The Effect of Institutional Changes on Judicial Behavior in the U.S. Courts of Appeals

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THE EFFECT OF INSTITUTIONAL CHANGE ON JUDICIAL BEHAVIOR IN THE U.S. COURTS OF APPEALS

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Periodically, over the past several years, various members of Congress debate whether to split the Ninth Circuit Court of Appeals into two separate courts. Arguments in favor of this decision consistently involve aspects such as large caseloads and the relatively high rate of review and reversal of Ninth Circuit decisions by the Supreme Court (see Posner 2000). What is unclear is whether splitting the Ninth Circuit into separate courts will address these (and other) concerns. While scholars of the judiciary recognize that institutional environments affect individual behavior (see Gillman and Howard 1999), it is not certain that splitting the Ninth Circuit (should legislation eventually pass out of Congress) will create different institutional environments for the new courts.

Such an empirical question needs to be addressed in order to determine whether splitting a judicial circuit adequately provides solutions to the issues offered by proponents of the measure. Yet, how does one conduct such an analysis before the action happens? Fortunately, a similar debate occurs approximately twenty years ago, when Congress decides to split the Fifth Circuit Court of Appeals into two separate courts. Additionally, this decision immediately follows a large expansion of federal judgeships across all circuits. Consequently, two substantial institutional changes occur within three years of each other that potentially influence the behavior of circuit judges.

While the substance of Congress' decision to split the Fifth Circuit into two separate courts has been well-documented by Barrow and Walker (1988), the effects of this action have received less attention from judicial scholars. Stidham, Carp, Songer, and Surratt (1992) conduct a limited analysis of voting outcomes resulting from the circuit split, but their analyses are lacking in two significant areas. First, they only conduct difference of means tests to determine if

average voting patterns change as a result of the circuit split. Second, they do not control for the expanded judiciary as a result of the Carter Omnibus Judgeship Bill passed in 1979.

Specifically, we focus on issues of court efficiency and the usage of *en banc* decisions. We examine whether various institutional changes (such as judicial expansions and splitting the Fifth Circuit) affect the workload of the federal appellate bench and the frequency of *en banc* review. To address these considerations, we employ a series of comprehensive analyses (using cross-sectional time-series models) on data provided by the Administrative Office of the U.S. Courts and the Courts of Appeals Database (compiled by Donald Songer).

INSTITUTIONAL STRUCTURE AND THE COURTS OF APPEALS

Scholars of political science have well documented the impact of institutional structures on the decision making of political actors across multiple contexts. For example, the literature on legislative politics (in particular, the U.S. Congress) is replete with research on the effects of institutionalization (e.g. Polsby 1968; Moe 1984) and institutional structure (e.g. Shepsle 1979; Riker 1980; Shepsle and Weingast 1987, 1994; and Schickler 2000) on legislative behavior. Additionally, several scholars demonstrate the impact of institutionalization and institutional structure on the U.S. presidency (e.g. Burke 1992; Weaver and Rockman 1993; Foley and Owens 1996; Cohen and Krause 1997; Foley and Owens 1996; and Krause 2002).

The literature on judicial politics has also witnessed a focus by scholars on aspects of institutionalization and institutional structure. Examples include McGuire's (2004) analysis of the institutionalization of the U.S. Supreme Court, and the numerous articles focused on hierarchical influences within the judicial institutional structure (e.g. Songer, Segal, and Cameron 1994; Martinek 2000; Benesh 2002; Benesh and Martinek 2002; Randazzo 2008). A

separate strand of judicial literature examines institutional changes based on the addition of new judgeships to courts. Though the process of increasing the number of federal judges is relatively slow – involving petitions from federal judges to Congress for increasing; followed by debate and votes on specific legislation – the impact of 'new blood' in the federal judiciary fundamentally changes its ideological composition (Barrow, Zuk, and Gryski 1996).

The expansion of federal judgeships also has an impact on caseload considerations, which coincidentally is one of the primary rationales arguing in favor of expansion. For example, Wright (1964) notes the dramatic increase of litigation following the passage of the Civil Rights Act; particularly for the Fifth and Ninth Circuits. Because of this substantial increase, the Fifth Circuit soon experienced substantial caseload burdens. Several short-term fixes were attempted, such as an increase in the reliance on visiting judges to sit on Fifth Circuit panels, and reductions in oral arguments and publication rates (Wright 1964; but, see Cohen 2002). Ultimately, calls for major institutional changes to the judiciary (including a massive judge expansion statute and a splitting of the Fifth Circuit) were needed to address these burgeoning caseload pressures.

An additional consideration possibly affected by institutional structures is the prevalence of *en banc* review by a circuit. This practice is often viewed as a way for the entire circuit to ensure specific panel decisions do not stray too far from the preferences of the whole. A recent article by Giles et al. (2007) demonstrates that ideological, strategic, and institutional factors (such as caseloads) affect the number if *en banc* cases in a given year. Regarding the Fifth and Ninth Circuits specifically, anecdotal evidence indicates that *en banc* considerations are a concern for appellate judges. In an anonymous interview (see Cohen 2002) a Ninth Circuit judge states:

> I do not think we could possibly hold *en bancs* with all of the judges. The history of the Fifth Circuit is that they found it intolerable with twenty-five, and I think

> that the eleven has worked extremely well. I think in general in all circuits there is a kind of dissatisfaction about *en bancs* because… you do not have them unless there is a certain amount of strife, so they are not the most pleasant things to go through. But, they are a kind of necessary evil…

Additionally, other anecdotal evidence indicates that judges on the Fifth Circuit viewed the difficulties with *en banc* decisions as a justification for splitting the circuit (Barrow and Walker 1988).

This begs the question of whether these major institutional changes significantly affected judicial behavior in the Fifth Circuit. In particular, our paper examines the impact of two institutional changes – a substantial increase in judgeships and the split of the Fifth Circuit into the Fifth and Eleventh Circuits – across several behavioral aspects, including caseload considerations and the frequency of *en banc* review. Given the repeated calls for similar institutional changes on the Ninth Circuit, it is imperative that scholars better understand whether these changes systematically affect judicial behavior and the institutionalization process.

Unfortunately, a limited amount of research examines how these changes affected decision making in the Fifth Circuit. Additionally, within the few existing studies none take a comprehensive approach to examine the impact of all institutional changes – they either examine the impact of judge expansion or the impact of the circuit split, but not the impact of both changes. This is disappointing given the historical importance of the Fifth Circuit, especially under Chief Judge Tuttle, in the racial integration of the Deep South (see Barrow and Walker 1988). Though the circuit as a whole was relatively balanced ideologically, this balance shifted dramatically once one considered geography; with the western portion of the circuit more liberal than the eastern portion. Consequently, proposals to split the Fifth Circuit generated substantial

concern among civil rights activists who feared the eastern half would attempt to overturn civil rights progress (Richardson and Vines 1970; Barrow and Walker 1988).

In hindsight, we 'know' that these fears were unrealized for the new Eleventh Circuit (comprised of the eastern portion of the 'old' Fifth Circuit). The limited analysis conducted by Stidham et al. (1992) demonstrates that voting patterns among judges after the split remained consistent (i.e. no statistically significant difference). Yet, little is known about *why noticeable differences did not emerge*. One plausible explanation involves the impact of the Omnibus Judicial Act of 1978 (hereafter the Carter Judge Act) – signed into law by President Carter three years prior to the Fifth Circuit split. This legislation expanded the Fifth Circuit from fifteen to twenty-six judges, giving Carter the ability to appoint eleven new circuit judges. Yet, political scientists have not examined whether this massive expansion of moderate to liberal Carter appointees also influenced judicial behavior. Our paper addresses this empirical question. Specifically, we examine the impact of the Carter Judge Act and the Fifth Circuit split across two areas –caseload considerations and use of *en banc* review.

THEORETICAL EXPECTATIONS

When considering the caseload pressures facing judges, one must be careful to separate two distinct aspects: absolute caseloads and relative caseloads. The former involves the total number of cases filed with a particular circuit; while the latter accounts for the number of judges on the circuit (i.e. per capita caseloads). These distinctions are important to note because changes to the institutional structure of the appeals courts are likely to affect each caseload aspect differently. For example, with regard to splitting the Fifth Circuit, one might expect this institutional change to affect the absolute caseload – because each newly created circuit (the

Fifth and the Eleventh) will have fewer cases overall to review – but not the relative caseload because the per capita number remains constant. This leads to our initial hypothesis:

H_1: Splitting the Fifth Circuit will lead to a decrease of the absolute caseload, but not affect the relative caseload.

Conversely, expanding the number of judges on the circuit will affect the relative caseload (i.e. by lowering the per capita number of cases) but not the absolute caseload. In fact, previous research examining expansion of judges indicates that reduction of relative caseloads is often a primary justification for increasing judgeships; doing so allows judges to maintain the quality of their decisions without facing undue workload burdens (see Wright 1964; Howard 1981; Posner 1996; Cohen 2003). This leads to our second hypothesis:

H_2: Increasing the number of judges will lead to a decrease in the relative caseload, but not affect the absolute caseload.

Examining the frequency of *en banc* review is important because these decisions provide the entire circuit an opportunity to monitor panels. While this is an important symbolic tool, the quote above indicates that judges rarely employ *en banc* review because of the substantial logistical difficulties involved. As Giles et al. (2007) discover a variety of strategic, ideological, and institutional factors affect the number of *en banc* decisions in a given year. Consequently, we believe that our two institutional changes will have differential impacts on *en banc* frequency. First, we expect that the splitting of the Fifth Circuit will increase the number of *en*

banc decisions because the new circuits will each have an incentive to more closely monitor panel decisions and settle precedential disputes.[1] Therefore, our third hypothesis states:

> *H_3: Splitting the Fifth Circuit will increase the number of* en banc *decisions in the newly created Fifth and Eleventh Circuits.*

Yet the expansion of judgeships may not operate in as straightforward a way as splitting the circuit. Giles et al. (2007) discovered a curvilinear relationship between expansion and *en banc* review – smaller circuits tended to increase the number of *en banc* decisions as they expanded while the frequency of *en banc* review actually decreased in larger circuits. Intuitively this makes sense because of diminishing marginal returns; that is, at a certain point increasing the number of judges on a circuit becomes too cumbersome to affect *en banc* review (as reflected by the quote from the Ninth Circuit judge). This leads to our fourth hypothesis:

> *H_4: Judicial expansion will have a curvilinear effect on the frequency of* en banc *review. For smaller circuits the number of* en bancs *will increase as new judgeships are added. However, for larger circuits, increases in judgeships will decrease the frequency of* en banc *decisions.*

RESEARCH DESIGN

To test these several hypotheses, we examine data on the Courts of Appeals, generally speaking (and in particular, the Fifth and Eleventh Circuits). To address questions concerning caseload considerations and *en banc* review we examine data from the Administration Office of the U.S. Courts (hereafter AO) for the time period 1975-1995.

[1] Giles et al. (2007) discuss this issue without making a theoretical statement on the likely effects of the Fifth Circuit split; rather, they just control for it without a stated theoretical expectation.

Caseload Considerations:

Our sample of cases, from 1975-1995, includes caseload information from all circuits as recorded by the AO. We rely on two separate dependent variables to measure caseloads. The first, *Absolute Caseload*, records the total number of cases litigated in specific circuits per year. The second dependent variable, *Relative Caseload*, measures the per capita caseload for circuits per year; and is calculated by taking the absolute caseload number in a given year, divided by the number of judgeships for a particular circuit.

The primary independent variable is the adoption of the *Carter Judge Act* which was passed in 1978 and went into effect in 1979. Therefore, we code this variable '1' from 1979 through 1984. In this year, Congress passed another judge expansion statute, the *Reagan Judge Act*, which created new positions beyond those established during the Carter administration. We therefore include a second dummy variable to measure this expansion; coded '1' from 1985 through 1990. In 1991, a third judicial expansion took effect – the *Bush Judge Act* – and consequently we include a final dummy variable, coded '1' from 1991-1995. Collectively, these three dummy variables allow us to control for specific moments of expansion among the federal judiciary. Additionally, we examine the effects across those circuits receiving the most judges compared to those circuits receiving the fewest to determine if a differential impact exists. The other primary independent variable measures the *Fifth Circuit Split* that occurred in 1981. This dummy variable is coded '1' for every year following the circuit split.

We also include multiple control variables to account for additional explanations of caseload changes in the federal appellate courts. Specifically, we control for increases in the *Crime Rate* (which we measure as the number of reported crimes per 1000 people) and the

Population. Both aspects allow us to account for potential increases in the demand for litigation by the Courts of Appeals and are expected to have a positive relationship to caseloads (see Shughart and Karajan 2002).

En Banc *Review*:

Data for this part of the analysis also comes from the AO, 1970-1995. Specifically, we examine influences on the number of *en banc* decisions. As such, our dependent variable is a count of the number of *en banc* cases per year. We maintain the same independent variables for these models as used to examine caseloads. Thus, we include dummy variables for each of the specific *Judge Acts* and a dummy variable to measure the *Fifth Circuit Split.*

In addition to these variables, we include several others to control for various aspects related to *en banc* review. The first variable, *Regime Change* is a dummy variable that measures when the partisan balance of a circuit changes. Giles et al. (2007) demonstrate that when the partisan balance shifts, circuits are more likely to conduct *en banc* reviews in order to monitor panel decisions and review policy changes. We therefore expect this variable to have a positive influence on the number of *en banc* decisions in a given year. Second, we include the variable *Caseload* to control for potential influences related to the number of cases on a circuit's docket. As this number increases (which signals that circuits are potentially becoming overloaded) then we expect the number of *en banc* decisions to decrease. Because of the wide variation involved, we take the natural logarithm to calculate this measure. Third, we control for the Ninth Circuit's use of mini *en bancs* after 1981. This option became available as part of the Carter Omnibus Judge Act and allowed for overloaded circuits to conduct *en banc* review with less than full circuit representation. In 1981, the Ninth Circuit formally took advantage of this provision, and

currently is the only circuit to do so. The dummy variable *Mini-En Banc* is coded '1' only for the Ninth Circuit after 1981. Theoretically, we expect this variable to be positively related to the number of *en bancs* per year. Finally, to control for the number of actual judges on a particular circuit we include the variable *Number of Judges*. We also include the squared term *Number of Judges*^{2} to account for potentially diminishing returns. Consequently, we expect the number of *en banc* decisions to increase initially as circuits gain more judges, but eventually decrease as circuits become too cumbersome to conduct *en banc* reviews.

EMPIRICAL RESULTS

To examine the influences pertaining to caseload considerations, we employ two distinct empirical specifications. The first is a static model using fixed effects regression and the second is a dynamic error correction model with fixed effects. The fixed effects regression model is appropriate (see Kennedy 2008) because the dependent variables (measuring absolute and relative caseloads) are continuous and because we need to account for specific idiosyncracies related to the circuits.[2] However, these models only provide static glimpses about the impact of our covariates on the dependent variables. To account for potential temporal influences brought about by the evolutionary influences of the covariates, we also employ fixed effects error correction models (see DeBoef and Keele 2008). The results of these models are presented in Table 1. An examination of the model statistics indicates that each provides valid estimates of caseloads.

Insert Table 1 Here

[2] We also ran specific diagnostic tests to check for issues related to temporal variation and discover there are no problems related to non-stationarity, autocorrelation, or unit roots.

The first two models examine the effects of covariates on absolute caseloads. Model 1 presents the results from the fixed effects regression model. These results indicate that passage of the *Carter Judgeship Act* significantly increased the absolute number of cases reviewed by the Courts of Appeals (even when controlling for circuit idiosyncracies). In fact, the presence of the Carter Act increased absolute caseloads by approximately 98 cases per year. Yet, the remaining judge expansions had no significant effect on caseloads. Consequently, the empirical evidence provides mixed support for our hypothesis that increases in the number of judgeships will have no affect on absolute caseloads. We also hypothesized that splitting the Fifth Circuit would lead to a decrease in the absolute caseloads of the Courts of Appeals. The results in Model 1 support this expectation. The variable *Fifth Circuit Split* is significant and negative and the coefficient indicates that the split decreased caseloads by approximately 537 cases per year across all circuits. Finally, the control variable *Crime Rate* is significant and negative but the coefficient reveals a negligible substantive impact (i.e. for every point increase in the crime rate, caseloads decrease by approximately 0.1 cases).

Model 2 provides results from a fixed effects error correction model (ECM) to better capture dynamic influences related to time. Generally speaking, error correction models operate by examining changes to a dependent variable, which in turn is calculated by first differencing the data.[3] Therefore, rather than examining values of the dependent variable (Y) at time *t*, as one would in a traditional cross-sectional analysis, the error correction model examines changes in the dependent variable (ΔY) where:

$$\Delta Y = Y_t - Y_{t-1}$$

[3] Differencing the data is an important step in removing any spurious temporal effects from trending data – thereby insuring its stationarity.

Additionally, ECMs require inclusion of a lag of the dependent variable as a regressor. Consequently, we include *Lag of Caseload* as an independent variable in the model. The remaining independent variables are included in two forms – to control for both short-term and long-term effects. To measure any immediate impact, each independent variable is included in a first differenced form (i.e. ΔX). The first differenced variables measure the instantaneous effects of the independent variable on the presence of rights cases on high court dockets. To measure any long-term effects on caseloads the independent variables are also included in lagged form (i.e. X_{t-1}). Consequently, the lagged versions allow us to measure the substantive impact of previous time periods on the development of rights litigation. Overall, the ECM provides analysts with the ability to measure both instantaneous effects and long-term impacts, while avoiding potential spurious relationships often exhibited in dynamically trending data.

Examining the results of the dynamic fixed effects error correction analysis in Model reveals that the variable *Lag of Caseload* is significant and negative – this is expected in an error correction model, since the statistical technique removes trending data by calculating first differences and then analyzing ΔY (in this case *Δ Caseload*). Substantively, a negative coefficient indicates that increases in the number of cases in year *t* are followed by a decrease in the next year that "erases some, but not all of the prior increase" (Hansford and Johnson 2008). Thus, we can interpret this negative coefficient as an acknowledgment that any increasing trends in caseloads are due only in part to litigation in the previous year.

Turning to the primary independent variables of interest in Model 2, we see that passage of the *Carter Judgeship Act* did not have an immediate effect on caseloads (i.e. the variable *Δ Carter Act* is not significant). However, this legislation had a long-term influence; increasing caseloads by 58 cases each year, starting a year after its implementation. The empirical results in

Model 2 also indicate that the other judgeship expansions had neither a short-term nor long-term impact on absolute caseloads. Conversely, splitting the Fifth Circuit had both a short-term and a long-term influence. Immediately upon the split, caseloads for the Fifth (and Eleventh) Circuit decreased by 445 cases. Moreover, this decrease continued in the long-term by an additional 367 cases per year.

Turning to the second dependent variable, relative caseloads, Model 3 presents the results from the static fixed effects regression analysis. The evidence indicates that all three judgeship expansions had a significantly positive influence on relative caseloads. Specifically, the passage of the *Carter Judgeship Act* increased relative caseloads by approximately 12 cases per judge per year; the *Reagan Judgeship Act* increased caseloads by almost 6 cases per judge per year; and, the *Bush Judgeship Act* increased caseloads by approximately 7.5 cases per judge per year. This evidence contradicts our hypothesis – our expectation, based on justifications for judge expansions, is that increases in the number of judges on a circuit should decrease the relative (i.e. per capita) caseload of that circuit. Additionally, the variable *Fifth Circuit Split* has a significant and negative effect on caseloads; after the split relative caseloads for these circuits decreased by approximately 17 cases per judge per year. Consequently, the evidence does not support our expectation that splitting the circuit would have no effect on relative caseloads. Finally, both the control variables *Crime Rate* and *Population* are significant and negative indicating that increases in each lead to a decrease in per capita caseloads across all circuits.

While these static results provide useful insights, the dynamic analysis (based on a fixed effects error correction model) presented in Model 4 offers additional intuitions. First, similar to the analysis examining absolute caseloads the lagged relative caseload variable possesses a significant and negative coefficient; indicating that increases in the number of cases in year *t* are

followed by a decrease in the next year that "erases some, but not all of the prior increase" (Hansford and Johnson 2008). Second, all three judgeship expansions possess significant long-term effects but do not impact relative caseloads in the short-term. Thus, the *Carter Judgeship Act* increases relative caseloads by approximately 9.5 cases per judge per year starting one year after its implementation; the *Reagan Judgeship Act* increases caseloads by 5 cases per judge per year; and the *Bush Judgeship Act* increases relative caseloads by 7.5 cases per judge per year. Moreover, we observe the *Fifth Circuit Split* possessing a long-term impact on relative caseloads but no short-term effect. One year after the split, per capita caseloads decreased by approximately 11 cases per year. Finally, Model 4 reveals that *Crime Rates* have both a short-term and a long-term impact; whereas *Population* only affects relative caseloads in the long-term.

Insert Table 2 Here

Turning our examination to the influence of institutional changes on the frequency of *en banc* review, we see different patterns in Table 2 than we observe for caseloads. Recall that the dependent variable in these analyses is the number of *en banc* cases in a given year. Consequently, because the dependent variable is a count, we employ a negative binomial regression model to examine the effects of particular covariates. According to Kennedy (2008) this model is appropriate to control for potential dispersion in the data (which is a legitimate concern, given the wide range of variation in the dependent variable). Model 5 provides the impact of institutional changes across all circuits regardless of their size – it assumes all circuits are equal. Examining the results in this model indicates that only the *Reagan Judgeship Act* had a

significant effect on the frequency of *en banc* review. Its passage led to an increase in *en banc* usage by almost 3 decisions per year (from a baseline of approximately 5 decisions per year). Additionally, the *Fifth Circuit Split* significantly decreased the number of *en banc* decisions by 2.6 cases per year. Finally, the variable *Number of Judges Squared* possesses a significant and negative coefficient, but its substantive impact (-0.017 cases) is relatively negligible.

A more interesting pattern is displayed in Model 6; where we separately examine the effects of institutional changes on the smallest and largest circuits. Looking at these results indicates that the *Carter Judgeship Act* had an effect on the frequency of *en banc* review, but only for the circuits with the smallest influx of judges[4] – their *en banc* decisions increased by approximately 2 cases per year (from a baseline of approximately 5 cases per year). The *Reagan Judgeship Act* had a more pervasive impact – not only did its passage affect the smallest circuits (increasing their *en banc* review by 1 case per year), but it also had a tremendous effect on the largest circuits (increasing their *en banc* review by over 14 cases per year). Additionally, the *Fifth Circuit Split* significantly decreased the frequency of *en bancs* by approximately 4 cases per year. Finally, the control variable *Number of Judges Squared* possesses a significant and negative coefficient, but does not have a large substantive impact.

CONCLUSIONS

To what extent did the splitting of the Fifth Circuit combined with the Carter Judgeship Act affect judicial behavior? Our analysis, albeit still in its preliminary stages, offers some

[4] To identify potential disparate effects based on the number of new judgeships added, we separately examine circuits with the smallest influx of judges and circuits with the largest influx. For the Carter Judgeship Act the First, Third, Seventh, Eighth and Tenth Circuits received the smallest number of new judgeships (one each); whereas the Fifth, Ninth, and DC Circuits received the largest influx of new judges. For the Reagan Judgeship Act, the First, Third, Fourth, Eighth, and DC Circuits received the smallest number of judgeships (one each) while the Ninth and the Sixth Circuits received the largest influx of new judges. Finally, for the Bush Judgeship Act, the First, Second, Seventh, Ninth, and Eleventh Circuits received the smallest influx of judgeships (none) while the Third, Fourth, and Tenth Circuits received the largest expansions.

insight into this question. First, it is apparent from the empirical evidence that judicial expansion tends to lead to increases in both the absolute and relative caseloads of appellate panels; though the impact is observed less with regards to absolute caseloads. Second, the dynamic models examining caseloads indicates that the effects of judicial expansion are primarily long-term influences rather than short-term. Third, splitting the Fifth Circuit significantly decreased both the absolute and relative caseloads for the judges in those two circuits (the reconstituted Fifth and the newly created Eleventh Circuits). Moreover, the circuit split had both an immediate and long-term effect on absolute caseloads, and a long-term effect on relative caseloads.

Finally, both the Carter and the Reagan Judgeship Acts significantly increased the frequency of *en banc* review, but these patterns become most distinct when one examines separately the circuits with the smallest and largest influx of new judgeships. Conversely, splitting the Fifth Circuit significantly decreased the number of *en banc* decisions.

Collectively, these results provide mixed support for our theoretical expectations concerning the influence on institutional changes on judicial behavior. While the conclusions shed light on interesting questions relevant to the current debate on splitting the Ninth Circuit, several questions remain that should be the topics of future research. In particular, did these institutional changes affect the ideological voting of judges on the Fifth and Eleventh Circuits? Are these effects, if any, consistent across all issue areas? Finally, can we predict what might happen if the Ninth Circuit were split in a similar fashion?

TABLE 1: FIXED EFFECTS REGRESSION MODELS OF CASELOAD CONSIDERATIONS

	Absolute Caseloads		**Relative (per capita) Caseloads**	
	Model 1 - Static Coefficients (Standard Errors)	**Model 2 - Dynamic** Coefficients (Standard Errors)	**Model 3 - Static** Coefficients (Standard Errors)	**Model 4 - Dynamic** Coefficients (Standard Errors)
Lag of Caseload		-0.616*** (.064)		-0.667*** (0.059)
Carter Judgeship Act	98.021*** (30.182)		12.241*** (2.656)	
Δ Carter Act		31.077 (47.369)		0.954 (4.090)
Carter Act_{t-1}		58.854* (33.707)		9.595*** (2.957)
Reagan Judgeship Act	-12.426 (29.274)		5.717** (2.576)	
Δ Reagan Act		20.352 (45.388)		5.478 (3.914)
Reagan Act_{t-1}		9.145 (32.979)		4.929* (2.883)
Bush Judgeship Act	21.995 (30.875)		7.578*** (2.717)	
Δ Bush Act		--		--
Bush Act_{t-1}		30.238 (33.137)		7.409** (2.844)
Fifth Circuit Split	-537.334*** (55.661)		-17.326*** (4.899)	
Δ Split		-445.102*** (126.919)		5.905 (10.715)
$Split_{t-1}$		-366.587*** (64.964)		-11.397** (4.936)
Crime Rate (per 1000 citizens)	-0.077*** (0.021)		-0.008*** (0.002)	
Δ Crime Rate		-0.075** (0.033)		-0.011*** (0.003)
$Crime\ Rate_{-1}$		-0.039 (0.025)		-0.004* (.002)
Population	1.815 (1.164)		-0.432*** (0.102)	
Δ Population		16.396 (10.016)		-0.281 (0.851)
$Population_{t-1}$		-0.022 (1.344)		-0.452*** (0.851)
Constant	453.491 (220.465)	454.795 (1248.174)	175.228 (19.403)	145.436 (23.921)
N	291	270	291	270
F Statistic	24.230	10.820	17.500	12.320
Probability > F	0.000	0.000	0.000	0.000
Number of Groups	12	12	12	12
Within-Group R^2	0.347	0.346	0.278	0.375

* p < .10 ** p < .05 *** p < .01

TABLE 2: NEGATIVE BINOMIAL REGRESSION OF *EN BANC* REVIEW

	Model 5		**Model 6**	
	Coefficients (Standard Errors)	Marginal Effects (Base = 4.898)	Coefficients (Standard Errors)	Marginal Effects (Base = 4.792)
Carter Judgeship Act	0.098 (0.152)			
For Smallest Influx			0.324** (0.156)	1.870
For Largest Influx			-0.348 (0.120)	
Reagan Judgeship Act	0.489*** (0.182)	2.720		
For Smallest Influx			0.260** (0.128)	1.288
For Largest Influx			1.489*** (0.354)	14.700
Bush Judgeship Act	0.313 (0.230)			
For Smallest Influx			-0.031 (0.120)	
For Largest Influx			-0.184 (0.238)	
Fifth Circuit Split	-0.724** (0.280)	-2.606	-1.786** (0.727)	-4.321
Regime Change	-0.020 (0.176)		-0.078 (0.171)	
Caseload	0.063 (0.123)		0.043 (0.119)	
Mini *En Banc*	0.474 (0.373)		-0.055 (0.389)	
Number of Judges	0.130 (0.086)		0.145 (0.096)	
# of Judges Squared	-0.004* (0.002)	-0.017	-0.007** (0.003)	-0.032
Constant	0.278 (0.907)		0.683 (1.007)	
N	290		290	
Log-Likelihood	-728.006		-716.323	
LR χ^2	234.95		258.320	
Probability > χ^2	0.000		0.000	
Pseudo R^2	0.139		0.153	

* p < .10 ** p < .05 *** p < .01

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