative for both Nomination Length and Confirmation Length, though these values are not statistically significant at a meaningful level. This, if taken as causal identification would indicate that there is some negative impact of emergency status on duration measures. Specifically, it would tell us that the entire process seems to be benefitted by the emergency designation; however the effect might be slightly muted in each component of the process owing to negative but insignificant coefficients on some duration variables.

Regardless, the estimated coefficients for emergency provision are unlikely to have a causal interpretation for reasons discussed earlier. OLS estimates are provided so as to allow us to do comparisons with the RD estimates.

2.3.2   The Discontinuity in Emergency Status

If the rule governing the allocation of the emergency title is binding, then I should observe a discontinuity in vacancy emergency provision right at the 600 weighted file per judgeship position cutoff, with vacancies above the cutoff receiving sharply more emergency distinctions than those below the cutoff. With this in mind, Figure 3 plots the average share of vacancies that are designated as emergencies immediately after the vacancy is declared. Just after the 600 cutoff we can visually notice a large jump in the allocation of emergencies to vacant positions (from just shy of 10% to above 80%)[61]. It is important to take notice of the fact that the jump is not from no provision to provision, nor is it to complete provision. The 10% group that exists as an emergency below the cutoff is mainly due to the clause in the emergency definition that allows for districts with

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[61] Each point on the graph is a cluster of observations in a bin of 40 WFPJ.

only one remaining judgeship position to be declared emergencies. Furthermore, the group past the cutoff falls short of 100% mainly a result of a small amount of measurement error in the measurement of WFPJ in the year 2005, so there are some districts that are incorrectly categorized.

Table 3 provides first stage results under Part B corresponding with the graph in Figure 3. The coefficient for exceeding the cutoff is positive and statistically significant across both a reduced sample and a sample which doubles the window size. More acutely, vacancies occurring in districts exceeding the threshold are almost 70 percentage points more likely to be declared an emergency as soon as the position becomes vacant relative to those below the threshold. This effect is quite large–a 13 fold increase between districts within a bandwidth of 100 WFPJ below the cutoff and those with the same length window above the cutoff. As such, I can conclude that the rule generates meaningful variation in Judicial Committee provision of emergencies for a particular vacancy.

2.3.3   Tests of the Validity of the Regression Discontinuity Design

Owing to the fact that emergencies are set up to alert the system to expedite the process of that vacancy, each district might have an incentive to declare itself above the cutoff. If any player in the process has power over the measurement of WFPJ and uses it to switch their positions from just below to just above the cutoff, then it is potentially the case that districts to the right of the cutoff are systematically different than other districts. In this example, my results would be biased by these districts which wield extra political clout. An unequal balance of political control might lead to shorter duration times along with potentially more qualified judges for those who are thus privileged. To assess this, in Figure 2 I plot the distribution of WFPJ. A discontinuity in the density of districts locating around the 600 WFPJ cutoff would suggest a manipulation of my running variable

(McCrary 2008). As the figure shows, there is little evidence of irregular heaps in the density of WFPJ counts. Specifically, the graph exhibits a strongly normal distribution with smooth rises and falls.[62] Applying the test proposed by McCrary (2008), however, I find some small evidence for a change in the density at the 600 WFPJ cutoff, p-value of .062. Specifically, there is a slight unexpected rise in density immediately following the cutoff. Hence, it could be the case that certain districts or judges are able to influence the process involving the emergency label. This situation might bias my data toward finding durational effects in the negative direction for the emergency designation, but, since the results aren't showing up in this fashion, this concern is less of an issue. Next, I check for indications of differences in observable characteristics across the threshold. Appendix Figure 2 graphs the covariates by WFPJ in the district. These covariates include the number of allocated judgeship positions, senior status vacancies, absolute levels of filings, whether the vacancy was a newly formed position, and various political alignment measures. For all covariates, the graphs show smooth distributions around the cutoff point. Additionally, I estimate Equation 3 using each of these observable characteristics as the dependent variable. These results are reported in Appendix Table 2 and confirm the visual evidence found in my graphs—there are no changes in covariates at the cutoff that are statistically significant. In order to combine these multiple tests into a single test statistic, I also estimate a Seemingly Unrelated Regression and perform a Chi-squared test for the hypothesis that the coefficient of Above600 across these regressions is jointly equal to zero. Here I find no evidence of heaping of any covariates with a p-value of 0.987. Nevertheless, I perform donut regressions of the duration outcomes in which vacancy districts within 20 WFPJ on either side of the cutoff are removed. These are done so as to

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[62] Barreca, Lindo and Waddell (2011) show that heaping in the running variable can lead to biased estimates even if the heaping occurs away from the cutoff. Figure 2 shows that there is no evidence of heaping at any value of the running variable in my sample. I also conducted donut regressions in which I dropped vacancy groups immediately around the 600 WFPJ cutoff and found results that are very similar to my main estimates.

exclude districts that might have moved their WFPJ count from one side of the border to the other and these estimates are included in Appendix Table 3.

2.3.4 Effect of Judicial Committee Provision of Emergencies on Vacancy Duration

Figure 4 provides visual evidence on the reduced-form relationship between the vacancy WFPJ count initially after the seat opens and the duration of the vacancy. It is evidenced by the lack of a clear jump that each graph shows no relationship between emergency designation and duration. In my setup, the first graph confirms that there is no overall effect in total vacancy duration. The second and third graphs dichotomize the process into the nomination period and the confirmation period respectively in order to piece out any differential effects between the incentives of the Executive and Legislative bodies. However, I find no discernable effect of the designation on either part of the process.

Table 3[63] presents the results from a formal evaluation of the duration of vacancies around the cutoff. I report results estimating the reduced-form effect of the vacancy exceeding the 600 WFPJ cutoff on the length of the entire vacancy process. Column 1 in Table 3 represents my main specification. In this regression I initially use a window of 100 WFPJ (i.e. test the bin of 500-599 WFPJ vs. 600-699 WFPJ). For purposes of cutting down on estimation error and shrinking my confidence intervals, I also include an expanded bandwidth regression for each outcome. Furthermore, I drop outliers that exceed in duration three years for either nomination length or confirmation length. In this manner I exclude one observation from the initial nomination regressions and three from the confirmation and commission regressions.[64] In column 1 the point estimates show that I cannot reject the null hypothesis that emergencies have no effect on total

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[63] Full regression results for this table are reported in the Appendix.
[64] Including these observations changes the point estimates only marginally but affects the confidence bands greatly.

vacancy duration. Since we are mainly concerned about a change in the negative direction (i.e. a decrease in any part of the commission process), I examine and find very little evidence for a decrease in any measure of vacancy duration. Specifically, the OLS point estimate of 57.64 in Column 2 (using the wider bandwidth for more precise estimates) allows me to reject a 1/4 standard deviation change of duration in the negative direction[65] at the 95% level.

Nevertheless, the process of nomination and confirmation, though likely linked from a game theoretic standpoint, must be examined individually for a multitude of reasons. If the Executive faces different incentives than the Legislative Branch, we might see different results for the individual parts.[66] Additionally, it could be the case that the nomination duration is more likely to see a clear result in my analysis owing to the second clause defining an emergency. Thus, I dichotomize the time to commission into its two key parts, nomination and confirmation.[67]

In column 3 I estimate the effect of emergency status on the nomination process. Here I find a point estimate very close to zero. Using the wider bandwidth and more precise estimates of column 4, I reject a 2/7 standard deviation change in the negative direction for nomination duration. Furthermore, my estimates on confirmation length, even more precisely estimated in columns 5 and 6, continue to confirm that the emergency label is a warning which is not providing a powerful enough incentive for either Congress or the President to respond. I additionally break down the confirmation process into time until first hearing in order to test more avenues of possible changes, but the results repeat themselves. On initial blush, the cutoff is not serving as a threshold past

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[65] I test changes in the negative direction since they are indicative of a speedy process.

[66] Both branches play at least a minor role in each part of the process, but it's clear that the President is the majority player in Nomination and the Senate in Confirmation with the Judiciary Committee being a minor player in each. It's also worthy to note that it is extremely rare to have a judicial candidate who is in from the opposing party as the president, regardless of the majority party of the Senate.

[67] There is a period between confirmation and commission but it's extremely random in duration and very short (less than two weeks on average).

which either the judicial nomination or confirmation process is to be shortened. Recalling earlier OLS estimates, these results give evidence of a slight negatively bias, which might be an indication of emergency districts' power to influence congress in some manner.

2.3.5 Effect on Judge Quality

Next, I examine the effect of on measures of judge quality. In testing this I employ two measurements of a nominee's[68] a priori ability similar to Choi et al. (2013), ABA Score and Law School Rank.

If either the Executive or the Senate is responding in a manner as to confirm judges which have higher ability scores, we would notice a perceptible jump at the 600 WFPJ cutoff. Figure 5 gives visual evidence at the cutoff for judge ability measures. In the first graph represented, there is a noticeable positive jump in the law school ranking of the confirmed judges. This positive jump indicates worse candidates on face (higher law score ranking corresponds to schools which score lower along the 12 measures of law school quality). My next graph provides visual evidence for the rating which the American Bar Association attributes to the judge post-nomination. It is important to note that the scores here are highly top-coded with the vast majority of judges receiving a perfect score of "Well Qualified."[69] Thus, absence of evidence in a change in the cutoff might result from the lack of variation in scores as opposed to an actual lack of difference between judges to the left and right of the cutoff. That said, here I notice a slight uptick, indicative of a higher initial evaluation of these judges by the ABA. This, analyzed against the law school ranking initially seems somewhat contradictory in nature with law schools attended worsening and individual ratings increasing.

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[68] I measure only nominees that eventually are confirmed. This might have other implications, but the idea was to test the incentives behind the process, not of the individual bodies here.

[69] 48 percent receive Well Qualified scores for the sample

To assess the visual results I observe in Figure 5, Table 4 provides empirical estimates of these regressions. Column 1 comes from a regression of "emergency" on the ranking of the judge's law school. My point estimate is positive but not significant. Nevertheless, I am able to reject a tenth of a standard deviation change in the negative (better) direction. Column 2 more accurately confirms that law school rankings are increasing with a point estimate of 30, corresponding to very large decrease in law school ranking from a mean ranking of 48. It is important to take pause and recognize that a candidate's law school rank does not encapsulate the entire body of evidence for or against them as a candidate. Here perhaps a better measure is the ABA score which takes into account the full measure of a judge's objective ability. Columns 3 and 4 report empirical results from the regression of the ABA scores on vacancy emergencies. With a widened bandwidth, I find a positive coefficient which is significant at the 5% level. Thus, while the law school rankings are worsening at the cutoff for these judges, the ABA is ranking them higher. This might be indicative of either a better rating system in which the judge's true ability to perform at the next level is put on trial or the result of incentives on the part of the ABA, which is more removed from the political process, to speed up the confirmation period for needy districts by providing higher scores for what are perhaps worse judges.[70] This paper is unable to parse out the exact incentives at play in this situation but provides a framework for further work on the matter.

2.3.6 Sensitivity Analysis

In Table 5 I examine the robustness of the results to several potential concerns. My baseline model assumes that the underlying relationship between WFPJ and vacancy outcomes is piecewise linear (recall I control for a linear function of district WFPJ count that allows for different slopes above and below the cutoff). To the extent that this functional form is incorrectly specified, the RD

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[70] Binder and Maltzman 2002report that the process of confirmation is quicker for judges with higher ABA ratings.

design will lead to a biased estimate of the treatment effect. Nevertheless, recalling Figure 4 my choice of a piecewise linear function was guided by the indications of a visually linear form. Table 5 tests the sensitivity of my results to both a modification of the running variable and a modification in the outcome variables of choice.

In order to address such sensitivity concerns as potential nonlinear effects of the running variable in my regression, I make use of a quadratic smoother (i.e. controlling for a quadratic function of WFPJ count with different functions above and below the cutoff) and test my duration outcomes once again. To gain more power to include these extra variables, I make use of my specification which widens the bandwidth to 150 WFPJ on either side of the cutoff. While the estimates generally have wider bands, the coefficients are all positive, further justifying the previous results. Moreover, there is some evidence for the necessity of the quadratic component as the coefficient is significant in the majority of the regressions.

Because the outcome variables are highly variable, the data might be better analyzed by smoothing the duration variables through some means. Unable to use a log transformation since a large number of nominations occur prior to a vacancy arising (i.e. the variable *NominationLength* is often negative), I impose an inverse hyperbolic sine transformation on both dependent variables.[71] Its interpretation is very similar to a log transformation at high values such as is the case here. In even columns of Table 5, I report estimates from the regression of the cutoff on my transformed values of both nomination and confirmation durations. Here the estimates are more precisely confirmed to be around zero, further adding to the case that emergencies don't affect the process in terms of duration.

2.5.1 Conclusion

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[71] The inverse hyperbolic sine is defined as $\log(y_i + (y_i^2 + 1)^{1/2})$.

Emergency vacancies are designated by the Judicial Conference as being those vacancies with the most deleterious effects to the efficacy of the courts. The rules that define them encapsulate the types of districts in which active judges are overburdened with heavy caseloads. While most scholars and policymakers concede that vacancies, especially those of the emergency category, cause large strains on the judiciary, few measures have been taken to ameliorate the issue. In contrast to the goal of expediting the treatment of important vacancies, I find that the emergency designation has no effect on duration on average. Nevertheless, because there are different parties of the political structure in charge during different parts of the process, I dissect the entire duration into its three most important components, nomination, confirmation and time to hearing. However, I find no individual effect among any of these three parts and can reject very small standard deviation changes in the negative direction. These findings highlight a policy that is ineffective in reducing vacancy duration at any level.

While duration outcomes are likely foremost in the discussion of emergency vacancies, there are reasons to believe this designation could have effects on the quality of confirmed judges. Theoretically, we could expect either higher or lower quality judges to make it through the process based on the preferences of the controlling political actors. My regression analysis finds that emergency vacancy judges have significantly worse law school rankings while maintaining nominally higher qualification scores from the American Bar Association. This might result from the ABA being a better and more widely used metric of measuring judicial quality, or could emanate from incentives on the part of the politically removed ABA to speed up the process for these types of vacancies by providing higher qualification scores for candidates.

These findings of a failure to reduce vacancy durations imply that the Judicial Conference is not helping their cause by declaring emergencies for those vacancies deemed in need. Furthermore, while these results pertain to the ten year period in my data sample and only district courts, it is a

long period of analysis in what is by far the largest federal court structure in the United States. Given the recent pattern of increasing vacancies and emergencies, it is likely to be the case that this will continue to be important in the political sphere. As such, these results that belie any effect of emergency labels will provide evidence to help inform the steps taken towards hastening the commissioning of new judges for important vacancies.

Figure 2.1: Evolution of Emergencies and Vacancies, 2002-2014
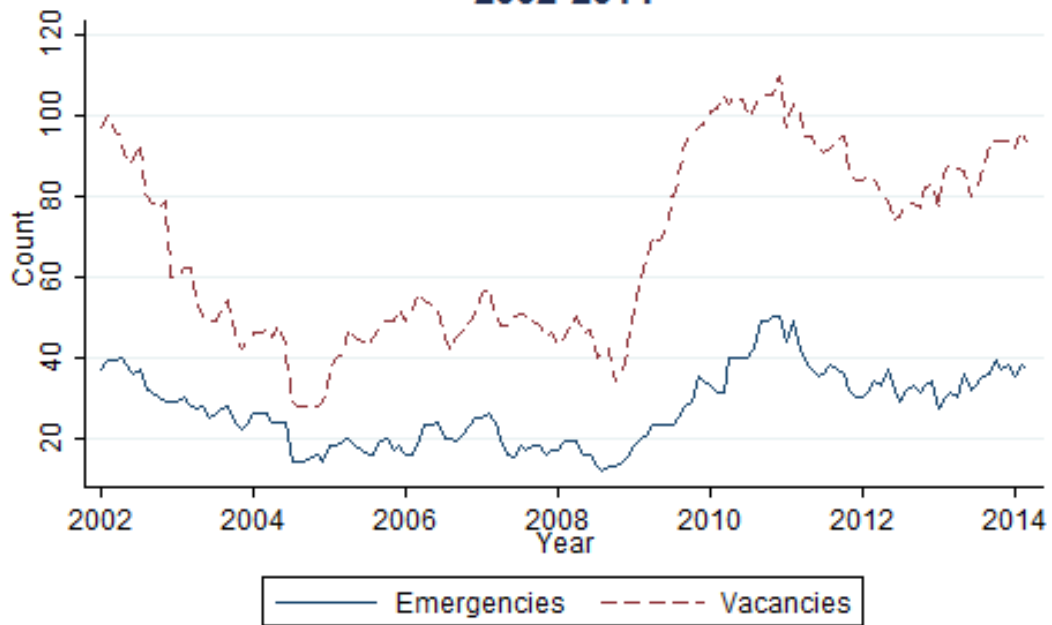
Notes: Monthly data of federal court vacancies taken from UScourts.gov.

Figure 2.2: Emergencies vs Weighted Filings

Notes: Observations are grouped into bins of 40 WFPJ emanating from the cutoff.
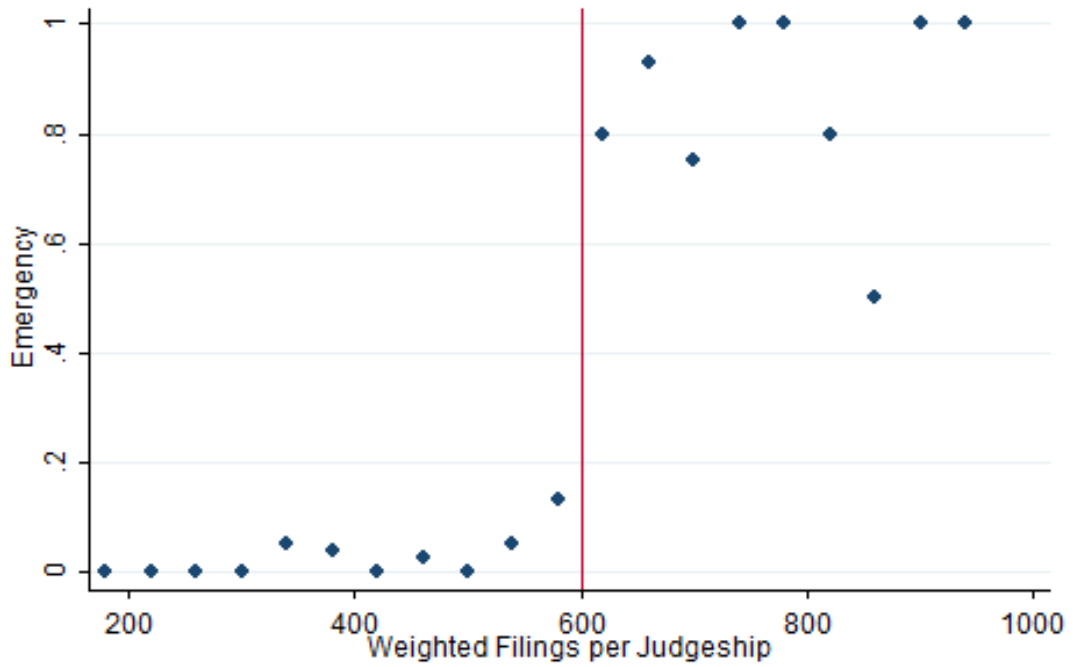
Figure 2.3: Distribution of District Court Vacancies, 2002-2011

Notes: Each bar includes observations in bins of 40 WFPJ emanating from the cutoff.

Figure 2.4: Emergency Outcomes on Duration
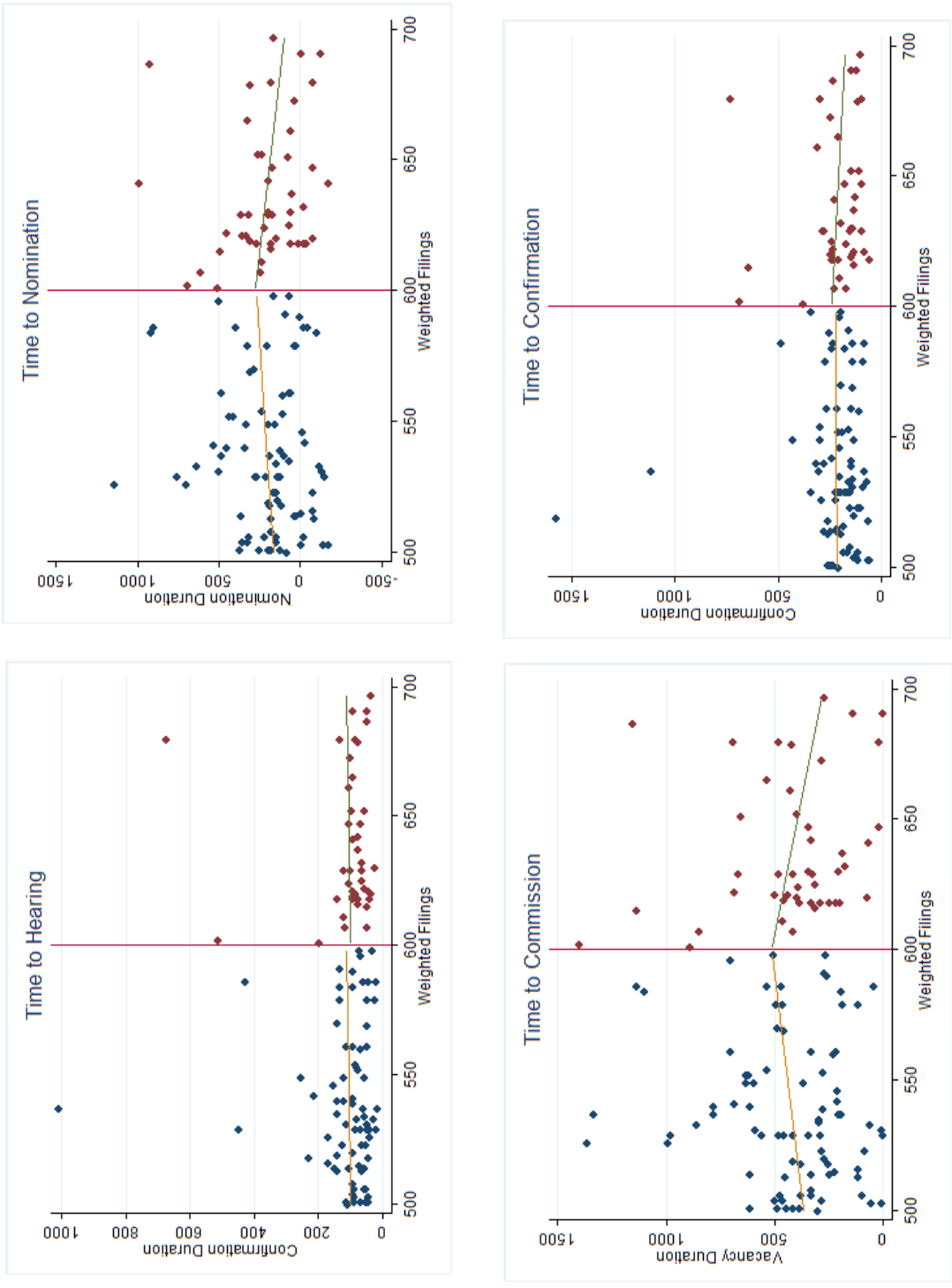
Figure 2.5: Emergency Outcomes on Judge Quality Measures
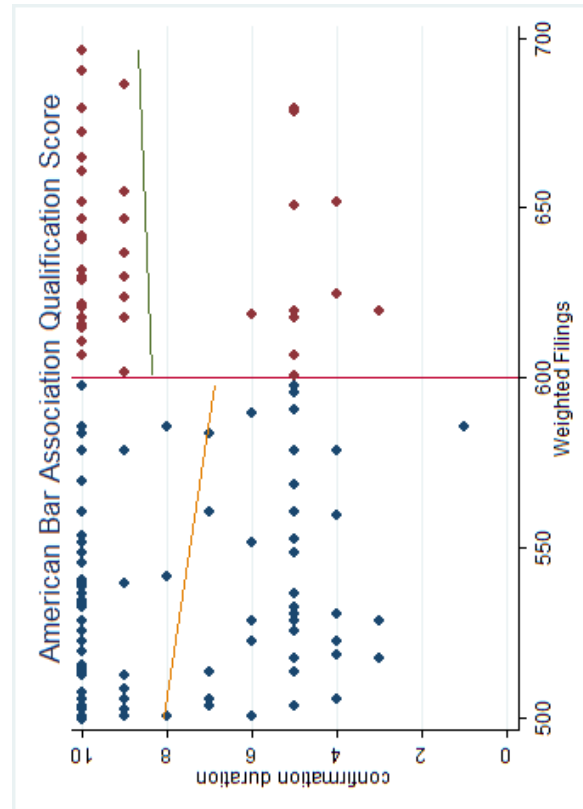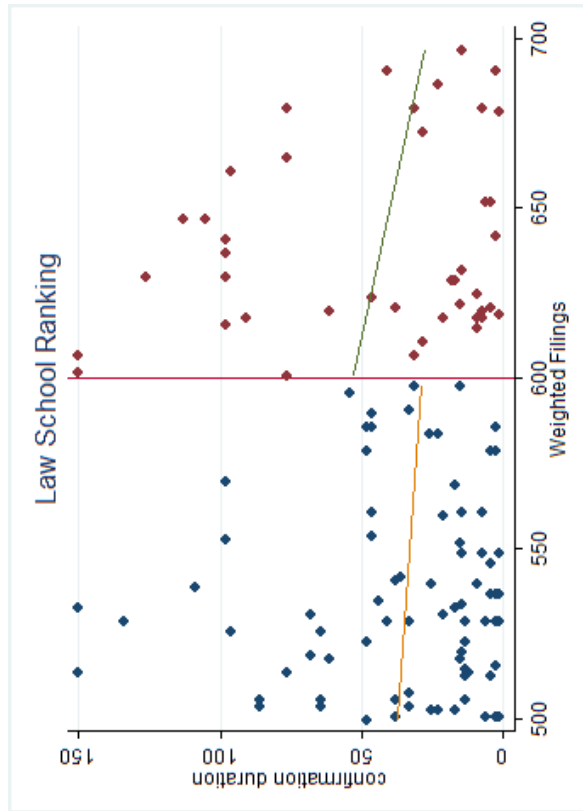
Table 2.1: Summary Statistics for Vacancies 2002-2012

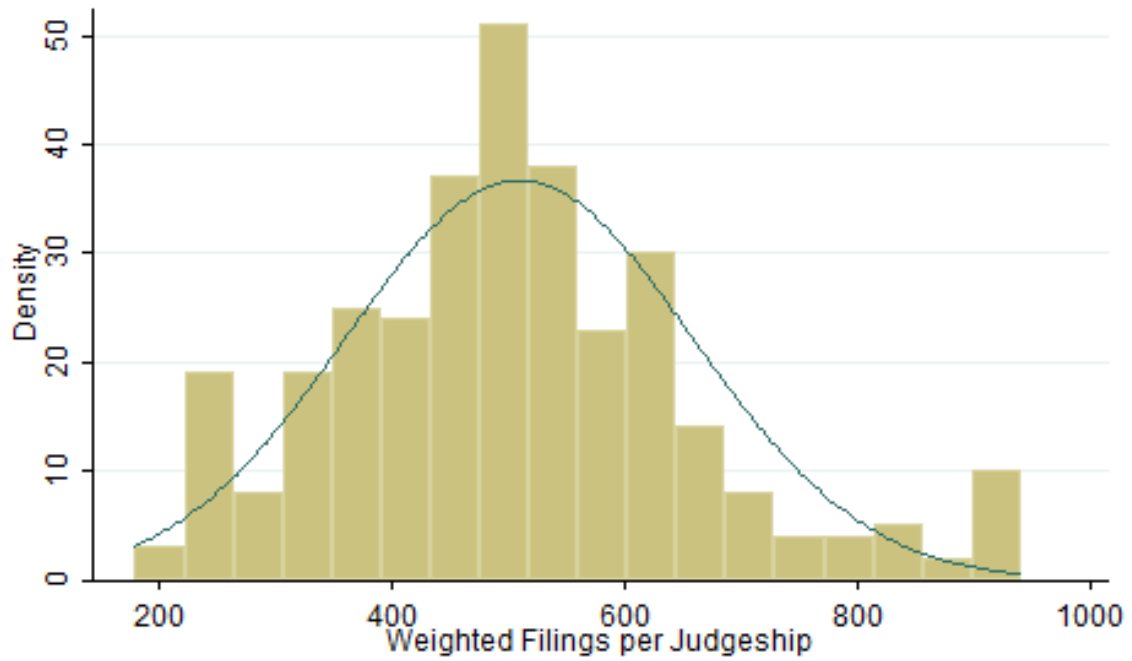|	Below 600 Cutoff	Above 600 Cutoff
Mean (Standard Errors) for Outcome Variables of Interest		
Nomination Length	247.54	177.34
	(19.270)	(29.100)
Confirmation Length	250.99	220.07
	(16.380)	(26.080)
Hearing Length	159.78	140.24
	(19.370)	(32.000)
Commission Length	499.29	410.84
	(24.100)	(41.210)
ABA Score[1]	8.05	8.11
	(0.168)	(0.328)
Law School Rank	49.4	47.78
	(3.082)	(5.508)
Mean (Standard Deviation) for Covariates		
Senior Status	0.763	0.575
	(0.028)	(0.058)
Senate-President Difference[2]	0.441	0.506
	(0.017)	(0.027)
District-Judicial Chair Difference	0.404	0.336
	(0.025)	(0.039)
District-Senate Difference	0.326	0.351
	(0.014)	(0.027)
Number of Vacancies	44.03	38.36
	(1.526)	(2.504)
New Position	0.0125	0.192
	(0.007)	(0.046)
Judgeships	11.32	13.26
	(0.505)	(0.831)
Filings	6159.8	8129.1
	(478.2)	(519.7)
WFPJ	446.8	705.7
	(6.070)	(12.620)
# of Vacancies	309	73
# of Districts	75	26

Notes: These are means of the variables in our analysis with standard deviations reported in the parentheses. Statistics are only reported for observations that appear in our sample for regression analysis. The sample of vacancies is taken for the period between 2002 and 2011. Observations with durations in excess of 1,000 days are excluded from analysis. [1]Ranking from 1-10 with 1 being Not Qualified and 10 being Well Qualified. [2] Differences rported as absolute value of DWN difference. Senate calculated at the median.

Table 2.2: OLS Estimates: Emergency Designation vs Duration Outcomes

	Commission Length		Nomination Length		Confirmation Length		Hearing Length[1]		ABA Score[2]	Law School Rank
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Emergency	-77.84*	-23.61	-23.25	18.74	-19.26	-10.55	-30.37**	-29.34*	-0.04	0.37
	(43.08)	(42.27)	(34.94)	(33.69)	(16.55)	(19.13)	(12.93)	(14.96)	(0.38)	(6.45)
Controls	N	Y	N	Y	N	Y	N	Y	N	N
Observations	298	298	309	309	298	298	293	293	298	298
R-squared	0.011	0.139	0.002	0.163	0.004	0.053	0.009	0.091	0.000	0.000

Notes: Length variables are measured in days. *Emergency* takes the value of 1 if the vacancy is given the emergency tag. I have dropped observations where the dependent variable is in excess of 1,000 days. For each regression, controls are used for *NumberInNomination, NumberInConfirmation, LengthKnown, ChairDifference, PresidentDifference* and *CongressDifference*. [1] I was unable to track down 5 observations of hearing dates in my sample. [2] For *ABA Score* and *Law School Rank* I report only observations which haven't been dropped in a previous regression. Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table 2.3: RD Estimates: Emergency Designation vs Duration Outcomes

	Commission Length		Nomination Length		Confirmation Length		Hearing Length	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. Reduced Form - OLS Coeficent for "District Has >=600 WFPJ"							
Above 600	24.18	40.72	42.20	38.15	20.34	34.43	-10.76	-74.35
	(92.16)	(68.36)	(69.78)	(58.93)	(52.69)	(44.44)	(43.22)	(80.48)
	B. 2SLS - Endogenous Regressor is "Vacancy is Labeled *Emergency*"							
1st Stage, OLS ceofficient for WFPJ>=600	0.64***	0.62***	0.67***	0.66***	0.64***	0.62***	0.67***	0.67***
	(0.13)	(0.11)	(0.12)	(0.10)	(0.13)	(0.11)	(0.13)	(0.10)
2nd Stage- Emergency	36.23	82.55	63.14	58.05	31.92	55.57	-15.23	27.78
	(141.50)	(120.60)	(98.60)	(85.84)	(78.80)	(69.26)	(65.74)	(57.14)
Window 150 WFPJ	N	Y	N	Y	N	Y	N	Y
Observations	128	176	136	185	128	176	127	174

Notes: The first row reports the OLS coefficient on the reduced form estimate. The first row for part B reports 1st stage coefficients from the IV regression. *Above 600* is a dummy for being above the cutoff while *WFPJ* and *WFPJ Above* allow for different slopes of the running variable. For each regression, controls are used for *NumberInNomination*, *NumberInConfirmation*, *LengthKnown*, *ChairDifference*, *PresidentDifference* and *CongressDifference*; each variable calculated at the beginning of the time calculation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.4: Measurements of Judge Quality

	Law School Ranking		ABA Ranking	
	(1)	(2)	(3)	(4)
	A. Reduced Form - OLS Coeficent for "District Has >=600 WFPJ"			
Above 600	21.83	31.26**	1.09	1.80**
	(13.60)	(13.07)	(0.85)	(0.84)
	B. 2SLS - Endogenous Regressor is "Vacancy is Labeled *Emergency*"			
1st Stage, OLS ceofficient	0.64***	0.62***	0.64***	0.62***
for WFPJ>=600	(0.13)	(0.11)	(0.13)	(0.11)
2nd Stage- Emergency	33.49	48.71**	1.63	2.81**
	(20.92)	(20.70)	(1.28)	(1.41)
Window 150 WFPJ	N	Y	N	Y
Observations	128	176	128	176

Notes: Only confirmed judges used in this analysis. Each regression controls for *NumberInNomination*, *NumberInConfirmation*, *ChairDifference, PresidentDifference* and *CongressDifference*. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.5: Sensitivity Analysis of RD Estimates

	Commission Length		Nomination Length		Confirmation Length		Hearing Length	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. Smoothed 2SLS Regression w/out Controls							
Emergency	209.20	0.07	178.50	1.34	123.70	0.38	55.29	0.35
	(208.90)	(0.52)	(128.90)	(1.98)	(88.03)	(0.28)	(74.00)	(0.42)
	B. Smoothed 2SLS Regression w/Controls							
Emergency	142.10	-0.35	206.70**	1.42	110.00	0.25	47.64	0.32
	(172.90)	(0.50)	(88.20)	(1.24)	(101.90)	(0.32)	(83.54)	(0.47)
Quadratic Smoothing	Y	Y	N	N	Y	Y	N	N
Hyperbolic Sine	N	N	Y	Y	N	N	Y	Y
Observations	128	128	136	136	128	128	127	127

Notes: The reported coefficients come from IV regression for the 100 WFPJ window specification. The first row estimates have control variables for a trend on the WFPJ variable above and below the cutoff and terms for the square of this trend (in "Quadratic Smoothing" columns). Full controls are present in the regression for the second row of estimates. The hyperbolic sine is calculated as $\log(y_i + (y_i^2+1)/2)$. The coefficients on the quadratic terms are not included in this table. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

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Appendix 2.1

There are two alternate time measures which I exclude from my main text. Here I test a measure for the time that exists between the confirmation of a judge and when they finally take up commission in the district along with the time between a judge's hearing and their confirmation by the Senate. I find no jumps in either variable at the cutoff.

Figure A2.1.1: Alternate Duration Specifications
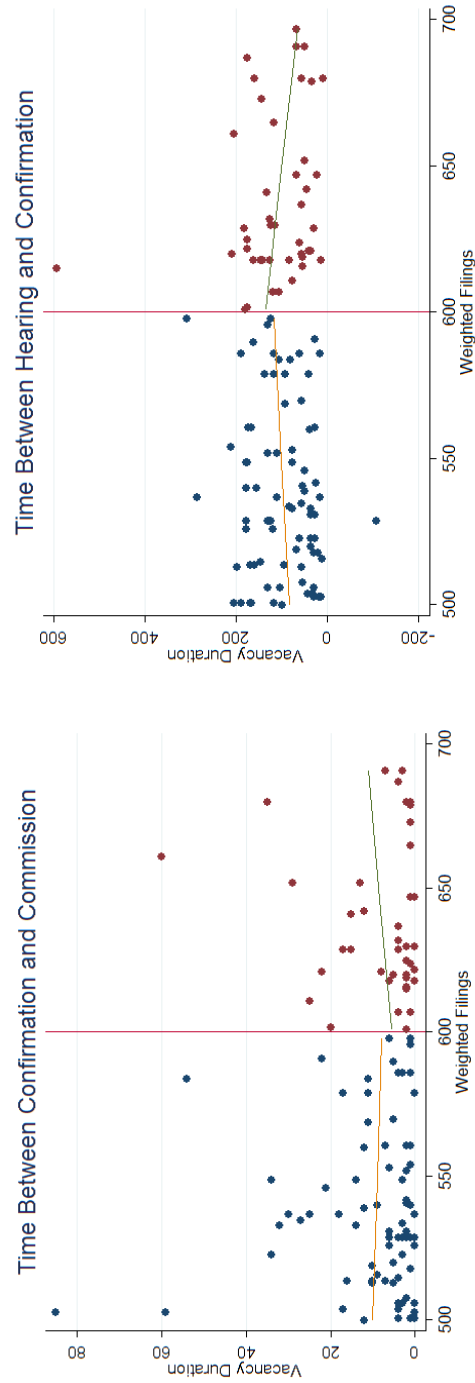
Table A2.1.1: Alternate Duration Specifications

	After Hearing		After Confirmation	
	(1)	(2)	(3)	(4)
	A. Reduced Form - OLS Coeficent for "District Has >=600 WFPJ"			
Above 600	28.76	115.10	-2.12	-4.00
	(26.93)	(77.48)	(4.31)	(3.05)
	B. 2SLS - Endogenous Regressor is "Vacancy is Labeled *Emergency*"			
1st Stage, OLS ceofficient	0.67***	0.67***	0.64***	0.62***
for WFPJ>=600	(0.13)	(0.10)	(0.13)	(0.11)
2nd Stage- Emergency	41.85	166.40	-3.19	-6.16
	(37.86)	(111.70)	(6.35)	(4.80)
Window 200 WFPJ	N	Y	N	Y
Observations	127	174	128	176

Notes: Only vacancies which last fewer than 1,000 days are included. Each regression controls for *NumberInNomination* , *NumberInConfirmation* , *LengthKnown, ChairDifference, PresidentDifference* and *CongressDifference* . Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix 2.2

I check for jumps at the cutoff of various variables that might relate to my outcome variables of interest. Using my main specifications, I test each covariate in turn against the 600 WFPJ cutoff and find no evidence of irregular heaping.

Figure A2.2.1. Covariate Tests
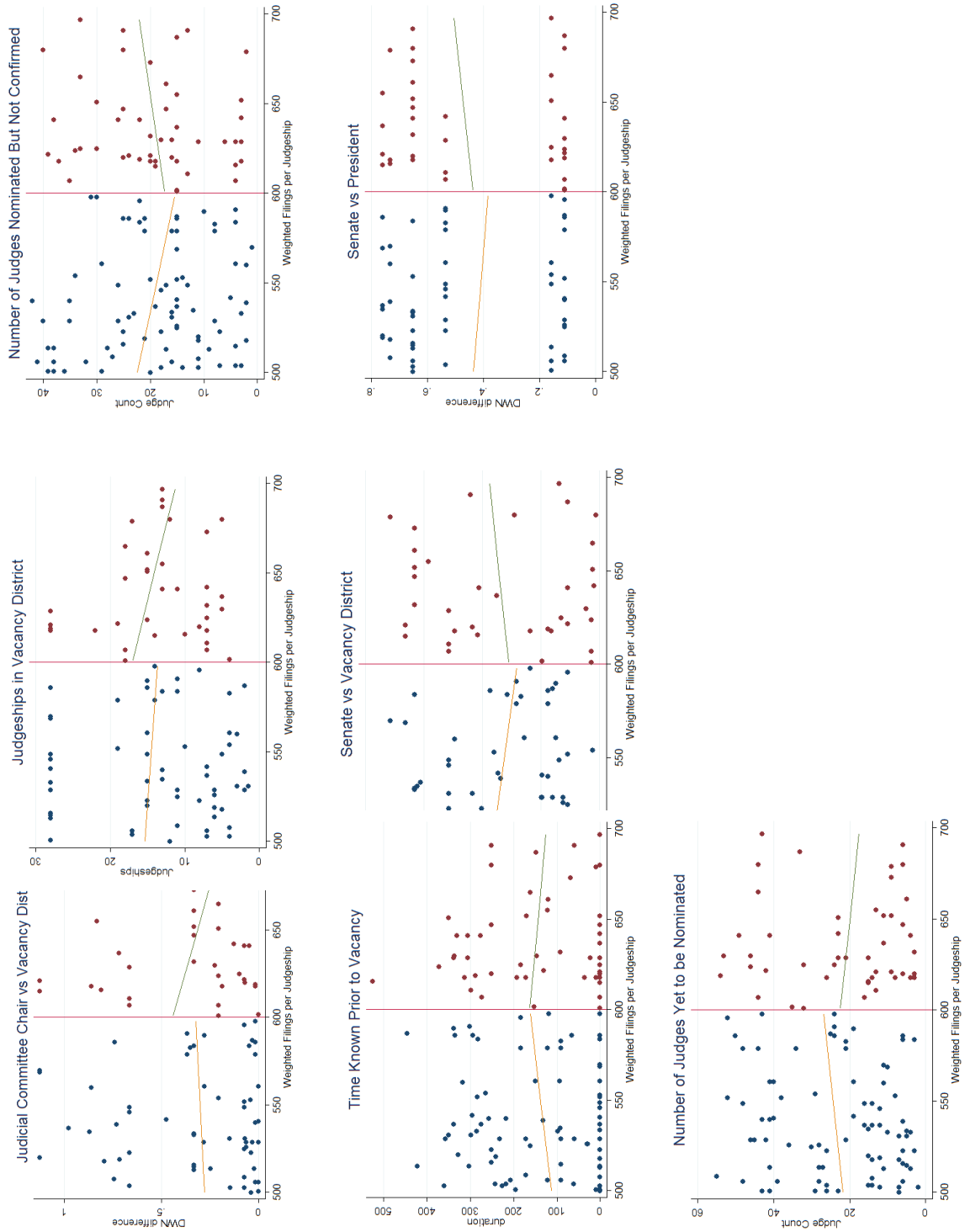
Table A.2.2.1: Covariate Tests

	AJ	FA	SA	OD	SD	PD	CD	NIN	NIC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Reduced Form - OLS Coeficent for "District Has >=600 WFPJ"									
Above 600	4.14	0.04	0.04	-0.12	0.06	0.12	0.21	-9.26	0.80
	(4.44)	(0.25)	(0.26)	(0.20)	(0.11)	(0.15)	(0.21)	(9.00)	(5.69)
B. 2SLS - Endogenous Regressor is "Vacancy is Labeled *Emergency*"									
2nd Stage- Emergency	2.68	0.03	0.02	-0.07	0.04	0.08	0.14	-6.00	0.52
	(2.82)	(0.17)	(0.17)	(0.13)	(0.07)	(0.10)	(0.13)	(5.88)	(3.76)
Observations	128	128	128	128	128	128	128	128	**128**

Notes: Dependent variables are as follows: Allocated judgeship positions, Senate, President and District aligned, Senate and district aligned, Opposition difference in DWN scores from district, Senate difference from district, President difference from Senate, Judiciary Chair difference from District, Number in Nomination and Number in Confirmation. Each regression only controls for a count of WFPJ above and below the cutoff. The window of 100 WFPJ is used in all

Appendix 2.3

Below I regress my outcome variables on the cutoff but I discard observations that occur within 20 WFPJ of the cutoff itself. Specifically, observations greater than 579 and less than 620 are excluded from the analysis. This is a test against districts that might have the ability to change their emergency designation. Here I do not find any changing results.

Table A2.3.1: Donut Regressions of Emergency Designation vs Duration Outcomes

	Commission Length		Nomination Length		Confirmation Length		Hearing Length	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Reduced Form - OLS Coeficient for "District Has >=600 WFPJ"								
Above 600	-33.10	46.03	41.94	38.26	-36.57	-5.88	-63.63	-5.26
	(104.60)	(83.57)	(96.34)	(74.12)	(44.28)	(34.77)	(44.12)	(35.44)
B. 2SLS - Endogenous Regressor is "Vacancy is Labeled *Emergency*"								
1st Stage, OLS coefficient for WFPJ>=600	0.61***	0.57***	0.63***	0.62***	0.61***	0.57***	0.68***	0.65***
	(0.13)	(0.15)	(0.20)	(0.14)	(0.13)	(0.15)	(0.20)	(0.15)
2nd Stage- Emergency	-49.48	126.40	66.23	62.17	-59.75	-10.31	-94.25*	-8.12
	(168.50)	(149.40)	(145.60)	(116.60)	(65.75)	(58.55)	(57.15)	(52.45)
Window 150 WFPJ	N	Y	N	Y	N	Y	N	Y
Observations	103	151	109	158	103	151	103	149

Notes: Controls are used for each regression. I report observations within the outer window and outside a donut of 20 WFPJ around the cutoff of 600 WFPJ. Specifically, for the first specification 580>WFPJ>500 or 700>WFPJ>620. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A2.3.2: Donut Regressions of Judge Quality

	Law School Ranking		ABA Ranking	
	(1)	(2)	(3)	(4)
	A. Reduced Form - OLS Coeficent for "District Has >=600 WFPJ"			
Above 600	15.50	34.72**	1.10	2.19*
	(20.94)	(17.41)	(1.35)	(1.26)
	B. 2SLS - Endogenous Regressor is "Vacancy is Labeled *Emergency*"			
1st Stage, OLS	0.61***	0.57***	0.61***	0.57***
ceofficient for	(0.13)	(0.11)	(0.13)	(0.11)
2nd Stage-				
Emergency	27.08	60.21*	1.92	3.81
	(34.48)	(31.50)	(2.44)	(2.55)
Window 200 WFPJ	N	Y	N	Y
Observations	103	151	103	151

Notes: Controls are used for each regression. I report observations within the outer window and outside a donut of 20 WFPJ around the cutoff of 600 WFPJ. Specifically, for the first specification 580>WFPJ>500 or 700>WFPJ>620. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix 2.4

The following are full regression results for the Tables 3 and 4 in the main text.

Table A2.4.1: Full Regression Results on Duration

	Commission Length	Nomination Length	Confirmation Length	Hearing Length	ABA Score	Law School Rank
	(1)	(2)	(3)	(4)	(5)	(6)
Above600	24.18	40.72	20.34	-10.76	1.09	21.83
	(92.16)	(68.36)	(52.69)	(43.22)	(0.85)	(13.60)
WFPJ	0.86	0.43	0.57*	0.13	-0.01	-0.15
	(0.83)	(0.80)	(0.31)	(0.27)	(0.01)	(0.11)
WFPJ*Above600	-1.86	-1.26	-1.22	0.01	0.02	0.03
	(1.74)	(1.56)	(1.06)	(0.95)	(0.02)	(0.24)
NumberInNomination	-1.83	1.40			-0.07*	-0.93
	(3.20)	(2.94)			(0.04)	(0.63)
NumberInConfirmation	-10.58***	-6.71***			0.02	-0.55
	(2.98)	(2.08)			(0.02)	(0.41)
ChairDifference		160.7**			1.85	22.34
		(79.79)			(1.27)	(18.41)
PresidentDifference		-702.80***				
		(200.10)				
CongressDifference		206.70			-1.69	-43.13
		(125.60)			(1.79)	(33.95)
LengthKnown	-0.35***	-0.29**			0.00	0.07***
	(0.13)	(0.12)			(0.00)	(0.03)
ConfirmationNIN	-8.65***		-2.25**	-0.41		
	(3.22)		(0.91)	(0.71)		
ConfirmationNIC	8.82**		0.86	0.92		
	(4.08)		(1.38)	(0.96)		
ConfirmationCD	299.4***		-0.06	0.03		
	(75.06)		(0.06)	(0.05)		
ConfirmationPD	-1,213***		-183.00**	100.90		
	(242.50)		(80.95)	(74.32)		
ConfirmationCD						
Observations	128	136	128	126	128	128
R-squared	0.44	0.36	0.15	0.05	0.06	0.13

Notes: Regressions are reported for the 100 WFPJ window. Control variables are chosen by what gave the highest adjusted r-squared. All variables involving DWN fall between 0 and 1. Variables beginning with "Confirmation" are calculated at the time the judge is nominated. Robust standard errors present in paranthesis. *** p<0.01, ** p<0.05, * p<0.1

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