

Measuring Judicial Discretion

Bert I. Huang*

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Abstract

Taking a novel empirical approach, I assess the leeway that federal appeals courts have in deciding the outcomes of cases. I observe that when the caseloads in two circuits recently jumped by 40 percent, due to a flood of appeals from a single federal agency, the outcomes in *other* cases changed: In a wide range of civil appeals from the district courts, the flooded circuit courts became more selective in “correcting errors.” They not only dismissed appeals more readily, before reaching a decision on the merits; but also reversed or remanded less often, in the cases that did proceed to a merits decision. For decades, federal judges have argued that changes such as these might occur if judicial resources failed to keep pace with growing caseloads. This study, by performing a differences-in-differences comparison of changes in the flooded and non-flooded circuits, credibly identifies (for the first time) the proposed causal link between appellate workload and case outcomes. More generally, it introduces a method for locating and measuring the room for discretion in decisionmaking.

* Law clerk to Justice David H. Souter, Supreme Court of the United States; and candidate for Ph.D. in Economics, Harvard University. I began this project as a Considine Fellow at Harvard Law School. Comments welcome at huang@post.harvard.edu. I am grateful to Claudia Goldin, Christine Jolls, Louis Kaplow, Lawrence Katz, and Steven Shavell for advice and guidance; to Robert Akerlof, Glenn Cohen, Rosalind Dixon, Benjamin Edelman, Sydney Foster, Scott Hemphill, Katerina Linos, J.J. Prescott, David Rosenberg, Daniel Schwarcz, and the Considine Writing Group for insightful comments; to Thomas Miles for sharing data; and to Michael Boudin, Noah Feldman, Robert Sack, Matthew Stephenson, and Jeannie Suk for helpful conversations. My colleagues at NBER and workshop participants at Vanderbilt Law School, Harvard Law School, and the Harvard Economics Department offered many useful suggestions. The Olin Center at Harvard Law School; the Harvard Project on Justice, Welfare, and Economics; and a National Science Foundation IGERT grant provided financial support.

[H]ow often and to what extent do modern federal appellate judges feel 'constrained' to decide cases in particular ways (i.e. to eschew reliance on ill-defined discretion), and in what senses are they in fact so bound?

- Federal appeals judge Harry Edwards¹

1. Introduction

Our nation's appeals courts are charged with correcting errors in lower-court rulings – but it is often unclear, based solely on legal sources, whether a given ruling should be overturned. Appellate judges have reported feeling that they must “exercise discretion,” relying on non-legal criteria, in deciding a certain number of cases;² and legal theorists have debated whether some quantum of discretion may be unavoidable.³ Yet, the most basic questions of discretion and constraint in common-law judging – centrally relevant to our notions of the rule of law – have so far proved elusive to empirical inquiry:⁴ How much leeway does an appeals court have, in deciding cases? How readily are its decisions swayed by concerns beyond “the facts” and “the law”?

In this paper, I introduce a simple method for addressing these questions: by seeing how a court's decisions respond to an external shock (here, a jump in workload) otherwise unrelated to the cases of interest. In essence, this approach locates discretion by observing changes in how it is exercised, under pressure.⁵ The ideal but infeasible experiment – seeing the same judge decide the same case under varying conditions – is here approximated by tracking a given court's decisions in a given set of cases.

¹ Edwards (1983b), at 389, posing empirically the question of “discretion” in appellate judging.

² Judge Edwards set the figure at 5 to 15 percent of appeals coming before him. Edwards (1983b), at 390. Then-judge Patricia Wald estimated 15 percent. Wald (1992), at 190. Famously, as a state appeals judge, Cardozo (1924) wrote that “nine-tenths, perhaps more, of the cases that come before a court are predetermined,” leaving a “range of free activity” in the rest. *Id.* at 60.

³ See, e.g., Raz (1972); Dworkin (1963). As Greenawalt (1975) put the theorists' concern: “Do judges in some cases have freedom in resolving legal issues to decide them more than one way, or are judges always legally bound to reach one conclusion rather than any others?” *Id.* at 364-65.

⁴ The many empirical studies focused on how judicial decisions vary across judges, or across case or litigant types, are not well-suited for addressing this core jurisprudential concern: How constraining, for a *given* adjudicator's decision in a *given* case, are the extant legal materials and the particulars of that case? I explain further in section 7.

⁵ This approach is analogous to “stress testing” or “tensile testing” in other contexts.

Using administrative data from the federal courts, I follow the decisions of two circuit courts recently flooded by tens of thousands of appeals from the federal immigration agency, due to deportation “streamlining” after the terrorist attacks of September 11, 2001. This flood of agency appeals increased the total caseload of each of the two courts by 40 percent. (See Figures 1 to 3.) My central finding is that this severe docket crowding affected the two courts’ decisions in a broad (and separate) set of cases: the vast pool of civil appeals arising from the federal district courts, a category ranging from civil rights to banking to copyright cases.

When flooded, these courts became more selective in “correcting errors” in civil cases. Specifically, they reversed or remanded lower-court decisions less often. Part of this decline resulted from a greater readiness to dismiss an appeal before it reached a panel of judges for a merits decision; but reversal-or-remand rates also fell among those cases that did proceed to a decision on the merits.⁶ Viewed differently: before the flood, with lighter dockets, these courts were free to overturn rulings at the same (lower) rates as during the flood; but they did not, choosing instead to correct errors more vigorously when they had more time.

Four features peculiar to this caseload shock help to strengthen an inference that the flood caused (not merely coincided with) the observed changes in case outcomes. First, the caseload flood was concentrated in the Second and Ninth circuits – leaving the remaining circuits to serve as a baseline for comparison, in a differences-in-differences approach (thereby accounting for any common changes in law or procedure, or in case quality). Second, the courts had no say in the deportation streamlining, which the Attorney General abruptly ordered in the aftermath of 9/11. Third, geographic accident determined which circuits absorbed the brunt of the flood. Fourth, the immigration agency’s decisions are appealed directly to the federal appeals courts –bypassing the district courts. The flow of agency appeals is thus unlikely to have tainted the quality or composition of civil appeals by crowding them in the district courts.

These features form a natural experiment useful for isolating the impact of the caseload crush, making causal inferences more credible. Given this simple experimental

⁶ Throughout, I use the terms “dismissal” and “merits” following the usage in the judiciary’s official statistics: Cases are decided “on the merits” if they reach oral argument or submission on briefs to a panel of judges. They are “dismissed” if they do not reach that stage.

design, the raw data pictured in Figures 4 and 5 essentially tell the story—showing how case outcomes in the two flooded circuits diverged from those in the other circuits, during the flood. Quantifying these divergences, a difference-in-differences model suggests that as of three years into the flood, a 6 to 7 percentage-point rise in dismissal rates, as well as a 4 to 7 point drop in reversal-or-remand rates, may be attributed to the flood’s effect. (Seen from the parties’ angle, appellees won more often, on both counts.) Reversals and remands also fell as a share of merits decisions, both in raw levels and according to difference-in-difference estimates.

Such changes suggest that the federal appeals courts do have a measurable degree of discretion in deciding civil cases—enough leeway that they can decide similar cases differently, for a reason unrelated to case quality and to the relevant law. Further results show that this revealed flexibility in case outcomes varies by the type of litigant (counseled versus *pro se*) and by legal subject matter (federal versus state law⁷). Perhaps surprisingly, the courts’ publication choices appear to be more rigid, in comparison.⁸

This study makes several contributions: First, although observers have surmised, and federal judges have reported, that crowded dockets may influence the outcomes of cases on appeal (see section 2), I analyze a unique natural experiment from which we might infer a causal link. This paper is, to my knowledge, the first empirical attempt to make such a showing.⁹

Second, this study offers a vivid example of how a change in one area of law can “spill over” to other areas, in our system of generalist courts: the post-9/11 change in deportation policy affected not only cases involving immigration, but also civil cases presenting a wide range of legal issues. Similar spillover effects might well arise, should Congress further expand the courts’ mandatory jurisdiction (for instance, by passing laws that generate appeals) but without also increasing judicial resources.¹⁰

⁷ As proxied by the federal-question versus diversity jurisdiction divide.

⁸ Another margin reported to be relatively “sticky” is delay in adjudication. See section 2.1.

⁹ Related empirical research includes Vladeck and Gulati (2005), who assume that caseloads affect publication rates, *id.* at 1673, but do not test the assumption. Helland and Klick (2007) find evidence that federal district court caseload affects attorney’s fee awards. Taha (2004) reports a correlation between federal district judges’ caseloads and their publication rates.

¹⁰ See Purcell (2003), Posner (1996), and Kramer (1991). Spillover effects have been a major concern for the federal judiciary since at least 30 years ago, when federal appeals judge Henry

Third, these findings highlight an often overlooked locus of judicial discretion. Discretion in correcting errors is revealed, in this study, to be available not only in judges' merits decisions – the focus of most legal commentary¹¹ – but also in the court's threshold choice whether to dismiss a case before it reaches that stage.¹² These two choices jointly determine individual case outcomes, and hence overall error-correction; and given that roughly half of all appeals are dismissed, a court's discretion in “deciding to decide” deserves greater scholarly attention.¹³

Finally, this paper demonstrates a general method for measuring discretion in decision-making: by seeing “what gives,” and what does not, when a decision-maker is put under pressure. For instance, to study where a schoolteacher has discretion, we might observe how his teaching practices change, when his class gains 10 transfer students: Does he end class at 2:15 instead of 2:05 p.m.? Does he write exams that are easier to grade? As to prosecutorial discretion, we might observe how a DA reacts when a drug-ring bust expands her pool of defendants: Does she drop existing indictments? Does she induce burglars to plea, making time for prosecuting traffickers?

The paper continues as follows: Section 2 reviews federal appeals judges' accounts of how, given the discretion inherent in their work, caseload pressure might affect a court's output. Section 3 details my empirical approach. Section 4 presents basic results; and extensions in Section 5 focus on cases reaching a merits decision and on the relative rigidity of the courts' publication choices. Section 6 addresses limitations and alternative explanations. A conclusion follows, setting this study apart from those examining variation across judges, and situating it within a broader inquiry into judicial production.

Friendly warned of the need to “avert the flood by lessening the flow,” namely, by removing certain types of cases from the jurisdiction of the generalist federal courts. Friendly (1974).

¹¹ See Kim (2006) for a recent survey.

¹² Throughout, I speak of the entire court as a single unit; but my findings (especially on dismissals) urge further inquiry into appeals courts' internal organization. See section 6.

¹³ See Shapiro (1985) for a related study of the “situations in which the federal courts have effectively been free to choose whether or not to exercise or assume jurisdiction.” *Id.* at 546.

2. The judges' hypotheses: first-hand accounts of caseload effects

In the federal judiciary, the courts of appeals are the “court[s] of last resort for all but the handful of cases that the Supreme Court will agree to hear.”¹⁴ Disposing of more than 60,000 appeals a year – about half by dismissal, half decided on the merits – these courts have the final say in nearly all of them. In addition to “correcting errors,” they also have a “law-declaring” role.¹⁵ As common-law courts whose 5,000 published opinions each year carry the force of legal precedent in their jurisdictions, these courts are also our nation’s most prolific interpreters of federal law.

It is uncontroversial that federal courts exercise a degree of “judicial discretion to administer [their] caseload to secure the just, speedy, and inexpensive determination of actions.”¹⁶ Federal appeals judges, moreover, are keenly aware that judicial resources are scarce,¹⁷ and that tradeoffs are necessary.¹⁸ Some have described their work as requiring “triage.”¹⁹ Tradeoffs, and the need for triage, have only become more acute as growing caseloads continue to outpace judicial resources; appeals filings have risen by over 30 percent in the past decade, but Congress has not created new appellate judgeships since 1990. As one federal appeals judge put it, “the bankruptcy of supply expansion suggests the need for some sort of rationing of federal judicial time, an undesirable *de facto* version of which may already be occurring in the courts.”²⁰

¹⁴ As described by federal appeals judge Carolyn Dineen King. King (2007), at 36.

¹⁵ These terms are borrowed from Breyer (1990).

¹⁶ Heckler v. Edwards, 465 U.S. 870, 879 n.12 (1984) (Marshall, J.).

¹⁷ According to Judge Stephen Reinhardt: “Most of us are now working to maximum capacity. As a result, when our caseload increases, we inevitably pay less attention to the individual cases.” Reinhardt (1993), at 52. Judge Jon Newman observed: “I have come to realize, in all candor, that I have been applying a shorter and shorter ruler to measure the time that ought to be devoted to each task. My standards are changing. Heavy volume is taking its toll.” Newman (1993), at 188. And Judge Robert Parker reported that caseloads were already “at levels that fundamentally undermine the ability of courts to administer justice.” Parker and Hagin (1994), at 211.

¹⁸ Federal appeals judge Ruggiero Aldisert described one such tradeoff: “Constraints of time demand the tradeoff. I would rather have adequate time for a decision conference, allowing for the discussion of complex and difficult issues . . . than be forced to shortchange those cases by the process of automatically granting oral argument in every case.” Aldisert (1978), at 321.

¹⁹ “Rational triage” is how federal appeals judge Frank Coffin described the “time-conscious evaluation of cases” for potential publication. Coffin (1994), at 175-76. See also Robel (1990), at 9.

²⁰ Edwards (1983a), at 922.

Several possible margins for “rationing” or “triage” are suggested by a sketch of the path of an appeal from filing to termination:²¹ In a first stage, the appeal is screened by the court for procedural defects, which may result in dismissal; and court officers may help the parties settle, also resulting in dismissal. In the second stage, a panel of judges decides the case “on the merits,” affirming or overturning the lower-court ruling; it also decides whether to publish an opinion.

As I detail below, several judges have suggested that—given the room for discretion in decisions at each of these two stages—adjustments due to ever-growing dockets might have the effect of changing case outcomes.

2.1. Caseloads and case outcomes

Federal appeals judge John Gibbons, noting the “troubling trend” that “while terminations in the courts of appeals have kept pace with filings, the reversal rates in those courts have declined markedly in recent times,”²² explained that reduced reversal rates may have served as a way of coping with growing caseloads:

The decline in reversals suggests that [our] remarkable achievement in productivity has been attained at least in part by the adoption of a posture of increased deference to the rulings of the courts we’re supposed to be supervising.²³

Federal appeals judge Richard Posner has also cautioned that “a possible consequence [of workload-related deference] is that fewer errors made by district courts are being corrected—an example of an undesired by-product of the growth in the caseload.”²⁴

²¹ Time reallocation is necessary if the courts do not permit enough delay or backlog to maintain prior levels of judicial time per case. Judge Posner explains that circuit courts use the “power of shame” to control delay—“at meetings of the judges, each judge is required to explain the status of every one of the opinions assigned to him that has not been issued within a specified period [e.g. 90 days].” Posner (1996), at 223. Yet rigidity in one margin may imply greater pressure on others: “If judges are led to think that the world is judging them exclusively on the speed with which they dispatch their business, they will speed up, all right, but the result may be a considerable deterioration in the quality of their decisions.” *Id.*

²² Gibbons (1989a), at 486.

²³ Gibbons (1989b), at 23; see also Gibbons (1989a), at 486.

²⁴ Posner (1996), at 176. Judge Posner identifies a similar effect in the district courts, observing that the “least visible but probably most important way in which the pressure of a growing caseload had resulted in streamlining or corner cutting” is the “sub rosa redefinition [by district courts] of standards for granting summary judgment and for dismissing a complaint.” *Id.* at 178.

In addition to inducing more-deferential review, time pressure may also affect case outcomes by prompting dismissals. As Judge Friendly noted of the district courts, in these days of crowded docket there is an inevitable risk of some degree of subconscious bias when [the] decision whether to dismiss a case . . . is made by the judge who will have to [hear] it.²⁵

The same logic may be thought to apply in appeals courts. Then-federal appeals judge Patricia Wald observed, analogously, as to the dismissal of specific arguments on procedural grounds: “I can tell you that the number of cases that go down on waiver or failure to raise the right point before the agency or trial court is too high. In an ideal system of justice, that might not be true, but realistically, time and docket pressures very definitely constrict the judge.”²⁶

These accounts accord with a basic time-budgeting story (call it “rationing” or “triage”) in which the reviewing court values accuracy in error correction:²⁷ As a court’s time demands rise, a court may reallocate its efforts; on those margins where discretion allows, the court may become more selective in choosing actions that are more time-intensive, or that become relatively less valuable when resources per case are reduced. As Judge Friendly urged, “defining the proper scope of review of trial court determinations requires considering in each situation the benefits of closer appellate scrutiny as compared to those of greater deference”;²⁸ and those relative benefits may depend on the cost of time. A rise in time costs makes time-saving options relatively more attractive; along the path of an appeal, dismissal saves the time required of a merits decision,²⁹ and affirmance tends to take less time than reversal.³⁰

²⁵ Friendly (1982), at 754.

²⁶ Wald (1984), at 10. This observation also speaks to case outcomes, because dismissing an otherwise viable argument in this way can lead to case dismissal or to affirmance. The practice tends to favor appellees, as it is usually the appellant who is required to have “preserved” a supposed error by raising an objection before the trial court.

²⁷ See Shavell (1995, 2006) and Daughety & Reinganum (2000) for formal models of the role of appeals courts in promoting accuracy in adjudication. See generally Kaplow (1994) for a theoretical treatment of the value of accuracy in adjudication.

²⁸ Friendly (1982), at 756.

²⁹ Cf. Friendly, (1982), at 754.

³⁰ As federal appeals judge Ruggiero Aldisert cautioned, “the danger is that some cases are affirmed rather than reversed because a reversal will require a time-consuming, researched opinion.” Aldisert (1997), at 43.

Moreover, the greater an appeals court's "institutional competence" for making accurate legal assessments, the more willing it may be to overturn lower-court decisions.³¹ Yet as Judge Edwards observed, "[t]he bigger the dockets, the less time we spend on the difficult cases and the more mistakes we make."³² Thus, as time demands increase, the appeals court senses that its institutional advantage, relative to the lower courts, is reduced.³³ Hence, the appeals court both scrutinizes and reverses lower-court rulings more selectively.

2.2. Deciding to publish opinions

In addition to case outcomes, a court's publication choices might also be responsive to caseload pressure. This is suggested by a general acceptance that the publication choice is expressly a matter of "triage"³⁴ or the "rationing of judicial time." As then-judge Ruth Bader Ginsburg observed:

Summary dispositions by order and unpublished memoranda are time savers . . . used to husband full opinion writing for decisions important to the development or clarification of the law.³⁵

Moreover, formal constraints are relatively light, for the publication decision,³⁶ as compared with choices such as dismissal (limited by procedural criteria and the willingness of parties to settle) and reversal (governed by relevant law). Judges tend to agree that it is within their courts' discretion to decide which cases merit a published opinion;³⁷ and indeed, the rules for publishing opinions invite discretion.³⁸

³¹ Consider, for instance, the familiar rationales for maintaining different standards of appellate review for issues of "fact" and of "law" (ambiguities aside).

³² Edwards (1983b), at 403.

³³ As federal appeals judge Calvert Magruder noted: "As to the trial judges, we must always bear in mind that they may be as good lawyers as we are or better. . . . [T]he main reason we on appeal may have a better chance of being right is that we have more time for reflection and study." Magruder (1958), at 3. See also Friendly (1982), at 757-58.

³⁴ Coffin (1994), 175-76.

³⁵ Ginsburg (1983), at 9-10. See also Coffin (1994), at 175-76.

³⁶ My study window precedes the effective date of the new Rule 32.1, in the Federal Rules of Appellate Procedure, which bars the circuit courts from prohibiting citation to any of their written dispositions ("published" or not) issued on or after January 1, 2007.

³⁷ For a prominent dissenting view, see Judge Richard Arnold's opinion in *Anastasoff v. United States*, 223 F.3d 898 (8th Cir. 2000).

³⁸ Consider the criteria in the Second Circuit:

2.3. Testing the judges' hypotheses

These judicial accounts present hypotheses that bear further empirical testing. For instance, a causal link between workload and “deference” cannot easily be inferred, based only on the dual trends that Judge Gibbons observed.³⁹ Swelling caseloads and falling reversal rates over the course of decades might be the joint result of a rising tide of meritless appeals, or due to other long-term factors. As the next section explains, my approach overcomes this difficulty by focusing on a sudden caseload increase—40 percent within a span of three years—and by using a control group to account for changing background factors. To my knowledge, no prior studies have attempted to show that the link between appellate caseloads and error-correction is causal.

3. Empirical strategy: a natural experiment

The essence of my empirical approach is to compare what actually happened, in a court flooded with extra cases, against what likely would have happened in that same court absent the flood. How historical reality and the counterfactual diverge is taken to be the true causal effect of the flood. Not observing the counterfactual, we can do our best to approximate it by constructing a baseline for comparison. In my differences-in-differences approach, I use the circuits that were not flooded (adjusted for characteristics and trends) as the baseline or control group.

Specifically, my preferred estimate of the causal effect will be this measure: how much more did the decisions of the flooded circuits diverge from their previous trends, during the flood, as compared with decisions in the control circuits?⁴⁰

Interim § 0.23. Dispositions by Summary Order. (a) Use of Summary Orders. The demands of contemporary case loads require the court to be conscious of the need to utilize judicial time effectively. Accordingly, in those cases in which decision is unanimous and each judge of the panel believes that no jurisprudential purpose would be served by an opinion (i.e., a ruling having precedential effect), the ruling may be by summary order instead of by opinion.

³⁹ Guthrie & George (2005), at 361, Table 1B. Professors George and Guthrie recognize that “[t]hese two phenomena may be related in any number of ways: one may be the cause of the other or both may be the product of other forces.” *Id.*, at 361.

⁴⁰ To address the possibility that prior trends might not have continued (in the counterfactual) throughout the study window, I also consider a variant of this measure—one which assumes that prior trends would have leveled out instead. See section 4.3.

To anticipate one potential concern: If case quality changed for the same reason as the caseload increase, it would be difficult to isolate the impact of caseload. As I explain next, a distinct advantage of this study is that I have chosen a source of caseload pressure (direct appeals from a federal agency's decisions) that is wholly separate from the cases whose outcomes I measure (civil cases from the district courts).

3.1. The source of the flood

Shortly after 9/11, in February 2002, Attorney General John Ashcroft pledged at a news conference that the DOJ would quickly clear out a deportation backlog, consisting of some 56,000 foreign nationals awaiting their hearings before the DOJ's Board of Immigration Appeals (BIA).⁴¹ In March, the BIA chairperson extended a special "streamlined" review process to all asylum and deportation appeals (that is, most of the BIA's cases).⁴²

As the federal judiciary newsletter observed, "[a]lmost immediately, the BIA doubled production, sending a deluge of petitions for review into the U.S. courts of appeals."⁴³ Moreover, the appeals rate from BIA decisions soared—because more of these decisions upheld deportation, and over half were unexplained summary orders⁴⁴—thereby sustaining the flood.⁴⁵

The flood continued unabated through the end of the study window in third-quarter 2005. As one Second Circuit staff attorney described the scene, at that point, it was one of literal "crowding":

In September 2005, one needed only to walk through the Second Circuit's case management offices to get a feel for the magnitude of this surge: Mountains of briefs had formed in almost every available space. Narrow

⁴¹ Press release available at <http://www.usdoj.gov/opa/pr/2002/February/02_ag_063.htm>

⁴² Dorsey & Whitney, LLP (2003), Appendix 22, "March 15, 2002 Memorandum to Board Members from Lori Scialabba, Acting Chairman."

http://www.dorsey.com/files/upload/DorseyStudyABA_8mgPDF.pdf The streamlined process authorized "affirmances without opinion" by any single member of the appeals board, and made single-member decisions the norm (rather than three-member decisions, as had been the standard). Palmer (2006), at 19.

⁴³ "Immigration Surge in Courts," The Third Branch: Newsletter of the Federal Courts, vol. 35, no. 9 (Sept. 2003). <<http://www.uscourts.gov/ttb/sep03ttb/immigration/index.html>>

⁴⁴ Dorsey & Whitney, LLP (2003), Summary of Findings and Conclusions, available at <http://www.dorsey.com/files/upload/Summary-Conclusion_DorseyABASTudy.pdf>

⁴⁵ Palmer (2006), at fig. 3.

paths snaked through the valleys, leading to desks fortified on all sides by thick walls of administrative records.⁴⁶

3.2. Using the flood as a natural experiment

Four features of this flood help us to isolate the causal effect of caseload on outcomes. First, the surge of immigration appeals was concentrated in two regional appeals courts, the Second and Ninth Circuits; other circuits were much less affected (Figures 1 to 3.) This pattern enables my differences-in-differences approach.

Second, the courts were not involved in the immigration agency's decision to clear its deportation backlog,⁴⁷ this fact relieves potential concerns about reverse causality. BIA officers are civil servants at DOJ, not a part of the judiciary; and by all indications "streamlining" was an internal agency decision.⁴⁸ Judges expressed surprise; for instance, Ninth Circuit Judge Dorothy Nelson reported: "It's just extraordinary. I've been on the court for 25 years, but I've never seen a rush overwhelming us like this."⁴⁹

Third, how the flood was spread among the circuits was a matter of geographic accident. The Second Circuit (covering New York) and the Ninth Circuit (covering California and all Pacific states) contain the locations where roughly three-quarters of the foreign nationals whose cases comprised the flood were initially processed by an immigration judge, making those two circuits the proper venues for their appeals.⁵⁰

Finally, by statute, these immigration appeals completely bypass the federal district courts; instead, they are appealed directly from the federal agency to the courts of appeals.⁵¹ This fact lessens the concern that the flood might have changed the composition or quality of cases being appealed from the district courts, by crowding them during that earlier stage as well. (To emphasize: I exclude the immigration cases

⁴⁶ Palmer (2006), at 14.

⁴⁷ None of the immigration agency's decision-makers seemed to recognize that streamlining might cause a flood of cases in the federal courts. Dorsey & Whitney, LLP (2003), at 11, 19-25, & Appendices. Available at <http://www.dorsey.com/files/upload/DorseyStudyABA_8mgPDF.pdf>

⁴⁸ Dorsey & Whitney, LLP (2003), at 11, 19-25, & Appendices. Available at <http://www.dorsey.com/files/upload/DorseyStudyABA_8mgPDF.pdf>

⁴⁹ Moore and Simmons (2005).

⁵⁰ 8 U.S.C. § 1252(b)(2) (2000).

⁵¹ 8 U.S.C. § 1252(b) (2000).

from all outcome measures; they are not classified as “civil” cases, and more to the point, their overall quality is plainly not comparable before and during the flood.)

3.3. Forty percent increases in caseloads

How this flood of cases affected the dockets of the Second, Ninth, and control circuits is shown in Figures 1 to 3. The control group consists of all other circuits from First to Eleventh; therefore, all federal appeals courts except the D.C. Circuit and the Federal Circuit (both of which have specialized dockets) are included.⁵² In the graphs, each dot represents the number of filings in a given quarter; lowess-smoothing curves are fitted to aid visualization. The lower curve tracks the immigration cases, and the upper tracks all other cases combined (civil, criminal, habeas, etc.). As these figures show, the flood was far more severe in the Second and Ninth Circuits than in the other courts.

4. Basic findings: discretion in “correcting errors” and “declaring law”

I present findings in two ways: first, using graphs of raw data; and second, using regression models. The data sample for the following graphs and regressions consists of civil appeals arising from the district courts, as documented by the Administrative Office of the U.S. Courts (AO); how I have limited this sample and cleaned the data is detailed in the Data Appendix.⁵³ The study window begins in fourth-quarter 1997, the earliest date at which the AO data indicate whether a case involved a *pro se* litigant; and ends with the latest available data, in third-quarter 2005. I thus examine eight October-to-September judicial terms, or 32 quarters. For visual convenience, on all graphs I have marked the start of the filings flood and its one-year mark with vertical lines.

⁵² As Judge Patricia Wald described it, the D.C. Circuit “is an animal of a different color from all others. We have no volume problems with Social Security or prisoner cases, and only a small number of diversity cases. Sixty percent of our appeals come not from the district court, but straight from the agencies.” Wald (1989), at 172.

⁵³ Most notably, I exclude habeas and prisoner suits. Like criminal cases, these suits are highly vulnerable as a class to shocks in federal criminal law, complicating the task of isolating caseload effects. For instance, my study window follows passage of a habeas statute (AEDPA) and is interrupted by major sentencing decisions in the Supreme Court (*Apprendi*, *Booker*). The portfolio of civil cases, by contrast, is diversified and less responsive to shocks in any single area of law.

During this study window, the number of authorized federal appeals judges remained constant; and the federal judiciary's staffing budget did not appreciably increase.⁵⁴ In October 2005, beyond the study window, the Second Circuit created a new "non-argument calendar" track for the BIA appeals, in order to relieve caseload pressure. In doing so, it revisited its long tradition of permitting oral argument in nearly all cases – providing an indication that it was indeed feeling the effects of the flood.

4.1. Visual evidence of divergence in case outcomes

How case outcomes in the flooded circuits diverged from those in the control circuits is readily seen in the graphs. Figures 4 to 7 track dismissals, reversals-or-remands, and the publication of opinions. In these figures, each dot represents the percentage of cases that reached a given outcome, in a given three-month period (fiscal quarter), in the two flooded circuits, combined. Plus signs mark the same measure for the control circuits, combined. As in Figures 1 to 3, lowess-smoothing curves are added to help visualize the path of the outcomes over time.

Dismissals. A sharp divergence between the flooded and the control circuits, in their dismissal rates, is readily apparent in Figure 4. These include all dismissals of an appeal before it reaches a judicial panel for a merits decision. The dots and the upper curve track the flooded circuits' average quarterly dismissal rates, which rise markedly during the flood (to the right of the vertical lines).

A visual guess suggests that the two flooded circuits were dismissing roughly 6 to 7 percent more cases by the end of the study window than at the start of the flood; over the same period, the dismissal rate in the control circuits appears roughly flat.⁵⁵ (Section 4 provides more rigorously derived estimates of these divergences.) Notably, because dismissal of an appeal pre-empts reversal, remand, and publication, an increase in dismissals may be viewed as lost opportunities for "correcting errors" or for "declaring law." The relationship between dismissals and the other outcomes will be explored in more depth, as an extension, in section 5.

⁵⁴ The use of senior and visiting judges (measured by either the number of judges or the number of cases they participated in) also did not systemically increase in the flooded circuits.

⁵⁵ Graphing dismissals individually by circuit (not presented here) shows that no circuit other than the Second and Ninth show a marked upturn coincident with the flood.

Reversals or remands. The two flooded circuits also show marked downturns in their reversal-or-remand rates (Figure 5). To ensure comparability, this measure includes reversals as well as remands;⁵⁶ circuits tend to vary in their usage of the two terms, at times interchanging them.⁵⁷ Combining these two categories serves as the best available measure of cases in which the appeals court chose to undo some aspect of a lower-court ruling—that is, to correct at least one error.⁵⁸

A visual guess suggests that by the end of the study window, roughly 4 to 5 percent fewer cases were being reversed or remanded, in the flooded circuits, than at the beginning of the flood. At the same time, the time path of reversals and remands in the control circuits seems to be flat.⁵⁹ This divergence is yet more pronounced if prior trends are considered; the error correction rate of the flooded circuits not only declined (in absolute terms) during the flood, but also turned around an upward trend.

Of particular interest, among reversals and remands, are those that were deemed important enough to deserve publication (Figure 6). Mirroring Figure 5, the evident decline in the rate of published reversals-or-remands confirms that the drop in error correction revealed in Figure 5 was not driven by changes only among the relatively trivial (*i.e.* unpublished) reversals or remands.⁶⁰

Publication. The percentage of terminated appeals that received a published and signed opinion is tracked in Figure 7.⁶¹ As noted in section 2, conventional wisdom suggests that this measure may be expected to show great flexibility under time pressure. There does appear to be a downturn, but the divergence is less clear, visually, than for dismissals or for reversals and remands.

⁵⁶ The coding of the administrative data does not distinguish between decisions that are “reversed” and those “vacated.”

⁵⁷ Newman (2005) documents these usages.

⁵⁸ Cases that result in “reversals in part and affirmances in part” are also included in this measure, because the aim is to count all appeals in which any material error was found. The same categorization of outcomes is used by Clermont & Eisenberg (2001).

⁵⁹ As with dismissals, graphing reversal-or-remand rates individually by circuit shows the marked downturns to be unique to the Second and Ninth Circuits.

⁶⁰ Changes in a court’s tendency to publish opinions would also affect this measure; but the analysis in section 5.2 suggests that greater selectivity in error correction (rather than in publishing opinions) is driving this decline in published reversals-or-remands.

⁶¹ Published and signed opinions are the best available proxy for precedential opinions (even though some *per curiam* opinions may also have been citable as precedent).

4.2. Quantifying the divergences

In the remainder of this section, I present the results of regressions quantifying the basic divergences shown in the graphs, and extend the analysis to comparing how outcomes changed among several major categories of civil cases. An advantage of doing separate regressions for each case category is that it controls for category in the most comprehensive and least restrictive way, being analogous to interacting category dummies with all other covariates in a single regression using all categories combined. Notably, case-category interactions with time and with circuit are fully accounted for, as quarter fixed effects and circuit fixed effects are included in all regressions.

The regression specification I adopt is a piecewise linear model intended to capture how much case outcomes diverged from their prior trends, after the flood began.⁶² The model is thus similar to the (nonparametric) smoothing functions shown in the graphs, except that it constrains the fitted function to be linear in each of three periods: before the start of the flood; a “transition” period (to allow for a lag); and a “flooded” period (by which time the flood’s effects, if any, should have been felt).

In the specification, as follows, the dependent variable is the fraction of all terminated appeals that reached a certain outcome, within the case category, in a given circuit, in a given quarter:⁶³

$$\begin{aligned}
 y_{ct} = & \beta_1 \cdot \text{Second} \cdot \text{trend} + \beta_2 \cdot \text{Ninth} \cdot \text{trend} \\
 & + \beta_3 \cdot \text{Second} \cdot \text{transition} + \beta_4 \cdot \text{Second} \cdot \text{change}_T \\
 & + \beta_5 \cdot \text{Second} \cdot \text{flooded} + \beta_6 \cdot \text{Second} \cdot \text{change}_F \\
 & + \beta_7 \cdot \text{Ninth} \cdot \text{transition} + \beta_8 \cdot \text{Ninth} \cdot \text{change}_T \\
 & + \beta_9 \cdot \text{Ninth} \cdot \text{flooded} + \beta_{10} \cdot \text{Ninth} \cdot \text{change}_F \\
 & + \mathbf{X}_{ct} \boldsymbol{\beta}_{11} + \varepsilon_{ct}
 \end{aligned}$$

Second and *Ninth* are indicators for whether the data point is from the Second and Ninth Circuits; *transition* and *flooded* are indicators for the transition and flooded periods; *c* indexes circuit; *t* indexes quarter of observation; *trend* is time since the start of the study window (capturing a linear trend); and *change_T* and *change_F* measure time since the start

⁶² In section 4.3, I also consider an adjustment that removes the contribution of the prior trend to the estimated effects.

⁶³ For instance, a data point for the publication-rates regression would be the percentage of cases decided, in a given circuit in a given quarter, that had a published opinion.

of the transition and flooded periods, respectively (capturing the change in trends in those periods). The covariates \mathbf{X}_{ct} include circuit fixed effects, quarter fixed effects, and a measure of each court's political composition in a given year.⁶⁴ The specification includes all components of the interacted terms; components not listed are those absorbed by the fixed effects in \mathbf{X}_{ct} .

Regressions are OLS, observations (which are circuit-by-quarter cell means) are weighted by cell population, and standard errors are clustered by circuit.⁶⁵ A case-level logit specification yields substantially similar results; to illustrate, Table L1 in the appendix presents the logit counterparts to the OLS results in Table 1. For ease of interpretation, I focus on OLS results in this discussion. The estimates presented here should be viewed as preliminary; further work, by developing a richer set of case-specific covariates, may improve explanatory power and the precision of estimates.

Using this basic model, I present estimates in Tables 1 to 5 that correspond to how much change in the levels of outcomes can be attributed to the flood, as of three years into the flood (second quarter of 2005). This measure is a within-sample prediction, as the study window extends through the third quarter of 2005. Specifically, the value is given by $\beta_5 + t \cdot \beta_6$ for the Second Circuit and $\beta_9 + t \cdot \beta_{10}$ for the Ninth, where t is the time in quarters between the start of the flooded period and the second quarter of 2005.⁶⁶ The units for this measure, as for observed outcomes y_{ct} , are percentage points.⁶⁷

⁶⁴ The share of active judges who were appointed by Republican presidents.

⁶⁵ The current specification, under OLS, with circuit-by-quarter observations weighted by cell population, is essentially equivalent to a case-level linear probability model when no case-level covariates are included. Coefficients are identical and standard errors are very close, between the case-level regressions (not reported here) and the circuit-level regressions (reported here).

⁶⁶ That is, 10 quarters for specifications allowing for a half-year transition period; and 8 quarters for the full-year transition period. The sum of t and the transition length is always 12 quarters (that is, three years into the flood).

⁶⁷ An alternative measure (not reported here) corresponding to the coefficients β_6 and β_{10} , for the Second and Ninth Circuits respectively, may be viewed as capturing dynamics: a negative coefficient indicates a downturn in a flooded circuit's outcome measures, relative to baseline—as would be expected if docket pressure did affect decision-making as hypothesized in section 2, because docket pressure grows over the course of the flood. In virtually all cases where the reported estimates (predicted levels changes) are statistically significant, these alternative measures (predicted slope changes) are also significant and signed in the expected direction.

I present these estimates using a separate regression for each of two definitions of the transition period: one half-year and one year.⁶⁸ As is evident from the tables, the estimates are largely robust to the chosen length; moreover, a simpler two-period model, wherein the transition periods are folded into the first period, also yields similar results (not reported). The two-period model may have the virtue of simplicity, but the advantage of my preferred three-period specification is that it relies less on assumptions about the exact length of the lag.

4.3. All civil cases, flooded circuits combined

First, in Table 1, I present results for the full range of civil cases, with case outcomes from the Second and Ninth Circuits pooled together. This presentation corresponds closely to the graphs in Figures 4, 5, and 7. Columns (1) and (2) report results from regressions with the transition period set, respectively, at one half-year and one year. The estimates represent how much change in the levels of the outcome variable is attributable to caseload pressure, as of three years into the flood (as described above). Confirming the sense from the graphs, the first two rows report that in the two flooded circuits combined, roughly a 7 percentage-point increase in dismissal rates and a 7 point drop in the reversals-or-remand rates are attributable to caseload pressure from the flood. (Likewise, for a 2 point drop in publication rates, although at present levels of precision, that estimate is not statistically significant.)

To address the possible concern that estimating divergences from prior trends would be inapt if those trends were unlikely to have continued throughout the study window, in Table A1 of the appendix I report the same measures as in Table 1 –but discounted for any contribution of prior trends. This adjustment, in essence, assumes a counterfactual in which prior trends would have leveled out at the start of the flooded period; in other words, the adjusted estimates represent differences between outcomes levels at that initial point and at the 3-year mark.⁶⁹

⁶⁸ The tradeoff in defining the transition period is that a longer transition is more likely to cover the true lag (if any), but a shorter one leaves a longer “flooded” period and hence more data for estimating the desired coefficients.

⁶⁹ The adjustment replaces β_6 with β_6' , where $\beta_6' = \beta_6 + \beta_1$, and analogously for $\beta_{10}' = \beta_{10} + \beta_2$.

As expected, the adjustment reduces the magnitude of certain estimates; but it also leaves intact the basic conclusion that a sizeable increase in dismissals (6 points) and decrease in reversals-or-remands (4 to 5 points) is attributable to the flood.⁷⁰ Because assuming that prior trends would have leveled out is somewhat more arbitrary than assuming that the trends would have continued, I report remaining measures following the preferred calculations shown in Table 1.

4.4. Breakdown by type of litigant: counseled versus *pro se*

I next present two cuts of the data that capture important institutional and legal distinctions; many other breakdowns are potentially interesting and will be considered in future work. Here, I first distinguish counseled cases from *pro se* cases; in the latter, at least one of the parties is proceeding without an attorney at the time of filing. All civil cases in the sample are categorized as either counseled or *pro se*. Making the distinction is important because *pro se* cases receive specialized treatment from the circuit courts;⁷¹ for instance, they are typically shepherded by dedicated staff attorneys, are ineligible for standard mediation programs, and rarely receive a published opinion. Naturally, *pro se* appeals will also differ in average quality from those filed by an attorney.

In Table 2, I report results for the sample of counseled cases in the first two columns, and for *pro se* cases in the third and fourth columns; each row represents the estimates for a given flooded circuit, either the Second or Ninth. As in Table 1, three outcomes are covered: dismissal rate, reversal-or-remand rate, and publication rate. From the reversal-or-remand estimates, it appears that error correction rates are more flexible, at current levels, for counseled cases than for *pro se* cases (for both circuits).

More striking is how the relative responses in dismissals, between counseled and *pro se* cases, differs between the two circuits: the Second Circuit's flood-related increase in dismissals for *pro se* cases far outpaces that for counseled cases; but in the Ninth, *pro se* dismissals barely increase (if at all). Sorting out this twist would be an interesting extension to pursue. Part of the disparity may be the result of differences in how the staff attorneys in each circuit handle *pro se* cases. For instance, if for *pro se* cases in the

⁷⁰ The publication-rates estimates are virtually unaffected.

⁷¹ McKenna, et al. (2000), at 77 & 173.

Ninth Circuit, a merits decision takes little or no more time than a dismissal, then the *pro se* dismissal rate might be relatively insensitive to time pressure (as observed).

4.5. Breakdown by source of jurisdiction

I also separate cases based on diversity-of-citizenship jurisdiction (28 U.S.C. § 1332) from those based on federal-question jurisdiction (28 U.S.C. § 1331); for comparability, I limit the sample to counseled cases.⁷² This breakdown tracks a highly salient distinction to judges; many have expressed a disfavor for diversity-jurisdiction cases, which tend to involve issues of state law rather than federal law.⁷³ Moreover, limiting or abolishing diversity jurisdiction has often been proposed by judges and by Congress as a way to relieve caseload pressure in the federal courts.⁷⁴

The simple observation that judges may disfavor diversity cases does not, in theory, imply any firm predictions as to how the two types of cases might respond differently, under pressure.⁷⁵ Nonetheless, there may be reasons to suspect that diversity suits would be more greatly affected. As Judge Friendly has suggested, caseload pressure might increase the motivation for abandoning diversity jurisdiction:

[T]he arguments for retaining it will not hold water when the federal courts are overburdened with distinctively federal business. While the *Erie* decision eliminated the evil of forum shopping, it also stripped the federal courts of the power to “make law” in diversity actions.⁷⁶

The sudden “overburdening” of a court might trigger the exercise of some *de facto* discretion over its own (effective) jurisdiction – for instance, by dismissing diversity suits more readily.⁷⁷ Moreover, the federal appeals courts once had a tradition,

⁷² The coding of source of jurisdiction is such that cases involving the U.S. government as a party are omitted from both categories. This breakdown, therefore, is free of the potential concern that the government’s litigation resources may have been affected by the flood.

⁷³ As Judge Coffin has summarized the reasons for disfavor: *The results [of diversity jurisdiction] are to burden federal courts with one-fourth of their caseload and one-half of their jury trials, to require federal judges to engage in arcane efforts to guess what state law might be, to flout the basic idea of federalism, to foster the idea that state courts are second-rate...* Coffin (1994), at 64-65.

⁷⁴ See Posner (1996), at 210-21.

⁷⁵ For instance, a federal court disfavoring diversity cases may be doing so few of them that it would be reluctant to drop still more. What matters for relative responses in outcomes is not the degree of relative disfavor but how it *changes*, as the court adjusts to pressure.

⁷⁶ Friendly (1974), at 641.

⁷⁷ Cf. Shapiro (1985).

enshrined in appellate doctrine, of deferring to district judges on issues of state law. The doctrine has been rejected by the Supreme Court,⁷⁸ as a formal matter; but the informal practice may be a tempting one to revive, during an unexpected time crunch.

In Table 3, I report results for federal-question cases (first two columns) and for diversity cases (other two columns), in the same format as in Table 2. Although all estimates bear the signs expected from the aggregate results in Table 1 and from the graphs, there is again a puzzling twist. The two circuits appear to have responded differently to the flood, as between the two types of cases: in the Second Circuit, dismissal rates rose farther for federal-question cases than for diversity cases; but in the Ninth, they rose more evenly.⁷⁹ Yet reversal-or-remand rates fell more evenly in the Second; in the Ninth, they fell in diversity cases but changed little (if at all) in federal-question cases. As for publication rates, the most noticeable change is a large drop in the Ninth, for diversity suits; for the Second, however, the greater drop appears to be for federal-question suits.⁸⁰

Without knowing more about the cases in each circuit-by-jurisdiction group, it is hard to conclude what is driving these disparities; that is an avenue for further inquiry. Notably, however, the Second Circuit's outcomes are hard to reconcile with the hypothesis (suggested above) that diversity cases might bear more the brunt of a caseload increase, due to the courts' *de facto* ability to limit their own jurisdictions, or due to a revived deference to lower courts on state-law issues.

5. Extensions: merits decisions and publication rates

The scaling-back of error correction – as summarized by drops in overall reversal-or-remand rates – may be attributable in part to dismissals of some cases that would have been reversed or remanded, had they reached a merits decision; and in part to less reversing or remanding among those cases that did reach a merits decision. This section extends the analysis by considering discretion in correcting errors among the

⁷⁸ *Salve Regina College v. Russell*, 499 U.S. 225 (1991).

⁷⁹ Although the estimated changes in the dismissal rates of the diversity cases in each circuit are sizeable, they are not statistically significant at current levels of precision.

⁸⁰ The latter estimates have magnitudes that are sizeable but not statistically significant.

latter – cases presented to a panel of judges for decision. It also further investigates the possibility (suggested by results reported above) that the flooded courts exhibit more flexibility in correcting errors than in publishing opinions.

5.1. Cases that reach a merits decision

In Table 4, I present the same measures as in Table 1, but limit the sample to cases reaching a panel of judges for a decision on the merits. These may be viewed as cases whose legal arguments are squarely presented to judges (the results reported so far have included both these cases and some that may not have received any judicial attention). Thus, the flexibility of case outcomes revealed in this merits-only sample may serve as an arguably closer proxy to what judges and theorists often have in mind, in speaking of discretion: how much leeway does a *judge* have (rather than the *court* as a whole), given the constraints of the law and the case record before her?⁸¹

The large, significant estimates reported in the first row of Table 4 suggest that the fall in overall reversal-or-remand rates is not being entirely driven by increased dismissals (lost opportunities to correct errors); rather, a decline is apparent even among those cases ultimately decided by a panel of judges. This is confirmed in the second row of Table 5,⁸² which reports the share of cases reaching a merits decision that were reversals or remands receiving a published opinion (again, proxying for the more “important” reversals and remands). Unless reversal-worthy cases were being disproportionately screened-out via dismissals, judicial panels in the flooded courts evidently became less willing to overturn lower-court rulings, in their merits decisions.

5.2. The puzzle of publication rates

By contrast with reversal-or-remand rates, the publication rate among cases reaching a merits decision shows no statistically meaningful change, as seen in the second row in Table 4. Also telling is the contrast between the third and fourth rows in Table 5: Published reversals-or-remands fell dramatically as a share of all published opinions. But they did not fall as a share of all reversals-or-remands; if anything, their share may have increased, perhaps due to a falling denominator. Together, these

⁸¹ See, e.g., Edwards (1983b), at 389-90.

⁸² The first row of Table 5 corresponds to Figure 6, as described in section 4.1.

patterns suggests that the reversals-or-remands rate is a flexible margin, even among cases slated to receive a published opinion; but that publication rates appear to be more rigid, possibly pegged to a fixed share of merits decisions.

The constancy of the publication rate among merits decisions is perplexing, given the common view that publication choices are largely discretionary and responsive to time pressures. It may be that law-declaring is such a priority that publication is an inflexible margin (at least, once a case reaches a judicial panel).⁸³ But the pattern also suggests an institutional “stickiness,” or even “targeting.” For instance, if a set fraction of merits cases are pre-sorted into a track almost surely to lead to publication (say, assigned as “chambers” work for closer attention from judges and law clerks), then it would be less surprising that publication rates continued as before. Moreover, those cases would also be the ones with “harder” legal issues, entailing more room for discretion in the merits decision; such pre-sorting would thus be consistent with the large response in reversal-or-remand rates among the published cases (Table 5).

6. Further thoughts

6.1. Assessing alternative explanations

First, could the trauma of 9/11 itself have created stresses on the work of the Second Circuit, thus accounting for the observed changes? The pattern of the changes in case outcomes suggests otherwise. A direct effect of 9/11 should have caused a sharp change, and then a rebound, in observed outcomes.⁸⁴ But for all outcomes, the divergence from prior trends grows, rather than diminishes, over time – consistent with increasing pressure from the continuing flood. Moreover, one might expect the Third Circuit (which includes New Jersey and Pennsylvania), the Fourth Circuit (which includes Virginia), and the D.C. Circuit also to have been affected by direct 9/11 stress;

⁸³ In terms of an optimization story, the marginal benefit of publishing may rise extremely steeply as the court publishes less (starting from the present quantity).

⁸⁴ Tellingly, the *total* number of civil appeals terminated was lower than usual in third-quarter 2001 (ending September 30) – but it immediately rebounded by the next quarter (October 1 to December 31). It then remained high in following quarters. (Likewise for the number of civil cases reaching a judicial panel for decision.)

but their case outcomes evolved very differently from those of the Second. The Ninth Circuit's changes are even less likely to be due to direct effects of 9/11.⁸⁵

Second, could the observed changes be the result of shifts in case composition? For instance, 9/11 may have changed the types of cases being brought and later appealed in New York-area federal courts. But the data reveal no sudden changes in the number or nature of civil appeals filed in the Second and Ninth Circuits; both the quantity and composition of civil filings are steady, during the study window. Notably, this observation also suggests that civil litigants appear not to have responded in large number to whatever caseload effects the savvier among them might have anticipated.

6.2. Some technical limitations

First, lacking measures of the time or effort required by each circuit to process the agency appeals, I have not attempted in this paper to calibrate how many extra hours of work the flood generated for each court. Comparisons across case types within a given circuit are sound,⁸⁶ but direct comparisons across circuits requires knowing (or else assuming) the relative difficulty of the cases flooding into each circuit.⁸⁷

Second, because individual judges are not identified in the data I use, I cannot control for judge-specific factors beyond what is captured by circuit and time fixed-effects and by the political composition measure. Similarly, very few case-specific measures are available; this disadvantage is reduced to a degree (but not entirely) by the differences-in-differences approach, by doing separate regressions for subcategories of cases, and by including fixed effects for circuits and for time. This shortcoming also hampers inferences about certain distributional consequences, such as whether "stronger" or "weaker" cases are most affected by docket crowding.

Third, which dismissals are the result of settlement, rather than a discovered procedural defect, is unclear in the administrative data. The latter dismissals leave lower-court errors intact (and are clear wins for the appellee); but some settlements

⁸⁵ Such an explanation would need to assert that the Ninth Circuit (along with the Second) was *more* affected by 9/11 than all other circuits (including the Third, Fourth, and D.C. Circuits).

⁸⁶ As is contrasting (across circuits) such comparisons (across case types within a circuit).

⁸⁷ Immigration cases can be very legally complex; but federal-court review of the immigration agency's actions is also tightly restricted by the governing statute. See 8 U.S.C. § 1252 (2000); see also *Reno v. American-Arab Anti-discrimination Committee*, 525 U.S. 471 (1999).

might informally relieve errors in lower-court rulings, from the parties' perspective (by splitting the difference). My analysis is thus limited to *formal* error correction; and I am unable to draw inferences about the how constraining are the rules for dismissing a case based on true procedural defects.

6.3. Limitations of scope

This study has been limited to the effects of time pressure on the courts; although these findings are revealing of discretion and constraints in appellate judging, a court may use its leeway differently under other types of pressure. This paper has also been limited to selected quantitative measures of judicial output; analyzing a richer set of outcomes might help to identify other dimensions of judicial discretion,⁸⁸ and might suggest answers to puzzles introduced by the present findings. For instance, publication rates may have changed relatively little only because other margins—such as opinion length, use of citations, mode of reasoning, or number of issues addressed—have more “give.” Exploring such possibilities, along with the judicial tools that preserve flexibility in dismissal and reversal-or-remand rates,⁸⁹ are among the aims of my broader project.

Finally, qualitative inquiry into the courts' divisions of labor, information flow, and governance structures would also complement the present analysis, which has treated the entire court as a single decision-making unit. (Notably, however, the observed changes in outcomes do not coincide with turnover of the Chief Judges in the flooded circuits.) The extension in section 5.1 distinguishing cases dismissed—in some cases by court staff alone—from those actually reaching a panel of judges, is in this spirit.

⁸⁸ For instance, there may be flexibility in the use of two-judge (rather than three-judge) panels, in delaying or otherwise time-shifting decisions, or in the allocation of oral arguments (consider the Second Circuit's creation of a “non-argument calendar” track in October 2005).

⁸⁹ As to dismissals, such mechanisms may include inducing settlement, denying interlocutory appeals, and prescreening for potential merit. As to reversals and remands, they may include devices for overlooking noticed errors, such as “harmless error,” or for barring scrutiny of potential errors, such as waiver and forfeiture of arguments (as noted in section 2); as well as everyday methods of legal reasoning, cf. Shleifer & Gennaioli (2005).

7. Conclusion

In this paper, I have linked changes in a court's decisions to changes in its caseload. My experimental design, along with special features of the caseload flood I have chosen to examine, combine to strengthen an inference that the links are causal. I take these findings to mean that under current judicial norms and legal constraints, a federal appeals court exercises a measurable degree of discretion over how selectively it corrects lower-court "errors." This revealed leeway appears comparable to, if not greater than, the flexibility in the courts' choices as to publishing opinions.

This paper's inquiry has been directed at a core concern of jurisprudence: How constraining are the facts and the law, on a given adjudicator's decision in a given case? Empirical studies showing how decisions vary among judges may be suggestive; but even if such a study showed "that each judge judges differently from every other judge,"⁹⁰ still, every decisionmaker might have felt fully constrained in each decision.⁹¹ The present study's solution is to assess how much leeway a *given* judicial body has, to decide differently in similar cases.⁹²

In attempting to locate and measure the leeway in judicial decisions, this study also serves as the initial piece of my broader inquiry into the "black box" of judicial production. Observing where discretion exists and where it appears to be lacking is a useful first step in identifying the choices and constraints facing a common-law court, in jointly producing legal interpretations and the resolution of disputes.

⁹⁰ As Judge Friendly observed when he first joined the bench. Friendly (1961), at 229.

⁹¹ In particular, evidence of "biases" attributable to aspects of judicial biography is not necessarily evidence of "discretion." Given the question of discretion, as posed here, such idiosyncrasies may be better seen as confounding factors, to be "controlled-for" or averaged-out.

⁹² Here, "similar cases" mean groups of cases constructed to be comparable, on average, after controlling for baseline trends and for case type.

Data Appendix

The Administrative Office of the U.S. Courts and the Federal Judicial Center distribute data on all cases filed in the 94 district courts and all appeals filed in the 12 regional appeals courts. These data are used in the federal judiciary's official publications, such as the reports of the Chief Justice and statistics published online.⁹³ General background on the AO data (and limitations of the district court data, not relevant to the appeals data used here) are detailed by Eisenberg and Schlanger (2003). That informative essay also catalogs (at notes 2-13) the growing academic literature using AO data.

I limit my analysis to variables that are available across all years in the study window, "statistical years" 1998 to 2005 (fourth-quarter 1997 through third-quarter 2005). This window begins where data is first available on whether a *pro se* litigant was involved in the case, and continues through to the end of the available data. The *pro se* / counseled distinction is important for this study, given the special handling of *pro se* cases by staff attorneys in each appeals court.

In addition, I have limited the sample as follows:

(1) I examine only cases categorized by the AO as "civil" cases. Subject areas within this category include (among others) contracts, torts, property, employment discrimination, other civil rights, intellectual property, regulatory actions, tax, labor laws, social security laws, and commercial law. The "civil" category is distinct from these remaining categories: "criminal," "original writs," and "administrative" (federal agency cases, including the BIA appeals).

(2) I exclude habeas and prisoner suits, even though they are formally categorized as civil cases. Cases involving federal criminal law, being less diversified than civil cases, are especially vulnerable as a class to such shocks as Supreme Court decisions (see note 4, *supra*). Such shocks affect not only cases involving the affected legal issue, but also the cases of those incarcerated litigants who may share the same pool of litigation resources, such as prison lawyers.

(3) To avoid double-counting, I keep only the record for the "lead" case among consolidated cases. Likewise, I omit reopened appeals and en banc cases.

(4) I omit the D.C. Circuit from all analyses, given its specialized docket. (The specialized Federal Circuit is not in the AO appeals data.) What remains are all other regional circuits, from First to Eleventh.

⁹³ I have been able to replicate official tables using the raw data from the AO database.

Data Summary: Civil Appeals Terminated, October 1997 - September 2005

	Litigant status, as share of civil cases		Source of jurisdiction, as share of counseled cases		Civil cases, as share of all cases
	<i>Counseled</i>	<i>Pro se</i>	<i>Federal question</i>	<i>Diversity of citizenship</i>	
Full sample (N = 126,179)	71.8	28.2	60.5	22.7	30.9
Second Circuit (N = 12,673)	65.7	34.3	64.5	24.3	38.8
Ninth Circuit (N = 21,422)	70.2	29.8	60.8	17.5	27.4
"Flooded" Circuits	68.5	31.5	62.2	19.9	30.7
"Control" Circuits	72.9	27.1	59.9	23.8	30.9

All civil cases	As share of merits decisions (i.e. not dismissed)		Share among reversals and remands	Share among published opinions
	<i>Dismissed</i>	<i>Reversed or remanded</i>	<i>Published opinion</i>	<i>Reversed or remanded</i>
Full sample	45.2	21.9	29.0	43.8
Second Circuit	50.9	17.1	28.3	41.9
Ninth Circuit	48.6	24.7	22.7	47.2
"Flooded" circuits	49.4	22.0	24.8	45.0
"Control" Circuits	43.6	21.8	30.4	43.5

Notes: Numbers are percentages. N refers to the number of civil cases. "Flooded" circuits are the Second and Ninth. "Control" circuits are all other circuits from First to Eleventh; the D.C. Circuit and the Federal Circuit are not included. "Civil cases" includes all categories of cases coded as "civil" by the Administrative Office of the U.S. Courts, excluding habeas and prisoner suits (as explained in the text and the data appendix). The federal-question and diversity-of-citizenship figures are for counseled cases only; they do not add to 1, because the data includes two other categories: U.S. government and "local question" cases. The sample is limited and cleaned as described in the data appendix.

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TABLES AND FIGURES

FIGURES 1-7

TABLES 1-5

APPENDIX TABLES

TABLE A1

TABLE L1

Figure 1: Appeals Filed in the Second Circuit 1994-2005

(Number of filings per quarter year. Curves are fitted for visualization using lowess smoothing; upper curve tracks all cases other than immigration, and lower curve tracks immigration cases.)

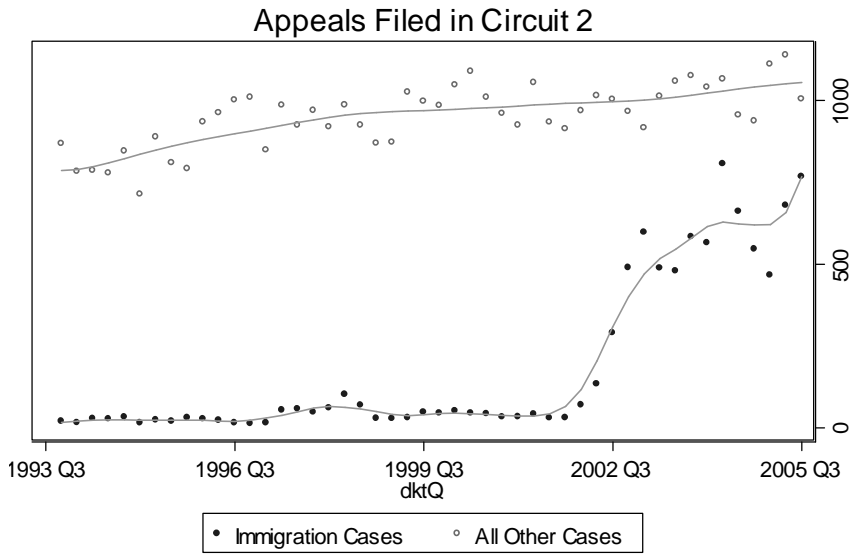


Figure 2: Appeals Filed in the Ninth Circuit 1994-2005

(Number of filings per quarter year. Curves are fitted for visualization using lowess smoothing; upper curve tracks all cases other than immigration, and lower curve tracks immigration cases.)

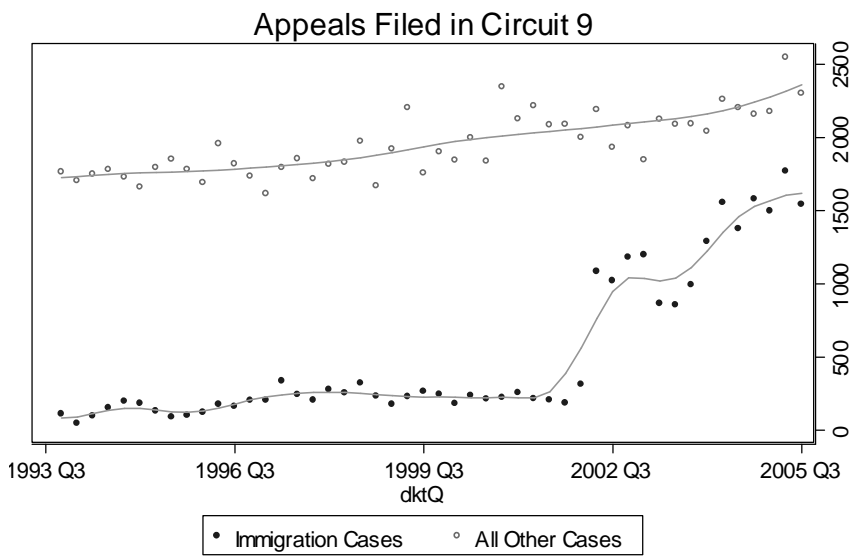
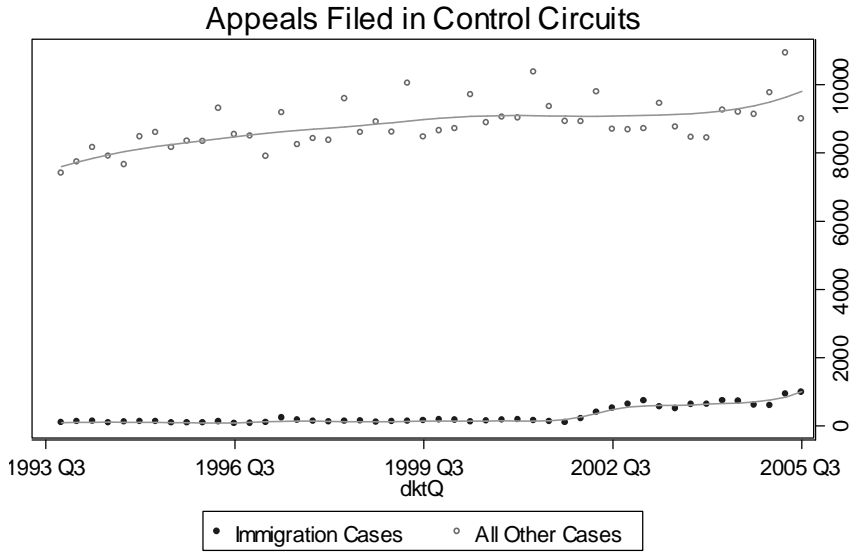


Figure 3: Appeals Filed in the “Control” Circuits 1994-2005

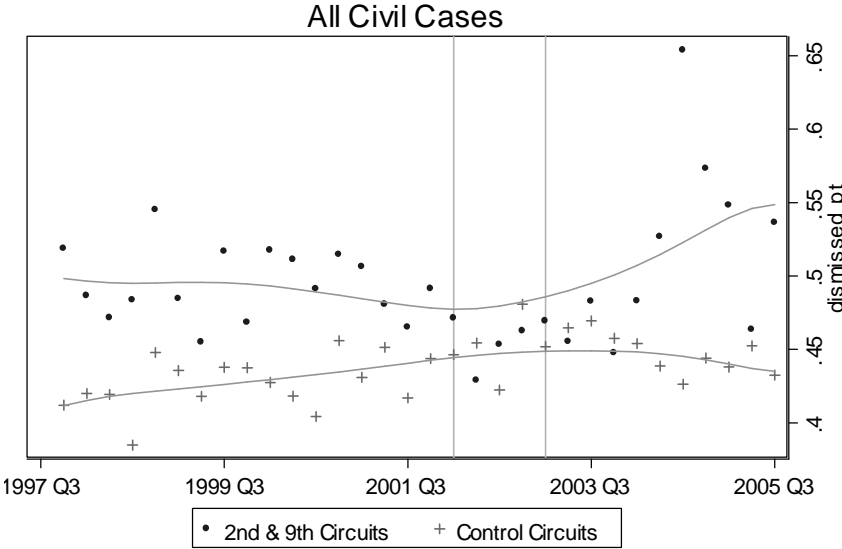
(Number of filings per quarter year. “Control” circuits are the First through Eleventh, excluding Second and Ninth. Curves are fitted for visualization using lowess smoothing; upper curve tracks all cases other than immigration, and lower curve tracks immigration cases.)



graphs_apr10_biagroup_by_treat

Figure 4: Percentage Dismissed Before Submission to Judicial Panel for Decision

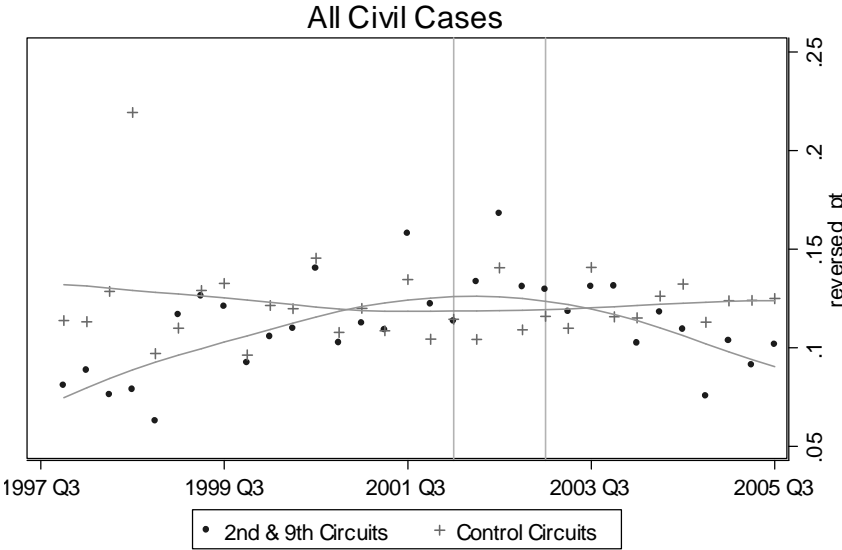
(By quarter year. Lower curve tracks the “control” circuits. Upper curve tracks the Second and Ninth Circuits, combined. Vertical lines mark start of filings flood and one year later.)



graphs_apr30_biggroup_by_treat_x_12

Figure 5: Percentage Reversed or Remanded

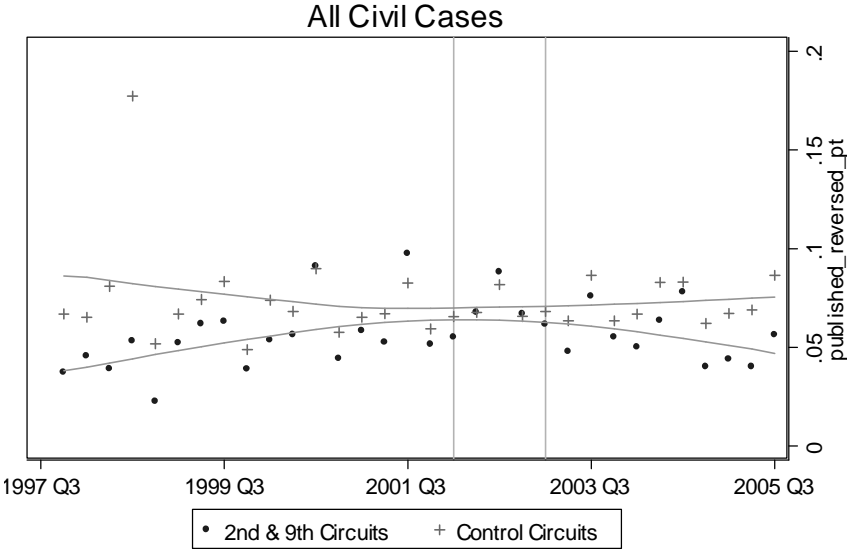
(By quarter year. Flat curve tracks the “control” circuits. Other curve tracks the Second and Ninth Circuits, combined. Vertical lines mark start of filings flood and one year later.)



graphs_apr30_biggroup_by_treat_x_12

Figure 6: Percentage Reversed or Remanded, with a Published and Signed Opinion

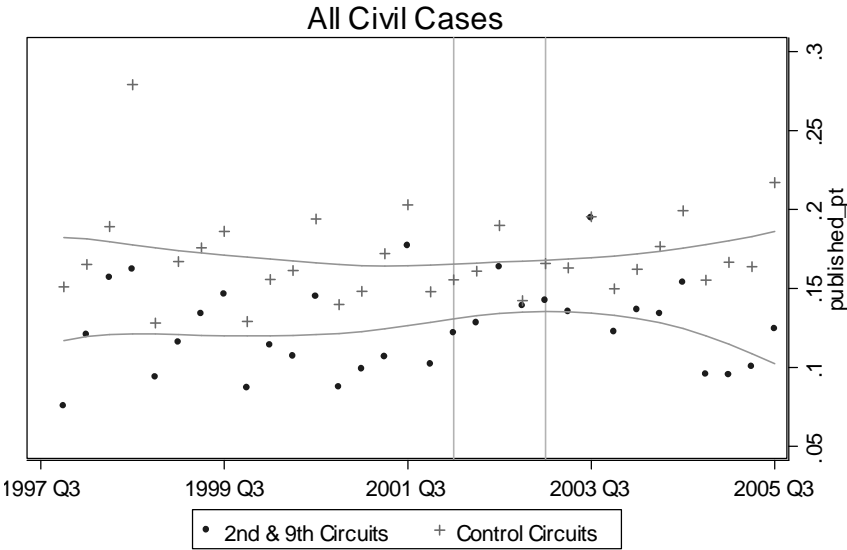
(By quarter year. Upper curve tracks the “control” circuits. Lower curve tracks the Second and Ninth Circuits, combined. Vertical lines mark start of filings flood and one year later.)



graphs_apr30_biggroup_by_treat_x_12

Figure 7: Percentage with Published and Signed Opinions

(By quarter year. Lower curve tracks Second and Ninth Circuits, combined. Upper curve tracks the “control” circuits. Vertical lines mark start of filings flood and one year later.)



graphs_apr30_biggroup_by_treat_x_12

Table 1: Civil Appeals, All Cases
Predicted Divergence from Trend, at 3-Year Mark

<i>As share of all terminated appeals</i>	<i>Second and Ninth, pooled</i>	
	(1)	(2)
Dismissals		
<i>Flooded circuits</i>	.070 (.034)*	.071 (.034)*
Reversals or remands		
<i>Flooded circuits</i>	-.068 (.011)***	-.067 (.011)***
Published opinions		
<i>Flooded circuits</i>	-.022 (.017)	-.024 (.017)
<i>Transition period</i>		
<i>½ year</i>	X	
<i>1 year</i>		X
Observations	352	352

Notes: Estimates represent the predicted divergence of the outcome, three years into the flood (by second quarter of 2005), based on the estimated changes in intercept and slope for the flooded period. The dependent variables are percentages of all terminated appeals in a given quarter, in a given circuit (observations are circuit-by-quarter percentages). All regressions are OLS, weighted by cell population. N = 32 quarters x 11 circuits = 352. All regressions include the following controls: political composition of the circuit; quarter fixed effects; and circuit fixed effects. Standard errors (in parentheses) are clustered by circuit. *** represents significance at the 1%, ** at the 5%, and * at the 10% levels.

**Table 2: Civil Appeals (Counseled and *Pro Se*)
Predicted Divergence from Trend, at 3-Year Mark**

<i>As share of all terminated appeals</i>	<i>Sample: All parties have counsel</i>		<i>Sample: At least one party is pro se</i>	
	(1)	(2)	(3)	(4)
Dismissals				
<i>Second Circuit</i>	.090 (.041)**	.083 (.042)*	.162 (.026)***	.180 (.027)***
<i>Ninth Circuit</i>	.064 (.033)*	.063 (.033)*	.004 (.028)	.005 (.031)
Reversals or remands				
<i>Second Circuit</i>	-.100 (.020)***	-.097 (.021)***	-.031 (.012)**	-.028 (.013)**
<i>Ninth Circuit</i>	-.056 (.015)***	-.059 (.016)***	-.033 (.008)***	-.0321 (.008)***
Published opinions				
<i>Second Circuit</i>	-.020 (.034)	-.023 (.035)	-.013 (.007)*	-.010 (.007)
<i>Ninth Circuit</i>	-.016 (.019)	-.015 (.020)	-.020 (.005)***	-.024 (.005)***
<i>Transition period</i>				
<i>½ year</i>	X		X	
<i>1 year</i>		X		X
Observations	352	352	352	352

Notes: Estimates represent the predicted divergence of the outcome, three years into the flood (by second quarter of 2005), based on the estimated changes in intercept and slope for the flooded period. The dependent variables are percentages of all terminated appeals in a given quarter, in a given circuit (observations are circuit-by-quarter percentages). $N = 32$ quarters \times 11 circuits = 352. All regressions are OLS, weighted by cell population. All regressions include the following controls: political composition of the circuit; quarter fixed effects; and circuit fixed effects. Standard errors (in parentheses) are clustered by circuit. *** represents significance at the 1%, ** at the 5%, and * at the 10% levels.

Table 3: Civil Appeals (Federal Question and Diversity Jurisdiction)
Predicted Divergence from Trend, at 3-Year Mark

<i>As share of all terminated appeals</i>	<i>Sample: Federal question cases</i>		<i>Sample: Diversity jurisdiction cases</i>	
	(1)	(2)	(3)	(4)
Dismissals				
<i>Second Circuit</i>	.128 (.035) ^{***}	.124 (.036) ^{***}	.068 (.067)	.066 (.068)
<i>Ninth Circuit</i>	.063 (.035) [*]	.065 (.036) [*]	.077 (.049)	.067 (.051)
Reversals or remands				
<i>Second Circuit</i>	-.132 (.016) ^{***}	-.135 (.017) ^{***}	-.190 (.052) [*]	-.109 (.051)
<i>Ninth Circuit</i>	-.027 (.013)	-.029 (.013)	-.111 (.038) ^{**}	-.107 (.039) [*]
Published opinions				
<i>Second Circuit</i>	-.068 (.042)	-.073 (.043)	-.009 (.050)	-.018 (.050)
<i>Ninth Circuit</i>	-.023 (.029)	-.019 (.030)	-.123 (.024) ^{***}	-.126 (.025) ^{***}
<i>Transition period</i>				
<i>½ year</i>	X		X	
<i>1 year</i>		X		X
Observations	352	352	352	352

Notes: Estimates represent the predicted divergence of the outcome, three years into the flood (by second quarter of 2005), based on the estimated changes in intercept and slope for the flooded period. The dependent variables are percentages of all terminated appeals in a given quarter, in a given circuit (observations are circuit-by-quarter percentages). N = 32 quarters x 11 circuits = 352. All regressions are OLS, weighted by cell population. All regressions include the following controls: political composition of the circuit; quarter fixed effects; and circuit fixed effects. Standard errors (in parentheses) are clustered by circuit. *** represents significance at the 1%, ** at the 5%, and * at the 10% levels.

Table 4: Civil Appeals, All Cases
Predicted Divergence from Trend, at 3-Year Mark

<i>As share of merits decisions</i>	<i>Second and Ninth, pooled</i>	
	(1)	(2)
Reversals or remands		
<i>Flooded circuits</i>	-0.107 (.019) ^{***}	-0.106 (.019) ^{***}
Published opinions		
<i>Flooded circuits</i>	-0.001 (.035)	-0.005 (.035)
<i>Transition period</i>		
<i>1/2 year</i>	X	
<i>1 year</i>		X
Observations	352	352

Notes: Estimates represent the predicted divergence of the outcome, three years into the flood (by second quarter of 2005), based on the estimated changes in intercept and slope for the flooded period. The dependent variables are percentages of all terminated appeals in a given quarter, in a given circuit (observations are circuit-by-quarter percentages). N = 32 quarters x 11 circuits = 352. All regressions are OLS, weighted by cell population. All regressions include the following controls: political composition of the circuit; quarter fixed effects; and circuit fixed effects. Standard errors (in parentheses) are clustered by circuit. *** represents significance at the 1%, ** at the 5%, and * at the 10% levels.

Table 5: Civil Appeals, All Cases
Predicted Divergence from Trend, at 3-Year Mark

<i>Published reversals and remands</i>	<i>Second and Ninth, pooled</i>	
	(1)	(2)
As share of all terminated appeals		
<i>Flooded circuits</i>	-0.035 (.011) ^{***}	-0.035 (.011) ^{***}
As share of merits decisions		
<i>Flooded circuits</i>	-0.054 (.019) ^{**}	-0.053 (.020) ^{**}
As share of published opinions		
<i>Flooded circuits</i>	-0.246 (.080) ^{**}	-0.236 (.078) ^{**}
As share of reversals and remands		
<i>Flooded circuits</i>	.042 (.042)	.040 (.038)
<i>Transition period</i>		
<i>½ year</i>	X	
<i>1 year</i>		X
Observations	352	352

Notes: Estimates represent the predicted divergence of the outcome, three years into the flood (by second quarter of 2005), based on the estimated changes in intercept and slope for the flooded period. The dependent variables are percentages of all terminated appeals in a given quarter, in a given circuit (observations are circuit-by-quarter percentages). N = 32 quarters x 11 circuits = 352. All regressions are OLS, weighted by cell population. All regressions include the following controls: political composition of the circuit; quarter fixed effects; and circuit fixed effects. Standard errors (in parentheses) are clustered by circuit. *** represents significance at the 1%, ** at the 5%, and * at the 10% levels.

Table A1: Civil Appeals, All Cases
Predicted Divergence at 3-Year Mark,
Assuming Leveling-Out of Prior Trends

<i>As share of all terminated appeals</i>	<i>Second and Ninth, pooled</i>	
	(1)	(2)
Dismissals		
<i>Flooded circuits</i>	.058 (.022)**	.061 (.024)**
Reversals or remands		
<i>Flooded circuits</i>	-.042 (.007)***	-.046 (.007)***
Published opinions		
<i>Flooded circuits</i>	-.021 (.011)*	-.024 (.012)*
<i>Transition period</i>		
<i>1/2 year</i>	X	
<i>1 year</i>		X
Observations	352	352

Notes: Estimates represent the predicted divergence of the outcome, three years into the flood (by second quarter of 2005), based on the estimated changes in intercept and slope for the flooded period. The dependent variables are percentages of all terminated appeals in a given quarter, in a given circuit (observations are circuit-by-quarter percentages). All regressions are OLS, weighted by cell population. N = 32 quarters x 11 circuits = 352. All regressions include the following controls: political composition of the circuit; quarter fixed effects; and circuit fixed effects. Standard errors (in parentheses) are clustered by circuit. *** represents significance at the 1%, ** at the 5%, and * at the 10% levels.

Table L1: Civil Appeals, All Cases
Predicted Divergence from Trend, at 3-Year Mark
(Case-Level Logit, Odds Ratios Shown)

<i>As share of all terminated appeals</i>	<i>Second and Ninth, pooled</i>	
	(1)	(2)
Dismissals		
<i>Flooded circuits</i>	1.326 (.169)**	1.327 (.172)**
Reversals or remands		
<i>Flooded circuits</i>	.475 (.089)***	.479 (.087)***
Published opinions		
<i>Flooded circuits</i>	.846 (.093)	.830 (.093)*
<i>Transition period</i>		
<i>1/2 year</i>	X	
<i>1 year</i>		X
Observations	126,179	126,179

Notes: Estimates shown are predicted odds ratios, from a case-level logit specification corresponding to the circuit-by-quarter level OLS specification presented in Table 1. An odds ratio of 1 (not 0) corresponds to “no effect.” All regressions include the following controls: political composition of the circuit; quarter fixed effects; and circuit fixed effects. Standard errors (in parentheses) are clustered by circuit. *** represents significance at the 1%, ** at the 5%, and * at the 10% levels.