Can Social Sanctions Improve Bureaucratic Performance? Evidence from U.S. Courts

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I study a judicial reform—the "six-month list"—which requires U.S. courts to publish the names of judges with overdue matters. Using a regression-discontinuity design and related methods, I exploit quasi-random variation in exposure to the list. Matters most exposed are resolved roughly 14% faster than those least exposed, with larger effects among young, non-white, and female judges. I find suggestive evidence of quality trade-offs: less-exposed matters are more likely to be affirmed on appeal. A bunching analysis estimates that the list reduces total motion-processing time by about 4%, highlighting the power of reputational incentives in shaping bureaucratic behavior.

Courts are essential institutions: beyond safeguarding rights and liberties, they promote economic growth by enforcing contracts, protecting property,

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and resolving disputes (North, 1990; Djankov et al., 2003; Acemoglu and Johnson, 2005). Yet courts cannot deliver these public goods if adjudication is slow. This paper examines a reform in bureaucratic accountability—the “six-month list”—which since the early 1990s has required U.S. federal courts to publish a semiannual list of judges with civil matters pending more than six months. I present new evidence on how this reform affects judicial efficiency and performance.

Like judges elsewhere, U.S. federal judges are largely insulated from ordinary workplace incentives. The Constitution guarantees them life tenure and fixed, non-diminishable salaries to safeguard judicial independence. Yet judges, like other public-sector professionals, still respond to social (Ashraf and Bandiera, 2018) and career incentives (Holmstrom and Milgrom, 1991; Dewatripont, Jewitt and Tirole, 1999; Alesina and Tabellini, 2008), as well as institutional norms (Akerlof and Kranton, 2005) and other nonmonetary incentives (Cassar and Meier, 2018). The six-month list leverages such nonmonetary incentives by publicly identifying judges with overdue decisions, effectively “shaming” them into greater timeliness. But whether social pressure actually improves bureaucratic performance—and at what cost—remains an open question.

To evaluate the effects of the six-month list, I construct a dataset of approximately 212,000 summary judgment motions with known dispositions in federal civil cases filed between 2005 and 2014. Summary judgment motions are requests to resolve a case after evidence has been gathered but before trial; they are time- and resource-intensive, and often represent a major chokepoint in litigation. Identification relies on a structural feature of the list: while motions are filed continuously, the list is prepared only twice per year. As a result, motions vary quasi-randomly and discontinuously in their exposure to the list. Motions filed just before certain cutoff dates give judges only seven months of what I term "reporting time"—that is, the time a judge has to act before the motion becomes reportable. Motions filed on or just after cutoff dates enjoy up to 13 months of reporting time. I show that filing dates are as-if random around the cutoff, allowing me to leverage this variation for causal inference.

Using linear regression and a regression-discontinuity design, I show that exposure to the six-month list significantly accelerates adjudication. Each additional month of reporting time delays motion resolution by about four days, and motions most exposed to the list are resolved roughly three-quarters of a month (14%) faster than those least exposed. Notably—and consistent with mechanisms based in reputational and career concerns—I find larger effects among younger, non-white, and female judges.

Motivated by multitask principal-agent theory (Holmstrom and Milgrom, 1991), I next ask whether faster adjudication comes at the cost of other dimensions of judicial performance, including adjudicative quality. The results are mixed. Exposure to the six-month list does not significantly affect motions' grant or denial rates. However, among the relatively small subset of appealed motions, those least exposed are about 3.8 percentage points ($\approx$ 9%) more likely to be affirmed. And while list exposure substantially shortens the time to resolve summary judgment motions, it yields more modest reductions in total case duration. Taken together, these results suggest that deadline incentives may lead judges to "cut corners," squandering some of the time savings and increasing the likelihood of appellate reversal.

Last, I apply a notched bunching design (Kleven and Waseem, 2013;

Kleven, 2016) in a novel setting to estimate the six-month list's impact on system-wide court delay. Relative to a counterfactual with no deadlines, I find that the list reduces total summary judgment processing time by about 4%. These findings reinforce the conclusion that reputational and career incentives can improve bureaucratic performance, even among public officials like judges who are largely insulated from standard workplace pressures.

This paper contributes to several important literatures in economics. First, it adds to a growing literature on the determinants and consequences of judicial performance and judicial efficiency. A central finding of this literature is that judicial speed yields tangible economic benefits (Visaria, 2009; Ponticelli and Alencar, 2016; Chemin, 2020; Kondylis and Stein, 2023). Researchers have studied a variety of successful judicial reforms, including the establishment of new tribunals with streamlined procedures (Visaria, 2009), procedural or legal reform within existing court systems (Chemin, 2012; Kondylis and Stein, 2023), as well as the adoption of case management protocols (Chemin, 2024). While promising, these reforms can prove difficult to scale, often requiring governments to write new laws or dedicate scarce resources. By contrast, this paper shifts the focus to judges' workplace incentives, which can be altered with comparative ease.

More broadly, this paper contributes to work on how public officials respond to institutional incentives. It builds on studies of judicial behavior and career concerns (Epstein, Landes and Posner, 2013; Levy, 2005), and complements work exploiting variation in judicial caseloads (Yang, 2016), salaries (Baker, 2008), and selection mechanisms (Lim, 2013; Ash and MacLeod, 2024). Outside the judiciary, it contributes to literatures on public sector performance, including the effects of deadlines (Frakes and Wasserman, 2020, 2024), social incentives (Ashraf, Bandiera and Jack, 2014; Chetty, Saez and Sandor, 2014; Ashraf and Bandiera, 2018; Gauri et al., 2021), and reputational and career motivations (Chen, Li and Lu, 2018; Bertrand et al., 2020) in shaping bureaucratic output.

This paper proceeds as follows. Section I provides background on the six-month list and the problem of delay in U.S. civil litigation. Section II describes the data and empirical strategy, and Section III discusses identification. Section IV presents the main results, including effects on speed, quality, and heterogeneity across judges. Section V uses bunching methods to estimate the list's aggregate impact on motion-processing time. Section VI concludes.

I. U.S. Civil Litigation and the "Six-Month List"

U.S. federal courts play a critical role in resolving civil disputes, including disputes between private parties and cases involving the government. District courts serve as the main trial-level courts in the federal system, handling both civil and criminal matters. The six-month list applies only to civil cases, of which district courts have received just over 300,000 annually since 2015. These cases span a wide range of issues, including contracts, product liability, intellectual property, and civil rights. While parties can often choose between state and federal court, federal courts are typically preferred for high-stakes or legally complex litigation.

Federal courts are also highly decentralized: over 1,100 district judges serve across 94 judicial districts, each with life tenure, guaranteed salaries, and broad discretion over case management. Cases are randomly assigned to a single judge, who oversees all aspects from filing to resolution, including setting deadlines, scheduling hearings, and presiding over trial when necessary. One of a judge's most time-intensive responsibilities is ruling on "motions"—formal requests by parties for the court to take specific actions.

In the late 1980s, rising caseloads and concerns over litigation delays prompted calls for reform. Congress responded with the Civil Justice Reform Act (CJRA) of 1990. The "six-month list"—also sometimes known as the "Biden List" in honor of its chief sponsor, then-Senator Joe Biden—was one of the law's key provisions, requiring the judiciary to publish a semi-annual report disclosing the names of federal judges with motions pending for more than six months. The provision was controversial, but both supporters and detractors agreed that the law's intent was to use the threat of social sanctions to incentivize judges to adjudicate matters more quickly. As one judge candidly observed in a letter to Senator Biden: "We all recognize that peer pressure plays an important role in our everyday lives, and it likewise is important in the judicial setting."[1] To that end, the contents of the six-month list can easily be found on the federal judiciary's website,[2] and in recent years the federal judiciary has even announced its publication on social media.[3]

Importantly for this paper, the six-month list is prepared on two fixed dates each year: March 31 and September 30. On either date, a motion is included on the list only if it remains currently unresolved *and* more than 214 days have passed since the motion was filed.[4] As a result, motions

[1] Letter from Justin L. Quackenbush, Chief Judge of the U.S. District Court for the Eastern District of Washington, to Senator Joseph R. Biden, Jr. (Apr. 23, 1991).

[2] Available at: `https://www.uscourts.gov/statistics-reports/civil-justice-reform-act-six-month-list`.

[3] See, e.g., `https://x.com/uscourts/status/1942677569238601897`.

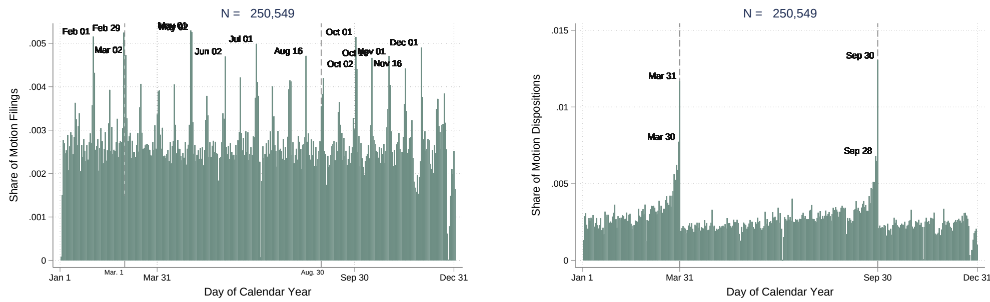
[4] Although the statute refers to matters pending "more than six months," the judiciary has come to define "pending" as beginning 30 days after filing and interprets "six months" as 184 days, making the effective threshold 214 days.

vary quasi-randomly in exposure: those filed exactly 215 days before a list deadline are most exposed, while those filed just afterward are least exposed. I describe the resulting identification strategy in Section III.

The six-month list covers nearly all types of motions in civil cases, from routine requests to extend a deadline to substantive requests like motions to dismiss. This paper focuses on one of the most consequential: the motion for "summary judgment." Filed after evidence has been gathered through discovery but before trial, it asks the judge to rule that the opposing party lacks sufficient evidence to proceed. While either side can request summary judgment, defendants do so more often. Summary judgment motions often involve complex legal issues and extensive records, making them among the most time-consuming tasks a federal judge undertakes.

In practice, the six-month list clearly matters to judges. Figure 1 shows sharp spikes in motion resolutions just before the March 31 and September 30 list deadlines, followed by noticeable dips immediately after. In contrast, motion filings are roughly uniform throughout the year, aside from routine biweekly spikes near the beginning, middle, and end of each month.

Despite its influence on judges, the six-month list has attracted limited scholarly attention. One prior empirical study, de Figueiredo, Lahav and Siegelman (2020), provides suggestive evidence that the list may influence judicial behavior and lead to unintended consequences. While informative, their analysis is based on a relatively small hand-coded dataset, and many of their findings lack causal identification. By contrast, this paper is the first to use large-scale administrative data and quasi-experimental methods to rigorously evaluate the six-month list's effects on judicial performance.



(a) Motions Filed (b) Motions Resolved

Figure 1. Summary Judgment Motions Filed & Resolved by Calendar Day (2005–2014)

Note: Figure shows total motions filed (a) and resolved (b) by calendar day from January 1, 2005 through December 31, 2014. Calendar days with either filings or dispositions greater than two standard deviations above the per-day mean are labeled with the date.

II. Data & Empirical Framework

A. Summary Judgment Motion Data

Although the federal judiciary maintains detailed records of court activity, much of this information is difficult for researchers to access. Because the six-month list operates at the motion level, what I require is a dataset of individual motions, including when the motion was filed and when and how a judge decided the motion. The judiciary maintains this information on a case's "docket sheet," which is the official administrative record of a case's proceedings. Docket sheets are available from the judiciary's Public Access to Court Electronic Records (PACER) system, but PACER does not facilitate bulk downloads, and access fees are generally prohibitive to large-scale empirical research.

To overcome these limitations, I constructed a dataset of more than 350,000 summary judgment motions—representing nearly all such motions filed in federal district courts between 2005 and 2014. I began with a database of digital docket sheets covering the universe of civil cases filed during this period. The raw docket data were provided as structured XML files by the West Publishing Company, which sourced the information from PACER.[5]

The docket sheets contain date-stamped text entries for every case action, including motion filings, rulings, and related updates. They also include basic case metadata: party names, attorneys, the case's basic subject matter (as reflected in a "nature-of-suit" code), and the identity of the assigned judge.

Using natural language processing, I identified all docket entries corresponding to motions for summary judgment. Although formatting is inconsistent, summary judgment filings tend to follow predictable patterns, making them suitable for dictionary-based classification. I then used similar methods to identify corresponding judicial rulings, including whether each motion was granted or denied.

Finally, I merged the resulting motion-level data with two external sources. To control for judge characteristics, I used the Federal Judicial Center's Biographical Directory of Federal Judges.[6] To validate case metadata and link cases to appellate activity, I merged with case-level data from the judiciary's Integrated Database (IDB), which includes both district and appellate court records.[7]

In order to give motions in my sample time to be resolved, I restrict the sample to motions filed at least one year before the end of 2014. This

[5]I am grateful to Jonah Gelbach, who obtained the data via contract between Yale University and Thomson Reuters, West's parent company. The same data have been used in several studies (e.g., Gelbach, 2014, 2024).

[6]Available at: `https://www.fjc.gov/history/judges`.

[7]Available at: `https://www.fjc.gov/research/federal-court-cases-fjc-integrated-database-1979-present`.

mitigates but does not eliminate concerns about survivorship bias (in my context, the concern that still-pending motions differ systematically from motions for which I observe a resolution)—an issue I revisit in the results section below.

Table 1 summarizes the resulting dataset. From approximately 2.5 million docket sheets, I identified over 363,000 motions for summary judgment filed in more than 242,000 unique cases. Of these, I identified an explicit disposition—generally granted, denied, or granted in part—for 212,814 (58.5%). The remainder were either unresolved at the end of 2014, were resolved without an explicit ruling (e.g., due to settlement of the lawsuit), or were resolved in ways that eluded my classification methods. This is my main analysis sample, although some of my analyses exploit the full sample including motions with unknown dispositions. Of motions with known dispositions, 67% were filed by defendants and 33% by plaintiffs. They span all 94 federal judicial districts as well as a wide variety of subject matters. On average, judges took 5.4 months to resolve the motions. Approximately 51% were granted, and about 26% of rulings were subsequently appealed.

B. Empirical Framework

I estimate the effect of the six-month list on motion resolution speed and other aspects of judicial performance by exploiting quasi-random variation in exposure to the list, generated by its fixed, semiannual reporting dates. Recall that a motion becomes reportable only after at least 215 days have passed, creating two key cutoff dates each year: March 1 and August 30, which fall exactly *214* days before the September 30 and March 31 deadlines, respectively. Motions filed just before these cutoffs are *most* exposed—they

Table 1—Summary Statistics: Summary Judgment Motions

Motion- & Case-level Characteristics	Full Sample	Known Disposition	Reporting Time < 10 months	Reporting Time ≥ 10 months
N (total motions)	363,881	212,814	104,612	108,202
Total unique cases	242,050	154,529	84,773	87,202
Reporting Time (mos.)	10.0 (1.7)	10.1 (1.7)	8.5 (0.9)	11.5 (0.9)
Case duration at filing (mos.)	13.4 (10.5)	13.3 (10.1)	13.3 (10.0)	13.3 (10.2)
Entries prior to filing	60.0 (568.4)	56.7 (572.8)	57.7 (608.9)	55.6 (535.5)
Moving Party				
Defendant	66.7%	67.3%	67.2%	67.3%
Plaintiff	33.3%	32.7%	32.8%	32.7%
Pro se movant	5.3%	4.9%	5.0%	4.9%
In Forma Pauperis	16.4%	17.9%	18.0%	17.9%
Nature of Suit				
Civil Rights	12.4%	13.5%	13.6%	13.4%
Contract	17.6%	17.6%	17.5%	17.6%
Employment	10.6%	11.9%	11.8%	11.9%
Intellectual Property	3.2%	3.0%	3.0%	2.9%
Labor	6.0%	6.4%	6.3%	6.4%
Other	8.5%	8.5%	8.4%	8.7%
Prisoner Petitions	13.4%	13.2%	13.4%	13.0%
Real Property	2.2%	2.2%	2.2%	2.1%
Social Security	11.7%	13.3%	13.4%	13.3%
Torts	14.3%	10.4%	10.3%	10.6%
Jurisdictional basis				
Diversity	27.1%	24.0%	23.9%	24.0%
Federal Question	54.2%	55.3%	55.4%	55.2%
Government Party	18.6%	20.7%	20.6%	20.8%
Motion-level Outcomes				
Months until disposition		5.4 (4.5)	5.2 (4.3)	5.6 (4.7)
Ruling on Motion				
Denied		35.3%	35.4%	35.2%
Granted		51.2%	50.9%	51.4%
Granted in Part		13.6%	13.7%	13.4%
Ruling Appealed		26.0%	25.8%	26.1%

Note: Table presents summary statistics. Column (1) shows the full sample of summary judgment motions, including motions with unknown dispositions. Column (2) shows the subsample of motions for which I was able to identify an explicit disposition. Columns (3) and (4) split the known-disposition sample into motions with low reporting time (< 10 months) and high reporting time (≥ 10 months), respectively. Standard deviations are in parentheses.

become list-eligible after about seven months—while those filed on or just after the cutoffs are *least* exposed, only becoming reportable at approximately 13 months. Figure 2 shows how a motion's "reporting time"—the number of days before it becomes list-eligible—varies with filing date.

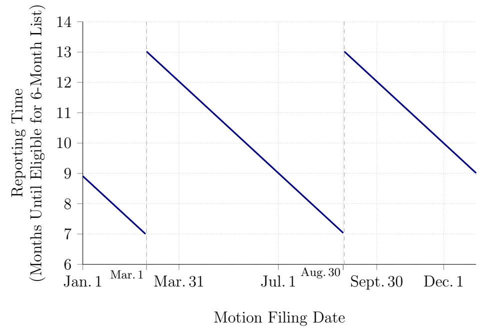


Figure 2. "Reporting Time" as a Function of Motion Filing Date

Note: Figure plots "reporting time"—i.e., the amount of time a judge has to review a motion before it could potentially appear on a six-month list—as a function of the motion's filing date.

My basic approach is to compare motion and appellate outcomes on the basis of their exposure to the six-month list. The core hypothesis is that greater exposure to the list leads judges to resolve motions more quickly. If judges prioritize speed over other dimensions of judicial performance, then I may also observe changes in other outcomes, including whether motions are granted or denied and whether litigants subsequently appeal.

As illustrated by Figure 2, reporting time varies continuously across most filing dates as well as discontinuously at the March 1 and August 30 cutoffs. This variation supports estimation via both OLS and regression discontinuity (RD), although the two approaches capture distinct causal parameters and require slightly different identifying assumptions.

First, assuming that filing dates are plausibly exogenous conditional on observables, I estimate the average effect of additional reporting time using the following OLS regression:

$$(1)\quad Y_{ijt} \;=\; \alpha + \theta \text{Reporting Time}_{ijt} + \mathbf{X}'_{ijt}B + \rho t + \lambda_t + \mu_j + \epsilon_{ijt}$$

where Y_{ijt} is the outcome of interest (e.g., resolution time, or any other motion or appellate outcome) for motion i before judge j at time t, Reporting Time$_{ijt}$ captures exposure to the six-month list, and $\mathbf{X}ijt$ includes motion- and case-level controls. The baseline model includes judge fixed effects (μ_j) as well as filing date time trends (ρt) and fixed effects (λ_t, which can include fixed effects for filing year, month-of-year, and day-of-month, depending upon the specification). The coefficient θ captures the average effect of one additional month of reporting time.

The regression discontinuity (RD) design estimates the local average treatment effect (LATE) of six additional months of reporting time for motions filed near the March 1 and August 30 cutoff dates. I pool data around both cutoffs and define a single running variable as the number of days separating the motion's filing date from the nearest cutoff. I then estimate a sharp RD of the form:

$$(2)\quad Y_{ijt} \;=\; \alpha + \tau \mathbb{1}(d_{ijt} \geq c) + f(d_{ijt} - c) + \mathbf{X}'_{ijt}\Gamma + \epsilon_{ijt},$$

where d_{ijt} is the motion's ordinal or "Julian" filing date (e.g., January $1 = 1$) and c is the nearest cutoff. The function $f(d_{ijt} - c)$ is a flexible polynomial in the running variable, estimated separately on either side of the cutoff, and $\mathbf{X}_{ijt}$ includes motion- and case-level controls, which are added for precision

(Calonico et al., 2019). The coefficient of interest is τ, which captures the LATE of six additional months of reporting time on the outcome of interest.

The two approaches complement one another in important ways. The OLS estimates are more precise and more general, drawing on variation across the full reporting time distribution, but they rely on the stronger assumption that filing dates are independent of potential outcomes, conditional on observables. The RD estimates are less precise and less general, but rely on a weaker assumption of continuity—that is, that expected outcomes evolve smoothly across the list cutoffs. I discuss these identifying assumptions and potential threats to inference in the following section.

III. Identification

A. Plausible Exogeneity of Filing Dates?

A key identifying assumption is that, conditional on observables and fixed effects, filing dates are as-good-as-random with respect to unobserved factors influencing judicial behavior or motion outcomes. This assumption is supported by institutional features of federal civil litigation. Although the decision to file a summary judgment motion is itself strategic, the precise filing date is typically determined by external scheduling constraints—such as court-imposed deadlines,[8] discovery timing, opposing party availability, and attorney workload—rather than by the merits of the motion or the incentives of the judge or litigants.[9]

[8]Federal rules require summary judgment motions to be filed within 30 days of the close of discovery, which is set early in the case by the judge and well before parties know whether a motion will be filed. Extensions are common and usually granted for non-substantive reasons (e.g., attorney scheduling conflicts).

[9]Interviews with practitioners suggest that most attorneys are unaware of the six-month list and its deadlines. Judges are familiar with the list but may not know the precise relationship between filing dates and reporting times. Publicly available inclusion

Moreover, filing dates are approximately uniform throughout the year (Figure 1), consistent with the idea that motions are not strategically timed around list deadlines. Observable pre-treatment characteristics are also generally well balanced across high- and low-exposure motions (Table 1). My baseline OLS models nonetheless include a rich set of case-, motion-, and court-level pre-treatment controls, including judge, filing party (i.e. plaintiff vs. defendant), case type, and district-by-year fixed effects. These aim to absorb any systematic variation in docket pressure, procedural timelines, or local litigation practices.

B. Evidence of Continuity at Cutoff Dates

My RD design relies on the assumption that potential outcomes evolve smoothly across the six-month list cutoff dates—that is, that motions are not strategically sorted around the threshold. I assess this in two standard ways: first, by plotting and testing for discontinuities in the density of filing dates (McCrary, 2008); and second, by comparing observable characteristics of motions filed just before and after the March 1 and August 30 cutoffs.

Importantly, although the running variable is based on time, my setting largely avoids typical pitfalls of regression discontinuity in time (RDiT) designs (Hausman and Rapson, 2018). Since many motions are filed on each day, I have sufficient cross-sectional variation to estimate treatment effects within a narrow bandwidth around the cutoff dates; relatedly, a McCrary-style density test is informative here since motion filings need not be uniformly dense across time.

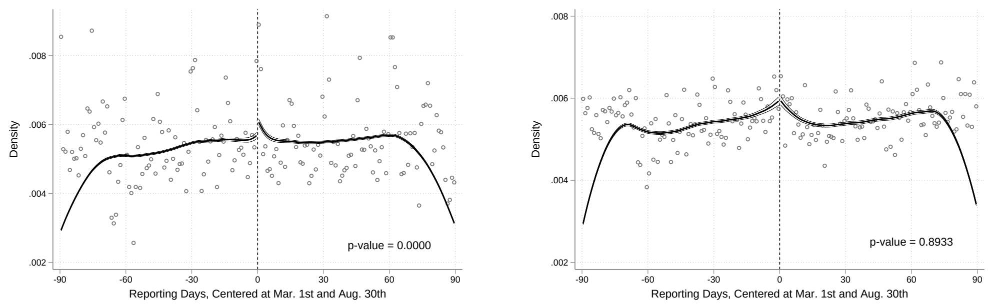
criteria for the six-month list are vague, and correspondence with the Administrative Office of the U.S. Courts revealed that actual inclusion criteria differ from those published online.

Figure 3 shows the density of motion filing dates around the six-month list cutoffs. As seen earlier in Figure 1, filings are roughly uniform throughout the year but spike on "round" calendar dates—typically the 1st, 15th, and last day of each month. Such heaping likely arises due to lawyers' tendency to file motions—and judges' tendency to set deadlines—on "round" dates, but since these patterns occur in each month of the year, they do not appear to reflect strategic responses to the six-month list.

Panel (a) shows a small but statistically significant discontinuity in the raw filing density at the cutoff dates, but this disappears after adjusting for day-of-week, day-of-month, holiday, and end-of-month effects (Panel b). These results hold when restricting to motions with known dispositions or when estimating separately around the March 1 and August 30 cutoffs (Appendix Figure A2). Although non-random heaping can complicate RD inference (Barreca, Lindo and Waddell, 2016), the evidence here suggests that any heaping is likely unrelated to the six-month list. I nonetheless return to this issue and address it with both a donut RD design and placebo tests below.

I supplement the density analysis with covariate balance tests, reported in Appendix Table A1. Each test estimates equation 2 using a pre-treatment motion- or case-level characteristic as the dependent variable. I find no significant discontinuities in most observables, including the filing party (plaintiff vs. defendant), attorney representation, jurisdictional basis,[10] or how long a case had been pending at the time of the motion for summary judgment (which likely reflects the complexity of the case). However, I detect small but significant jumps in the share of certain nature-of-suit categories, as well as marginal discontinuities in judge age and gender. To the extent

[10]Federal cases must satisfy one or more jurisdictional criteria, such as diversity or federal question jurisdiction.



(a) Raw Filings (b) Adjusted Filings

Figure 3. Density of Motion Filing Dates Relative to Six-Month List Cut-offs

Note: Figures show the density of summary judgment motions filing dates relative to the six-month list maximum-reporting-time cut-offs of March 1 and August 30. Sample includes all summary judgment motions filed from 2005–2014, including motions with unknown dispositions. P-values displayed in lower right are from McCrary density tests (McCrary, 2008). Figure (a) shows the raw filing date density, and (b) shows the filing date density after adjusting for day-of-month (including a dummy for the last day of any month), day-of-week, and holiday effects.

that younger and female judges respond more strongly to list exposure (a pattern for which I find evidence in Section IV.B below), these discontinuities could suggest that certain judges are manipulating their court-imposed filing deadlines to reduce their exposure to the six-month list.

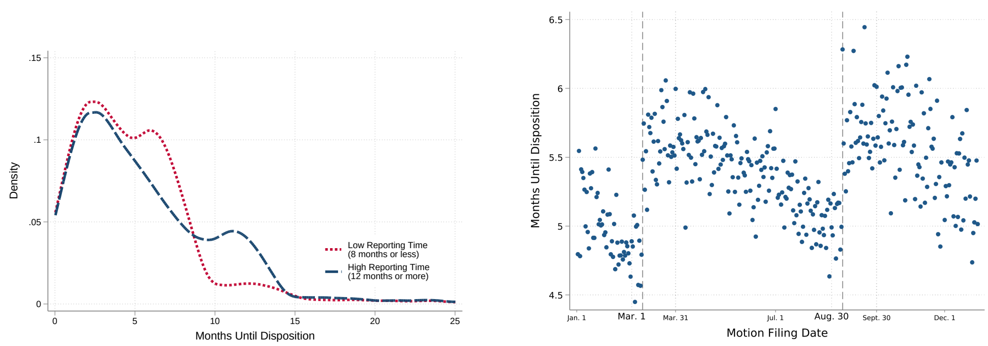
While I find no strong evidence of sorting around the six-month list cutoffs, I nonetheless take steps to address potential concerns. First, I supplement my main RD models with a "donut" design that excludes observations near the cutoff (Barreca, Lindo and Waddell, 2016). Second, I conduct placebo tests using counterfactual cutoff dates before and after the true thresholds, which help to mitigate concerns of spurious discontinuities.

The following section presents my main results, beginning with the six-month list's effect on the speed of motion resolution.

IV. Main Results

A. *Effect on Speed of Motion Disposition*

I begin by examining how the six-month list affects the speed of motion resolution. Figure 4 presents two descriptive figures that illustrate the basic relationship. Panel (a) compares the distribution of resolution times for motions with high ($\geq$ 10 months) versus low ($<$ 10 months) reporting time, and panel (b) plots average resolution time by filing date. Both figures reveal a clear negative relationship between reporting time and resolution speed, with visible discontinuities at the list cutoff dates in panel (b).



(a) Months until Motion Disposition by Reporting Time

(b) Avg. Months until Motion Disposition by Filing Day

FIGURE 4. MONTHS UNTIL MOTION DISPOSITION RELATIVE TO REPORTING TIME

Note: Figure (a) plots the kernel density of months until motion disposition by high vs. low reporting time bins. Figure (b) plots a scatterplot of average months until motion disposition against the motion's filing date. Dashed vertical lines indicate dates on which reporting time is at its highest (approximately 13 months).

Table 2 formalizes these results with OLS estimates corresponding to equation 1. The coefficients are consistent in magnitude and highly significant across specifications, indicating that each additional month of reporting time slows motion resolution by approximately 0.13 months (or about four days). Exposure to the six-month list, in other words, speeds resolution. Appendix Figure A1 shows that the relationship is approximately linear throughout the reporting time distribution.

TABLE 2—OLS ESTIMATES: EFFECT OF ONE MONTH ADDITIONAL REPORTING TIME ON MONTHS UNTIL MOTION DISPOSITION

	(1)	(2)	(3)	(4)
Reporting Time (Months)	0.132***	0.134***	0.133***	0.133***
	(0.005)	(0.005)	(0.005)	(0.005)
Observations	250,063	250,063	250,057	250,057
Case & Motion Controls	✓	✓	✓	✓
Calendar Trends		✓	✓	✓
District*Year FEs			✓	✓
Day-of-Month FEs				✓
Mean Motion Duration	5.32	5.32	5.32	5.32
Mean Reporting Time	10.03	10.03	10.03	10.03

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents OLS estimates of the effect of additional reporting time on months until motion disposition. Reporting time is defined as the number of months between the day on which a motion was filed and the earliest possible date on which it could appear on a six-month list. All columns include basic case- and motion-level controls, including a dummy for the party (plaintiff or defendant) filing the motion and nature-of-suit, judge, district, and filing-year fixed effects. Robust standard errors are in parentheses.

I turn next to my RD estimates, which are visually summarized in Figure 5. The figure plots average motion resolution time against the motion's filing date relative to the March 1 and August 30 cutoff dates. There is a clear discontinuity at the cutoffs: motions filed just before the cutoffs—those most exposed to the six-month list—are resolved nearly a month faster than those filed just after.

Table 3 reports RD estimates of the local average treatment effect of six additional months of reporting time on resolution speed. I follow Calonico, Cattaneo and Titiunik (2014) in selecting MSE-optimal bandwidths and

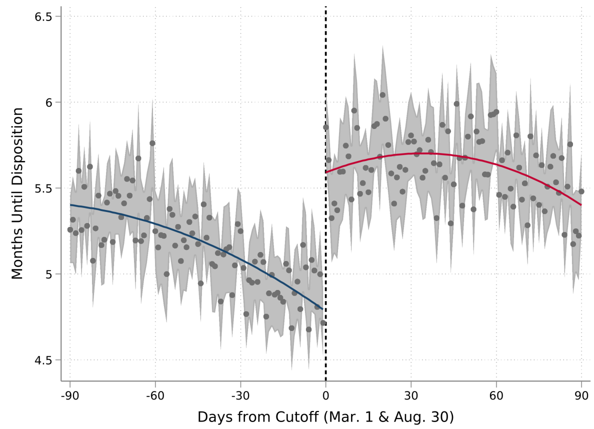


FIGURE 5. RD PLOT: MONTHS UNTIL MOTION DISPOSITION BY FILING DAY

Note: Figure plots average months until motion disposition against the motion's filing date relative to the two six-month list cutoff dates of March 1 and August 30. Fitted lines with 95% confidence intervals are estimated with second-order global polynomials.

estimating effects using local polynomials. Columns indicating controls include list (i.e., March vs. September), filing party, and nature-of-suit fixed effects. The estimates are positive and statistically significant throughout, suggesting that increased exposure to the list accelerates adjudication. The estimated magnitude—0.7 to 0.8 months (14% of the unconditional mean)—closely matches the OLS estimates in Table 2, which imply a 0.78-month effect for the same six-month difference in reporting time.

I report several RD robustness checks in Appendix Section A. The results are stable across alternative bandwidths (Appendix Table A2) and alternative polynomial specifications (Table A4). To account for the possibility that error terms are serially correlated (Hausman and Rapson, 2018), which would almost certainly occur only within a judge's docket, I show that results are robust to clustering the standard errors at the judge level (Table

Table 3—RD Estimates: Effect of six months additional reporting time on months until motion disposition

	(1)	(2)	(3)	(4)
Filed After Cutoff	0.752***	0.767***	0.735***	0.755***
	(0.111)	(0.094)	(0.150)	(0.118)
Dep. Var. mean	5.4	5.4	5.4	5.4
Polynomial order	First	First	Second	Second
Controls		✓		✓
Bandwidth (days)	33.7	32.1	42.1	45.9
Effective N	80,264	78,120	99,208	106,296
N (left)	39,125	38,144	48,741	52,285
N (right)	41,139	39,976	50,467	54,011

Note: Table presents bias-corrected RD estimates of the effect of six months additional reporting time on months until motion disposition. MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014). Effects are estimated with first or second order local polynomials using triangular kernels. Columns indicating controls include list (i.e., March vs. September) fixed effects, a dummy for the filing party (plaintiff vs. defendant), and nature-of-suit and filing-year fixed effects. Robust standard errors clustered at the Julian date of filing are in parentheses.

A3). Results also hold under a donut RD design that excludes motions near the cutoff (Table A5). Finally, placebo tests using counterfactual cutoff dates detect no spurious discontinuities (Table A6; Figure A3), with one marginal exception. Together, these checks reinforce the conclusion that the observed discontinuity reflects a true effect of exposure to the six-month list.

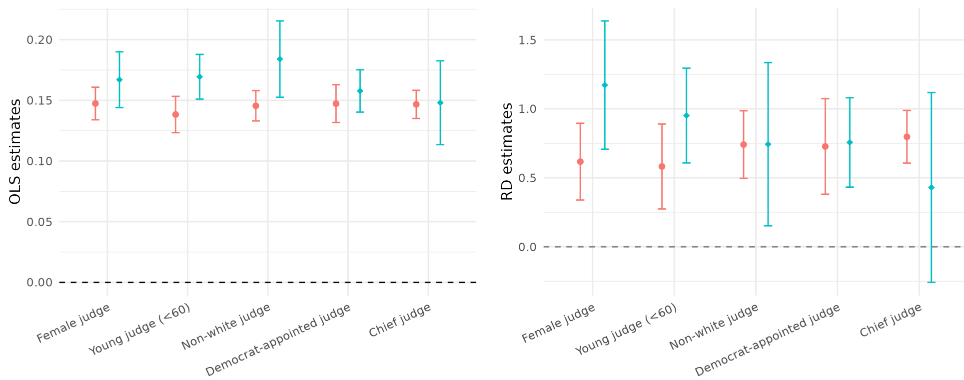
As previewed above, a potential concern with my research design is that motions with known dispositions may differ systematically from those with unknown dispositions, introducing a form of reverse survivorship bias. To assess this possibility, I re-estimate my main OLS and RD models including motions with unknown dispositions, imputing their time to disposition as the number of days between the motion's filing date and the end of the overall case. The OLS results are slightly attenuated but remain broadly consistent with my main estimates (Appendix Table A12). The RD estimates lose statistical significance, likely due to increased measurement error, but remain directionally consistent with the main findings (Appendix Table A13)

B. Heterogeneous Effects by Judge Characteristics

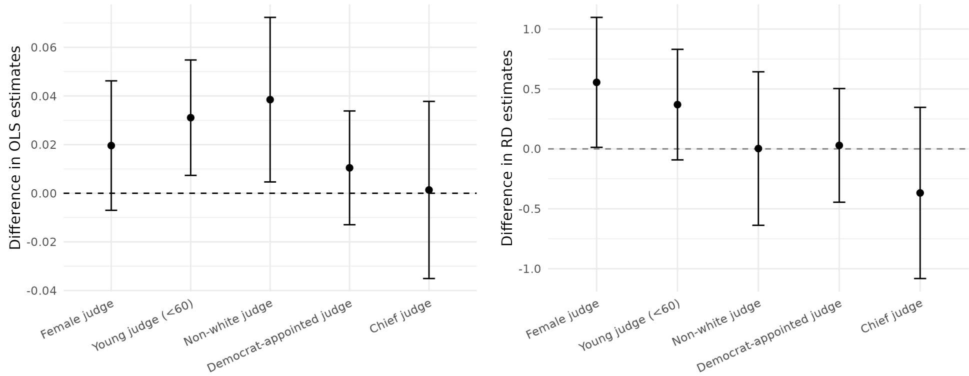
Do judges respond differently to the six-month list according to their demographic or professional characteristics? Figure 6 plots OLS and RD estimates of the effect of additional reporting time on motion resolution speed, disaggregated by judge characteristics. Subgroups include gender, age (above vs. below 60), race (non-white vs. white), political affiliation of the appointing president (Democrat vs. Republican), and status as the district's Chief Judge. OLS estimates are obtained from adding interaction terms to Eq. 1; RD estimates follow Calonico et al. (2025) by adding linear interactions to Eq. 2. Panel A presents group-specific estimates; Panel B plots differences between subgroups with 95% confidence intervals.

These results should be interpreted with caution: judge characteristics are not randomly assigned, subgroup sizes are small, and many traits—such as age, gender, and race—are correlated. Still, suggestive patterns emerge. In the OLS estimates, younger, female, and non-white judges appear more responsive to the six-month list than their older, male, and white counterparts, with differences statistically distinguishable at conventional levels. By contrast, there is no evidence of heterogeneity by political affiliation or Chief Judge status. The RD estimates yield similar patterns, though differences by race are not statistically detectable, likely due to limited sample size near the cutoff.

While not conclusive, the subgroup patterns are consistent with a theory that the six-month list operates partly through reputational and career incentives. Such pressures may be more salient for judges early in their careers



(a) Panel A: OLS & RD Estimates by Judge Subgroup



(b) Panel B: Difference in Subgroup Estimates

Figure 6. Heterogeneous Effects by Judge Subgroups

Note: Panel (a) shows OLS (left) and RD (right) estimates of the effect of additional reporting time on months until motion disposition by judge subgroup. Diamond markers correspond to estimates for judges with the characteristic indicated on the x-axis. Panel (b) shows differences between subgroup estimates. RD subgroup estimates are obtained following Calonico et al. (2025).

or from historically underrepresented groups, who may face greater scrutiny or be more attuned to institutional expectations around productivity. Opportunities for advancement (either through elevation to a higher court or selection for internal leadership roles) may heighten these concerns, especially for younger judges, who have more working years ahead of them, but also potentially for female and non-white judges, who received new attention as part of President Obama's efforts to diversify the federal judiciary.[11]

C. *Effect on "Quality" of Adjudication*

While the six-month list appears to accelerate motion dispositions, it may also affect other aspects of judicial performance. A multi-task principal-agent framework (Holmstrom and Milgrom, 1991) might predict that judges would reallocate effort toward speed at the expense of other dimensions of performance. In fact, skeptics of the six-month list have long argued that it could result in inaccurate and unfair rulings. I explore this possibility using OLS and RD designs to estimate the effect of list exposure on motion and appellate outcomes, including whether the motion was granted or denied, whether the ruling was appealed, and whether the ruling was affirmed on appeal. I also assess the relationship between motion-level exposure to the six-month list and overall *case* duration, which may shed light on whether judges are "cutting corners" to resolve motions more quickly.

Motion and Appellate Outcomes

Judicial quality is inherently difficult to measure: I do not observe the "correct" outcome for any motion, nor do I observe the quality of a judge's

[11] On at least one recent occasion, a Senator cited a district judge's six-month list track record as grounds to oppose her elevation to a Court of Appeals.

written opinion.[12] To the extent that I find differences in outcomes between more and less exposed motions, I can only infer that the six-month list must have affected—and potentially compromised—certain unobserved dimensions of judicial performance beyond mere speed.

Appellate outcomes are more plausibly related to adjudicatory quality, but they too are imperfect measures, both because the vast majority of rulings (approximately 74% in my sample) are never appealed, and because there is similarly no "correct" answer on appeal. Despite these limitations, appellate outcomes are often viewed as imperfect but reasonable proxies for decisional quality (e.g., Choi, 2012; Halberstam, 2016). If the list prompts district judges to make rushed and low-quality decisions, we might expect motions with longer reporting times to show lower appeal rates and higher affirmance rates, consistent with higher quality.

Table 4 reports OLS estimates of the effect of reporting time on motion and appellate outcomes. Judges with more reporting time are slightly more likely to grant motions, but the effect is small—less than 0.3% per month of reporting time—and only marginally significant. Reporting time has no detectable effect on appeal rates. However, among motions that are appealed, each additional month of reporting time is associated with a 0.3 percentage point (0.7%) increase in the likelihood of affirmance.

Table 5 reports RD estimates for the same outcomes, and Appendix Figure A4 shows corresponding RD plots. With the exception of affirmance rates, all other RD estimates are small and statistically insignificant, indicating little effect of list exposure on grant, denial, or appeal rates. Among

[12]Some have suggested that a rushed judge may be more likely to deny summary judgment, since denials are harder to appeal and require less formal justification, but such a relationship is purely speculative.

Table 4—OLS Estimates: Effect of one month additional reporting time on motion and appellate outcomes

	(1) Granted	(2) Denied	(3) Appealed	(4) Affirmed
Months until Report	0.0013**	-0.0003	-0.0001	0.0030***
	[0.0006]	[0.0006]	[0.0004]	[0.0010]
Observations	211,902	211,902	362,216	79,831
Dep. Var. mean	.49	.34	.22	.43
Indep. Var. mean	10.05	10.05	10.05	10.03

Note: Table presents OLS estimates of the effect of additional reporting time on motion and appellate outcomes. Reporting time is defined as the number of months between the day on which a motion was filed and the earliest possible date on which it could appear on a six-month list. All columns include basic case- and motion-level controls, calendar trends, district*year fixed effects, and day-of-month fixed effects. Robust standard errors are in parentheses.

appealed motions, however, those that are least exposed to the six-month list are approximately 3.8 percentage points (or about 8.6%) more likely to be affirmed on appeal than those that are most exposed. This estimate is larger but directionally consistent with the OLS result, supporting the interpretation that increased reporting time may improve decisional quality.

Somewhat reassuringly, the same overall pattern is visible even in a simple scatterplot of affirmance rates by filing date (Appendix Figure A5d), which shows a noisy but visible negative correlation between reporting time and affirmance rates, including small but visible discontinuities at the six-month list cutoffs. The Appendix also shows that the RD estimate for affirmance rates is robust to alternative specifications (Table A10), including alternative bandwidths (Table A7), clustering at the judge-level (Table A3), and donut RDs of various radii (Table A8).

Table 5—RD Estimates: Effect of six months additional reporting time on motion and appellate outcomes

	(1) Granted	(2) Denied	(3) Appealed	(4) Affirmed
Filed After Cutoff	-0.015	0.014	-0.006	0.038**
	(0.011)	(0.013)	(0.012)	(0.017)
Dep. Var. mean	.49	.34	.26	.44
Polynomial order	First	First	First	First
Bandwidth (days)	32.7	26.4	30.1	23.6
N (left)	38,144	29,455	35,382	6,892
N (right)	39,976	31,381	36,477	7,409

Note: Table presents bias-corrected RD estimates of the effect of six months additional reporting time on motion and appellate outcomes. MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014). Effects are estimated with first-order local polynomials using triangular kernels. All columns include list (i.e., March or September) fixed effects, a dummy for the part (plaintiff or defendant) filing the motion, and nature-of-suit and filing-year fixed effects. Robust standard errors clustered at the Julian date of filing are in parentheses.

Effect on Overall Case Duration

As a final test of adjudicatory quality, I examine how list exposure affects overall case duration. Case duration is important in its own right: it reflects not only the total outlay of judicial resources on a case but also the private cost of litigation to the parties.

Additionally, however, total case duration may also shed light on adjudicatory quality. Having found exposure to the six-month list to expedite motion resolution, we might expect list exposure to also shorten overall case duration by a roughly similar amount of time, if only because the motion must be resolved before the overall case can be resolved. However, if judges are "cutting corners" under pressure from the six-month list, they may defer complex issues to later stages, possibly resulting in longer trials or delays elsewhere in the process. In that case, we should expect to see that some of the motion-level time savings are offset by greater delays later in the case, resulting in a less than one-for-one relationship between motion-level time savings and case-level time savings.

Table 6 reports estimates of the effect of motion reporting time on total case duration. Column (1) shows that each additional month of reporting time is associated with a 0.09-month increase (about 3 days) in case duration, consistent with faster motions leading to faster overall case resolution.

Table 6—Effect of one month additional motion reporting time on total case duration

	Naive OLS		ACDE
	(1)	(2)	(3)
Reporting time (months)	0.091***	-0.031*	-0.031*
	(0.016)	(0.018)	(0.016)
Motion duration (months)		0.914***	
		(0.010)	
Observations	361,806	211,500	211,500
Dep. Var. mean	21.22	22.3	22.3
Indep. Var. mean	10.05	10.05	10.05

Note: Table presents estimates of the effect of additional motion reporting time on total case duration (measured in months). Columns (1)–(3) show naive OLS estimates; column (1) shows the baseline effect of motion reporting time on total case duration unconditional motion disposition time; column (2) adds motion duration as a control. Column (3) reports the estimated Average Controlled Direct Effect of motion reporting time on case duration using the two-step "sequential-g" method proposed by Acharya, Blackwell and Sen (2016). All columns include basic pre-treatment case- and motion-level controls, calendar day trends, and district*year fixed effects. Column (3) standard error is calculated using a bootstrap procedure.

To explore potential channels, I next control for motion disposition time, in essence treating motion disposition time as a mediator between exposure to the six-month list and overall case duration. This analysis is suggestive only, since conditioning on a post-treatment variable may introduce bias. Nonetheless, if the reporting time coefficient remains significant, that might suggest that the six-month list affects case duration through channels beyond simply speeding motions. If the coefficient on motion reporting remains significant *and flips signs*, that might suggest that judges are cutting corners at the summary judgment stage, ultimately squandering some of the motion-level time savings by causing delays later in the case.

Indeed, that is largely what I find. Controlling for motion disposition time in Column (2), the effect of reporting time reverses: each additional month of reporting time increases case duration by about 0.03 months (roughly one day). Column (3) replicates this result using the two-step "sequential-g" estimator of Acharya, Blackwell and Sen (2016), which isolates the direct effect of reporting time on case duration and addresses post-treatment bias under certain assumptions.[13]

Altogether, the results in this section paint a mixed picture. I find little evidence that exposure to the six-month list affects how motions are decided—whether they are granted or denied—or whether they are appealed, but I do find some evidence that motions with longer reporting times are more likely to be affirmed on appeal. I also find suggestive evidence that judges may be inefficiently cutting corners at the summary judgment phase.

V. Aggregate Effects of the Six-Month List: A Bunching Analysis

What would happen if the six-month list were eliminated altogether? To quantify the aggregate effect of the six-month list on motion-processing time, I implement a notched bunching estimator adapted from Kleven and

[13]Specifically, the method assumes "sequential unconfoundedness." Here, that means I assume (1) no omitted variable with respect to the effect of reporting time on case duration, conditional on pretreatment controls, and (2) no omitted variable with respect to the effect of motion disposition time on case duration, conditional on reporting time, pretreatment controls, and intermediate motion-level outcomes (e.g., whether the motion was granted or denied).

Waseem (2013) and Kleven (2016). The list deadline acts as a behavioral notch: judges have a strong incentive to resolve motions just before the March 31 and September 30 reporting deadlines, but little incentive to rule earlier, and no additional penalty for delay beyond the deadline. This creates the bunching pattern visible in Figure 1b, with spikes in motions resolved at each reporting date.

Conceptually, I compare the observed distribution of motion resolution times to a smooth counterfactual distribution fitted via polynomial regression, excluding a pre-specified window around the notch. More formally, let d be the number of days from motion filing to disposition, and let l denote a motion's reporting time in days. I observe densities $f_1^l(d)$ for all motions with $l \in L$, where L spans reporting times from roughly seven to thirteen months. The goal is to estimate a common counterfactual distribution $f_0(d)$ that approximates motion durations under a no-list regime.[14]

I group motion dispositions into five-day bins indexed by j—for example, motions resolved in 1–5 days, 6–10 days, and so on. Rather than estimating separate counterfactual distributions for each reporting time l, I estimate a single counterfactual distribution $\hat{f}_0(d)$ using motions with maximum reporting time ($l \geq 390$). These least-exposed motions best approximate a no-list scenario, since a far-off deadline is most similar to no deadline at all.

To estimate the counterfactual distribution $\hat{f}_0(d)$, I regress counts of motion resolutions in duration bin j on a high-order polynomial in d_j, excluding

[14]My approach is similar to that of Gruber, Hoe and Stoye (2023), who use a similar estimator to study wait-time targets in U.K. emergency rooms. Unlike their setting, which involves a single notch, my setting includes many *different* notches corresponding to different reporting times.

bins around the notch. The fitted distribution is given by:

$$\hat{f}_0(d) \equiv \sum_{i=0}^{p} \hat{\beta}_i \cdot (d_j)^i + \sum_{r \in R} \hat{\rho}_r \cdot \mathbb{1}\left[\frac{d_j}{r} \in \mathbb{N}\right], \tag{3}$$

with coefficients estimated via:

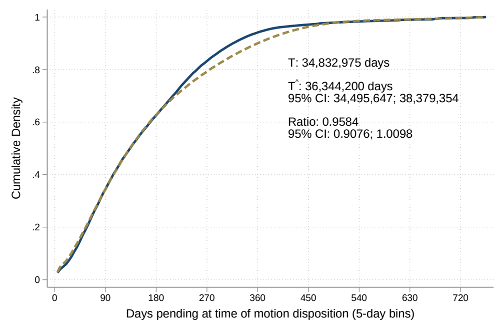
$$c_j = \sum_{i=0}^{p} \beta_i \cdot (d_j)^i + \sum_{i=d_-}^{d_+} \gamma_i \cdot \mathbb{1}[d_j = i] + \sum_{r \in R} \rho_r \cdot \mathbb{1}\left[\frac{d_j}{r} \in \mathbb{N}\right] + \nu_j, \tag{4}$$

where c_j is the number of motions with maximum reporting time resolved in duration bin j, d_j is the upper bound of duration bin j, and $p = 8$ in the baseline specigation. The final term accounts for round-number heaping (e.g., at multiples of 30 days).

The key assumption of the bunching estimator is that bunching responses are entirely confined to an excluded window (i.e., the range $[d_-, d_+]$) around the notch. Finally, with a counterfactual distribution in hand, I calculate the ratio $T/\hat{T}$, where $T = \sum_{j \in J} \sum_{l \in L} c_j^l \cdot d_j$ represents total actual resolution time for all summary judgment motions in my sample, and $\hat{T} = \sum_{j \in J} \sum_{l \in L} \hat{c}_j^l \cdot d_j$ represents the estimated total counterfactual resolution time for the same sample of motions.

I summarize the aggregate results with Figure 7, which plots the cumulative distribution functions of observed and counterfactual motion resolution times. The figure reveals a clear gap between the two. I estimate a ratio $T/\hat{T}$ of approximately 0.958 (95% CI: 0.908–1.010), indicating that the six-month list reduces total motion-processing time by about 4.2% (95% CI: -1%–9.2%). This result is robust to alternative polynomial orders and exclusion window bounds (Appendix Table B1).

FIGURE 7. ACTUAL VS. COUNTERFACTUAL RESOLUTION TIMES



Note: Figure plots the cumulative distribution functions of actual and counterfactual motion resolution times for all summary judgment motions in the sample. Actual total resolution time (T), estimated counterfactual resolution time ($\hat{T}$), ratio $T/\hat{T}$, 95% confidence intervals are indicated in northeast corner of plot.

In Appendix Section B I present separate reporting-time-specific CDFs and show that the ratio $T^l/\hat{T}^l$ is increasing in reporting time l. Motions filed with 250 days or less are resolved about 12% faster than the counterfactual, while those with 320 days or more show less than 2% time savings (with 95% confidence intervals that include *no* time-savings). These results suggest that the aggregate time savings are largely driven by the most exposed motions, particularly those with fewer than 9 months of reporting time.

VI. Conclusion

This paper studies whether nonmonetary incentives—specifically, social sanctions—can improve bureaucratic performance in the context of the U.S. federal judiciary. I study the "six-month list," a Congressionally-mandated policy requiring courts to publicly identify judges with motions pending longer than six months. Leveraging quasi-random variation in list exposure, I find that judges significantly accelerate resolution of summary judgment motions under deadline pressure. Motions most exposed to the six-month list are resolved approximately 14% faster than those least exposed.

These effects are not uniform. Young, female, and non-white judges respond more strongly than older, male, and white judges. These patterns are consistent with a theory that the six-month list operates partially through reputational incentives and career concerns. Regardless of the mechanism, these results could also be cause for concern, potentially damaging morale among judges.

While the six-month list clearly promotes timeliness, the evidence on adjudicative quality is more nuanced. Motion grant and denial rates are unaffected, but among appealed cases, less-exposed motions are more likely to be affirmed—suggesting that some speed gains may come at the expense of quality. Moreover, the list's effect on total case duration is smaller than its effect on motion duration, consistent with judges deferring effort to later litigation stages—what I term "corner-cutting" at the motion phase.

A bunching analysis confirms that the six-month list shapes judicial behavior and improves efficiency in the aggregate. I estimate that the list reduces summary judgment processing time by approximately 4% relative to a no-list counterfactual. These results demonstrate that the policy has modest but measurable effects on overall judicial efficiency, which previous studies have shown to be critical for economic growth.

More broadly, this paper deepens our understanding of how bureaucrats respond to social and reputational incentives. When formal mechanisms of accountability are weak or absent, transparency-based policies like the six-month list may offer a pragmatic alternative—but policymakers should remain attentive to potential distortions and unintended effects.

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Supplemental Appendix: Robustness Checks and Additional Results

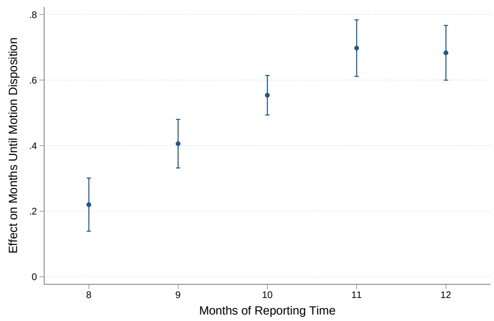

Figure A1. Non-parametric OLS Estimates

Note: Figure plots non-parametric OLS estimates of the effect of reporting time on months until motion disposition. Reporting time is defined as the number of months between the day on which a motion was filed and the earliest possible date on which it could appear on a six-month list. Estimates are obtained by regressing months until motion disposition on a series of reporting time dummies (one for each month of reporting time) in addition to motion and case controls. Error bars indicate 95% confidence intervals.

Table A1—: Balance Table: RD Estimates for Pre-Treatment Characteristics.

	Linear		Quadratic	
	(1)	(2)	(3)	(4)
Dependent Var.	No Controls	Controls	No Controls	Controls
% Filed by Defendant	0.00 (0.01)	0.00 (0.01)	-0.00 (0.02)	-0.00 (0.02)
% Class Action	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
Docket Entries Prior to Motion	-12.14 (11.56)	-14.39 (10.69)	-12.27 (12.86)	-14.51 (12.53)

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Table A1 – ...continued from previous page

	Linear		Quadratic	
	(1)	(2)	(3)	(4)
Dependent Var.	No Controls	Controls	No Controls	Controls
Days Prior to Motion Filing	-22.72	-18.72	-33.11	-35.94
	(18.26)	(14.49)	(25.56)	(24.52)
% Civil Rights Cases	0.01	0.01	-0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)
% Employment Cases	0.01*	0.01*	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
% Prisoner Rights Cases	-0.02***	-0.02***	-0.02***	-0.02***
	(0.01)	(0.01)	(0.01)	(0.01)
% Intellectual Property Cases	-0.00	-0.00	-0.01*	-0.01*
	(0.00)	(0.00)	(0.00)	(0.00)
% Labor Cases	0.01**	0.01**	0.01**	0.01**
	(0.01)	(0.01)	(0.01)	(0.01)
% Real Property Cases	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% Soc. Sec. Appeals	-0.02	-0.01	-0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.02)
% Tort Cases	-0.01	-0.01	-0.01	-0.02
	(0.02)	(0.02)	(0.02)	(0.03)
% Contract Cases	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
% Motion Filed by Pro Se Party	-0.01	-0.01	-0.01	-0.01
	(0.00)	(0.00)	(0.01)	(0.01)
% In Forma Pauperis	-0.01	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
% Diversity Jurisdiction	-0.00	-0.01	-0.02	-0.02

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Table A1 – ...continued from previous page

	Linear		Quadratic	
	(1)	(2)	(3)	(4)
Dependent Var.	No Controls	Controls	No Controls	Controls
	(0.02)	(0.02)	(0.03)	(0.03)
% Federal Question Jurisdiction	0.02*	0.02*	0.03*	0.03*
	(0.01)	(0.01)	(0.02)	(0.02)
% Government Defendant	-0.01	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)
Assigned Judge Age (Years)	-0.33**	-0.35**	-0.39*	-0.41**
	(0.16)	(0.15)	(0.20)	(0.19)
% Assigned Female Judge	0.02	0.02*	0.03*	0.03**
	(0.01)	(0.01)	(0.02)	(0.02)
% Assigned Non-White Judge	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
% Assigned Magistrate Judge	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)

Note: Table shows regression discontinuity estimates for various pre-treatment motion- and case-level characteristics. MSE-optimal bandwidths are selected using the procedure from Calonico et al. (2019) and based on a local linear regression. All columns estimated with first- or second-order local polynomials. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2—RD Estimates for Months Until Motion Disposition, Alternative Bandwidths

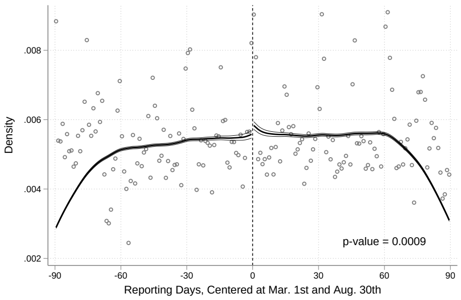
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Filed After Cutoff	0.788***	0.723***	0.735***	0.746***	0.777***	0.792***	0.806***
	(0.115)	(0.113)	(0.108)	(0.100)	(0.091)	(0.084)	(0.078)
Dep. Var. mean	5.4	5.4	5.4	5.4	5.4	5.4	5.4
Polynomial order	First	First	First	First	First	First	First
Controls	✓	✓	✓	✓	✓	✓	✓
Bandwidth (days)	7.0	14.0	21.0	28.0	35.0	42.0	49.0
Effective N	15,958	31,318	48,173	63,142	82,440	96,850	113990
N (left)	7,264	14,963	23,190	30,654	40,223	47,563	55,998
N (right)	8,694	16,355	24,983	32,488	42,217	49,287	57,992

Note: Table presents RD estimates of the effect of reporting time on months until motion disposition using a variety alternative bandwidths. All columns are estimated with first first-order local polynomials and triangular kernels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

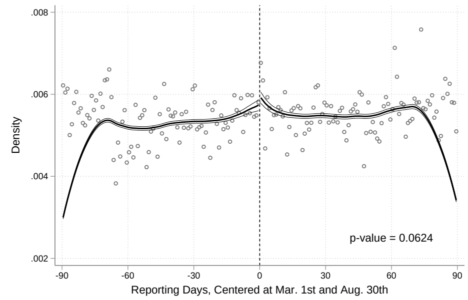
Table A3—RD Estimates: Effect of six months additional reporting time on months until motion disposition, clustered at judge-level

	(1)	(2)	(3)	(4)
Filed After Cutoff	0.764***	0.785***	0.732***	0.758***
	(0.146)	(0.135)	(0.192)	(0.167)
Dep. Var. mean	5.4	5.4	5.4	5.4
Polynomial order	First	First	Second	Second
Controls		✓		✓
Bandwidth (days)	38.9	38.5	44.6	48.7
Effective N	90,754	90,549	103572	113813
N (left)	44,440	44,340	51,001	55,931
N (right)	46,314	46,209	52,571	57,882

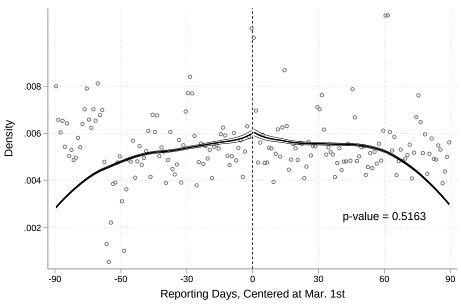
Note: Table presents bias-corrected RD estimates of the effect of six months additional reporting time on months until motion disposition. MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014). Effects are estimated with first or second order local polynomials using triangular kernels. Columns indicating controls include list (i.e., March or September) fixed effects, a dummy for the part (plaintiff or defendant) filing the motion, and nature-of-suit and filing-year fixed effects. Robust standard errors clustered at the judge-level are in parentheses.



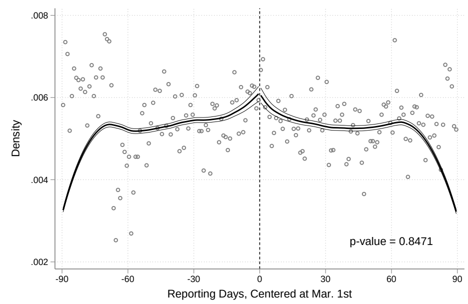
(a) Raw Filings, both cutoffs



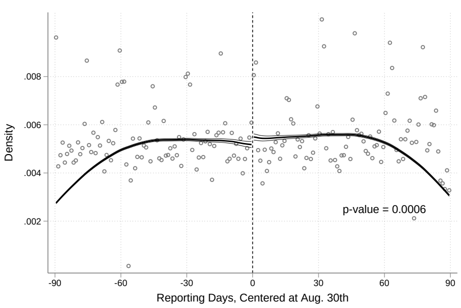
(b) Adjusted Filings, both cutoffs



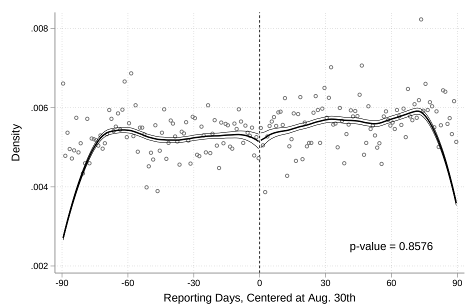
(c) Raw Filings, March cutoff



(d) Adjusted Filings, March cutoff



(e) Raw Filings, August cutoff



(f) Adjusted Filings, August cutoff

FIGURE A2. DENSITY OF MOTION FILING DATES RELATIVE TO SIX-MONTH LIST CUT-OFFS

Note: Figures show the density of summary judgment motions filing dates relative to the six-month list maximum-reporting-time cut-offs of March 1 and August 30. Sample includes summary judgment motions filed from 2005–2014 with known dispositions. P-values displayed in the lower right are from McCrary density tests (McCrary, 2008). Panels (a)–(f) show raw and adjusted filing date densities for both, March, and August cutoffs respectively.

Table A4—Parametric RD Estimates: Effect of six months additional reporting time on months until motion disposition

	(1)	(2)	(3)	(4)
Filed After Cutoff	0.773***	0.814***	0.717***	0.766***
	(0.091)	(0.083)	(0.132)	(0.110)
Dep. Var. mean	5.32	5.32	5.33	5.33
Global poly. order	First	First	Second	Second
Controls		✓		✓
Bandwidth (days)	33.68	32.09	42.15	45.95
Effective N	80,264	78,120	99,208	106296
N (left)	39,125	38,144	48,741	52,285
N (right)	41,139	39,976	50,467	54,011

Note: Table presents bias-corrected RD estimates of the effect of six months additional reporting time on months until motion disposition. MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014) but effects are estimated with first or second global polynomials. Columns indicating controls include list (i.e., March or September) fixed effects, a dummy for the part (plaintiff or defendant) filing the motion, and nature-of-suit and filing-year fixed effects. Robust standard errors clustered at the Julian date of filing are in parentheses.

Table A5—Donut RD Estimates for Months Until Motion Disposition

	(1)	(2)	(3)	(4)	(5)	(6)
Filed After Cutoff	0.778***	0.785***	0.790***	0.797***	0.802***	0.808***
	(0.091)	(0.088)	(0.085)	(0.082)	(0.079)	(0.077)
Donut radius (days)	3	6	9	12	15	18
Dep. Var. mean	5.4	5.4	5.4	5.4	5.4	5.4
Polynomial order	First	First	First	First	First	First
Controls	✓	✓	✓	✓	✓	✓
Bandwidth (days)	35.1	38.1	41.1	44.1	47.1	50.1
Effective N	84,507	90,684	96,850	103496	111594	118476
N (left)	41,251	44,393	47,563	50,947	54,724	58,180
N (right)	43,256	46,291	49,287	52,549	56,870	60,296

Note: Table presents "Donut" RD estimates of the effect of reporting time on months until motion disposition using a variety of donut radii. All columns are estimated with triangular kernels using first-order local polynomials.

Table A6—Placebo Tests: RD Estimates for Months Until Motion Disposition, Counterfactual Cutoffs

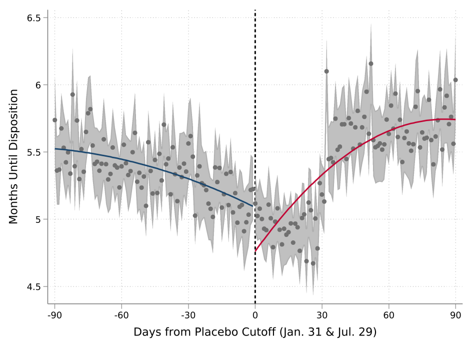
	(1)	(2)	(3)	(4)
Filed After Cutoff	-0.205	0.053	0.012	-0.258**
	(0.149)	(0.118)	(0.134)	(0.107)
Cutoff Dates	2 months left	1 month left	1 month right	2 months right
Dep. Var. mean	5.4	5.39	5.4	5.4
Polynomial order	First	First	First	First
Controls	✓	✓	✓	✓
Bandwidth (days)	22.6	18.6	19.3	20.6
Effective N	49,858	43,197	46,214	49,434
N (left)	25,456	20,321	22,346	23,488
N (right)	24,402	22,876	23,868	25,946

Note: Table presents placebo RD estimates of the effect of reporting time on months until motion disposition using counterfactual six-month publication and cutoff dates (shifted either one or two months to the left or right of actual dates). All columns are estimated with local linear polynomials using triangular kernels.

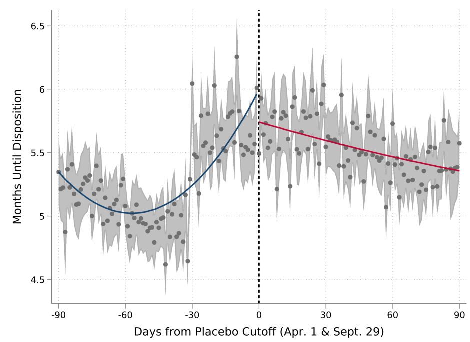
Table A7—RD Estimates for Motion & Appellate Outcomes, Alternative Bandwidths

BW (days):	7	14	21	28	35	42
% Granted	-0.01	-0.02	-0.02	-0.02	-0.01	-0.01
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
% Denied	0.01	0.01	0.01	0.01	0.01	0.01
	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
% Appealed	0.02	0.00	-0.00	-0.01	-0.01	-0.01
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
% Affirmed	-0.00	0.03	0.04**	0.04**	0.03**	0.03**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)

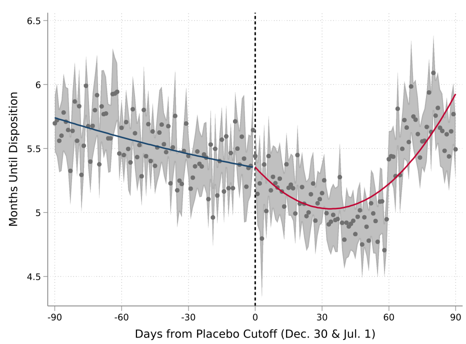
Note: Table presents RD estimates of the effect of reporting time on motion and appellate outcomes using a variety alternative bandwidths. All columns are estimated with first first-order local polynomials and triangular kernels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



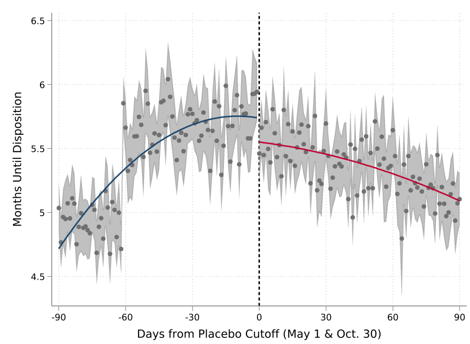
(a) One Month Left



(b) One Month Right



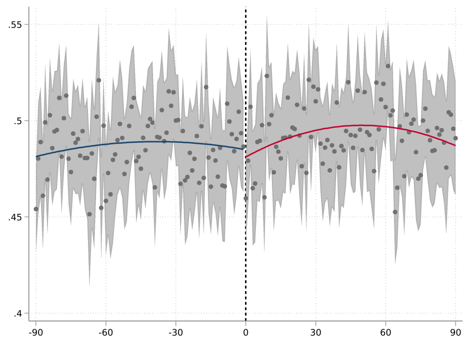
(c) Two Months Left



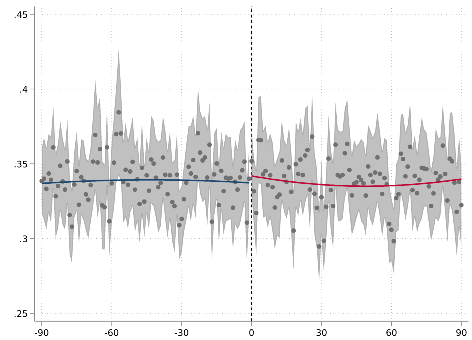
(d) Two Months Right

Figure A3. RD Estimates of Effect on Months Until Disposition, Placebo Cutoffs

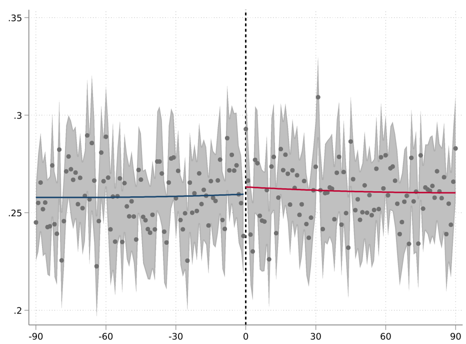
Note: Figures show RD placebo plots of the effect of six months of additional reporting time on months until motion disposition using counterfactual six-month publication and cutoff dates. Publication and cutoff dates are shifted either one or two months to the left or right. MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014). All columns are estimated with local linear polynomials using triangular kernels.



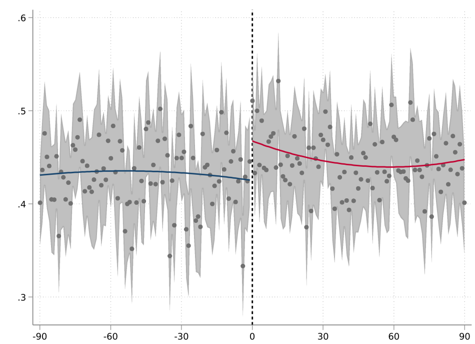
(a) % Granted



(b) % Denied



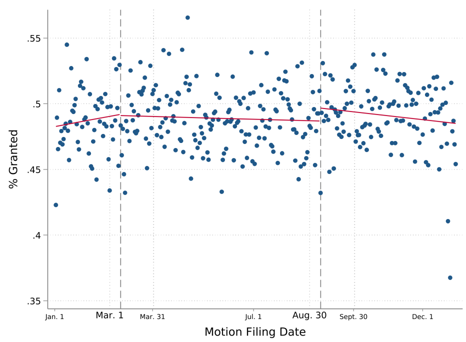
(c) % Appealed



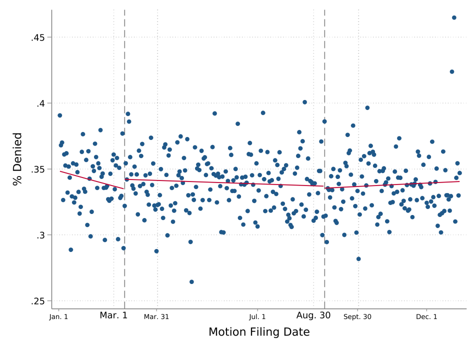
(d) % Affirmed on Appeal

Figure A4. RD Plots: Motion and Appellate Outcomes

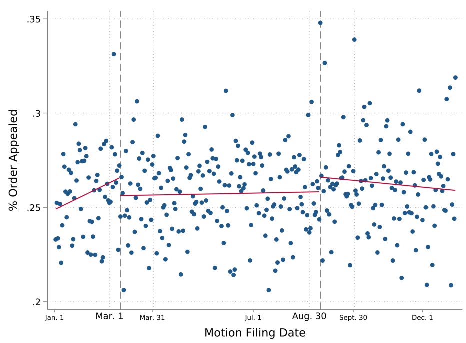
Note: Figures plot average motion and appellate outcomes against the motion's filing date relative to the two six-month list cutoff dates of March 1 and August 30. The sample comprises all summary judgment motions filed from 2005–2014 with known dispositions.



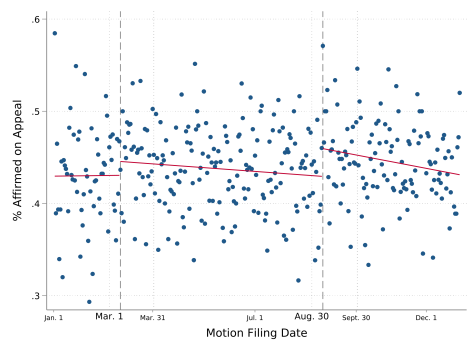
(a) % Granted



(b) % Denied



(c) % Appealed



(d) % Affirmed on Appeal

Figure A5. Scatterplots: Motion and Appellate Outcomes

Note: Figures plot average motion and appellate outcomes against the motion's calendar filing date. Best-fit lines are separately estimated for the periods January 1st–February 29th, March 1–August 29th, and August 30–December 31st. The sample comprises all summary judgment motions filed from 2005–2014 with known dispositions.

Table A8—Donut RD Estimates for Motion & Appellate Outcomes

Donut radius (days):	3	6	9	12	15	18
% Granted	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
% Denied	0.01	0.01	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
% Appealed	-0.01	-0.01	-0.01	-0.01	-0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
% Affirmed	0.04**	0.03**	0.03**	0.03**	0.03**	0.03**
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)

Note: Table presents "Donut" RD estimates of the effect of reporting time on motion and appellate outcomes using a variety of donut radii. MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014). All columns are estimated with triangular kernels using first-order local polynomials. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9—Placebo Tests: RD Estimates for Motion & Appellate Outcomes, Counterfactual Cutoffs

Cutoff Dates:	2 Mos. Left	1 Mo. Left	1 Mo. Right	2 Mos. Right
% Granted	-0.01	-0.01	-0.01*	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)
% Denied	0.01	0.01	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
% Appealed	-0.01	-0.01	0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
% Affirmed	0.01	-0.02	0.01	-0.02
	(0.02)	(0.02)	(0.02)	(0.01)

Note: Table presents placebo RD estimates of the effect of reporting time on motion and appellate outcomes using counterfactual six-month publication and cutoff dates. Publication and cutoff dates are shifted either one or two months to the left or right. MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014). All columns are estimated with local linear polynomials using triangular kernels.

Table A10—RD Estimates: Effect of six months additional reporting time on motion & appellate outcomes, clustered at Julian date of filing

	(1)	(2)	(3)	(4)
% Granted	-0.02	-0.01	-0.02	-0.02
	(0.01)	(0.01)	(0.02)	(0.02)
% Denied	0.01	0.01	0.01	0.02
	(0.01)	(0.01)	(0.02)	(0.02)
% Appealed	0.00	-0.01	0.02	-0.00
	(0.01)	(0.01)	(0.02)	(0.02)
% Affirmed	0.05^{**}	0.04^{**}	0.06^{**}	0.04^{**}
	(0.02)	(0.02)	(0.03)	(0.02)
Polynomial order	First	First	Second	Second
Controls		✓		✓

Note: Table presents bias-corrected RD estimates of the effect of six months additional reporting time on motion & appellate outcomes. MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014). Effects are estimated with first or second order local polynomials using triangular kernels. Columns indicating controls include list (i.e., March or September) fixed effects, a dummy for the part (plaintiff or defendant) filing the motion, and nature-of-suit and filing-year fixed effects. Robust standard errors clustered at the Julian date of filing are in parentheses.

Table A11—RD Estimates: Effect of six months additional reporting time on motion & appellate outcomes, clustered at judge-level

	(1)	(2)	(3)	(4)
% Granted	-0.02	-0.01	-0.03	-0.03
	(0.01)	(0.01)	(0.02)	(0.02)
% Denied	0.01	0.01	0.01	0.02
	(0.01)	(0.01)	(0.02)	(0.02)
% Appealed	0.00	-0.01	0.02	0.00
	(0.01)	(0.01)	(0.02)	(0.02)
% Affirmed	0.04**	0.04*	0.07*	0.04
	(0.02)	(0.02)	(0.04)	(0.03)
Polynomial order	First	First	Second	Second
Controls		✓		✓

Note: Table presents bias-corrected RD estimates of the effect of six months additional reporting time on motion & appellate outcomes. MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014). Effects are estimated with first or second order local polynomials using triangular kernels. Columns indicating controls include list (i.e., March or September) fixed effects, a dummy for the part (plaintiff or defendant) filing the motion, and nature-of-suit and filing-year fixed effects. Robust standard errors clustered at the judge-level are in parentheses.

Table A12—OLS Estimates: Effect of one month additional reporting time on months until motion disposition, motion disposition dates imputed from case closures

	(1)	(2)	(3)	(4)
Reporting Time (Months)	0.093***	0.106***	0.099***	0.101***
	(0.011)	(0.012)	(0.011)	(0.010)
Observations	362,117	362,117	362,114	362,114
Dep. Var. mean	✓	✓	✓	✓
Case & Motion Controls		✓	✓	✓
Calendar Trends			✓	✓
DistrictYear FEs				✓
Dep. Var. mean	5.79	5.79	5.79	5.79
Indep. Var. mean	10.05	10.05	10.05	10.05

Note: Table presents OLS estimates of the effect of additional reporting time on months until motion disposition. For motions with unknown dispositions but known case closure dates, the case closure date is used as the motion disposition date. MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014). Effects are estimated with first or second order local polynomials using triangular kernels.

Table A13—RD Estimates: Effect of six months additional reporting time on months until motion disposition, motion disposition dates imputed from case closures

	(1)	(2)	(3)	(4)
Filed After Cutoff	0.819	0.337	1.233*	0.458
	(0.535)	(0.245)	(0.670)	(0.300)
Dep. Var. mean	5.79	5.79	5.79	5.79
Polynomial order	First	First	Second	Second
Controls		✓		✓
Bandwidth (days)	24.5	21.6	30.7	35.5
Effective N	97,704	86,441	123,254	144,333
N (left)	48,012	42,137	61,157	71,016
N (right)	49,692	44,304	62,097	73,317

Note: Table presents RD estimates of the effect of additional reporting time on months until motion disposition. For motions with unknown dispositions but known case closure dates, the case closure date is used as the motion disposition date.

Supplemental Appendix: Bunching Methodology & Additional Results

Figure 1 shows clear bunching of motion dispositions at the six-month list deadlines. To quantify the aggregate effect of the six-month list on motion-processing time, I implement a notched bunching estimator adapted from Kleven and Waseem (2013) and Kleven (2016).

Let d be the number of days from motion filing to motion disposition, and let l be the number of days from the filing date to the six-month list deadline (i.e., the motion's reporting time in days). I observe data generated by the density functions $f_1^l(d)$—the actual distribution of motion resolution times given a six-month reporting time of l—for $l \in L$ and the goal is to obtain a counterfactual distribution $f_0(d)$. My setting and estimator are similar to those of Gruber, Hoe and Stoye (2023), who study the effect of a policy in the U.K. that penalized emergency room providers for failing to admit patients within four hours of their arrival. Unlike their setting, however, where doctors were subjected to a single universally-applicable wait time target, my setting involves many *different* notches corresponding to the amount of reporting time a motion receives, between approximately seven and thirteen months.

I first group motion dispositions into five-day bins indexed by j—for example, motions decided in five days or less, motions decided in six to ten days, and so on. From there, I can bin estimate separate counterfactual distributions $\hat{f}_0^l(d)$ for each possible reporting time l using a series of polynomial regressions of the form:

$$c_j^l = \sum_{i=0}^{p} \beta_i^l \cdot (d_j)^i + \sum_{i=d_-}^{d_+} \gamma_i^l \cdot \mathbb{1}[d_j = i] + \sum_{r \in R} \rho_r^l \cdot \mathbb{1}\left[\frac{d_j}{r} \in \mathbb{N}\right] + \nu_j^l, \tag{B1}$$

where c_j^l is the number of individual motions with reporting time l disposed of in bin j, d_j is the maximum duration of a motion disposed of in bin j (for example, $d_j = 5$ for motions adjudicated in 1–5, $d_j = 10$ for motions decided in 6–10 days, and so on), and p is the order of the polynomial ($p = 8$ in my baseline specification). The term $\sum_{r \in R} \rho_r \cdot \mathbb{1}\left[\frac{d_j}{r} \in \mathbb{N}\right]$ reflects the inclusion of round-number fixed effects. These are necessary to account for the fact that dispositions tend to spike on multiples of seven, 30, and 365.

The range $[d_-, d_+]$ is the "excluded window" of excess and missing mass around the notch point l. In addition to smoothness of the counterfactual distribution, the key assumption of the bunching estimator is that bunching responses are entirely confined to the excluded window. For example, I assume that judges might respond to the March 31 deadline by postponing dispositions that otherwise would have occurred in late February or early March, and they might similarly respond by expediting dispositions that otherwise would have occurred in April or May; they are unlikely, however, to either postpone a disposition they would have otherwise reached much sooner or expedite a disposition they would have otherwise reached much later.

This is a relatively strong assumption, and it is likely most defensible for motions filed on or just prior to March 1 and August 30 (i.e. those with maximum reporting time). Specifically, when judges have a full 13 months to review a motion before it becomes eligible for the six-month list, the pull of the six-month list is at its weakest. It is reasonable to assume that, during the first several months of a motion's pendency, judicial behavior will not be greatly affected by a deadline that is still many months away. Somewhat more intuitively, it is reasonable to believe that there is little difference between a distant deadline that hardly ever binds and a regime with no deadline at all.

The estimated counterfactual distribution is defined as the predicted values of equation (B1) omitting the contribution of the excluded window dummies so that $\hat{c}_j^l = \sum_{i=0}^{p} \hat{\beta}_i \cdot (d_j)^i + \sum_{r \in R} \rho_r^l \cdot \mathbb{1}\left[\frac{d_j}{r} \in \mathbb{N}\right]$. I depart from the Kleven and Waseem (2013)—which chooses the lower bound of the exclusion window d_- by visual inspection—by implementing a Quandt Likelihood Ratio (QLR) test. The QLR test is frequently used to identify structural breaks in time-series data. For my purposes, the QLR test identifies the duration bin d_- where dispositions have most strongly broken from their prior trend. The upper bound d_+ chosen recursively by starting at an initial value $d_+^0 \approx l$, estimating equation (B1), and increasing d_+ by small increments until we identify the value of d_+ that minimizes the difference between estimated excess mass $\hat{B}$ in the range $[d_-, l]$ and estimated missing mass $\hat{M}$ in the range $[l, d_+]$.

While my data make it feasible to estimate these separate reporting time-specific counterfactual distributions, I instead choose to estimate a single counterfactual distribution $\hat{f}_0(d)$ using only motions with close to the maximum possible reporting time (i.e., 390 days or greater). I make this simplification for two reasons: first, it makes sense to impose a single density function since, theoretically, the counterfactual distribution should be the same for all reporting times; and second, because far-off deadlines are similar to no deadline at all, the core bunching assumption (i.e., that the distribution of adjudications is unaffected by the six-month list outside of a local "exclusion window" around the deadline itself) is most likely to be satisfied in the maximum reporting time scenario. Figure B1 plots the distribution of motion resolution times for motions with reporting times of 390 days or greater, along with the fitted counterfactual distribution.[15] Using the approach described above, I estimate an exclusion window beginning 15 days to the left of the six-month list deadline (375 days) and ending 95 days to the right (485 days).

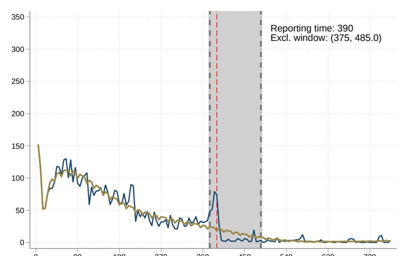


Figure B1. Actual vs. Counterfactual Dispositions, Maximum Reporting Time

Note: Figure plots the observed and counterfactual frequencies of motion disposition times (in days) for motions with maximum reporting time (at least 390 days). Counterfactual distribution is estimated using an 8th-order polynomial regression. Exclusion window is shaded in gray.

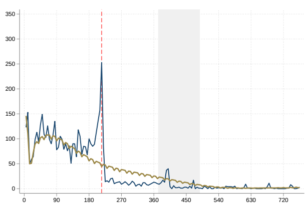
With a counterfactual distribution in hand, I can then estimate the aggregate effect of the six-month list by comparing the actual and counter-

[15]Notably, the observed density of motion dispositions appear to spike slighlty at approximately 215 days, which aligns with the six-month list deadlines for more exposed motions. This suggests that some judges may be responding to the six-month list even when they have a full 13 months to resolve a motion. This is not entirely surprising since, as discussed above, the precise six-month list inclusion criteria are somewhat opaque and may not always be known even by the judges themselves. To the extent that judges are responding to deadlines that are not actually binding on them, this would tend to bias my estimates of the effect of the six-month list downward.

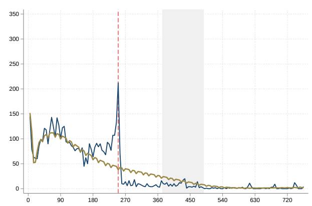
factual distributions. Specifically, I calculate the ratio $T/\hat{T}$, where $T = \sum_{j \in J} \sum_{l \in L} c_j^l \cdot d_j$ represents the total actual disposition time for all summary judgment motions in my sample, and $\hat{T} = \sum_{j \in J} \sum_{l \in L} \hat{c}_j^l \cdot d_j$ represents the estimated total counterfactual disposition time for the same sample of motions. I assume that the counterfactual for any given reporting time l is equal to the counterfactual estimated from maximum reporting time data ($l = 390$ days) scaled by the number of filings with reporting time l, so that $\hat{c}_j^l = \hat{c}_j^3 90 * \frac{\sum_j c_j^l}{\sum_j c_j^3 90}$.

Table B1 presents ratios $T/\hat{T}$ for a variety of different polynomial orders and exclusion window lower bounds. A ratio of less than one suggests that the six-month list has reduced aggregate disposition times relative to the scenario without a six- month list, and a value of greater than one would suggest that the six-month list has actually slowed aggregate dispositions. Across all specifications, I estimate that the six-month list reduces total motion disposition time by approximately 3.5–4.5%.

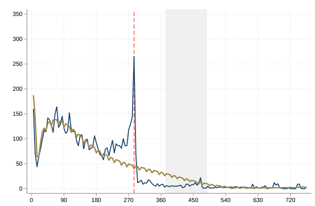
As discussed above, our setting is complicated by the fact that motions are subject to a variety of different reporting times—more than 180 in total—depending upon the day of the year on which they were filed. Thus, we might hypothesize that the extent of bunching is likely to vary with the amount of reporting time. Figure B2 confirms our intuition by plotting the actual versus counterfactual density of motion disposition times for six selected reporting time bins, ranging from the minimum (just over seven months, depicted in the northwest sector of the plot) to the maximum (just over thirteen months, depicted in the southeast sector of the plot). We can see that the bunching is prominent for all possible reporting times, but it is most prominent for motions with a relatively low amount of reporting time.



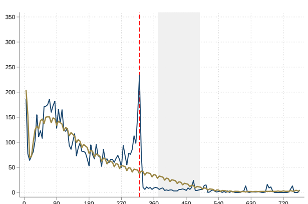
(a) 215 days R.T.



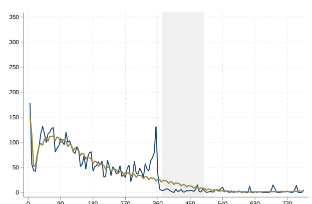
(b) 250 days R.T.



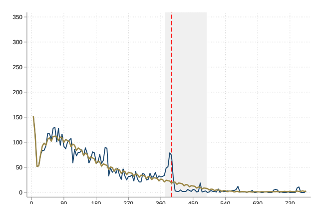
(c) 285 days R.T.



(d) 320 days R.T.



(e) 355 days R.T.

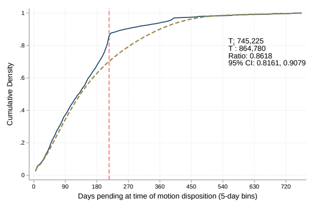


(f) 390 days R.T.

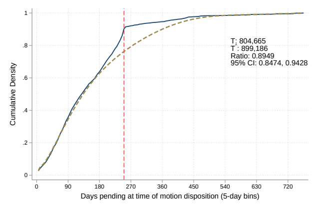
Figure B2. Counterfactual & Observed Disposition Times (Days) for Six Selected Reporting Times

Note: Figure plots observed and counterfactual disposition times (in days) for six different reporting times. Counterfactual distributions are estimated from motions with maximum reporting time ($\geq$ 390 days) using an 8th-order polynomial regression. Exclusion windows are shaded in light gray.

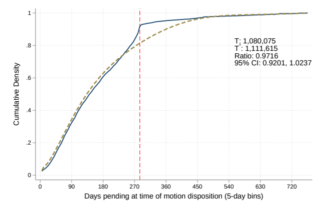
Finally, Figure B3 plots observed versus counterfactual CDFs for the same six reporting time bins. Reporting time-specific ratios $T^l/\hat{T}^l$ and confidence intervals are indicated in the northeast corner of each plot.[16]



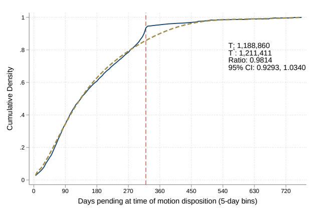
(a) 215 days R.T.



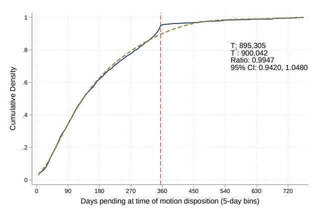
(b) 250 days R.T.



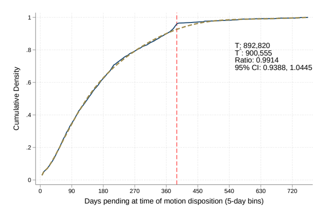
(c) 285 days R.T.



(d) 320 days R.T.



(e) 355 days R.T.



(f) 390 days R.T.

Figure B3. CDFs of Counterfactual & Observed Disposition Time (Days) for Six Selected Reporting Times

Note: Figure plots observed and counterfactual disposition time CDFs (in days) for six different reporting times. Counterfactual distributions are estimated from motions with maximum reporting time ($\geq$ 390 days) using an 8th-order polynomial regression. Exclusion windows are shaded in light gray.

[16]Standard errors and confidence intervals are obtained with a Wild bootstrap procedure.

Table B1—Robustness Checks: Bunching Estimates by Polynomial Order and Lower Bound

	Lower bound of exclusion window			
Order (p)	**360**	**370**	**375**	**380**
6	0.9605	0.9581	0.9566	0.9609
	(0.8916–1.0001)	(0.8901–0.9995)	(0.8881–0.9957)	(0.8796–1.0056)
7	0.9573	0.9546	0.9588	0.9637
	(0.9065–1.0077)	(0.9056–0.9966)	(0.9006–1.0021)	(0.9006–1.0094)
8	0.9532	0.9548	0.9584	0.9590
	(0.9003–1.0176)	(0.9072–1.0036)	(0.9076–1.0098)	(0.9032–1.0076)
9	0.9453	0.9586	0.9643	0.9603
	(0.8987–0.9982)	(0.8979–1.0093)	(0.8953–1.0026)	(0.8959–1.0041)

Note: Table presents estimates of the ratio $T/\hat{T}$ for varying polynomial orders and lower bounds of the exclusion window. Polynomial of order 8 and lower bound of 375 are the baseline. 95% confidence intervals are presented in parentheses below each estimate.