Author Pre-Print—Accepted for publication in the Journal of Public Economics, Vol. 262 (2026)

Non-Monetary Incentives and Bureaucratic Performance: Evidence from U.S. Courts

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July 2026

Abstract

Federal judges—who enjoy lifetime tenure and constitutionally protected salaries—represent an especially hard test case for incentive-based bureaucratic reform. I study the "six-month list," a reform requiring U.S. federal courts to publicly identify judges with overdue matters. Using a regression discontinuity design and other methods, I find that matters most exposed are resolved approximately 14% faster than those least exposed, with larger effects among younger, non-white, and female judges. The speed gains come with tradeoffs: upfront time savings are partially offset by downstream delays, and more-exposed matters are more likely to be reversed on appeal. A bunching analysis estimates aggregate time savings of approximately 4%, demonstrating that non-monetary levers can shift behavior even among highly insulated elite professionals.

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1 Introduction

Courts are essential institutions. Beyond safeguarding rights and liberties, they promote economic growth by enforcing contracts, protecting property, 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—leaving little room for the material incentives that feature prominently in standard models of worker effort. Yet judges, like other public-sector professionals, may still respond to social (Ashraf and Bandiera, 2018) and career incentives (Holmstrom, 1999; Dewatripont, Jewitt and Tirole, 1999; Alesina and Tabellini, 2008), institutional norms (Akerlof and Kranton, 2005), and other non-monetary incentives (Cassar and Meier, 2018). The six-month list leverages such incentives by publicly identifying judges with overdue decisions, potentially creating reputational and career pressure to resolve matters more quickly. The six-month list provides a unique opportunity to study the effect of non-monetary incentives among a relatively understudied group of elite bureaucrats, in a setting where monetary confounders are largely eliminated.

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 2013. 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. While judges perform a variety of tasks beyond deciding motions, I rely on summary judgment motions because they are more subject to judicial control—and more susceptible to judicial delay—than nearly any other aspect of judging.

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 a cost to other dimensions of judicial performance. The results suggest yes, though the picture is nuanced. Exposure to the six-month list does not significantly affect grant and denial rates, suggesting judges are not simply gaming the list by issuing more denials, which are harder to appeal and often require less work than orders granting summary judgment.[1] At the same time, motions least exposed to

[1]A granted summary judgment motion resolves the relevant issues without trial, producing a final decision that the losing party may immediately appeal. A denied motion, by contrast, merely preserves the issues for trial; because no final decision has been rendered, the losing party generally cannot appeal until after trial. Judges are also likely aware that granted motions face immediate appellate scrutiny, creating additional pressure to write carefully reasoned opinions.

the list are approximately 1.3 percentage points ($\approx$ 8.7%) less likely to be reversed on appeal, consistent with deadline pressure compromising decisional quality. List exposure also generates meaningful reductions in overall case duration, though the motion-level time savings appear to be partially offset by downstream delays, consistent with judges cutting corners and deferring substantive work to later stages of the case.

Last, I apply a notched bunching design (Kleven and Waseem, 2013; Kleven, 2016) in a novel setting to address a potential concern with the motion-level results: judges may simply be reshuffling effort across more and less exposed motions, in which case the estimated effects would reflect timing manipulation rather than genuine aggregate time savings. Comparing the observed distribution of motion resolution times to a smooth counterfactual representing a no-deadline regime, I find that the list reduces total summary judgment processing time by approximately 4%, suggesting genuine aggregate time savings rather than mere reshuffling across motions.

This paper contributes to several literatures in economics. First, it adds to a growing body of work on judicial performance and 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 (Visaria, 2009), procedural reform within existing court systems (Chemin, 2012; Kondylis and Stein, 2023), case management protocols (Chemin, 2024), and information and accountability interventions (Chemin et al., 2022). While promising, these reforms can prove difficult to scale, often requiring lawmakers to write new laws or dedicate scarce resources. This paper shifts the focus to judges' non-monetary 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; Ash, Chen and Naidu, 2026) 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; 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. The paper also adds to a literature demonstrating the effects of public disclosure and "shaming" mechanisms on a variety of individual and firm behaviors, including tax compliance (Perez-Truglia and Troiano, 2018; Dwenger and Treber, 2022), regulatory compliance (Johnson, 2020), voting (Gerber, Green and Larimer, 2008), delay in academic peer review (Chetty, Saez and Sandor, 2014), and energy conservation (Delmas and Lessem, 2014). Relative to the prior literature, I study a group of "elite" bureaucrats—U.S. federal judges—who enjoy greater professional prestige and insulation from ordinary workplace incentives than most previously studied public officials.

This paper proceeds as follows. Section 2 provides background on U.S. civil litigation and the six-month list. Section 3 describes the data and empirical strategy, and Section 4 discusses identification. Section 5 presents the main results, including effects on motion resolution speed, heterogeneous effects by judge characteristics, and evidence on tradeoffs and downstream consequences. Section 6 uses a bunching design to estimate the list's aggregate impact on motion-processing time. Section 7 concludes.

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

Understanding what motivates judges requires understanding the institutional context in which they operate. 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 written "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 semiannual report disclosing the names of federal judges with motions pending for more than six months.[2] The provision was controversial, but both supporters and detractors

[2] In addition to motions pending more than six months, the law also requires disclosure of certain civil trial decisions pending for more than six months and overall cases pending more than three years. I focus in this paper on overdue motions because they are far more frequent and more subject to judicial control than other events requiring dislosure under the law.

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."[3] To that end, the contents of the six-month list can easily be found on the federal judiciary's website,[4] and in recent years the federal judiciary has even announced its publication on social media.[5]

It is unsurprising that lawmakers chose to focus the six-month list on overdue civil motions. Federal judges have a wide range of responsibilities, but few are as common or as subject to judicial control—or as susceptible to judicial delay—as deciding dispositive motions. Trials are exceedingly rare in modern federal litigation, occurring in just two percent of federal criminal cases and less than one percent of federal civil cases (Smith and MacQueen, 2017). Other judicial tasks are either relatively pro forma (e.g. criminal arraignments) or largely driven by the parties themselves, with judges playing a relatively passive role (e.g., pretrial conferences). Criminal adjudication on the whole is an unlikely source of judicial delay: strict statutory and constitutional timing requirements designed to protect the rights of criminal defendants leave judges little discretion to either slow or expedite criminal proceedings. That leaves civil motion practice—and in particular, dispositive motions, where one party has asked the judge to dispose of part or all of the case without trial—as the key bottleneck in modern litigation. And while litigation delay is no doubt an equilibrium phenomenon involving many actors, the speed with which a judge resolves a dispositive motion is one of the few aspects of that equilibrium that is primarily within the judge's own control.

This paper focuses on one particularly consequential form of dispositive motion: the

[3]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).

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

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

motion for “summary judgment.” Compared to the rarity of trial, motions for summary judgment are common, filed in nearly 20% of federal civil cases (Cecil et al., 2007). Filed after evidence has been gathered through discovery but before trial, they ask 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.

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.[6] As a result, motions vary quasi-randomly in their exposure to the six-month list based on their filing dates. To give a concrete example: a motion filed on February 28 will be exactly 215 days old on September 30, giving the judge just 7 months to review it before it first appears on a six-month list. An otherwise identical motion filed on March 1 is saved from the September 30 list by just one day and will instead face its first potential reporting date the following March 31—giving the judge nearly 13 months to resolve it before it appears on the list. I describe the resulting identification strategy in Section 4.

In practice, the six-month list clearly matters to judges.[7] Appendix Figure A1 shows an excerpt from a recent six-month list report, illustrating how the list is organized by judge with each judge’s outstanding motions individually listed underneath. Figure 1 shows sharp spikes in motion resolutions just before the March 31 and September 30 list deadlines, followed by

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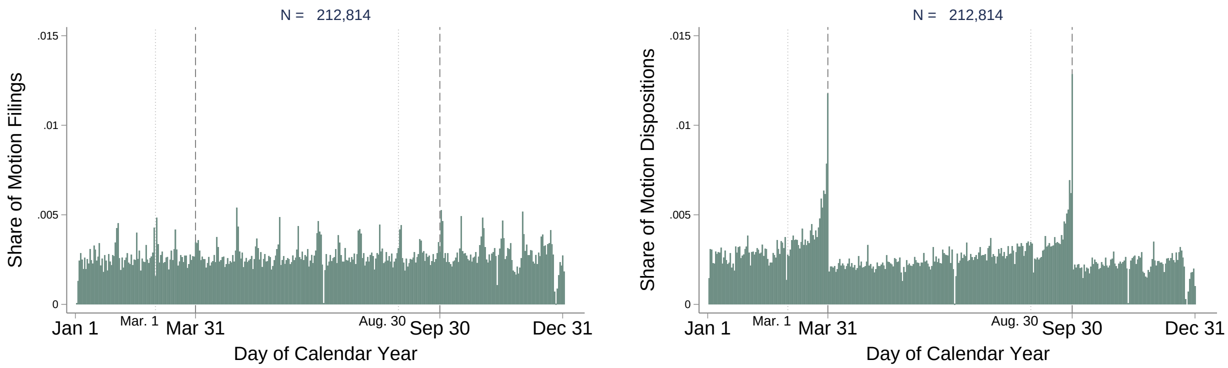
[6] Although the statute refers to matters pending “more than six months,” the judiciary has come to define “pending” as beginning 30 days after filing and interprets “six months” as 184 days, making the effective threshold 214 days.

[7] Statistics gathered from recent six-month list reports suggest that judges are generally careful to minimize their outstanding motions. In a recent report for the Northern District of California, for example, 12 of 26 district judges had zero outstanding motions and 8 others had two or fewer. Two outlier judges reported 11 and 24 outstanding motions respectively.



(a) Motions Filed

(b) Motions Resolved

Figure 1: Summary Judgment Motions Filed & Resolved by Calendar Day

Note: Panel (a) shows the share of summary judgment motions filed on each calendar day of the year, and Panel (b) shows the share resolved on each calendar day of the year. Both panels include all motions filed between January 1, 2005 and December 31, 2013 (N = 212,814). Dotted vertical lines indicate the March 1 and August 30 cutoff dates at which reporting time is at its highest; dashed vertical lines indicate the March 31 and September 30 six-month list reporting deadlines.

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), used a hand-coded sample of 758 summary judgment motions and an identification strategy that, like mine, relied in part on quasi-random variation in motions' filing dates to show that motions filed in time to be eligible for a March 31 list were resolved between 10 and 30 days faster than motions not eligible for that list. They also documented suggestive evidence of quality effects including increases in denial rates and appellate remand rates in motions and cases decided just prior to a six-month list deadline. Relative to their paper, the larger and more comprehensive dataset on which I rely allows me to deploy more rigorous methods, including a formal regression discontinuity design and bunching estimator, and to examine judges' heterogeneous responses to the six-month list—an analysis their sample size precluded.

3 Data & Empirical Framework

3.1 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 U.S. district courts between 2005 and 2013. I began with a database of digital docket sheets covering the universe of civil cases pending during this period.[8] The raw docket data were provided as structured XML files by the West Publishing Company, which sourced the information from PACER.[9] 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

[8]Although the database includes docket activity through 2014, I restrict my analysis to motions filed by the end of 2013. This helps to mitigate concerns of censoring bias by giving all motions in the sample at least one year to be resolved before the end of the data coverage period in 2014.

[9]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).

identity of the assigned judge.

I used natural language processing (NLP) methods to identify docket entries containing motions for summary judgment and, separately, to classify entries containing judicial rulings on those motions. Although formatting and phrasing can be inconsistent, docket entries for summary judgment filings and resolutions tend to follow predictable patterns, making them suitable for rules-based classification, where rules were developed iteratively through manual review. The result is a motion-level dataset with detailed information on when each motion was file and by which party, when and how it was resolved (including whether it was granted or denied), and various attributes of the larger case.

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.[10] 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.[11]

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 of motions (41.5%) were either resolved after the end of my sample period, were resolved without an explicit ruling (for example, due to settlement or voluntary dismissal of the lawsuit before the judge had an opportunity to rule on the motion itself), or were resolved in ways that eluded my classification methods, likely due to idiosyncratic phrasing in the judge's docket order or data entry errors by the court.[12]

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

[11]Available at: `https://www.fjc.gov/research/federal-court-cases-fjc-integrated-database-1979-present`. Because the IDB data are updated continuously, I am able to observe overall case dispositions and appellate activity that occur even after the end of my motion-level sample period in 2014.

[12]Data on overall case dispositions from the IDB suggest that settlements, voluntary dismissals, and other

While I cannot rule out censoring or selection bias due to missing resolutions, it is nonetheless reassuring that Table 1 shows the sample of motions with known resolutions to be relatively well-balanced with the overall sample along a variety of observable dimensions. Of motions with known resolutions, 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 all rulings were subsequently appealed.

3.2 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 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 amount of time before it becomes eligible for inclusion on a six-month list—varies with 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

case-wide resolutions comprise a majority of the motions with unobserved resolutions. Approximately 98.5% of motions in the full sample occur in cases that were fully resolved before the end of the data coverage period in 2014, suggesting that censoring is relatively rare. Of motions in the full sample with unknown resolutions, 39.9% occur in cases with a disposition code explicitly indicating settlement between the parties—likely an underestimate since parties sometimes fail to report settlements to the court. Just 23.9% occur in cases with disposition codes reflecting a judgment on the merits, which would ordinarily be expected if a motion for summary judgment had been granted or if a motion had been denied and the case had proceeded to trial.

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 (months)	10.0 (1.7)	10.1 (1.7)	8.5 (0.9)	11.5 (0.9)
Case duration at filing (months)	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%
Motion Filed by Pro Se Party	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%
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 for all summary judgment motions filed in U.S. federal district courts between January 1, 2005 and December 31, 2013. Column (1) shows the full sample, 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 by reporting time—defined as the number of months between a motion's filing date and the first six-month list deadline by which the motion must be resolved to avoid appearing on a six-month list report—into motions with low (< 10 months) and high (≥ 10 months) reporting time, respectively. Case duration at filing is the number of months between the case's filing date and the motion's filing date. Entries prior to filing is the number of docket entries in the case that preceded the motion's filing. *Pro se* movant indicates that the party moving for summary judgment was representing themselves without an attorney. *In forma pauperis* indicates that the plaintiff was granted a waiver of court filing fees. The jurisdictional basis indicates whether the case was brought under federal law (federal question), between parties from different states (diversity), or involved a federal government party. Standard deviations are in parentheses.

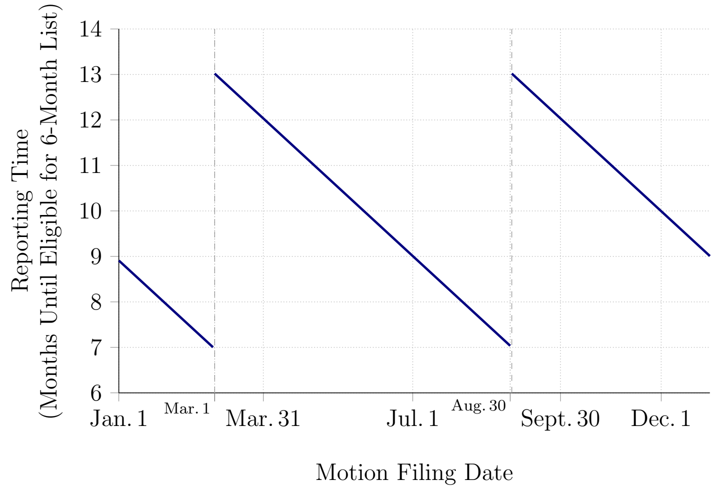


Figure 2: "Reporting Time" as a Function of Motion Filing Date

Note: Figure plots reporting time (in months) as a function of the calendar day on which a motion for summary judgment was filed. Reporting time is defined as the number of months between a motion's filing date and the first six-month list deadline by which the motion must be resolved to avoid appearing on a six-month list report.

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:

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

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, $\text{ReportingTime}_{ijt}$ 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:

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

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 design identifies a more general parameter and is likely to yield more precise estimates—drawing on variation across the full reporting-time distribution—though it relies on the stronger assumption that filing dates are independent of potential outcomes, conditional on observables. The RD design identifies a more local parameter and is likely to yield less precise estimates, but identification relies on a weaker continuity assumption—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.

4 Identification

4.1 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,[13] discovery timing, opposing party availability, and attorney workload—rather than by the merits of the motion or the incentives of the judge or litigants.[14]

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

[13] 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).

[14] Interviews with practitioners suggest that most attorneys are unaware of the six-month list and its deadlines, making strategic filing-date manipulation by litigants unlikely. Judges are familiar with the list but may not know the precise relationship between filing dates and reporting times. Judges typically rely on automatically generated reports to identify motions that are currently reportable, but the ex ante inclusion criteria are not straightforward. Published guidance suggests that "motions pending more than six months" are eligible for reporting, but key definitions are not specific enough to infer precise cutoff dates. Correspondence with officials from the Administrative Office of the U.S. Courts confirmed that the AO's actual policy is to include motions that remain unresolved after 215 days—a rule that is absent from the published guidance.

motions (Table 1), and regressions of pre-treatment characteristics on reporting time reveal little evidence of imbalance across a variety of covariates and specifications (Appendix Table A1). 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.[15]

4.2 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 density test is informative here since motion filings need not be uniformly dense

[15] Importantly, my identification does not depend on random assignment of cases to judges. Judge, district, and nature-of-suit fixed effects are included to account for the possibility that reporting times are distributed unevenly across judges, districts, or case types that also vary in baseline motion-level outcomes. A separate potential concern is that within-judge spillovers across motions could violate SUTVA and bias both OLS and RD estimates. Several features of the analysis provide reassurance. First, the results are stable across a wide range of OLS and RD specifications and robustness checks, including donut RD specifications with varying exclusion widths (Appendix Table A7), suggesting the findings do not hinge on any particular set of treatment-control comparisons. Second, the motion-level results are broadly consistent with the bunching estimates in Section 6, which do not rely on cross-motion variation in reporting time and are therefore unaffected by within-judge spillovers. Third, Appendix Table A3 presents between-judge and within-judge-between-year specifications alongside a Mundlak decomposition (Mundlak, 1978); the within-judge estimate is nearly identical to the baseline, and the between-judge estimate is if anything larger, suggesting spillovers are not upward-biasing the main results.

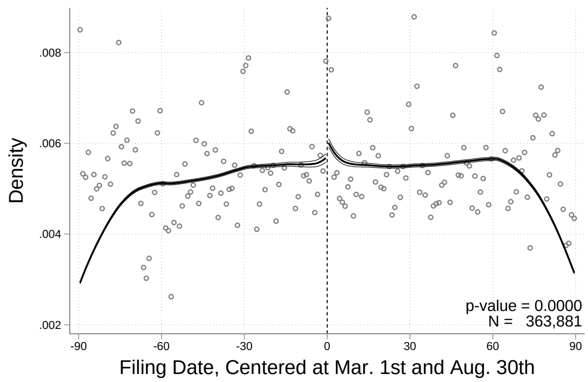
across time.

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.

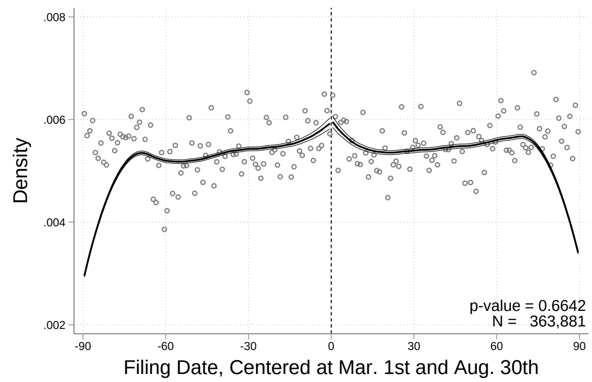
Panel (a) shows a small but statistically significant discontinuity in the raw filing density at the cutoff dates, but this disappears after adjusting for day-of-week, day-of-month, holiday, and end-of-month effects (Panel b). These results generally hold when restricting to motions with known dispositions or when estimating separately around the March 1 and August 30 cutoffs (Appendix Figure A4). 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 A2. 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,[16] 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, Appendix Table A2 also reveals 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 that younger and female judges respond

[16]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 motion filing dates relative to the six-month list reporting cutoffs of March 1 and August 30. Sample includes all summary judgment motions filed from 2005–2013, including motions with unknown dispositions. P-values from McCrary density tests (McCrary, 2008) and an observation count for the relevant sample of motion filings are displayed in the lower right of each panel. Panel (a) shows the raw filing date density; panel (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.

more strongly to list exposure (a pattern for which I find evidence in Section 5.2 below), these discontinuities could suggest that certain judges are manipulating their court-imposed filing deadlines to reduce their exposure to the six-month list.

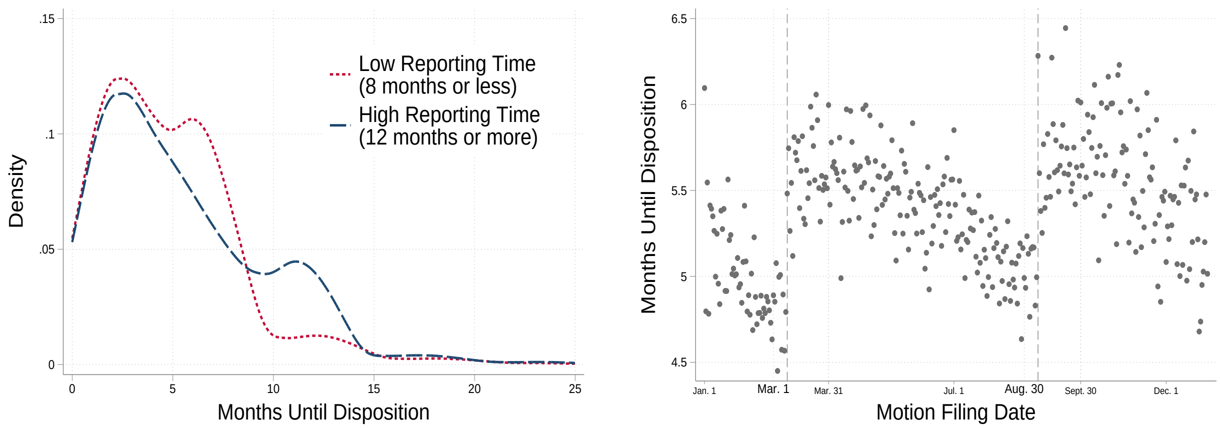
While I find no strong evidence of sorting around the six-month list cutoffs, I nonetheless take steps to address potential concerns. First, I supplement my main RD models with a "donut" design that excludes observations near the cutoff (Barreca, Lindo and Waddell, 2016). Second, I conduct placebo tests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

5 Main Results

5.1 Effect on Speed of Motion Resolution

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 ($\geq$ 12 months) versus low ($<$ 8 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).[17]



(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: Panel (a) plots kernel density estimates of months until motion resolution separately for motions with high (12 months or more) and low (8 months or less) reporting time. To improve readability, motions resolved in more than 25 months are excluded (resulting in a sample of 211,389 motions, or more than 99% of all motions with known dispositions). Panel (b) plots mean months until motion resolution against filing date. Each point represents the mean resolution time for all motions filed on that calendar day from 2005–2013 (out of 212,814 motions with known dispositions). Dashed vertical lines in panel (b) indicate the March 1 and August 30 cutoff dates at which reporting time is at its highest.

[17]Appendix Figure A2, which reproduces Figure 4b using percentiles of months until motion disposition instead of the mean, shows that this pattern is concentrated in the upper tail of the distribution: while the 10th and 25th percentiles of resolution time show little variation with filing date, the 75th and 90th percentiles track reporting time closely, including visible discontinuities at the list cutoff dates.

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 0.14 months (or about four days). Exposure to the six-month list, in other words, speeds resolution. Appendix Figure A3 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)	0.142***	0.142***	0.142***	0.142***
	(0.006)	(0.006)	(0.006)	(0.006)
Observations	211,908	211,908	211,902	211,902
Case & Motion Controls	✓	✓	✓	✓
Calendar Trends		✓	✓	✓
District×Year FEs			✓	✓
Day-of-Month FEs				✓
Dep. Var. mean	5.4	5.4	5.4	5.4
Indep. Var. mean	10.05	10.05	10.05	10.05

Note: This table presents OLS estimates of the effect of one additional month of reporting time on months until motion disposition. Reporting time is defined as the number of months between a motion's filing date and the first six-month list deadline by which the motion must be resolved to avoid appearing on a six-month list report. All columns include case- and motion-level controls: a dummy for the party (plaintiff or defendant) filing the motion, a dummy for whether a motion was the only summary judgment motion filed in its case, and fixed effects for nature-of-suit category, judge, district, and motion filing year. Columns (2)–(4) include filing date calendar trends, including linear day-of-month, day-of-quarter, and day-of-year ("Julian" date) trends corresponding to the motion's filing date. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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

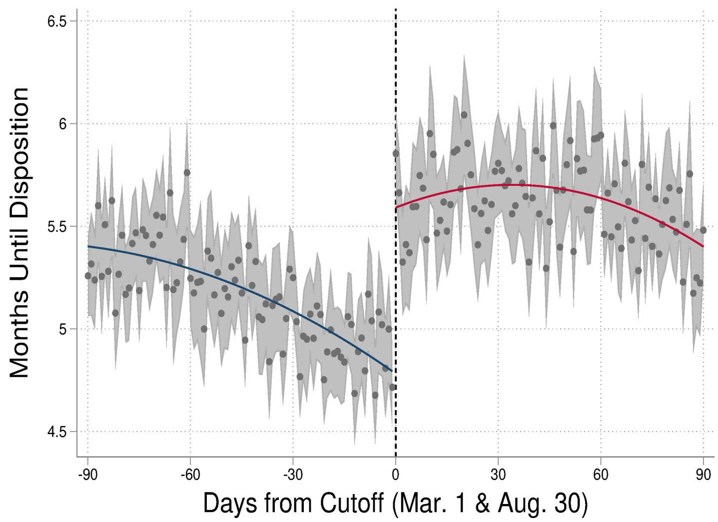


Figure 5: RD Plot: Months until motion disposition by filing day

Note: Figure plots mean months until motion disposition against the motion's filing date relative to the nearest six-month list cutoff (March 1 or August 30), pooling both cutoffs. Each point represents the mean disposition time for motions filed within a one-day bin. Fitted lines with 95% confidence intervals are estimated separately on each side of the cutoff using second-order global polynomials. Sample includes all summary judgment motions with known dispositions filed from 2005–2013.

of reporting time on resolution speed. I follow Calonico, Cattaneo and Titiunik (2014) in selecting MSE-optimal bandwidths and 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 0.85-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 A4) and alternative polynomial specifications (Appendix Table A6). To account for the possibility that error terms are serially correlated (Hausman and Rapson, 2018), which would almost certainly occur only within an individual

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)	33.6	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 reporting time on months until motion disposition, pooling both six-month list cutoffs (March 1 and August 30). 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 (March vs. September) fixed effects, a dummy for the filing party (plaintiff vs. defendant), a dummy for whether a motion was the only summary judgment motion filed in its case, and nature-of-suit and filing-year fixed effects. Effective N reports the number of observations within the optimal bandwidth; N (left) and N (right) decompose this into observations filed before and after the cutoff, respectively. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

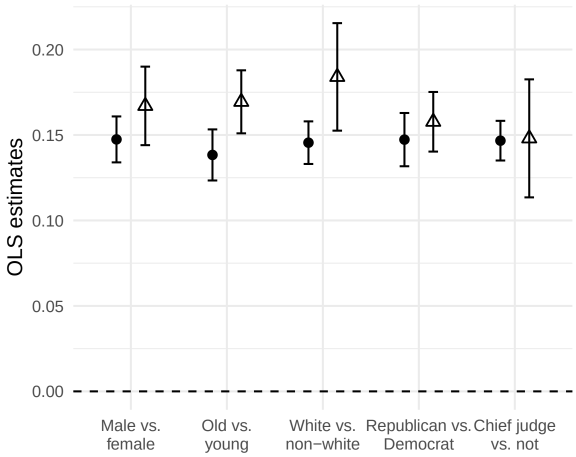
judge's docket, I show that results are robust to clustering the standard errors at the judge level and even at the district level (Appendix Table A5). Results also hold under a donut RD design that excludes motions near the cutoff (Appendix Table A7). Finally, placebo tests using counterfactual cutoff dates detect no spurious discontinuities (Appendix Table A8; Appendix Figure A5), 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 possible censoring or selection 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 A14). The RD estimates lose statistical significance, likely due to increased measurement error, but remain directionally consistent with the main findings (Appendix Table A15)

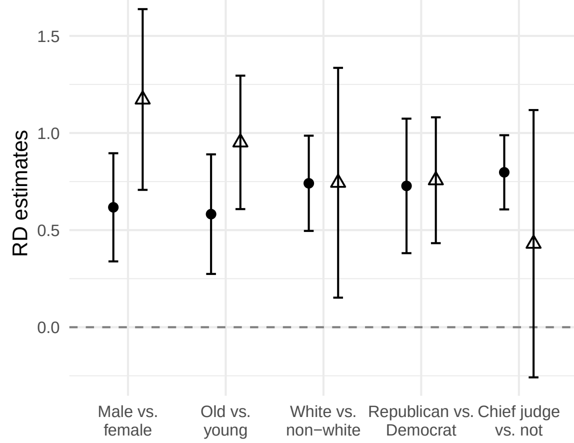
5.2 Heterogeneous Effects by Judge Characteristics

Having established the six-month list's baseline effect on the speed with which judges resolve motions, I turn next to examining whether judges respond differently to the list depending on their demographic and 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) and (b) present group-specific estimates; panels (c) and (d) plots differences between subgroups with 95% confidence intervals.

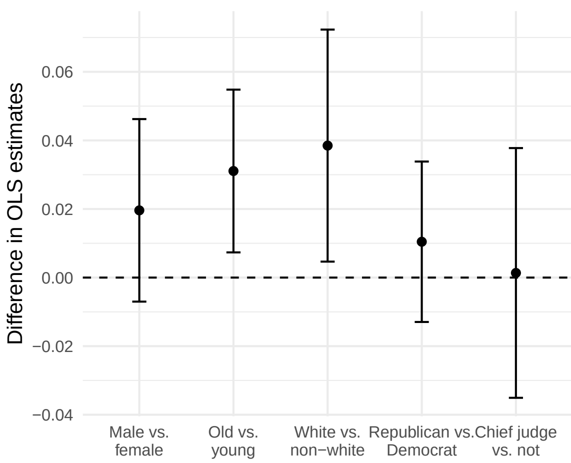
These results should be interpreted with caution: judge characteristics are not randomly assigned, subgroup sizes are small, and many traits—such as age, gender, and race—are correlated. Still, suggestive patterns emerge. In the OLS estimates, younger, female, and non-white judges appear more responsive 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.



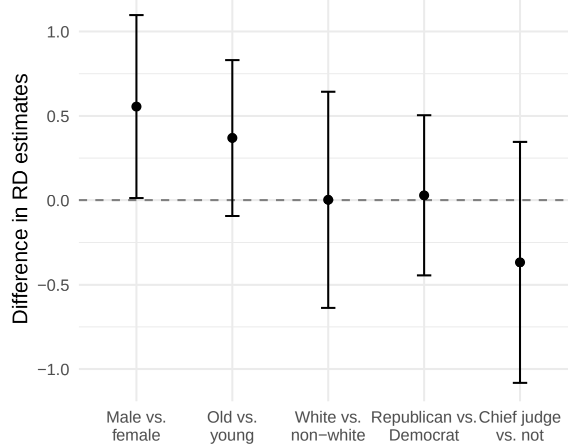
(a) OLS Estimates by Judge Subgroup



(b) RD Estimates by Judge Subgroup



(c) Differences in OLS Subgroup Estimates



(d) Differences in RD Subgroup Estimates

Figure 6: Heterogeneous Effects by Judge Subgroups

Note: Figure presents OLS and RD estimates of the effect of reporting time on months until motion disposition by judge subgroup. Panels (a) and (b) plot point estimates with 95% confidence intervals for each judge subgroup, with OLS estimates in panel (a) and RD estimates in panel (b). In Panels (a) and (b), estimates for male judges, judges 60 and over, white judges, judges appointed by Republican presidents, and non-Chief judges are indicated with closed circles; estimates for female judges, judges under 60, non-white judges, judges appointed by Democratic presidents, and Chief judges are indicated with open triangles. Panels (c) and (d) plot the difference in estimates between the two subgroups indicated on the x-axis, with OLS differences in panel (c) and RD differences in panel (d). RD estimates and subgroup differences are obtained following Calonico et al. (2025).

While not conclusive, the subgroup patterns are consistent with a theory that the six-month list operates partly through reputational and career incentives. We might expect such incentives to be more salient for judges who have the most to gain from establishing a strong professional record—either because they have more working years ahead of them, or because they belong to groups that have historically faced higher scrutiny and lower baseline presumptions of competence—and for whom promotion is a realistic possibility—presidents typically prefer to elevate young judges, and President Obama made a concerted effort to diversify the federal judiciary by appointing or elevating women and people of color.

5.3 Tradeoffs and Downstream Consequences

The results in Sections 5.1 and 5.2 establish that the six-month list substantially accelerates motion resolution, and that this effect is concentrated among judges who plausibly face the strongest reputational and career incentives. But speed is not the only dimension of judicial performance that matters, nor is time until *motion* resolution necessarily the best metric for overall litigation delay. In this section, I examine whether the motion-level speed gains come at a cost—either in the form of downstream delays that offset case-level time savings, or in the form of compromised decisional quality. The evidence suggests the answer is yes on both counts, though the effects are modest and should be weighed against the substantial motion-level speed gains documented above.

5.3.1 Effect on Overall Case Duration

The proximate goal of the six-month list was to accelerate the resolution of individual motions, but Congress's broader objective was to reduce overall cost and delay in civil litigation. Did the list accomplish that broader goal? To answer this question, I examine how motion-level exposure to the six-month list affects overall case duration—the total elapsed time from case filing to final disposition.[18]

If the list causes judges to resolve motions more efficiently, we would expect motion-level time savings to translate roughly one-for-one into case-level time savings, since the motion must be resolved before the overall case can be resolved. If instead judges are responding to deadline pressure by cutting corners—resolving motions quickly but deferring substantive work to later stages of the case—we would expect the case-level time savings to be smaller than the motion-level savings, or possibly absent altogether, as downstream delays partially offset the motion-level gains.

Table 4 reports OLS estimates of the effect of motion reporting time on total case duration. I rely here on OLS estimates of the total case duration effect because corresponding RD estimates are largely too noisy to be statistically informative. Noise in the RD results is not surprising, both because of the data-intensive nature of RD designs and because—unlike motion resolution—overall case duration is influenced by many factors beyond the judge's control. Nonetheless, Appendix Figure A6 presents RD plots for the same case duration outcomes and shows results that are at least directionally consistent with the OLS results shown here. Column (1) of Panel A shows that, on average, exposure to the six-month list generates a small but genuine improvement in overall case duration: overall cases last nearly a tenth of a month (0.4%) longer for each additional month of motion reporting time.

However, this average effect masks important heterogeneity by motion outcome. Columns (2) and (3) divide the sample into motions that were granted and denied, respectively. Importantly, this conditioning on a post-treatment variable introduces potential collider bias

[18] Faster summary judgment resolutions may be inherently valuable to litigants regardless of their effect on overall case duration, since the disposition of a summary judgment motion itself resolves meaningful uncertainty about whether contested issues will proceed to trial. The case-duration analysis nonetheless speaks directly to Congress's broader goal of reducing overall litigation cost and delay.

and the corresponding estimates should not be interpreted as causally identified, but to the extent they show differences cross cases with granted and denied motions, they may be suggestive of the mechanisms at play. Column (2) shows that improvements in case duration are concentrated among cases where the summary judgment motion was granted: each additional month of reporting time is associated with a nearly 0.17-month (0.8%) increase in total case duration for granted motions, consistent with the list generating meaningful case-level time savings in these cases. This makes intuitive sense—we have already documented that exposure to the six-month list expedites motion resolutions, and a granted summary judgment often resolves the case entirely, so motion-level speed gains translate directly into case-level speed gains.

Column (3) tells a strikingly different story for denied motions: the effect on total case duration is small and statistically indistinguishable from zero. Importantly, Appendix Table A9 shows that motion-level time savings are similar for granted and denied motions, so the difference in case-level effects is not driven by differences in motion-level speed gains. Thus, when motions are denied, it appears that some of the motion-level time savings documented in Section 5.1 are partially offset by delays elsewhere in the case.

Panel B of Table 4 examines remaining case duration—the time elapsed from motion disposition to final case resolution—which provides an even more direct test of whether judges are deferring work to later case stages. For granted motions, there is no detectable effect on remaining case duration, consistent with a granted summary judgment often ending the case with little remaining work. For denied motions, however, each additional month of reporting time is associated with a statistically significant *reduction* of 0.05 months in remaining case duration—confirming that, among cases where the motion for summary judgment was denied, greater exposure to the six-month list actually *prolongs* subsequent case resolution. This

Table 4: Effect of one month additional motion reporting time on total & remaining case duration

Panel A: Total Case Duration			
	(1) Full Sample	(2) Granted	(3) Denied
Reporting Time (Months)	0.099***	0.169***	0.044
	(0.019)	(0.022)	(0.028)
Observations	211,500	103,671	98,715
Mean Dep. Var.	22.3	20.48	24.34
Mean Reporting Time	10.05	10.06	10.05
Panel B: Remaining Case Duration			
Reporting Time (Months)	-0.024*	0.016	-0.050**
	(0.013)	(0.016)	(0.021)
Observations	211,500	103,671	98,715
Mean Dep. Var.	22.3	20.48	24.34
Mean Reporting Time	10.05	10.06	10.05
Case & Motion Controls	✓	✓	✓
Calendar Trends	✓	✓	✓
District×Year FEs	✓	✓	✓
Day-of-Month FEs	✓	✓	✓

Note: Table presents OLS estimates of the effect of one additional month of reporting time on total case duration (Panel A) and remaining case duration after summary judgment motion disposition (Panel B), both measured in months. Column (1) presents estimates for the full sample of summary judgment motions; columns (2) and (3) restrict to subsamples of granted and denied motions, respectively. Because they condition on post-treatment outcomes, columns (2) and (3) should be interpreted as suggestive evidence of heterogeneous effects rather than causally identified estimates. All columns include case- and motion-level controls (a dummy for the filing party, a dummy for whether a motion was the only summary judgment motion filed in its case, and nature-of-suit category and judge fixed effects), calendar trends, district×year fixed effects, and day-of-month fixed effects. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

is precisely the pattern one would expect if judges are cutting corners on denied motions under deadline pressure: the motion is resolved quickly, but the work deferred to later stages extends the remaining case duration, at least partially offsetting time saved on the summary judgment motion itself.[19]

5.3.2 Motion and Appellate Outcomes

If judges are indeed cutting corners under deadline pressure, we might also expect to see evidence of compromised decisional quality in how motions are decided and what happens on appeal. In this subsection I examine these outcomes directly.

Decisional quality is inherently difficult to measure: I do not observe the "correct" outcome for any motion—i.e., whether it should have been granted, granted in part, or denied in whole—nor do I observe the quality of a judge's written opinion. Nonetheless, to the extent that I find systematic differences in grant or denial rates between more and less exposed motions, I can infer that the six-month list affected not just *when* judges decide motions but *how* they decide them. Appellate outcomes are more directly informative in this regard: an affirmance is effectively an independent endorsement of the district judge's decision, while a reversal reflects a finding of error, and if deadline pressure causes rushed or poorly reasoned decisions, we might expect those decisions to be more likely reversed on appeal.

Table 5 reports OLS estimates of the effect of reporting time on motion and appellate outcomes. One margin of response worth examining is whether judges respond to deadline pressure by artificially denying difficult motions—since denials require less formal justification and are harder to appeal, a judge facing an imminent reporting deadline might be tempted to deny rather than do the harder work of granting summary judgment. If this were occurring,

[19] Appendix Figure A7 presents scatterplots of case duration outcomes against motion filing date, disaggregated by motion outcome, and shows that the patterns described above are visually apparent in the raw data.

we would expect motions with more reporting time (i.e., those that are less exposed to the list) to be denied at lower rates and granted at higher rates. The OLS results are directionally consistent with this pattern, but the effects are small and marginally significant at best. The estimated effects on appeal rates and reversal-on-appeal rates are also small and statistically insignificant.[20]

Table 5: OLS Estimates:
Effect of one month additional reporting time on motion and appellate outcomes

	(1) % Granted	(2) % Denied	(3) % Appealed	(4) % Reversed
Months until Report	0.0013**	-0.0003	0.0006	-0.0007
	[0.0006]	[0.0006]	[0.0006]	[0.0005]
Observations	211,902	211,902	211,902	211,902
Case & Motion Controls	✓	✓	✓	✓
Calendar Trends	✓	✓	✓	✓
District×Year FEs	✓	✓	✓	✓
Day-of-Month FEs	✓	✓	✓	✓
Dep. Var. mean	.49	.34	.26	.15
Indep. Var. mean	10.05	10.05	10.05	10.05

Note: Table presents OLS estimates of the effect of one additional month of reporting time on motion and appellate outcomes. Reporting time is defined as the number of months between a motion's filing date and the first six-month list deadline by which the motion must be resolved to avoid appearing on a six-month list report. Motions are coded as reversed on appeal if the district court's order is appealed and reversed (or otherwise not affirmed). All columns include case- and motion-level controls (a dummy for the filing party, a dummy for whether a motion was the only summary judgment motion filed in its case, and nature-of-suit category and judge fixed effects), calendar trends, district×year fixed effects, and day-of-month fixed effects. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6 reports RD estimates for the same outcomes, with corresponding RD plots in Appendix Figure A8. Grant rates, denial rates, and appeal rates all show small and statistically insignificant discontinuities at the cutoff, and the signs on grant and denial rates are if anything opposite what would be implied by a strategic denial story. The estimated effect on reversal-on-appeal is somewhat more conclusive: motions filed just after the RD

[20]Motions are coded as reversed on appeal if the district court's order is appealed and reversed (or otherwise not affirmed) and coded as not reversed on appeal if the order is either not appealed or appealed and affirmed.

cutoff, when they are least exposed to the six-month list, are approximately 1.3 percentage points (8.7%) less likely to be reversed on appeal than motions filed just before the cutoff—a result that is statistically significant at the 5% level and robust to alternative polynomial orders, bandwidths, donut specifications, and clustering schemes (Appendix Tables A10, A11, A13). Although the RD estimate for reversal-on-appeal is larger and more precise than the OLS estimate in Table 5, the two estimates are nonetheless directionally consistent and statistically compatible, with substantially overlapping confidence intervals when the OLS coefficient is scaled by six to match the difference in reporting time across the RD cutoff. Taken together, these results suggest that judges are not systematically "gaming" the list by denying more motions, but that deadline pressure may nonetheless compromise the thoroughness or overall quality of their decisions—an interpretation that aligns with the post-motion-resolution delays documented in Section 5.3.1.

Altogether, the results in this section paint a nuanced picture of the six-month list's effects. The list generates genuine speed gains at the motion level, and these speed gains translate into meaningful case-level time savings on average. At the same time, the evidence on remaining case duration—especially the finding that exposure to the list is associated with longer post-motion-resolution delays in cases where the motion was denied—suggests that judges may be cutting corners at the summary judgment phase, ultimately trading short-term speed gains for downstream delays. Appellate outcomes point in the same direction: exposed motions are somewhat more likely to be reversed on appeal, consistent with deadline pressure compromising decisional quality at the margin. Whether the net effect of the list is beneficial depends on how one weighs these costs against the speed gains—a question that ultimately requires a social welfare function and lies beyond the scope of this paper. What the evidence does establish is that the six-month list involves real tradeoffs, and that policymakers

Table 6: RD Estimates:
Effect of six months additional reporting time on motion and appellate outcomes

	(1) % Granted	(2) % Denied	(3) % Appealed	(4) % Reversed
Filed After Cutoff	-0.015	0.014	-0.006	-0.013**
	(0.011)	(0.013)	(0.012)	(0.006)
Dep. Var. mean	.49	.34	.26	.15
Polynomial order	First	First	First	First
Controls	✓	✓	✓	✓
Bandwidth (days)	32.7	26.4	30.1	36.1
Effective N	78,120	60,836	71,859	86,650
N (left)	38,144	29,455	35,382	42,257
N (right)	39,976	31,381	36,477	44,393

Note: Table presents bias-corrected RD estimates of the effect of reporting time on motion and appellate outcomes, pooling both six-month list cutoffs (March 1 and August 30). MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014). Motions are coded as reversed on appeal if the district court's order is appealed and reversed (or otherwise not affirmed). Effects are estimated with first-order local polynomials using triangular kernels. All columns include list (March vs. September) fixed effects, a dummy for the filing party (plaintiff vs. defendant), a dummy for whether a motion was the only summary judgment motion filed in its case, and nature-of-suit and filing-year fixed effects. Effective N reports the number of observations within the optimal bandwidth; N (left) and N (right) decompose this into observations filed before and after the cutoff, respectively. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

evaluating transparency-based accountability reforms should expect such tradeoffs to be a feature rather than an anomaly.

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

By comparing outcomes across different levels of exposure to the six-month list, Section 5 showed that the list accelerates both motion resolution and, to a lesser extent, overall case resolution. One might worry, however, that these motion-level effects could reflect mere reshuffling—judges prioritizing exposed motions at the expense of less-exposed ones—rather than genuine aggregate time savings. The bunching analysis in this section addresses this concern directly: by comparing the observed distribution of motion resolution times to a smooth counterfactual representing a regime with no reporting deadlines, I find that the six-month list reduces total motion-processing time by approximately 4%, ruling out pure reshuffling within the motion sample. One important caveat is that this analysis cannot speak to reshuffling across fundamentally different task types— I cannot rule out that judges prioritize exposed motions at the expense of other judicial duties—but within the universe of summary judgment motions, the aggregate time savings appear to be genuine.

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). 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.[21]

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 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 specification. The final

[21] 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.

term accounts for round-number heaping (e.g., at multiples of 30 days).

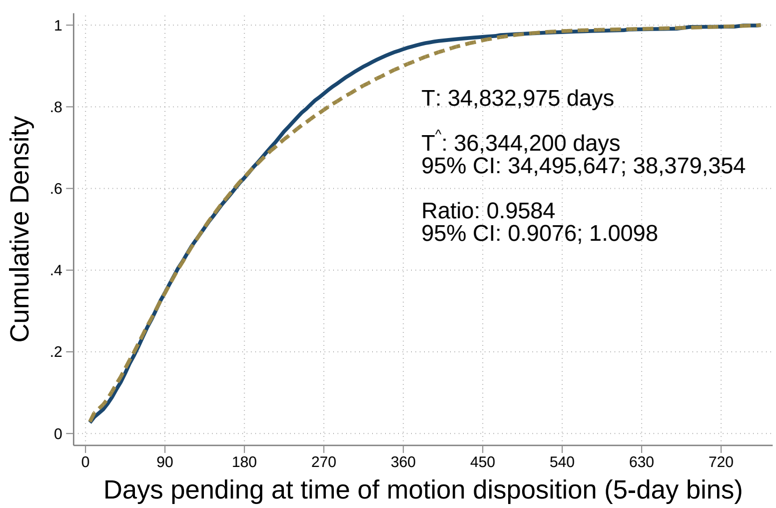
The key assumption of the bunching estimator is that bunching responses are entirely confined to an "excluded window" (i.e., the range $[d_-, d_+]$) around the notch. Intuitively, I assume that judges might respond to a reporting deadline by expediting dispositions that otherwise would have occurred shortly after the deadline, or by postponing dispositions that otherwise would have occurred shortly before it; they are unlikely, however, to either expedite a disposition they would have otherwise reached much later or postpone one they would have otherwise reached much sooner. As discussed above, I estimate the counterfactual distribution using only motions that enjoyed maximum reporting time ($l \geq 390$). Since that far-off deadline is most similar to no deadline at all, it is for this group of motions that the excluded-window assumption is most defensible.

In fact, Appendix Figure B11, which shows the empirical distribution of actual motion resolution times for six selected reporting time bins, and Appendix Figure B12, which shows corresponding cumulative distribution functions for the same reporting time bins, both provide visual support for this assumption. It appears from both figures that empirical resolution rates look nearly identical across reporting time bins during the first several months after motions are filed. While not definitive, this visual evidence suggests that differences across reporting time groups are concentrated in narrow windows around the six-month list deadline, with resolutions spiking in the weeks just before the deadline and dropping off quickly thereafter. This interpretation is further supported by Appendix Figure A2, which shows that reporting time is highly correlated with the 75th and 90th percentiles of motion resolution time but not with the 10th or 25th percentiles. This too suggests that the six-month list primarily causes judges to expedite motions that would have otherwise lingered near or past the deadline with little discernible effect on motions that would have been resolved quickly regardless.

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 excluded-window bounds (Appendix Table B16).

Figure 7: Actual vs. Counterfactual Resolution Times



Note: Figure plots cumulative distribution functions of actual and counterfactual motion resolution times for all summary judgment motions in the sample filed from 2005–2013. The counterfactual distribution is estimated from motions with maximum reporting time ($l \geq 390$ days) using an eighth-order polynomial, following the notched bunching estimator of Kleven and Waseem (2013). A ratio $T/\hat{T} < 1$ indicates that the six-month list reduces total motion-processing time relative to the no-list counterfactual. Actual total resolution time (T), estimated counterfactual resolution time ($\hat{T}$), ratio $T/\hat{T}$, and 95% confidence intervals are indicated in the northeast corner of the 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.

7 Conclusion

This paper studies whether non-monetary incentives can improve bureaucratic performance among an unusually hard-to-motivate group: U.S. federal judges, who enjoy fixed salaries and near-absolute job security. I study the "six-month list," a reform requiring federal courts to publicly identify judges with civil motions pending longer than six months. Leveraging quasi-random variation in list exposure based on motion filing dates, I find that judges significantly accelerate the resolution of summary judgment motions under deadline pressure. Motions most exposed to the list are resolved approximately 14% faster than those least exposed, and aggregate motion-processing time is reduced by about 4% relative to a no-list counterfactual.

These effects are not uniform. Younger, female, and non-white judges respond more strongly than their older, male, and white counterparts, consistent with the list operating partly through reputational and career incentives that are more salient for judges who have more working years ahead of them or who face greater professional scrutiny.

The speed gains come with tradeoffs. Motion grant, denial, and overall appeal rates are largely unaffected, but orders on motions most exposed to the list are almost 9% more likely to be reversed than orders on motions least exposed, consistent with deadline pressure compromising decisional quality at the margin. And while list exposure meaningfully reduces overall case duration on average, the motion-level time savings appear to be partially offset by downstream delays—particularly for cases where the motion was denied—consistent with judges cutting corners and deferring substantive work to later stages of the case.

Taken together, these results speak to the power and limits of transparency-based accountability as a tool for improving bureaucratic performance. Non-monetary levers can shift behavior even among highly insulated elite professionals—but not without tradeoffs. Policymakers evaluating such reforms should expect speed gains to be real but partial, and should remain attentive to the possibility that deadline pressure distorts effort allocation in ways that are difficult to monitor.

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Appendix Contents

A Supplemental Appendix: Robustness Checks and Additional Results

Figure A1: Excercpt of September 30, 2014 Six-Month List

CJRA Table 8—Report Of Motions Pending Over Six Months
For Period Ending September 30, 2014

3RD Circuit
U.S. District Court for PENNSYLVANIA MIDDLE
District Judge KOSIK, EDWIN M.

Office	Docket Number	NOS Code	Case Title	Motion Text	CJRA Deadline	Status	Status Description
3	11-cv-01368	550	Dockery v. Wentzel et al	MOTION for Summary Judgment	09/28/2014	B	Opinion/Decision in Draft
3	12-cv-01653	350	Barnett et al v. United States Department of Interior,	MOTION to Dismiss or in the Alternative, for Su	06/10/2014	B	Opinion/Decision in Draft

Total Cases for District Judge : KOSIK, EDWIN M. **2**

Note: Figure shows an excerpt of the September 30, 2014 Six-Month List. The list is organized by judge, with each judge's outstanding motions individually listed underneath and the judge's total number of outstanding motions at the bottom. Judges with no outstanding motions are included on the list with a total of zero.

Table A1: Balance Table: OLS Estimates for Pre-Treatment Characteristics.

Dependent Var.	(1)	(2)	(3)	(4)
% Filed by Defendant	0.00	0.00	0.00*	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% Class Action	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Docket Entries Prior to Motion	-0.73	-0.79	-0.85	-0.77
	(0.81)	(0.85)	(0.84)	(0.84)
Days Prior to Motion Filing	0.18	-0.40	-0.18	-0.64
	(0.71)	(0.70)	(0.62)	(0.48)
% Civil Rights Cases	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% Employment Cases	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% Prisoner Rights Cases	-0.00	-0.00*	-0.00**	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% Intellectual Property Cases	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% Labor Cases	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% Real Property Cases	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% Soc. Sec. Appeals	-0.00	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% Tort Cases	0.00	0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% Contract Cases	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)

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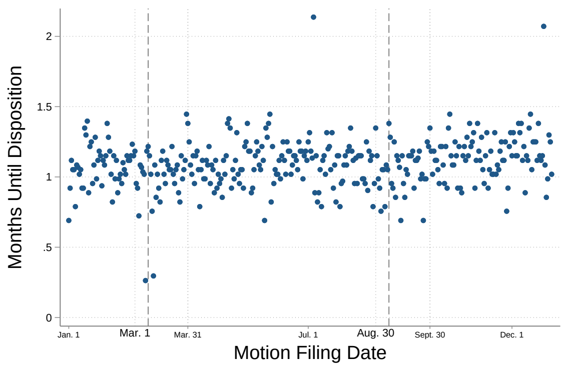
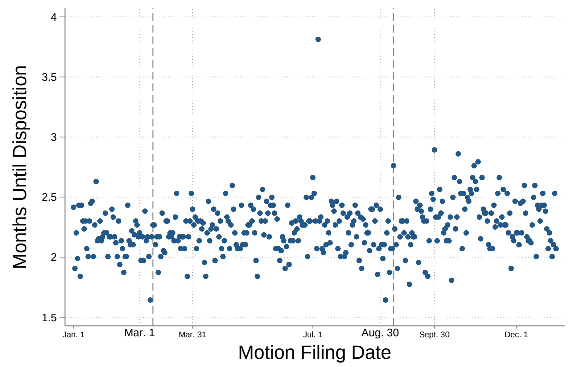
Table A1 – ...continued from previous page

Dependent Var.	(1)	(2)	(3)	(4)
% Motion Filed by Pro Se Party	-0.00	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% In Forma Pauperis	0.00	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% Diversity Jurisdiction	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% Federal Question Jurisdiction	0.00	0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% Government Defendant	-0.00	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Assigned Judge Age (Years)	-0.01	-0.02*	-0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
% Assigned Female Judge	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% Assigned Non-White Judge	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
% Assigned Magistrate Judge	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)

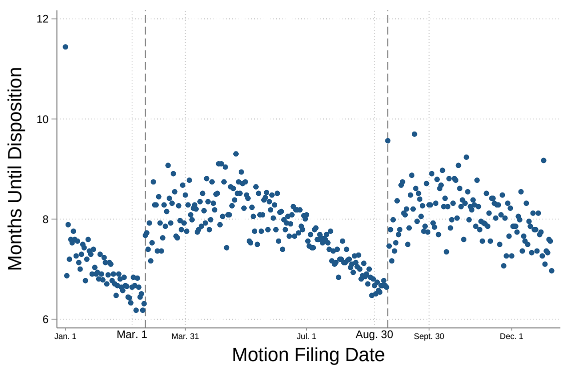
Note: Table shows OLS estimates for the effect of reporting time on the pre-treatment motion- or case-level characteristic indicated in each row. All columns include district and filing-year fixed effects. Column (2) adds linear day-of-month, day-of-quarter, and day-of-year time trends. Column (3) adds district-by-filing-year fixed effects. Column (4) adds day-of-month fixed effects. Observation counts vary by outcome and are omitted to facilitate presentation in a single table. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



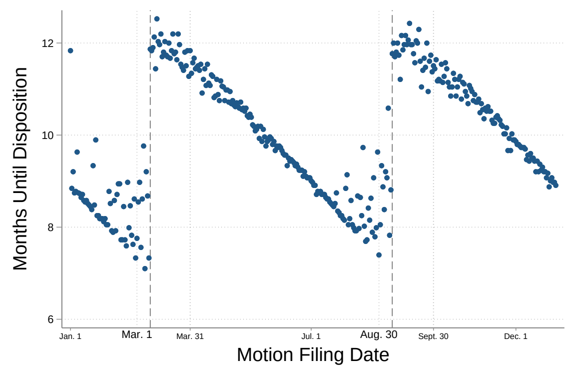
(a) 10th Percentile



(b) 25th Percentile



(c) 75th Percentile



(d) 90th Percentile

Figure A2: Percentiles of Months until Motion Resolution by Motion Filing Date

Note: Panels (a)–(d) show scatterplots of 10th, 25th, 75th, and 90th percentiles, respectively, of months until motion resolution against the motion's filing date. Each point represents the relevant percentile of resolution time for all motions filed on a given calendar day in any year from 2005–2013. Dashed vertical lines indicate the March 1 and August 30 cutoff dates, on which reporting time is at its highest.

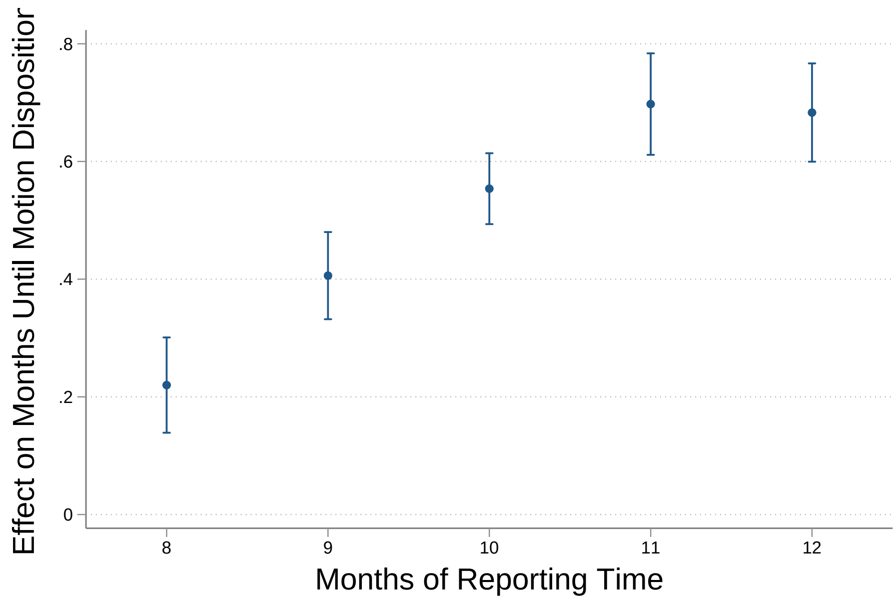
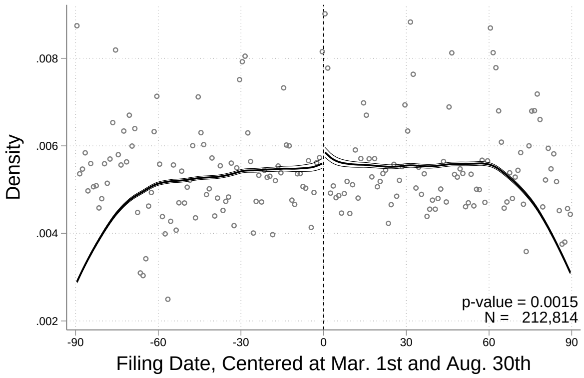
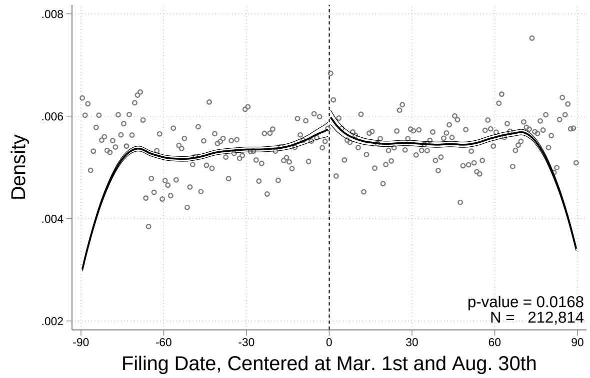


Figure A3: Non-parametric OLS Estimates

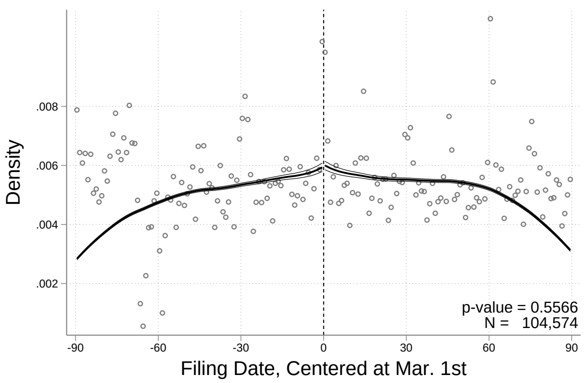
Note: Figure plots non-parametric OLS estimates of the effect of reporting time on months until motion disposition. Reporting time is defined as the number of months between a motion's filing date and the first six-month list deadline by which the motion must be resolved to avoid appearing on a six-month list report. Estimates are obtained by regressing months until motion disposition on a series of reporting time dummies (one for each month of reporting time), case- and motion-level controls, calendar trends, district×year fixed effects, and day-of-month fixed effects. Error bars indicate 95% confidence intervals. Standard errors are clustered at the Julian date of filing.



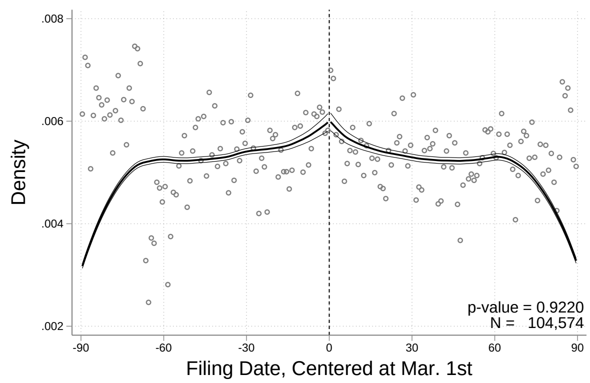
(a) Raw Filings, both cutoffs



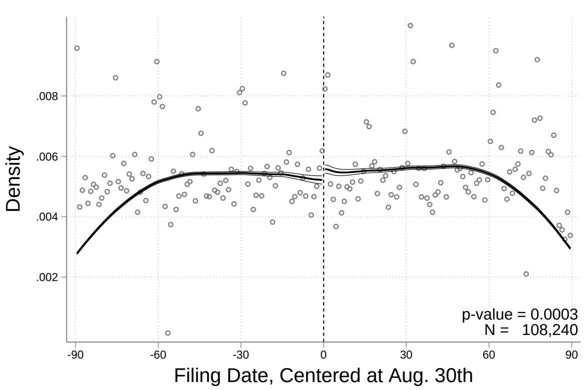
(b) Adjusted Filings, both cutoffs



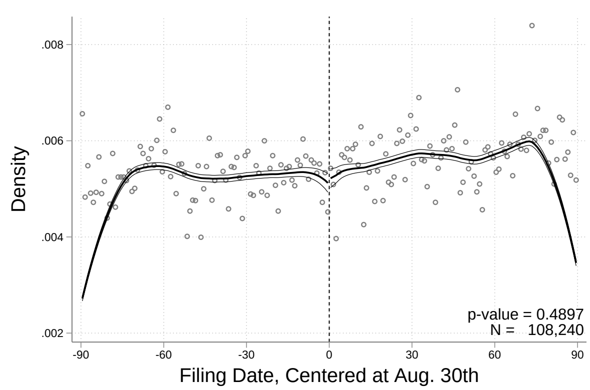
(c) Raw Filings, March cutoff



(d) Adjusted Filings, March cutoff



(e) Raw Filings, August cutoff



(f) Adjusted Filings, August cutoff

Figure A4: Density of Motion Filing Dates Relative to Six-Month List Cut-offs, Sample of Motions with Known Dispositions

Note: Figures show the density of summary judgment motion filing dates relative to the six-month list reporting cutoffs of March 1 and August 30. Sample includes summary judgment motions filed from 2005–2013 with known dispositions only. P-values from McCrary density tests (McCrary, 2008) and observation counts for the relevant sample are displayed in the lower right of each panel. Panels (a) and (b) show densities pooling both cutoffs; panels (c) and (d) show densities relative to the March 1 cutoff only; panels (e) and (f) show densities relative to the August 30 cutoff only. Within each pair, the first panel shows the raw filing date density and the second shows the density after adjusting for day-of-month (including a dummy for the last day of any month), day-of-week, and holiday effects.

Table A2: 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	0.00	-0.00	-0.00
	(0.01)	(0.01)	(0.02)	(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	-14.39	-12.27	-14.51
	(11.56)	(10.69)	(12.86)	(12.53)
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)

Continued on next page...

Table A2 – ...continued from previous page

Dependent Var.	Linear (1) No Controls	Linear (2) Controls	Quadratic (3) No Controls	Quadratic (4) Controls
% 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
	(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 presents bias-corrected RD estimates for the pre-treatment motion- and case-level characteristics indicated in each row, pooling both six-month list cutoffs (March 1 and August 30). MSE-optimal bandwidths are selected following the approach of Calonico et al. (2019). Effects are estimated with first- or second-order local polynomials using triangular kernels. Columns indicating controls include list (March vs. September) fixed effects, a dummy for the filing party (plaintiff vs. defendant), a dummy for whether a motion was the only summary judgment motion filed in its case, and nature-of-suit and filing-year fixed effects. Bandwidths and effective observation counts vary by outcome and are omitted to facilitate presentation in a single table. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: SUTVA Robustness Checks: Between-Judge and Within-Judge OLS Specifications

	(1) Baseline	(2) Between-Judge	(3) Between-Year	(4) Mundlak
Reporting Time (Months)	0.142***	0.207	0.120*	
	(0.006)	(0.185)	(0.067)	
Rep. Time, Within-Judge				0.141***
				(0.006)
Rep. Time, Between-Judge				0.486***
				(0.081)
Observations	211,902	1,594	11,200	212,301

Note: Table presents robustness checks addressing potential SUTVA violations from within-judge spillovers across motions. Column (1) reproduces the baseline OLS estimate from Table 2. Column (2) collapses the data to judge-level means and regresses mean motion duration on mean reporting time with district fixed effects; within-judge spillovers cannot bias this specification since it compares across rather than within judges. Column (3) collapses the data to judge-year means and includes judge, year, and district×year fixed effects; within-judge spillovers would need to affect a judge's entire annual docket simultaneously to bias this specification. Column (4) presents a Mundlak decomposition that includes both within-judge demeaned reporting time and judge-level mean reporting time as separate regressors; the within-judge coefficient is nearly identical to the baseline, and the between-judge coefficient is if anything larger, inconsistent with upward bias from within-judge spillovers. Standard errors are clustered at the Julian date of filing in columns (1) and (4), at the judge level in column (3), and are heteroskedasticity-robust in column (2); all are reported in parentheses.

Table A4: 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 bias-corrected RD estimates of the effect of reporting time on months until motion disposition using a variety of alternative bandwidths, pooling both six-month list cutoffs (March 1 and August 30). All columns are estimated with first-order local polynomials and triangular kernels and include list (March vs. September) fixed effects, a dummy for the filing party (plaintiff vs. defendant), a dummy for whether a motion was the only summary judgment motion filed in its case, and nature-of-suit and filing-year fixed effects. Effective N reports the number of observations within the indicated bandwidth; N (left) and N (right) decompose this into observations filed before and after the cutoff, respectively. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: RD Estimates: Effect of six months additional reporting time on months until motion disposition, clustered at various levels

	(1)	(2)	(3)	(4)
Filed After Cutoff	0.751	0.768	0.736	0.755
	(0.112)	(0.094)	(0.151)	(0.118)
	[0.157]	[0.144]	[0.198]	[0.172]
	{0.312}	{0.302}	{0.332}	{0.316}
Dep. Var. mean	5.4	5.4	5.4	5.4
Polynomial order	First	First	Second	Second
Controls		✓		✓
Bandwidth (days)	33.2	32.3	41.9	46.0
Effective N	80,143	78,010	96,923	106,133
N (left)	39,076	38,099	47,614	52,223
N (right)	41,067	39,911	49,309	53,910

Note: Table presents bias-corrected RD estimates of the effect of reporting time on months until motion disposition, pooling both six-month list cutoffs (March 1 and August 30). Standard errors clustered at the Julian date of filing, judge, and district levels are shown in parentheses, brackets, and braces, respectively. Significance stars are omitted to facilitate comparison across standard error specifications. To ensure common point estimates across all clustering schemes, the sample is restricted to observations with non-missing judge identifiers, and MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014) and optimized for standard errors clustered at the Julian date of filing. Effects are estimated with first- or second-order local polynomials using triangular kernels. Columns indicating controls include list (March vs. September) fixed effects, a dummy for the filing party (plaintiff vs. defendant), a dummy for whether a motion was the only summary judgment motion filed in its case, and nature-of-suit and filing-year fixed effects. Effective N reports the number of observations within the optimal bandwidth; N (left) and N (right) decompose this into observations filed before and after the cutoff, respectively.

Table A6: 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 reporting time on months until motion disposition using global polynomials, pooling both six-month list cutoffs (March 1 and August 30). MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014); effects, however, are estimated with first- or second-order global polynomials rather than local polynomials. Columns indicating controls include list (March vs. September) fixed effects, a dummy for the filing party (plaintiff vs. defendant), a dummy for whether a motion was the only summary judgment motion filed in its case, and nature-of-suit and filing-year fixed effects. Effective N reports the number of observations within the optimal bandwidth; N (left) and N (right) decompose this into observations filed before and after the cutoff, respectively. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Donut RD Estimates for Months Until Motion Disposition

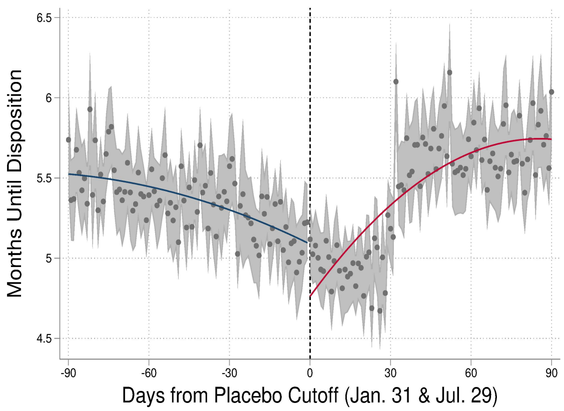
	(1)	(2)	(3)	(4)	(5)	(6)
Filed After Cutoff	0.778***	0.785***	0.790***	0.797***	0.802***	0.808***
	(0.091)	(0.088)	(0.085)	(0.082)	(0.079)	(0.077)
Donut radius (days)	3	6	9	12	15	18
Dep. Var. mean	5.4	5.4	5.4	5.4	5.4	5.4
Polynomial order	First	First	First	First	First	First
Controls	✓	✓	✓	✓	✓	✓
Bandwidth (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 bias-corrected "donut" RD estimates of the effect of reporting time on months until motion disposition using a variety of donut radii, pooling both six-month list cutoffs (March 1 and August 30). MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014) for the corresponding specification without a donut; for each column, the donut radius is added to this baseline bandwidth so that the effective estimation window remains approximately constant across specifications. All columns are estimated with first-order local polynomials and triangular kernels and include list (March vs. September) fixed effects, a dummy for the filing party (plaintiff vs. defendant), a dummy for whether a motion was the only summary judgment motion filed in its case, and nature-of-suit and filing-year fixed effects. Effective N reports the number of observations within the optimal bandwidth excluding the indicated donut radius; N (left) and N (right) decompose this into observations filed before and after the cutoff, respectively. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

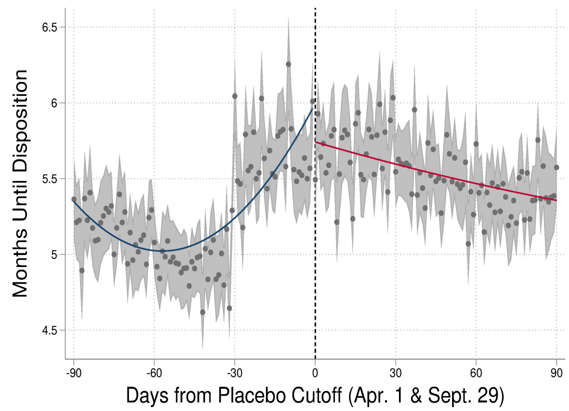
Table A8: Placebo Tests: RD Estimates for Months Until Motion Disposition, Counterfactual Cutoffs

	(1)	(2)	(3)	(4)
Filed After Cutoff	-0.203	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)	22.7	18.6	19.3	20.6
Effective N	49,893	43,197	46,214	49,434
N (left)	25,459	20,321	22,346	23,488
N (right)	24,434	22,876	23,868	25,946

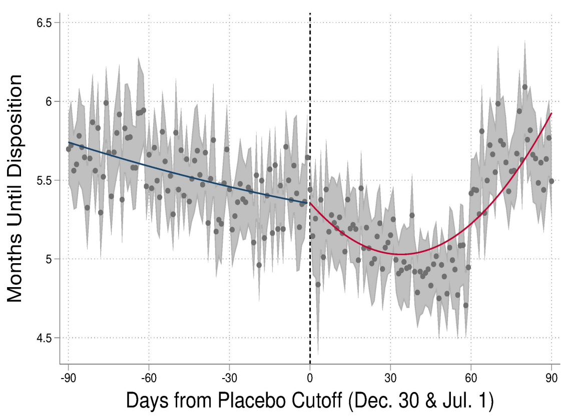
Note: Table presents bias-corrected placebo RD estimates of the effect of reporting time on months until motion disposition using counterfactual cutoff dates shifted one or two months before or after the actual March 1 and August 30 cutoffs. All columns are estimated with first-order local polynomials and triangular kernels and include list (March vs. September) fixed effects, a dummy for the filing party (plaintiff vs. defendant), a dummy for whether a motion was the only summary judgment motion filed in its case, and nature-of-suit and filing-year fixed effects. MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014). Effective N reports the number of observations within the optimal bandwidth; N (left) and N (right) decompose this into observations filed before and after the placebo cutoff, respectively. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



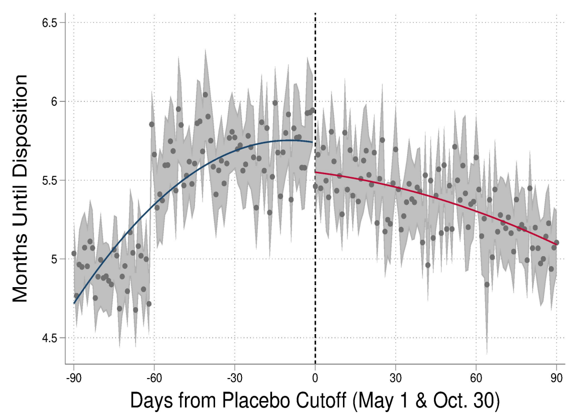
(a) One Month Left



(b) One Month Right



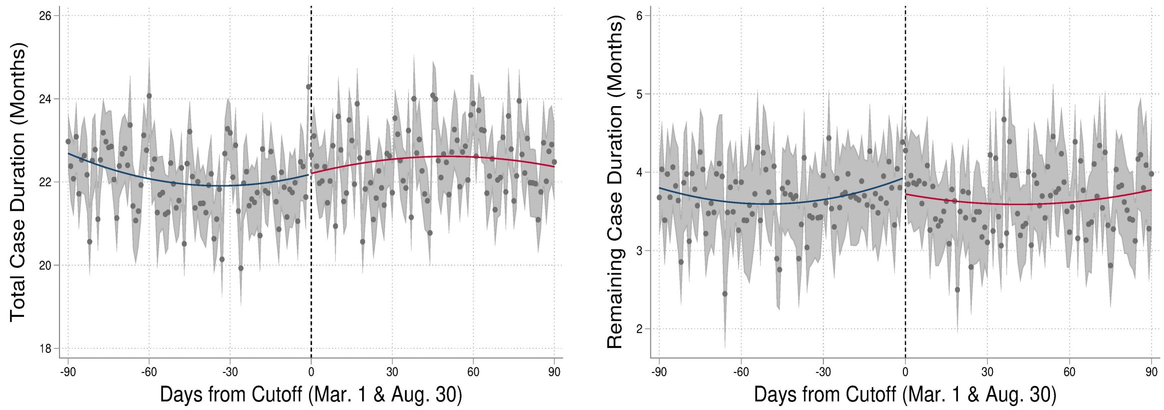
(c) Two Months Left



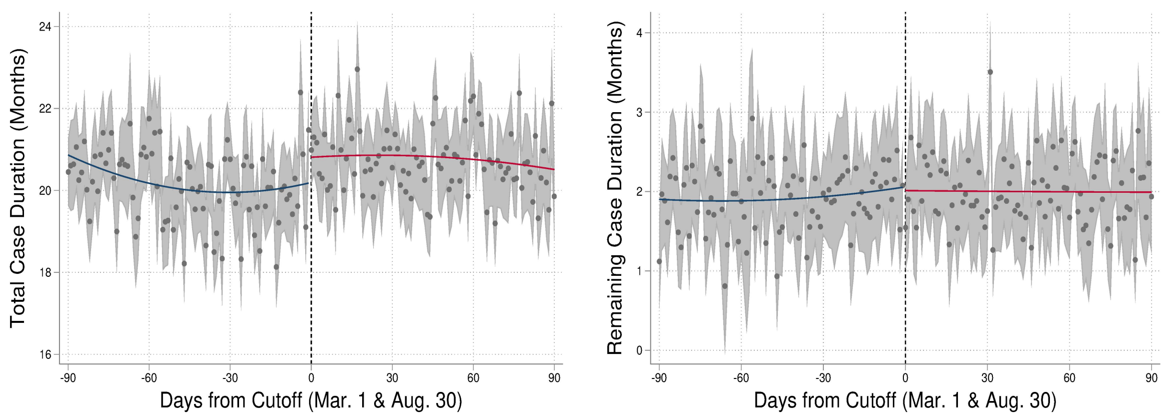
(d) Two Months Right

Figure A5: RD Estimates of Effect on Months Until Disposition, Placebo Cutoffs

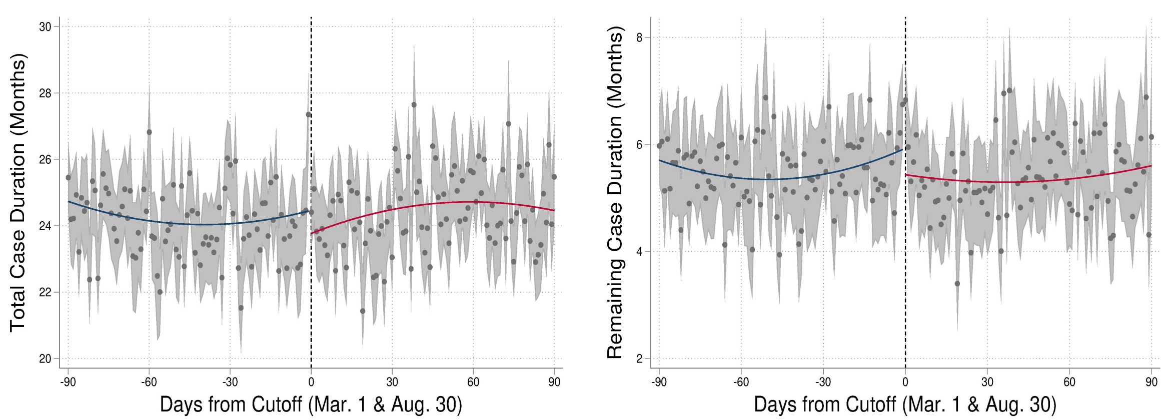
Note: Figures plot mean months until motion disposition against the motion's filing date relative to counterfactual cutoff dates shifted one or two months before or after the actual March 1 and August 30 cutoffs. Each point represents the mean disposition time for motions filed within a one-day bin. Fitted lines with 95% confidence intervals are estimated separately on each side of each placebo cutoff using second-order global polynomials. Sample includes all summary judgment motions with known dispositions filed from 2005–2013.



(a) Panel A: Full Sample



(b) Panel B: Granted Motions



(c) Panel C: Denied Motions

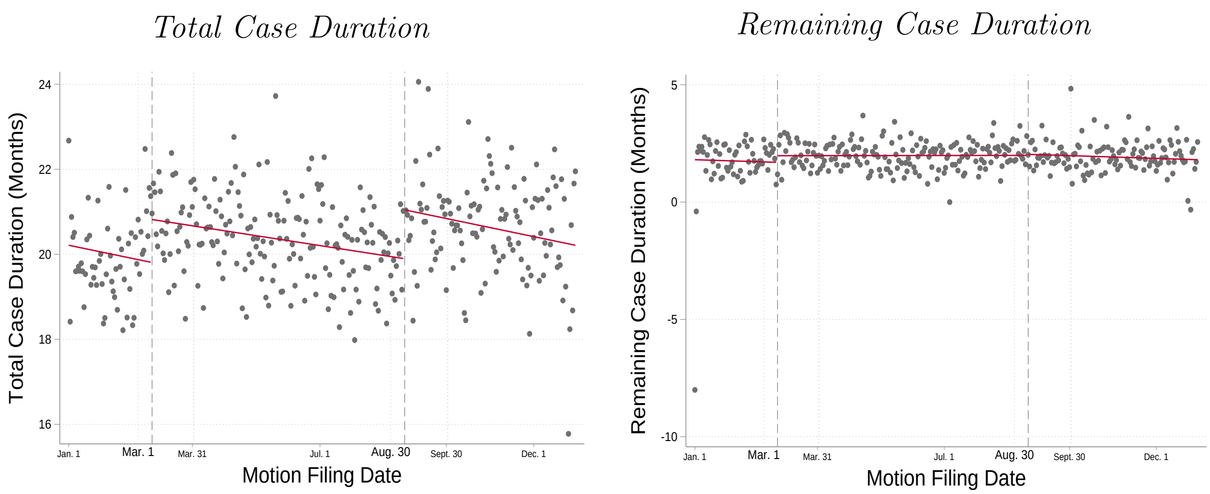
Figure A6: RD Plots: Case Duration Outcomes by Motion Filing Date

Note: Figures plot mean total case duration (left column) and mean remaining case duration after motion disposition (right column) against the motion's filing date relative to the nearest six-month list cutoff (March 1 or August 30), pooling both cutoffs. Each point represents the mean duration for motions filed within a one-day bin. Fitted lines with 95% confidence intervals are estimated separately on each side of the cutoff using second-order global polynomials. Panel (a) includes all summary judgment motions with known dispositions filed from 2005–2013; panel (b) restricts to granted motions; panel (c) restricts to denied motions.

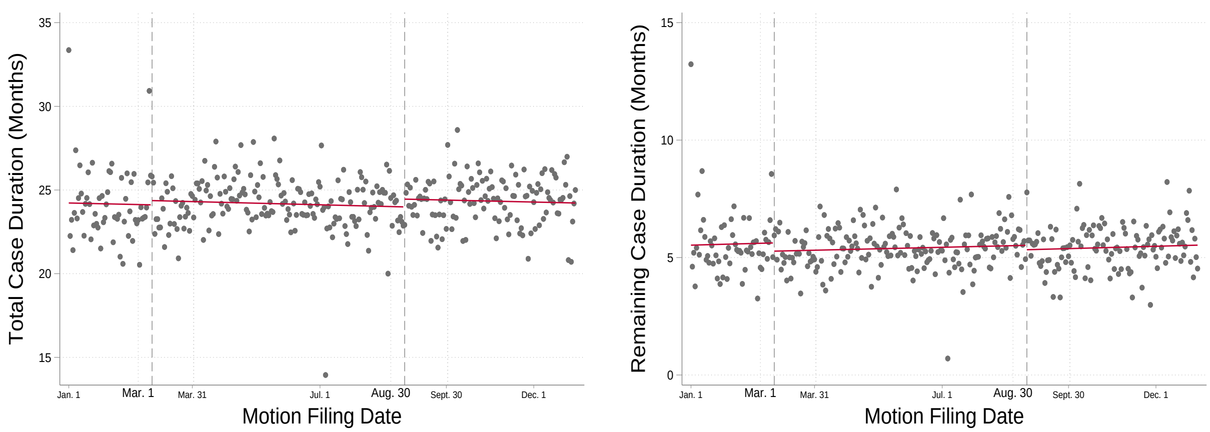
Table A9: OLS Estimates: Effect of one month additional reporting time on months until motion disposition, by motion outcome

	(1) Full Sample	(2) Granted	(3) Denied
Reporting Time (Months)	0.142***	0.144***	0.140***
	(0.006)	(0.008)	(0.008)
Observations	211,902	103,794	98,969
Case & Motion Controls	✓	✓	✓
Calendar Trends	✓	✓	✓
DistrictYear FEs	✓	✓	✓
Day-of-Month FEs	✓	✓	✓
Dep. Var. mean	5.4	5.26	5.65
Indep. Var. mean	10.05	10.06	10.05

Note: Table presents OLS estimates of the effect of one additional month of reporting time on months until motion disposition. Reporting time is defined as the number of months between a motion's filing date and the first six-month list deadline by which the motion must be resolved to avoid appearing on a six-month list report. Column (1) includes the full sample of summary judgment motions with known dispositions; column (2) restricts to granted motions; column (3) restricts to denied motions. All columns include case- and motion-level controls (a dummy for the filing party, a dummy for whether a motion was the only summary judgment motion filed in its case, and fixed effects for nature-of-suit category, judge, district, and filing year), calendar trends, district×year fixed effects, and day-of-month fixed effects. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



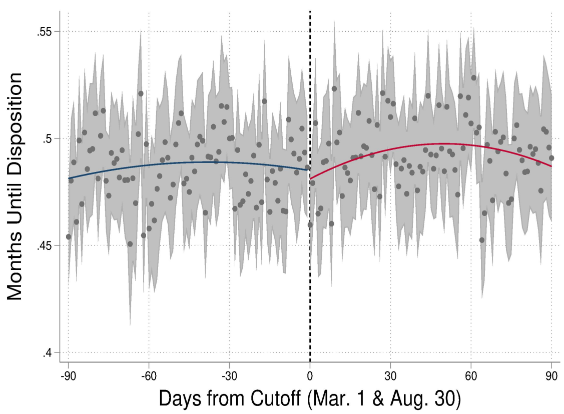
(a) Panel A: Granted Motions



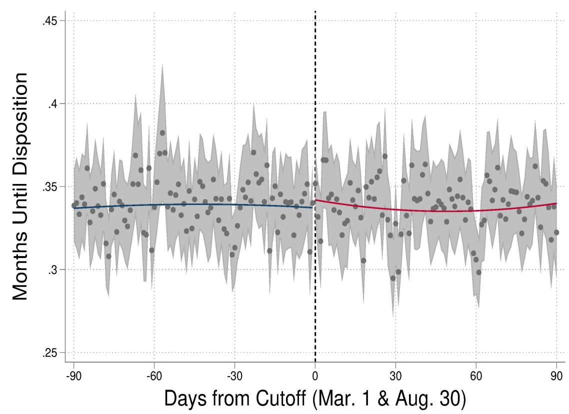
(b) Panel B: Denied Motions

Figure A7: Case Duration Outcomes by Motion Filing Date

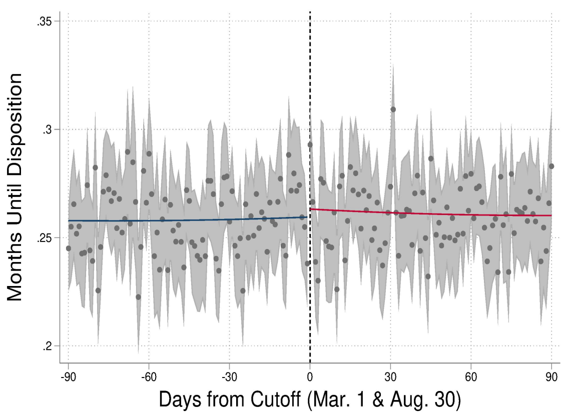
Note: Figures plot mean total case duration (left column) and mean remaining case duration after motion disposition (right column) against the motion's filing date. Each point represents the mean duration for motions filed on a given calendar day from 2005–2013. Panel (a) restricts to granted motions and panel (b) restricts to denied motions. Best-fit lines are estimated without controls via OLS and separately for the periods March 1–August 29 and August 30–February 29. Dashed vertical lines indicate the March 1 and August 30 cutoff dates, on which reporting time is at its highest.



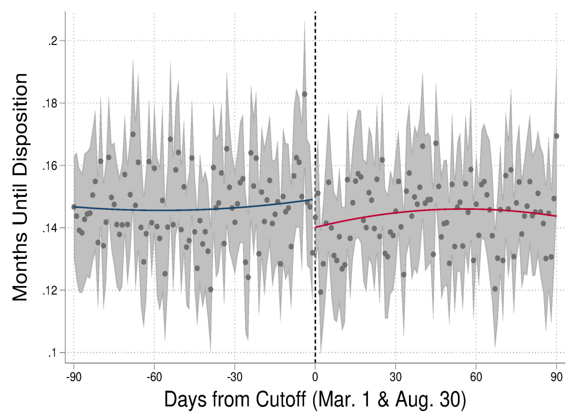
(a) % Granted



(b) % Denied



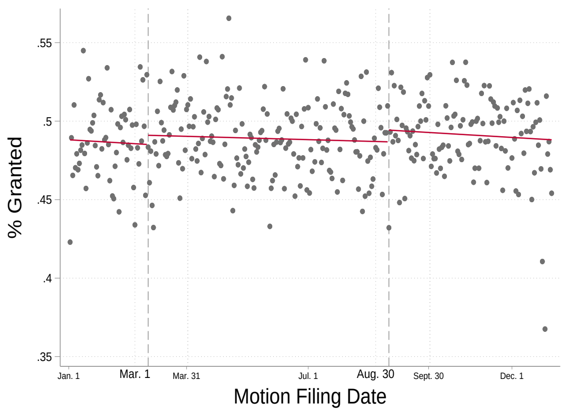
(c) % Appealed



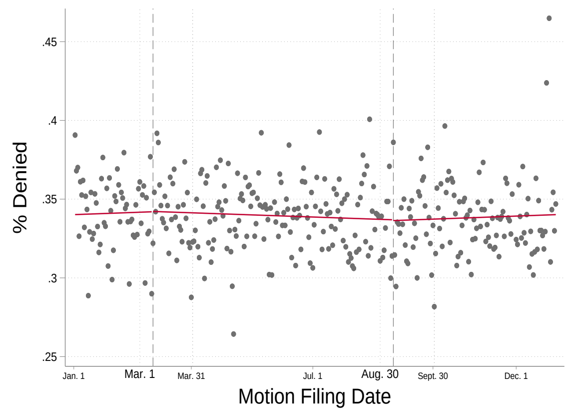
(d) % Reversed on Appeal

Figure A8: RD Plots: Motion and Appellate Outcomes

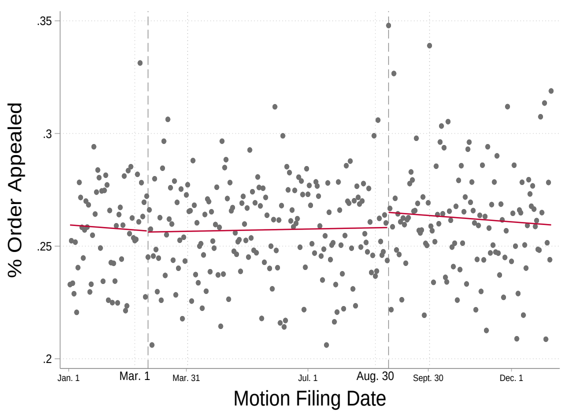
Note: Figures plot mean motion and appellate outcomes against the motion's filing date relative to the nearest six-month list cutoff (March 1 or August 30), pooling both cutoffs. Each point represents the mean outcome for motions filed within a one-day bin. Fitted lines with 95% confidence intervals are estimated separately on each side of the cutoff using second-order global polynomials. Motions are coded as reversed on appeal if the district court's order is appealed and reversed (or otherwise not affirmed). Sample includes all summary judgment motions with known dispositions filed from 2005–2013.



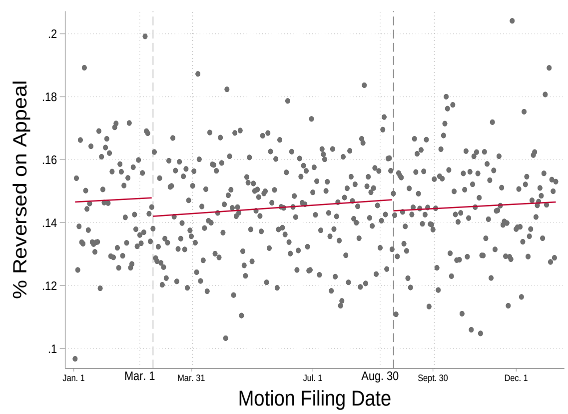
(a) % Granted



(b) % Denied



(c) % Appealed



(d) % Reversed on Appeal

Figure A9: Scatterplots: Motion and Appellate Outcomes

Note: Figures plot mean motion and appellate outcomes against the motion's calendar filing date. Each point represents the mean outcome for motions filed on a given calendar day from 2005–2013. Best-fit lines are estimated without controls via OLS and separately for the periods March 1–August 29 and August 30–February 29. Motions are coded as reversed on appeal if the district court's order is appealed and reversed (or otherwise not affirmed). Sample includes all summary judgment motions with known dispositions filed from 2005–2013.

Table A10: RD Estimates for Motion & Appellate Outcomes, Alternative Bandwidths

BW (days):	7	14	21	28	35	42	49
% Granted	-0.01	-0.02	-0.02	-0.02	-0.01	-0.01	-0.01
	(0.02)	(0.02)	(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
	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
% Appealed	0.02	0.00	-0.00	-0.01	-0.01	-0.01	-0.00
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
% Reversed on Appeal	0.01	-0.01	-0.01^{**}	-0.01^{**}	-0.01^{**}	-0.01^{**}	-0.01^{**}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)

Note: Table presents bias-corrected RD estimates of the effect of reporting time on motion and appellate outcomes using a variety of alternative bandwidths, pooling both six-month list cutoffs (March 1 and August 30). All columns are estimated with first-order local polynomials and triangular kernels and include list (March vs. September) fixed effects, a dummy for the filing party (plaintiff vs. defendant), a dummy for whether a motion was the only summary judgment motion filed in its case, and nature-of-suit and filing-year fixed effects. Motions are coded as reversed on appeal if the district court's order is appealed and reversed (or otherwise not affirmed). Effective observation counts vary by outcome and are omitted to facilitate presentation in a single table. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: 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)
% Reversed on Appeal	-0.01**	-0.01**	-0.01**	-0.01**	-0.01**	-0.01**
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)

Note: Table presents bias-corrected "donut" RD estimates of the effect of reporting time on motion and appellate outcomes using a variety of donut radii, pooling both six-month list cutoffs (March 1 and August 30). All columns are estimated with first-order local polynomials and triangular kernels and include list (March vs. September) fixed effects, a dummy for the filing party (plaintiff vs. defendant), a dummy for whether a motion was the only summary judgment motion filed in its case, and nature-of-suit and filing-year fixed effects. MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014) for the corresponding specification without a donut; for each column, the donut radius is added to this baseline bandwidth so that the effective estimation window remains approximately constant across specifications. Motions are coded as reversed on appeal if the district court's order is appealed and reversed (or otherwise not affirmed). Bandwidths and effective observation counts vary by outcome and are omitted to facilitate presentation in a single table. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: 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)
% Reversed on Appeal	-0.01	0.00	0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)

Note: Table presents bias-corrected placebo RD estimates of the effect of reporting time on motion and appellate outcomes using counterfactual cutoff dates shifted one or two months before or after the actual March 1 and August 30 cutoffs. MSE-optimal bandwidths are selected following the approach of Calonico, Cattaneo and Titiunik (2014). All columns are estimated with first-order local polynomials and triangular kernels and include list (March vs. September) fixed effects, a dummy for the filing party (plaintiff vs. defendant), a dummy for whether a motion was the only summary judgment motion filed in its case, and nature-of-suit and filing-year fixed effects. Motions are coded as reversed on appeal if the district court's order is appealed and reversed (or otherwise not affirmed). Bandwidths and effective observation counts vary by outcome and are omitted to facilitate presentation in a single table. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: RD Estimates: Effect of six months additional reporting time on motion & appellate outcomes, clustered at various levels

	(1)	(2)	(3)	(4)
% Granted	-0.021	-0.015	-0.025	-0.025
	(0.014)	(0.011)	(0.016)	(0.017)
	[0.015]	[0.012]	[0.017]	[0.016]
	{0.022}	{0.024}	{0.022}	{0.025}
% Denied	0.009	0.015	0.013	0.017
	(0.011)	(0.014)	(0.015)	(0.015)
	[0.014]	[0.015]	[0.018]	[0.018]
	{0.018}	{0.020}	{0.020}	{0.022}
% Appealed	0.003	-0.006	0.016	0.000
	(0.013)	(0.012)	(0.019)	(0.016)
	[0.013]	[0.012]	[0.020]	[0.016]
	{0.018}	{0.016}	{0.024}	{0.020}
% Reversed on Appeal	-0.010	-0.013	-0.011	-0.015
	(0.005)	(0.006)	(0.008)	(0.008)
	[0.007]	[0.007]	[0.010]	[0.009]
	{0.011}	{0.009}	{0.011}	{0.010}
Polynomial order	First	First	Second	Second
Controls		✓		✓

Note: Table presents bias-corrected RD estimates of the effect of reporting time on motion and appellate outcomes, pooling both six-month list cutoffs (March 1 and August 30). Standard errors clustered at the Julian date of filing, judge, and district levels are shown in parentheses, brackets, and braces, respectively. Significance stars are omitted to facilitate comparison across standard error specifications. To ensure common point estimates across all clustering schemes, the sample is restricted to observations with non-missing judge identifiers, and MSE-optimal bandwidths are selected for each outcome-specification combination following the approach of Calonico, Cattaneo and Titiunik (2014) and optimized for standard errors clustered at the Julian date of filing. For each outcome-specification combination, optimal bandwidths and effective observations are the same as those reported in Table 6. Motions are coded as reversed on appeal if the district court's order is appealed and reversed (or otherwise not affirmed). Effects are estimated with first- or second-order local polynomials using triangular kernels. Columns indicating controls include list (March vs. September) fixed effects, a dummy for the filing party (plaintiff vs. defendant), a dummy for whether a motion was the only summary judgment motion filed in its case, and nature-of-suit and filing-year fixed effects.

Table A14: 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 one additional month of 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. All columns include case- and motion-level controls (a dummy for the filing party, a dummy for whether a motion was the only summary judgment motion filed in its case, and fixed effects for nature-of-suit category, judge, district, and filing year), calendar trends, district×year fixed effects, and day-of-month fixed effects. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: 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 bias-corrected RD estimates of the effect of reporting time on months until motion disposition, pooling both six-month list cutoffs (March 1 and August 30). 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. Columns indicating controls include list (March vs. September) fixed effects, a dummy for the filing party (plaintiff vs. defendant), a dummy for whether a motion was the only summary judgment motion filed in its case, and nature-of-suit and filing-year fixed effects. Effective N reports the number of observations within the optimal bandwidth; N (left) and N (right) decompose this into observations filed before and after the cutoff, respectively. Standard errors are clustered at the Julian date of filing and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B 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 estimate separate counterfactual distributions $\hat{f}_0^l(d)$ for each possible reporting time l using a series of polynomial regressions of the form:

$$c_j^l = \sum_{i=0}^{p} \beta_i^l \cdot (d_j)^i + \sum_{i=d_-}^{d_+} \gamma_i^l \cdot \mathbb{1}[d_j = i] + \sum_{r \in R} \rho_r^l \cdot \mathbb{1}\left[\frac{d_j}{r} \in \mathbb{N}\right] + \nu_j^l, \tag{5}$$

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 (5) 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 excluded 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 (5), 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 "excluded window" around the deadline itself) is most likely to be satisfied in the maximum reporting time scenario. Figure B10 plots the distribution of motion resolution times for motions with reporting times of 390 days or greater, along with the fitted counterfactual distribution.[22] Using the approach described above, I estimate an excluded window beginning 15 days to the left of the six-month list deadline (375 days) and ending 95 days to the right (485 days).

With a counterfactual distribution in hand, I can then estimate the aggregate effect of the six-month list by comparing the actual and counterfactual 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 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 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 B16 presents ratios $T/\hat{T}$ for a variety of different polynomial orders and excluded-window lower bounds. A ratio of less than one suggests that the six-month list has reduced aggregate resolution times relative to the scenario without a six-month list, and a value

[22] 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.

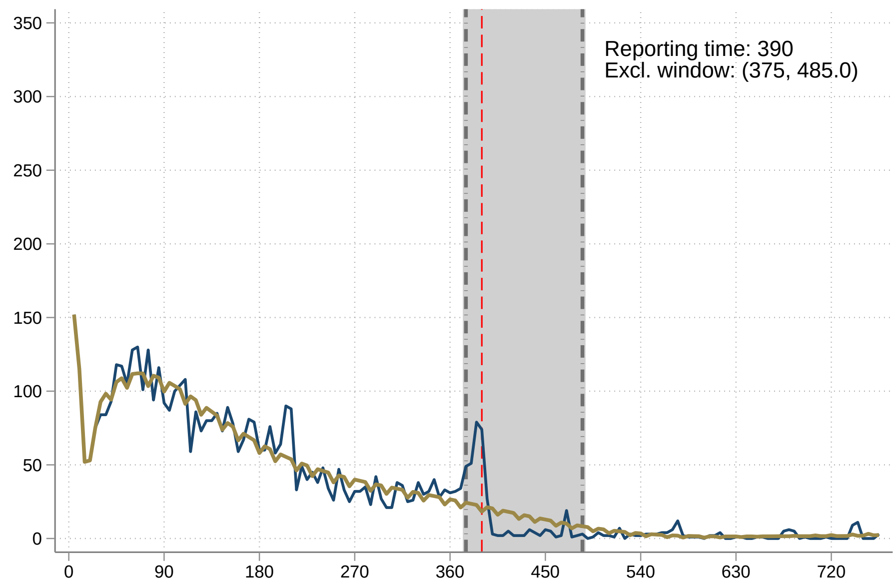


Figure B10: Actual vs. Counterfactual Resolutions, Maximum Reporting Time

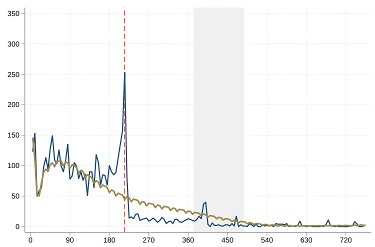
Note: Figure plots observed and counterfactual frequencies of motion resolution times (in days) for motions with maximum reporting time (at least 390 days). The counterfactual distribution is estimated using an eighth-order polynomial regression following the bunching estimator of Kleven and Waseem (2013), excluding a window around the six-month list deadline (shaded in gray) within which judges may respond to reporting incentives. Excluded window bounds are displayed in figure's upper-right corner.

of greater than one would suggest that the six-month list has actually slowed aggregate resolutions. Across all specifications, I estimate that the six-month list reduces total motion resolution time by approximately 3.5–4.5%.

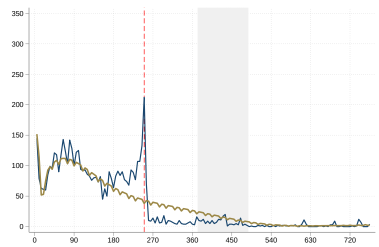
As discussed above, our setting is complicated by the fact that motions are subject to a variety of different reporting times—more than 180 in total—depending upon the day of the year on which they were filed. Thus, we might hypothesize that the extent of bunching is likely to vary with the amount of reporting time. Figure B11 confirms our intuition by plotting the actual versus counterfactual density of motion resolution 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.

Finally, Figure B12 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.[23]

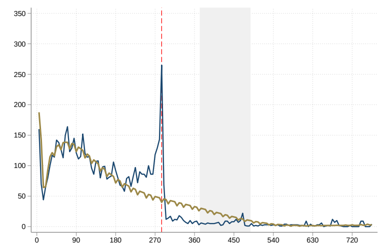
[23] Standard errors and confidence intervals are obtained with a Wild bootstrap procedure.



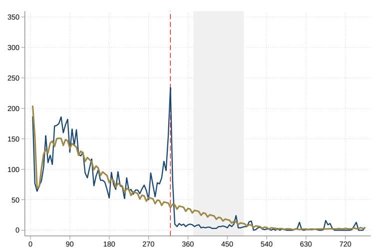
(a) 215 days R.T.



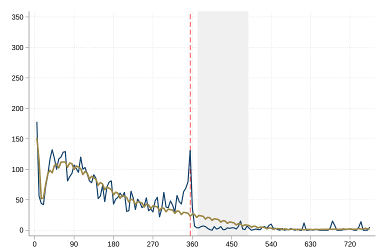
(b) 250 days R.T.



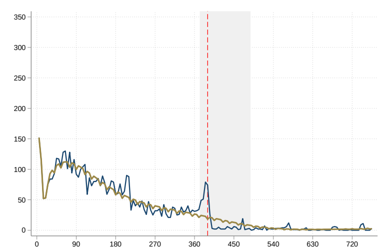
(c) 285 days R.T.



(d) 320 days R.T.



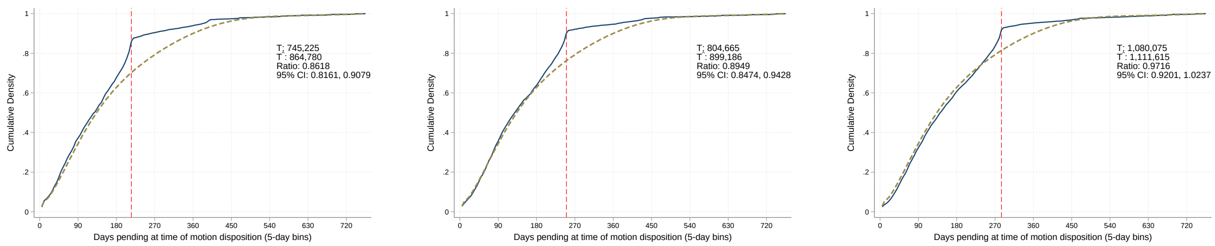
(e) 355 days R.T.



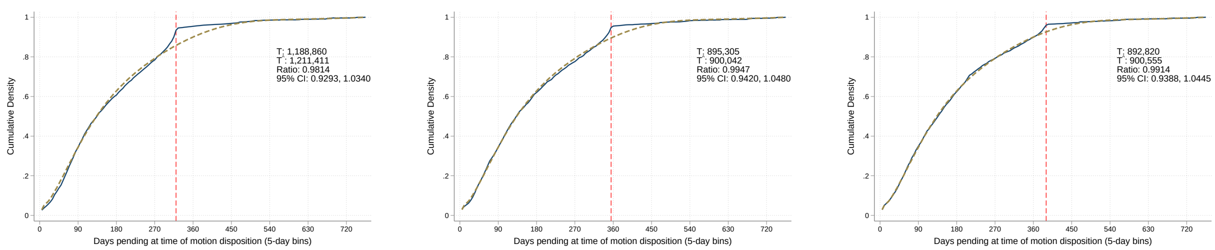
(f) 390 days R.T.

Figure B11: Actual vs. Counterfactual Resolutions for Six Selected Reporting Times

Note: Figure plots observed frequencies of motion resolution times (in days) for six selected reporting times (215, 250, 285, 320, 355, and 390 days) as well as a common counterfactual distribution of resolution times. The counterfactual distribution is estimated from motions with maximum reporting time ($\geq$ 390 days) using an eighth-order polynomial regression following the bunching estimator of Kleven and Waseem (2013), excluding a window around each reporting time's six-month list deadline (shaded in gray) within which judges may respond to reporting incentives. Excluded window bounds are displayed in each panel's upper-right corner of Figure B10.



(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 B12: CDFs of Counterfactual & Observed Resolution Time (Days) for Six Selected Reporting Times

Note: Figure plots observed and counterfactual cumulative distribution functions of motion resolution times (in days) for six selected reporting times (215, 250, 285, 320, 355, and 390 days). Counterfactual distributions are estimated from motions with maximum reporting time ($\geq$ 390 days) using an eighth-order polynomial regression following the bunching estimator of Kleven and Waseem (2013), excluding a window around each reporting time's six-month list deadline (shaded in gray) within which judges may respond to reporting incentives. Excluded window bounds are displayed in each panel's upper-right corner. Actual total resolution time (T), estimated counterfactual resolution time ($\hat{T}$), ratio $T/\hat{T}$, and 95% confidence intervals are indicated in the northeast corner of each plot.

Table B16: Robustness Checks: Bunching Estimates by Polynomial Order and Lower Bound

	Lower bound of excluded 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 $T/\hat{T}$—where T is actual total motion resolution time and $\hat{T}$ is the estimated counterfactual resolution time under a no-list regime—for varying polynomial orders and lower bounds of the excluded window, following the bunching estimator of Kleven and Waseem (2013). A ratio less than one indicates that the six-month list reduces total motion-processing time relative to the counterfactual. Polynomial order 8 and lower bound of 375 days are the baseline specification. 95% confidence intervals are reported in parentheses.

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