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The Speed of Justice

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Abstract
Can procedural reforms improve judicial efficiency? And do improvements in judicial efficiency benefit firms? We study a reform that gave judges in Senegal the powers to desk reject cases and the responsibility to complete pre-trials within four months. We combine three years of hearing-level caseload data and monthly firm tax filings with the staggered roll-out of the reform to produce three key results. First, the reform improved judicial efficiency, with no detrimental effect on quality. Second, firm monthly revenues drop by 8-11 percent upon entering pre-trial, with the effect concentrated on slower pre-trials. Third, monthly firm revenues decline by on average 3.2-5.0 percent for every 100 days a case spends in pre-trial. Survey results show firms are willing to pay higher legal fees to achieve post-reform speed, suggesting net positive benefits of the reform on firms.

Keywords: Economic development, Firms, Judicial efficiency.

JEL Classification: K41, D73, O12.

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1 Introduction

Can judicial reforms improve judicial efficiency and increase firms' economic performance? Cross-country and within-country evidence show that legal efficiency, in the form of higher speed and lower procedural formalism, is a strong correlate of economic development and market performance (Alencar & Ponticelli, 2016; Djankov et al., 2008; Chemin, 2018). As legal origins account for much of the cross-country variation in legal efficiency (La Porta et al., 2008), what is the potential for *de jure* reforms to increase *de facto* efficiency? And, can improvements in legal efficiency produce firm-level economic gains?

In this study, we show that judicial reforms can improve judicial efficiency at a margin that can produce meaningful economic impacts for firms engaged in court cases. We do so in the context of a Senegalese judicial reform that aimed to increase the speed of civil and commercial adjudications and, in doing so, improve the investment climate. The reform gave first-instance judges the power to desk reject cases and the responsibility to complete pre-trials in four months—which, on average, accounted for over two-thirds of total case duration. As such, the reform we study explicitly aimed to curb high levels of procedural formalism, characteristic of civil law systems (Djankov et al., 2003).

We proceed in two steps. First, we exploit hearing-level court case data to document the causal impact of the judicial reform on procedural efficiency and the quality of legal decisions. Second, we combine monthly firm tax records and primary firm survey data to measure the firm-level economic effects of the improvement in pre-trial speed legislated by the reform.

We start by showing that a reduction in *de jure* procedural formalism increased *de facto* legal efficiency in the context of the civil and commercial court of Dakar, Senegal. In two ways, we contribute novel evidence to a literature that has linked legal reforms with judicial efficiency. First, we construct a flexible event study design, combining the staggered roll-out of the reform across the six chambers of the court between November 2013 and March 2014 with caseload data on all 5,297 civil and commercial cases that entered court between 2012 and 2015. Second, we exploit the granularity of our court data by tracing each case through the legal procedure and constructing case-level markers of speed (duration) and procedural formalism (number of steps in the procedure at both pre-trial and decision phases, and number of overturned steps). Court-level studies tend to be limited to richer economies (Chang & Schoar, 2007).[1] Even then, court data typically only record duration (Alencar & Ponticelli, 2016; Chemin, 2009b,a; Coviello et al., 2015; Lichand & Soares, 2014; Visaria, 2009). Detailed hearing-level data allow us to measure steps taken by judges to avoid dilatory actions by the parties. We also shed light on potential efficiency-quality trade-offs, using data on final judgments and intention to appeal to capture the quality of decisions. Understanding the trade-offs associated with changing the rules of the game in bureaucracies is particularly important in courts, as judges evolve in complex, multi-tasking environments with substantial authority and independence (Holmstrom & Milgrom, 1987; Finan et al., 2017).

We find the reform significantly increased the speed of justice through a reduction in procedural formalism with no adverse effect on the quality of legal decisions. We document a large reduction in pre-trial duration of 46.1 days (0.32 SD) off of a 157 day baseline, as judges are 49% more likely to meet the four-month deadline (an increase of 23.9 percentage points from a pre-reform level of 48.7%). We show this effect is attributable to a reduction in formalism. The number of case-level pre-trial hearings is reduced (0.31 SD), and the decisiveness of hearings increases: the number of desk-rejected and fast-tracked cases increase (by 16.9 and 9.2 percentage points, respectively), while judges are 48% more likely to issue a strict deadline for an adjournment. Both smaller and larger disputes are similarly affected by the reform, and the decree is equally applied by originally faster and slower judges. We later discuss robustness of our event study design to selection on level, selection on trend, selection on shock, and selection on trend shock.

[1] Chemin (2009b) uses yearly court-level data to identify the impact of a legal reform in Pakistan, exploiting district-level variation in coverage. Alencar & Ponticelli (2016) exploit yearly variation in case duration across courts to isolate the role of court efficiency on the impacts of a bankruptcy reform in Brazil.

Gains in speed do not appear to negatively affect procedural quality, as captured along three dimensions. First, the quality of the pre-trial itself is not negatively affected. The completeness of the evidence assembled, measured by the likelihood of pre-trial being declared insufficient and the decision being postponed, remains unchanged. Second, we do not find evidence of judges' effort displacement from decision to pre-trial stage: decision hearings are scheduled at the same speed, the number of decision hearings does not increase, and the quality of decisions (measured by the length of the justification and the number of articles cited) is not negatively affected. Finally, the decree does not affect parties' intentions to appeal court decisions. Taken together, these results contribute court-level causal evidence on the role of legal reforms in strengthening institutions, complementing and extending a literature that has relied on cross-country and within-country evidence (Djankov et al., 2003; La Porta et al., 2008; Chemin, 2018; Bosio et al., 2020).

Next, we combine tax administration records and primary firm survey data to show that the improvement in pre-trial speed legislated by the reform is associated with meaningful economic gains for firms. We use monthly firm revenue data from value-added tax (VAT) declarations to show that having a case significantly hurts firms activity: OLS results indicate that starting pre-trial is associated with an 8% reduction in revenue. This penalty is concentrated among firms whose cases spend over four months in pre-trial: these firms experience an 11% decline in revenue, relative to an imprecise 4% decline for firms whose cases have faster pre-trials (p-value of the difference < 0.1). We use the fact that the reform exogenously increased the speed of pre-trial without affecting other margins of judicial efficiency and quality to instrument for speed of treatment. We show that average monthly firm revenues decline by a significant 3.2-5.0% for every 100 days a case spends in pre-trial.

Firm surveys corroborate these results and point to the diversion of managerial capacity away from day-to-day operations, increased firm debt and failure to invest as key mechanisms of the economic cost of judicial inefficiencies. Firms state being willing to pay 50% more in legal costs to experience post-reform speed, relative to pre-reform speed. Comparing firms with cases before and after the reform, we find improvements in firms' perception of the justice system. Taken together, these results support the notion that firms involved in court cases after the introduction of the reform experienced net positive economic gains, bringing new case-firm-level evidence to the literature on the costs of procedural delays (Alencar & Ponticelli, 2016; Djankov et al., 2010, 2020; Lilienfeld-Toal et al., 2012).

2 Civil and commercial justice in Senegal

Senegal's civil and commercial legal procedure is associated with a high degree of formalism and low legal efficiency (Djankov et al., 2003). Senegal ranked 166 out of 185 economies in the contract enforcement category of the 2013 Doing Business Report, suggesting the speed of commercial dispute resolution could be significantly improved (Doing Business, 2013).[2] Yearly, the Regional First Instance Court of Dakar adjudicates a dispute amount equivalent to 3–6% of Senegal's GDP. As this capital is stuck in lengthy litigation, it is easy to infer large economic costs (Barro, 1991; Mankiw et al., 1992). We now detail the architecture of the court and legal procedure.

In the Regional First Instance Court of Dakar, judges are organized in chambers, consisting of a president and two additional judges. While the court adjudicates all types of affairs, we focus on civil and commercial justice. At the time of the reform we study, there were four commercial chambers and two civil chambers in the Court. Tables 1 and 2 describe the variation in caseload size and characteristics we have access to at the chamber and case

[2] The Doing Business Report's enforcing contracts indicator collects its data through a standardized case study with a pre-defined claim value and very specific assumptions. Among such assumptions is that the case is disputed on the merits and that an expert is appointed. The Doing Business Report's trial and judgment indicator includes pre-trial and decision proceedings, as well as the time to obtain a written judgment and the period within which any party can appeal the first instance decision. In 2014, the Doing Business Report indicated a 420-day duration for trial and judgment. Upon request from the Ministry of Finance of Senegal, and on the basis of the present analysis of Decree n° 2013-1071 combined with its own methodology, the Doing Business team adjusted this figure down to 390 days in the 2018 report (and adjusted the duration down retroactively going back to 2015).

levels, respectively.

Commercial and civil trial and judgment consist of the following steps: distribution (*répartition*), pre-trial hearings (*audiences de mise en état*), decision hearings (*délibérations*), and judgment (*jugement*). In 2012, 1,546 new civil and commercial cases were distributed in Dakar. This step consists in the assignment of the new caseload to the chambers by court president on the basis of chambers' relative ongoing caseloads and specialization.

In its assigned chamber, a case first undergoes public pre-trial hearings chaired by a pre-trial judge. The evidence is assembled and the parties submit supporting pieces to develop their arguments; they may also request expert reports. The pre-trial judge's role is largely administrative. The first hearing is crucial, as the pre-trial judge sets a path for each party to build their case, or, alternatively, may decide to fast-track a case to decision.

Once pre-trial is complete, a case moves to decision—collegiate closed-door deliberations, chaired by the president of a chamber. The judgment is then announced in a public decision hearing. Should the evidence presented in deliberations be insufficient, the judges can declare it so and send a case back to pre-trial. Alternatively, the decision may be postponed, allowing the judges to perform further diligence.

Each chamber follows a fixed schedule of two hearing dates per month. On average, a chamber takes in 16.4 new cases at each bi-monthly pre-trial hearing date, ranging from 9.1 to 26.8 across chambers and years (Table 1). During these, each pre-trial judge chairs her scheduled pre-trial hearings. At the end of each pre-trial hearing, the judge can either schedule an additional hearing at the request of one of the parties (adjournment) or close the pre-trial and move the case to decision phase. If the pre-trial judge estimates that adjourning would unnecessarily slow down the procedure, she can issue a "strict" adjournment (*"renvoi ferme"* or *"renvoi ultime"*). This signals to the parties that the following hearing will be the final hearing before the decision phase begins. If the judge feels a party is (still) not doing its due diligence, she can move a case to decision as is.

Commercial and civil disputes vary in their nature and complexity. Commercial cases include mostly payment and other contract disputes, such as sale and rent contracts involving at least one moral person (firm). Similarly, civil cases include contract and payment disputes between individuals (e.g., landlord and tenant), as well as other civil issues such as inheritance disputes. Sixty-three percent of civil and commercial disputes in our sample include a payment claim. Among these, the average claim amount is CFA franc 71,542,000 (approximately USD 135,000), ranging from CFA franc 75,000 to CFA franc 7,400,000,000 (approximately USD 142–13,962,000; Table 2).

3 Data

3.1 Caseload data

We digitized paper-based records of the civil and commercial chambers of the Regional First Instance Court of Dakar, Senegal, over the period January 2012 to June 2015 (Figure A1). We recorded hearing-level outcomes for each case across both pre-trial and decision phases and entered information on the minutes of the judgment. This data yields case-level information on the full civil and commercial caseload over the 2012/15 study period (5,297 cases). For each case, we recorded when it entered the court, when and to which chamber it was transferred for the pre-trial procedure (first hearing), which judge presided over its pre-trial, the date and outcome of each pre-trial and decision-stage hearing, the date and nature of the final decision, some elements of the text of the decision itself (judgment minutes), as well as scant case characteristics available in the files (civil or commercial, claim amount, type and number of parties on each side).

This yields a case-hearing-level dataset that retraces the whole first instance procedure for all cases entering the court over our study period. This dataset allows us to document whether a case was heard at a given chamber hearing date and what the outcome was. Hearings are scheduled on a bi-monthly basis, following a chamber-specific schedule that is set every six months by the president of the court; this allows for 21 hearing dates per chamber per year. Figure A2 shows the incoming and ongoing case volume at any hearing period.[3]

3.2 Firm data

To document the impact of the reform on court users, we combine and leverage two sources of complementary data: the universe of firm monthly VAT filings obtained from the tax administration (together with annual corporate and individual tax returns), and primary survey data on firms involved in court cases over our study period. These cases involved 5,401 parties that are firms, which correspond to 2,154 distinct firms. Matching these to the tax administration data is only possible when we are able to obtain their unique tax ID (NINEA); this is the case for 66% of distinct firms which represent 82% of parties that are firms in the court data. We then perform the following two merges.

First, we merge the court party data with the 2012 revenue data from annual corporate and individual returns to check for balance in pre-decree revenues. This yields a match for 46% of distinct firms in our court data (993 firms), representing 70% of the parties that are firms (3,785 parties, of which 1,991 had cases before the reform); these firms are involved in a total of 2,910 cases.

Second, we merge the court party data with revenue data from monthly VAT filings.[4] We restrict the revenue data to include only complete time series of twelve monthly data points around the start of a parties' pre-trial (six months before and six months after). We obtain

[3]A six-week summer break is established at the chamber level over the August–October period, on a rotating basis across chambers. All judges take leave during the period assigned to their respective chamber, and no hearings are scheduled.

[4]We focus on firm revenues and not value-added due to the frequency and granularity of the filing. While revenues are filed monthly, deductions are not, and we do not have transaction-level data to recover monthly deductions.

matches fulfilling the data criteria for 1,622 out of the 5,401 unique parties, which represent 582 out of the 2,154 distinct firms.[5] The discrepancies in matching success across tax data sources are due to revenue thresholds (as well as lower compliance) for monthly filing regimes and the exclusion of banks and insurance companies from the monthly revenue analysis (see last footnote). We perform robustness to differential attrition in Section 6.3. While our primary focus is on the impact of court proceedings on firm revenues, for some specifications we leverage the monthly revenue time series of all 8,575 firms in the VAT filings data without a case over our study period that meet analogous data criteria (complete time series of at least 12 months).[6]

Finally, between August 2016 and February 2017 we conducted a survey of firms involved in commercial cases over our study period. We recovered addresses and/or phone numbers in the Dakar region for 1,709 of these 2,154 firms.[7] Out of the remaining 445 firms, 218 were located outside the survey area (abroad or in a different region of Senegal), while for 227 no contact information could be obtained. We successfully located 812 of the 1,709 firms for which we had recovered some contact information,[8] and completed 277 surveys. Conditional on being located, we obtained a response rate of 34%. These 277 firms correspond to 925 parties that are firms; they were involved in 884 different cases. Again, we discuss robustness

[5]The universe of monthly VAT filings includes 15,652 distinct firms with revenue data for at least one month within our study period. Of these, 939 were party to at least one court case (3,441 parties in total) over our study period, while 14,713 were not. Of the former, the 12-month complete time series criterion was met for 2,410 parties (606 distinct firms). The 12-month window is chosen as a compromise between event study time series length and sample size, given the presence of data gaps in the monthly VAT filings. We further exclude all case parties that are banks or insurance companies (788 parties, for 24 distinct banks/insurance companies) to avoid assigning excessive weight to sectors where court cases are a frequent occurrence of regular operations and cannot be expected to affect revenues.

[6]These observations remain in our analysis sample, contributing to the estimation of (month $\times$ year) fixed effects to improve the precision of our event study and difference-in-differences estimates. Note that 6,138 of the 14,713 firms in the universe of monthly revenue data that were not party to a case did not meet the data criteria.

[7]Tracking of these firms was done through a combination of court records, name, and tax ID merged with a national registry of firms operating in Senegal which contains contact information fields (*Répertoire National des Entreprises et Associations*, RNEA), and searches in public address books and web search engines.

[8]Another 133 firms were found not to exist anymore and the remaining 743 did not have sufficient contact information.

of our results to differential attrition in the firm survey sample in Section 6.3. By order of preference we interviewed the CEO, legal counsel, or another suitable respondent. We surveyed a range of perceptions of the justice system, asked them to report the economic costs associated with having an ongoing case, and elicited their willingness to pay for judicial services associated with faster pre-trial proceedings.

4 The 2013 pre-trial reform

The legal reform we study aimed to increase judicial efficiency of the pre-trial trial procedure and, in doing so, improve the business and investment climate (Ministère de la Justice, 2013).[9] The decree application was staggered across the six civil and commercial chambers of the Regional First Instance Court of Dakar between November 2013 and March 2014 (Figure 1). It modifies the civil and commercial procedure in two main ways: first, it sets a four-month limit on the pre-trial procedure; and second, it gives pre-trial judges the ability to "desk reject" a case based on poor evidence (*"irrecevabilité en l'état"*).[10] Before the application of the decree, only half of all civil and commercial cases completed pre-trial in four months or less, and the average case underwent 8.3 pre-trial hearings over a 157-day period (Table 2).

This new instrument of "desk-rejection" can only be exercised in the first pre-trial hearing, which precludes its use for ongoing cases. Similarly, judges were not obligated to apply the new deadline to ongoing cases. We use these features for our identification (together with the fact that judges belong to only one chamber), as we define cases that enter after decree application as treated, while those that entered before decree application serve as our comparison group in our event study. However, judges may also try to meet the new four-

[9]Decree n° 2013-1071 was ratified by the ministerial council on July 18, 2013 and published on August 6, 2013.

[10]Before the reform, such cases would undergo the pre-trial procedure to be then judged on the limited evidence.

month deadline for recently started ongoing cases, or may be unable to distinguish between cases started just before and just after the decree application date. This would yield some fuzziness in the effective application of the new deadline in a small window around the application date. We return to this in Section 5.

5 Impacts of the decree on judicial efficiency

5.1 Empirical strategy

Our empirical strategy describes variation in our main outcomes of interest (case duration, judge's behavior in hearings, quality of pre-trial and judgment) relative to the staggered introduction of Decree n° 2013-1071. Specifically, we exploit cross-sectional variation that stems from the fact that, while the decree was ratified in July/August 2013, it was applied at different times across the six civil and commercial chambers of the Regional First Instance Court of Dakar. The staggered application began in early November 2013 and reached full coverage in late March 2014 (Figure 1).[11]

We use high-frequency data around the multiple decree application cutoffs and two years of pre-intervention data to identify the causal effect of the reform in a flexible event study

[11] A seventh chamber, the 2^{nd} civil chamber, closed in July 2013, before the decree was published (Figure 1). It does not contribute to the event study design, for two reasons. First, we do not know when the decree would have been introduced in that chamber. Consequently, there is no straightforward way to assign its pre-reform cases an entry period relative to decree application (see event study specification below). Second, we do not know which cases would have been assigned to this chamber, had it not closed. We check that this does not affect our conclusions by verifying the nature of the caseload assigned to this chamber over our study period. One main source of worry would be that cases in the 2^{nd} civil chamber had a systematically faster pre-trial than in the rest of the court in the pre-period. Hence, excluding these cases would make the pre-period artificially slow. A simple means comparison over the pre-period indicates that this is not the case, as pre-trial for cases in the 2^{nd} civil chamber lasted on average 163 days compared to 157 in our study sample in the pre-period. A further concern about the closing of the 2nd civil chamber a few months before decree application started (and of the 4th commercial chamber a month after full coverage was reached, Figure 1) may be a negative externality on cases in the other chambers due to case transferrals from those closing. We note that first, our event study graphs (see results below) do not show a worsening of outcomes at the time of closure of these chambers. Second, such an externality would negatively affect the period during and after decree application and would thus go against finding a decree impact, in particular as throughout the post-decree period, the new incoming caseload had to be handled by fewer chambers.

design. If the reform had an impact on our outcomes of interest, we expect to see a structural change in those outcomes at the time of the reform's application. For example, we should see a sharp increase in the speed of adjudication for cases entering court right around the application threshold, relative to those that entered earlier. The high-frequency multi-year nature of the court data, together with the staggered reform application across chambers, allows us to attribute this change to the reform, net of seasonality and other structural changes (internal or external) to the court.

In line with this identification strategy, we estimate three main models to measure the impact of the decree on the speed and nature of court procedures. The first event study model verifies our main identifying assumption across all application cutoffs. Letting $\tau_{i,c}$ be, for case i in chamber c, the number of hearing periods since chamber c was treated when i entered court (normalized to zero at -4, the last hearing before the adjustment period), we estimate a flexible functional form that estimates one treatment effect per case-entry period, as follows:

$$y_{i,c} = \beta_{\tau_{i,c}} + \gamma_c + \theta_{month(i)} + \epsilon_{i,c} \tag{1}$$

We include cases starting their pre-trial between 22 months before the decree application date (the before period) and 12 months after (the after period).[12] If the reform had an effect, we expect to see a significant jump or drop in these dummy coefficients around $\tau = 0$. Estimating one treatment effect per entry period allows us to flexibly capture pre- and post-reform changes in trends. Bias from potential selection on level either in time of introduction (seasonality) or in chambers' order of introduction (chamber characteristics) is eliminated by calendar month of entry and chamber fixed effects ($\theta_{month(i)}$ and γ_c, respectively). Hence, each event study coefficient $\beta_{\tau_{i,c}}$ compares the outcomes of cases entering in hearing period $\tau_{i,c}$ to the outcomes of cases entering court in the reference hearing period (-4) within the

[12] We construct the same time window around each of the chamber-level decree application dates: 38 pre-decree application and 21 post-decree application hearing periods (periods zero to 20 relative to decree application).

same chamber and controlling for calendar month of entry. Standard errors are clustered at the level of case assignment to treatment (chamber×period of entry level).

Appendix Figure A3 confirms our identification strategy: in each panel, we plot the uncontrolled event study coefficients on pre-trial duration around each individual chamber decree introduction cutoff (normalized to zero at -4); we display vertical lines on either side of the cutoff to visualize and motivate the [-3;2] adjustment period later used to estimate the average treatment effect of the decree.[13] The uncontrolled coefficients from each chamber display drops in duration at the chamber's cutoff, and only around that cutoff (Figure A3). This set of figures suggests no selection on trends, as the pre-period trends appear parallel. In Section 5.2.3 and Appendix B.1, we also consider selection on shocks and trend shocks as potential sources of bias.

Case treatment duration, one of our main outcomes of interest, is a censored variable. This is because not all cases were finished at the time of the final data extraction and, for a given period of entry, it is the duration of the longest cases that is missing. This censoring should only cause a negative trend in our dummy coefficients, and not a sudden drop at the cutoff. Nevertheless, we take duration censoring seriously and estimate a Cox proportional hazard model, combining the event study approach with survival analysis to estimate the effect of the reform on case duration, as follows:

$$h_{i,c}(t|\gamma_c + \theta_{month(i)}) = h_0(t)\exp[\beta_{\tau_{i,c}} + \gamma_c + \theta_{month(i)} + \epsilon_{i,c}] \tag{2}$$

$\beta_{\tau_{i,c}}$ is now interpreted as the impact of entering the court at τ on the hazard of exiting pre-trial stage, relative to a reference dummy with a hazard ratio of one.[14] Failure corresponds to exiting the pre-trial stage and coefficients below one imply a lower probability of exiting,

[13] Results including the adjustment period are reported in Tables A1 and A5; this does not affect any of the conclusions of the paper.

[14] In practice, we estimate the hazard rate $h(t)$, of a case exiting pre-trial at hearing period t, conditional on the same covariates as in (1). This approach corrects for censoring without being subject to selection bias, conditional on baseline (pre-reform) hazard rate $h_0(t)$.

and coefficients above one imply a higher probability of exiting.

Finally, we compute the average effect of the decree across all cutoffs, using the indicator function $\mathbb{1}\{\tau_{i,c} \geq 0\}$, allowing for different trends across the six chambers and introduction cutoffs.[15] For this, we estimate the following model:

$$y_{i,c} = \beta \mathbb{1}\{\tau_{i,c} \geq 0\} + \delta_{1,c} \times \tau_{i,c} + \delta_{2,c}\mathbb{1}\{\tau_{i,c} \geq 0\} \times \tau_{i,c} + \gamma_c + \theta_{\text{month}(i)} + \epsilon_{i,c} \tag{3}$$

where β is the effect of the decree, and $\theta_{month(i)}$ and γ_c are calendar month and chamber fixed effects, as before. $\delta_{1,c} \times \tau_{i,c}$ is a chamber specific trend, and $\delta_{2,c}\mathbb{1}\{\tau_{i,c} \geq 0\} \times \tau_{i,c}$ allows for different slopes across each chamber cutoff. While our preferred estimates exclude an adjustment period of three hearings on either side of each cutoff to retrieve a measure of the impact of the decree net of short-term adjustments, we also present all main results including the adjustment period and show this choice does not affect our main conclusions (Tables A1 and A5). We cluster our standard errors at the (chamber×period of entry) level; to account for potential serial correlation within chamber for a small number of chambers, we also report standard errors from a six-point wild cluster bootstrap following Cameron & Miller (2015).

Our analysis sample consists of all cases that entered the court between January 2012 and February 2015, thus allowing all cases four months to complete the pre-trial stage, as hearing outcomes and final decisions are recorded until the end of June 2015 in our data. This yields an analysis sample of 5,297 cases.[16] Decision-stage outcomes only apply to cases that reach this stage, and we allow all cases in our sample at least one month to complete the decision phase. For this, we restrict the analysis of decision-stage outcomes to cases finishing their pre-trial before June 2015. This yields a sample of 4,214 cases documenting decision-stage

[15] We also report results with common linear trends across chambers and find it does not affect our inferences.

[16] For specifications that exclude an adjustment period of three hearings on either side of the cutoff, we maintain an analysis sample of 4,795 cases, of which 2,671 are cases that had their first hearing before the decree was applied in their respective chamber (also referred to as pre-reform cases).

outcomes, of which 2,405 are pre-reform cases.[17]

5.2 Pre-trial delays

Did the reform affect the speed of pre-trials? We start by estimating our event study specification (1). Panel A, Figure 2, plots the pre- and post-decree hearing period coefficients. The results reveal a sudden drop in pre-trial duration for cases that entered a chamber close to the application cutoff in that chamber. The drop in pre-trial duration levels off three hearing periods after the cutoff. To provide an estimate of the average decree impact net of this adjustment period, we estimate (3) removing these six hearing periods, $\tau \in [-3; 2]$, from our sample. The results indicate an average 46.1 days reduction in pre-trial duration (p-value<0.01; col 1, Table 3) off of a 157 day baseline duration.[18] This is a large effect, on the order of 0.32 pre-reform standard deviations. Specification (3) allows for chamber-specific linear trends on either side of the cutoff. We obtain a remarkably similar point estimate (42.9 days reduction, p-value<0.01) when we assume a common linear trend across chambers on either side of the cutoff (col 1, Table A2), further ruling out selection on trends as a potential source of bias.[19]

Next, we reproduce the event study result, accounting for censoring in pre-trial duration.[20] We estimate the Cox proportional hazard model expressed in (2). Again, the event study specification exposes a clear jump in the hazard ratio of exiting pre-trial at the decree

[17] For decision-stage outcomes, 3,844 observations are used for specifications that exclude the adjustment period.

[18] Leaving the adjustment period in the sample does not affect our conclusions (Table A1).

[19] We present results fitting a common linear trend across chambers while allowing for a structural break at the cutoff, for all our main results (Tables A2, A3, A6, and A7). In addition to verifying the robustness of our results to various trend specifications, these models allow us to parsimoniously report a coefficient on these pre- and post-reform trends.

[20] This censoring is visible in Panel A, Figure 2, which displays a downward trend in the effect of the entry-period dummies on pre-trial duration as we move away from the cutoff in the post period. This is because for any late entry cohort, the longest-lasting cases are still ongoing and thus omitted from this sample. While censoring is present, the event study results in Figure 2 show a significant break from this pre-trend at the cutoffs. Similarly, the average effects show a large and significant treatment effect despite controlling for chamber-specific trends (and allowing these trends to be affected by the reform; cols 1 and 2, Table 3). Hence, we can credibly rule out that censoring explains the observed drop in pre-trial duration.

introduction cutoffs (Panel B, Figure 2). Estimating the average effect (3) indicates that the introduction of the decree significantly increased the hazard ratio of a case finishing pre-trial by 33.8% (p-value<0.01; col 2, Table 3). A similar size effect (32%) is obtained when assuming shared linear trends across chambers (col 2, Table A2).

The finding of a reduction in pre-trial duration is further supported by evidence of a similar jump in the likelihood of completing the pre-trial stage within the newly sanctioned four-month deadline (Panel C, Figure 2)—an outcome that is not affected by censoring. On average, the likelihood of meeting this deadline significantly increases by approximately 23.9 percentage points, a 49% increase (p-value<0.01; col 3, Table 3).

To further establish robustness, we check that these results qualitatively hold in each chamber. We display the average effect of the decree introduction on pre-trial duration and the likelihood of completing pre-trial stage within four months, estimating (3) at the chamber level (Panels A and B, Figure A4). The average effect within each chamber is within the confidence interval of the combined effect. We also report standard error adjustment through a six-point wild cluster bootstrap (Cameron & Miller, 2015) at the chamber level in the bottom panel of all results tables. Most of our inferences survive this adjustment, although we lose some precision.

To shed light on the impact of the reform across the distribution of delays, we compare distributions of pre-trial durations across the application cutoff. We report Kaplan-Meier survival estimates pre- and post-reform (Panel D, Figure 2). Figure A5 displays kernel densities of pre-trial delays across five hearing-period case cohorts (the vertical line indicates each cohort's sample means). These figures show that, after the decree is applied, cases in all ranges of the distribution see their pre-trial duration shift to the left. We also notice that the densities narrow post-reform, suggesting judges uniformly apply shorter timelines to all types of cases. We investigate specific sources of heterogeneity in Section 5.2.2.

5.2.1 Mechanisms

Did the reform increase the speed of justice by improving the efficiency of the procedure, or by increasing its intensity? We use our rich case and hearing-level court data to document the channels through which the decree affected procedural efficiency versus formalism at pre-trial stage. The results show that judges respond to the decree by increasing the decisiveness of the pre-trial proceedings. We make three main observations. First, cases are more likely to be desk-rejected or fast-tracked to deliberations (Panels A and B, Figure 3; cols 4 and 5, Table 3).[21] Second, judges schedule fewer hearings with no change in cases' likelihood of being heard (Panels C and D, Figure 3; cols 6 and 7, Table 3). Instead, we find judges are more likely to impose strict deadlines on parties requesting an adjournment (Panel E, Figure 3; col 8, Table 3). Overall, these results corroborate the notion that the decree led to actual efficiency gains at each step of the pre-trial procedure.

5.2.2 Heterogeneity

Did the reform similarly affect all case and judge types, or did it generate heterogeneous responses? We explore two dimensions of case and judge heterogeneity in the impact of the decree on procedural delays: case complexity, proxied by claim amount, and judge baseline speed.[22] Our results suggest that all case types and judge types responded to the reform by moving to a new equilibrium in which pre-trials are faster.

Investigating heterogeneity by case complexity, we first confirm that claim size indeed proxies for case complexity as it is associated with a longer, more complex pre-trial procedure before the reform (first row, Table 4). We then make three central observations. First,

[21] Again, assuming common trends across chambers does not change our point estimates (cols 4 and 5, Table A2).

[22] Table 2 shows substantial variation in case complexity. Among cases that include a payment claim, the average claim amount is CFA franc 71.5 million (approximately USD 135,000) and the median is CFA franc 8 million (approximately USD 15,000). We also observe substantial variation in judge-level speed by claim amounts (Figure A10).

the decree equally increased the speed of both small and large-claim cases (cols 1 and 2, Table 4). Second, claim size does not predict the rate at which cases are desk-rejected or fast-tracked after the decree, and we fail to detect a differential intensification of pre-trial hearings (cols 3–6, Table 4). Third, judges are more likely to impose stricter adjournment rules on the parties of larger cases after the decree, while the effect on smaller cases is small and imprecisely estimated (col 7, Table 4). We conclude that the decree improved judicial efficiency similarly for different types of cases, but to achieve these gains judges had to put more pressure on the parties of larger cases.

Testing for heterogeneity by judge baseline speed, we find no evidence of differential effects on pre-trial duration, desk rejection, fast tracking or stricter adjournment rules imposed on parties (cols 1–4 and 8, Table 5). Fast judges are significantly less likely to increase pre-trial likelihood of being heard relative to slow judges, although neither type of judge significantly responds to the decree at that margin (col 6, Table 5). This supports the notion that judges do not manipulate delays for their private gains.

5.2.3 Robustness of the court event study design

Our main identifying assumption is that in the absence of the reform, the speed of justice would have followed a steady trend within each chamber. We consider identification threats coming from four types of selection issues: selection on level, selection on trend, selection on shock, and selection on trend shock. First, bias from potential selection on level is eliminated by the within-chamber differencing. Second, the event study graphs allow us to rule out selection on pre-event trends. We now consider selection on shock and selection on trend shock.

To test for selection on shock, we carry out placebo checks. We remove one chamber from our sample at a time, following the chronology of decree application, and run our main event study specification displaying all entry cutoffs. Figures A6–A9 display the four sequential sample restrictions; dashed lines indicate placebo cutoffs, which did not apply to the chambers remaining in the sample, while solid lines show actual application cutoffs. As expected, this significantly dampens the sharpness of the jumps after the initial cutoff, and we do not observe any significant jump in our main outcomes of interest at the dashed lines. This provides reassurance that selection of chamber and cases on shock is unlikely to bias our event study estimates. Judges sharply respond to the decree application by desk-rejecting cases only after their respective chambers become treated. We return to these placebo checks in Appendix B.1.

Finally, we consider the possibility of selection on trend shock. While this is harder to test in the data, it is also quite unlikely that the president of the tribunal could have anticipated which chambers or cases were likely to have disproportionately high or low growth and target case or chamber assignment based on this. The placebo checks discussed above tend to reject this hypothesis. We run two additional checks to rule out selection on trend shock. First, we compare results with (Table 3) or without (Table A2) the inclusion of chamber-specific time trends. Second, we analyze changes in potential determinants of trend shock selection, such as changes in volume and composition of the incoming caseload: size of the caseload, claim size, case complexity, case type, and the 2012 revenue of firms entering court (overall and as plaintiff and defendant separately). We provide further details on the rationale behind, and results of, these robustness checks in Appendix B.1. Overall, these results do not corroborate selection of time of decree application on anticipated trend shock as a source of bias.

Taken together, these results provide reassurance that the event study design produces unbiased estimates of decree impact.

5.3 Impacts on decisions

Although the reform focused on improving pre-trial procedural efficiency, it may have affected the decision phase in two ways: either through spillovers from the pre-trial reform (stronger/weaker cases make it to the decision stage) or displacement of judges' effort. We use our rich case-level court data to shed light on these effects.

5.3.1 Effort displacement across pre-trial and decision

One possible unintended impact of the reform is that judges' zeal in pre-trial displaced attention away from their deliberations. This could lead to an increase in both duration and number of decision hearings. We do not identify a significant jump in the duration of deliberations (Panel A, Figure 5; col 1 Table 6), the hazard ratio of completing deliberations (Panel B, Figure 5; col 2, Table 6), or the likelihood of completing this stage within one month (Panel C, Figure 5; col 3, Table 6). Additionally, cases that entered a chamber after the decree did not, on average, experience a different number of decision-stage hearings (Panel D, Figure 5; col 4, Table 6). Similarly, we see no jump in the probability of a case being heard at any scheduled hearing over the course of the decision phase (Panel E, Figure 5; col 5, Table 6). Finally, pooling across pre-trial and decision phases, we see a jump in the hazard ratio of completing the overall case (Panel F, Figure 5).

5.3.2 Quality of pre-trial and decisions

We then investigate a potential quality-speed trade-off. As discussed above, the pre-trial procedure aims to prepare a case for judgment in the decision phase of the trial. We capture quality of the pre-trial along one dimension (completeness of the evidence brought forward) and quality of the judgment along two dimensions (judges' documentation of the decision, and parties' intention to appeal the decision).[23]

First, we assess completeness of the evidence by looking at the incidence of two decision hearing outcomes: pre-trial declared insufficient (and case sent back to pre-trial) and post-

[23] We additionally consider potential changes in judges' workload around decree introduction and conclude that the reform does not seem to have led to a meaningful increase in judges' backlogs (Appendix C).

ponement of the decision. To the extent that the deliberations are done collegiately, whereby each case is reviewed by all judges in the chamber, these decision hearing outcomes offer a plausible measure of pre-trial quality. Panel A, Figure 6 indicates no discernible jump in the probability that a case gets sent back to pre-trial after the introduction of the decree. This is corroborated by a small and statistically insignificant average effect coefficient (col 1, Table 7). Similarly, we find no significant change in the likelihood that a decision is postponed (Panel B, Figure 6; col 2, Table 7). For both outcomes, there is no change in trend across the decree application cutoff.

Second, we estimate the impact of the reform on the length of the written decision justification (within the judgment) and the number of articles cited. Again, we fail to detect any impact of the decree on these outcomes both through the event study and average effect estimations (Panels C and D, Figure 6; cols 3 and 4, Table 7).

Finally, an important measure of quality of a first-instance judgment is the probability that the decision gets appealed (Coviello et al., 2014). Again, we fail to detect an impact of the reform on parties' intention to appeal, both in the event study design and on average across the introduction cutoffs (Panel E, Figure 6; col 5, Table 7).

Taken together, these results suggest that accelerating the pace of the pre-trial procedure did not displace judges' attention away from deliberations and did not lead to a decline in the quality of either the evidence or the legal justification. Implicitly, these results also bring reassurance that cases did not select into court around the time of the decree based on the legal merits of their claims, further corroborating robustness checks discussed in Section 5.2.3.

6 Firm-level impacts

6.1 Event study of pre-trial entry and speed

6.1.1 Empirical strategy

Merging court data with tax administration records, we estimate a party-level event study model to capture the evolution of firms' revenues after they enter pre-trial. Letting $y_{i,f,t}$ be log monthly revenues at time t for firm f that is party to case i, and $\xi_{i,f,t}$ the number of months since f's case i started pre-trial (normalized to zero at -1), we estimate a flexible functional form that estimates one treatment effect per month relative to pre-trial start, as follows:[24]

$$y_{i,f,t} = \beta_{\xi_{i,f,t}} + \alpha_{i,f} + \theta_t + \epsilon_{i,f,t} \tag{4}$$

As described in Section 3.2, we study a time window that includes the last six months before the start of the pre-trial and the first six months after. θ_t and $\alpha_{i,f}$ are time (month$\times$year) and party fixed effects. If entering pre-trial is associated with a worsening of business activity, we expect to see a significant drop in these dummy coefficients around $\xi_{i,f,t} = 0$.[25]

We also estimate the following difference-in-differences specification to provide a magnitude of the change in firm log revenues after the start of pre-trial and test for heterogeneity with respect to pre-trial speed:

$$y_{i,f,t} = \mathbb{1}\{\xi_{i,f,t} \geq 0\} W'_{i,f}\beta + \alpha_{i,f} + \theta_t + \epsilon_{i,f,t} \tag{5}$$

[24]This regression is run at the party level. Hence, a firm appears in the data as many times as it has cases over the study period. We adjust our inference accordingly, clustering standard errors at the firm level. Firms that do not have cases but have filed VAT over the study period (see Section 3.2) appear only once, and only contribute to the estimation of the (month $\times$ year) fixed effects.

[25]As noted in Section 3.2, we discuss robustness of all party and firm-level results to differential attrition in Section 6.3.

where $\mathbb{1}\{\xi_{i,f,t} \geq 0\}$ is the indicator function that takes value 0 before pre-trial starts and 1 after pre-trial starts. We specify the following three models. First, we estimate a base difference-in-differences specification to provide a magnitude of the change in firm log revenues after the start of pre-trial, and $\mathbb{1}\{\xi_{i,f,t} \geq 0\}W'_{i,f}$ is just the indicator function for having started pre-trial (Model 1). Second, we test whether faster pre-trials (less than four months, Fast pre-trial) are associated with smaller reductions in firm log revenues after the start of pre-trial. In this specification, $\mathbb{1}\{\xi_{i,f,t} \geq 0\}W'_{i,f}$ is now the vector $(\mathbb{1}\{\xi_{i,f,t} \geq 0\} + \mathbb{1}\{\xi_{i,f,t} \geq 0\} \times \text{Fast pre-trial}_{i,f})$ (Model 2). Finally, we estimate the elasticity of firm revenues to pre-trial duration. In this specification, $\mathbb{1}\{\xi_{i,f,t} \geq 0\}W'_{i,f}$ is a scalar that takes value 0 if pre-trial has not started, and pre-trial duration (in 100 days) if pre-trial has started, $\mathbb{1}\{\xi_{i,f,t} \geq 0\} \times \text{Pre-trial duration}_{i,f}$ (Model 3). In all three models, we control for time (month $\times$ year) fixed effects (θ_t), and party fixed effects ($\alpha_{i,f}$).

To purge our estimates for Models 2 and 3 of equation (5) of potential simultaneity bias, we exploit the exogenous shift in the speed of pre-trial completion caused by the application of the decree described in Section 5, and estimate them via 2SLS using the following first stages:

$$\begin{aligned} &\mathbb{1}\{\xi_{i,f,t} \geq 0\} \times \text{Speed of pre-trial}_{i,f} \\ &= \gamma_1 \mathbb{1}\{\xi_{i,f,t} \geq 0\} \times \text{After decree}_{i,f} + \alpha_{i,f} + \theta_t + \eta_{i,f,t} \end{aligned} \tag{6}$$

where Speed of pre-trial is alternatively Fast pre-trial (Model 2) or Pre-trial duration (Model 3). For this IV estimate to be consistent, we need a first stage ($Cov(\mathbb{1}\{\xi_{i,f,t} \geq 0\} \times \text{Speed of pre-trial}_{i,f}, \mathbb{1}\{\xi_{i,f,t} \geq 0\} \times \text{After decree}_{i,f}) \neq 0$; Panel C, Figure 2 and col 4, Table 8) and we need to assume exogeneity of the instrument ($Cov(\epsilon_{i,f,t}, \mathbb{1}\{\xi_{i,f,t} \geq 0\} \times \text{After decree}_{i,f}) = 0$), that is, that the decree application did not affect short-term firm outcomes through other channels than the increase in probability of cases completing pre-trial in under four months.

6.1.2 Results

Do firms experience an economic slowdown at the outset of a court case? Estimating (4) produces striking results (Panel A, Figure 7): entering pre-trial is associated with a sharp reduction in firm revenues as reported in their VAT filing for the month immediately following the first pre-trial hearing (Figure 7). One concern with the interpretation of this finding is that a firm could end up in court as a result of its economic struggles. However, results from the event study support a causal interpretation of this drop in economic activity, as the absence of pre-trends rejects such reverse causality.[26] Similarly, difference-in-differences estimates from (5) suggest an 8.2% average monthly decline in firm revenues for the first six months after entering court (p-value < 0.01; col 1, Table 8).[27]

Do gains in the speed of pre-trial reduce these negative firm-level impacts? We start by showing that the pre-trial duration mandated by the reform is associated with lower economic losses for firms. Interacting $\xi_{i,f,t}$ with a dummy for completing pre-trial in under four months (Fast pre-trial) in the event study specification (4) shows that the adverse impact of starting pre-trial is concentrated among firms whose cases spend more than four months in these hearings (Panel B, Figure 7). The event study for slower pre-trials is now sharper, while the revenue of firms experiencing faster pre-trials does not seem affected. Estimating our difference-in-differences specification (5) with OLS confirms this: while having a case stuck in pre-trial for over four months is associated with an average 11.5% reduction in monthly firm revenue over the first six months following the start of the pre-trial, completing it in under four months is associated with a significantly smaller penalty (the point estimate of a 4.4% reduction for faster pre-trials is statistically insignificant; p-value of the difference $<$ 0.1; col 2, Table 8). The OLS estimate of the elasticity of firm revenues to pre-trial duration

[26]Robustness is further discussed in Section 6.3.

[27]We split the sample between plaintiff and defendant and estimate a 6% and a 10.4% average monthly decline in firm revenues, respectively (not reported; Figure A13 plots the event study coefficients from estimating (4) separately across sides); we fail to reject that the impact is similar across sides of the dispute (p-value = 0.30). Thus we pool across sides for power.

suggests an average drop in monthly revenues of 3.2% for every 100 days spent in pre-trial (p-value < 0.01; col 6, Table 8).[28]

A sharp impact of starting pre-trial concentrated on firms who go on to experience slower pre-trial is consistent with the procedure (Section 2) as well as the channels of the decrees impact on pre-trial speed (Section 5.2.1). The first pre-trial hearing is the occasion at which the judge sets the tone for the pre-trial and puts forth a roadmap; the parties learn whether audits and expert reports will be commissioned, and whether this will entail a lengthy process of submission of additional evidence and arguments over a number of hearings. Firms are thus able to form an expectation over the length of the pre-trial at the first hearing and adjust their operations in response. Short pre-trials are less likely to require much additional work on the side of the firm. In the case of a longer pre-trial, the management of audits and reports and preparation of additional arguments and evidence will require allocating managerial capacity away from day-to-day operations.[29] This would work to immediately and linearly hurt firms revenues. In line with this mechanism, our investigation into the channels of the decree's impact on pre-trial speed highlighted that judges scheduled fewer hearings and were more likely to impose strict deadlines during the pre-trial.[30]

Finally, responses in our firm survey as to the implications of having a case in court support the proposed mechanism through re-allocation of managerial capacity (Table A8). Indeed, nearly all firms (96–98%) report losses in terms of their own or employees' work hours; all qualify these losses as large, and those able to quantify them estimate their value at approximately CFA franc 1.3 million (approximately USD 2,450). A further potential mechanism hinted at in our survey data is an immediate cash flow problem affecting firms day-to-day

[28]Pre-trial duration is a censored variable in our dataset, as not all cases completed pre-trial within our study period; Fast pre-trial is not censored as we observe all cases for at least four months after the start of pre-trial. We establish robustness of our estimates for Models 1 and 2 to this sample selection (Table A9.

[29]This may be compounded by the time lost attending the hearings themselves, given that longer pre-trials are associated with more hearings; attending these can take most of the day, as the timing of a specific case within the chambers hearing date is not communicated in advance.

[30]We may expect that they would have manifested the intention to be strict in the first hearing, to maintain their credibility.

operations. 20–24% of firms report having had to take on debt because of their most recent court case as plaintiff and/or defendant (although the survey did not ask for the precise reason or timing). Firms estimate the direct costs of lawyer and court fees at CFA franc 1.755–3.386 million (approximately USD 3,310–6,390). Finally, our survey also suggests longer-term effects on firms, with 33–40% of respondents reporting that having the case prevented them from making investments, which would, however, be expected to affect revenues beyond our six month study window.

Despite these plausible mechanisms, our OLS findings discussed above could be explained by other factors, for example, firms whose disputes with business partners are less serious may experience smaller economic losses and the court may at the same time be able to resolve these disputes more speedily. We thus turn to our 2SLS specifications. Estimating the first stages (6) reproduces the results presented in Section 5, as starting pre-trial after the decree is applied substantially reduced pre-trial duration and increased the probability of completing it within the legislated four-month deadline (cols 5 and 8, Table 8).[31] Despite a relatively precise first stage, estimating the reduced form impact of entering after the decree application yields a small, imprecise coefficient (col 4, Table 8). While it follows that the 2SLS estimation of the impact of finishing pre-trial in under four months produces imprecise results, we note that the point estimates are very close to the OLS coefficient (col 2, Table 8), suggesting modest bias in the OLS estimate. The 2SLS estimation of pre-trial duration on firm revenues is more precise (p-value < 0.01) and close to the OLS estimate, suggesting that for every 100 days a case spends in pre-trial firm monthly revenues declines by on average 5% over the first six months following the start of the pre-trial.[32]

[31] The point estimate on Fast pre-trial appears smaller than in Section 5, while the point estimate on Speed of pre-trial is larger; this is not due to selective attrition, as we show in Section 6.3, but to specification changes imposed by having different variables across event studies.

[32] We also estimate an ANCOVA specification, adapting (5) and (6) controlling for log monthly firm revenues in all six months before pre-trial start. Our results are robust to this specification (Table A10) although less precise. Moreover, the first stages provide some reassurance to the interpretation of our findings, as revenue in the months preceding entry into court do not explain having a faster pre-trial (cols 3 and 7, Table A10).

6.2 Legal costs and perception of faster justice

We complement this evidence from firms' monthly tax records with primary firm survey data to shed light on the net benefits of faster justice on firms. We proceed in two steps. First, our survey instrument asks firms to evaluate the costs of legal services associated with shorter delays. We present two scenarios of pre-trial delays, using our empirical estimates of the average reform impact. First, the firm is told it should hire a lawyer to resolve a dispute of a median amount. Two types of lawyers are available: one who can reliably complete pre-trial proceedings in 5 months (the average pre-reform speed); and one who can reliably complete them in 3.5 months (the average post-reform speed). We then ask the firm, in an open-ended manner, how much they would be willing to pay each lawyer. The kernel densities corresponding to each response are shown in Figure 8. We find that firms unanimously report being willing to pay more for a faster lawyer, an average of CFA franc 853,522 (approximately USD 1,610), relative to a lawyer performing at pre-reform speed, for which they would pay CFA franc 559,462 (approximately USD 1,056). The mean difference of CFA franc 294,060 (approximately USD 555) represents a 50% increase over willingness to pay for baseline speed, and is significant at the 1% level.

Second, we exploit the fact that some firms only had court cases before the decree was applied, while others had one or more court cases after, to document before-after changes in firms' perceptions of the justice system around the decree introduction. For robustness, we present results on two samples: all surveyed firms, and a sub-sample of firms that had only one court case (Table 9).[33] We make three central observations. First, firms' perceived duration and lawyer costs did not change significantly across the decree application (cols 1–4, Table 9). Second, we discern a small, imprecisely estimated difference in hypothetical future

[33] Using the former sample, we compare firms that only had court cases before the decree was applied with those that had at least one case after. Using the latter sample, we compare—among firms that only had one case—those whose case was before decree application with those whose case was after. In Table 9, uneven columns report results on the full sample, and even columns report results on the restricted sample.

use of the court for commercial disputes across the decree application cutoffs (cols 5 and 6, Table 9). Third, firms that underwent legal disputes after the reform have, on average, a more positive perception of the justice system (cols 7 and 8, Table 9). Taken together, these results suggest net positive benefits of the reform on firms.

6.3 Robustness of the firm-level results

We now discuss robustness of the firm results along two dimensions: robustness of the event study of starting pre-trial, and robustness to attrition in the tax records and firm survey relative to the court sample.

As discussed in Section 5.2.3, our main identifying assumption in estimating the impact of entering pre-trial on firm revenues is that this event is the main source of variation in firm revenue in the year around pre-trial start and that, in the absence of a court case, firm revenues would have followed a steady trend, conditional on party and (month $\times$ year) fixed effects. Hence, we consider threats to identification from four classes of selection issues: selection on level, selection on trend, selection on shock, and selection on trend shock. First, bias from potential selection on level is eliminated by the within-party differencing. Second, the event study graph allows us to rule out selection on pre-event trends, conditional on party and (month $\times$ year) fixed effects. Third, selection on shock or selection on trend shock would introduce bias. Such bias would require that, for instance, the plaintiff targeted whom to sue and the timing of the case based on own or the defendant's economic performance, or on the expectation of a differential change in own or the defendant's growth. If this were true, we would likely observe different trends before starting the pre-trial across plaintiffs (who can influence case timing) and defendants (who cannot), which is not the case (Figure A13). Taken together, these results provide reassurance that the event study design produces an unbiased estimate of the impact of starting pre-trial.

Next, we test for differential attrition of firms across our court data (court-only) and the matched tax records and firm survey samples described in Section 3.2. We perform two attrition balance tests. First we look for balance on baseline case and firm characteristics across samples (Table A11). Reassuringly, we find no significant difference in the share of court cases a firm has after the decree across court-only and tax and survey samples (Panels B and C). However, the average firm in both the tax records and survey samples has higher baseline revenues and a larger number of cases than the average court-only firm (Panels B and C), and parties included in party-level analyses using the tax records sample have significantly smaller claim amounts (Panel A). These imbalances suggest care is needed in extrapolating our firm-level results to the full sample of firms having experienced a court dispute over our study period.

Second, we look for differential attrition across samples based on (in-court) decree impact. In Table A12, we replicate our main within court case-level analysis (presented in Table 3) first at the party level, testing for differential reform impacts across whether or not a party is in the tax records sample (cols 1–3), and then at the firm level, testing for differential impacts across whether or not a firm is in the survey sample (cols 4–6). In both of these specifications, we fail to reject the null hypothesis that firms appearing in either the tax records or the survey sample experience the same decree impact, in terms of speed and quality, as those that do not appear. These results suggest no selective attrition into the tax records and firm survey samples on decree impacts.

7 Conclusion

In this paper, we leverage a combination of case-level court data, monthly firm tax records and primary firm survey data to show that a simple procedural reform can have large impacts on the speed of justice, and that such efficiency gains can translate into meaningful economic impacts. A decree that imposed a new four-month deadline on the pre-trial, a key phase in the Senegalese civil and commercial court procedure, reduced its duration by 46 days relative to the pre-decree mean of 157 days. In other words, imposing a reduction in delays by one day reduced mean delays by 1.24 days (46 days / [157 days - 120 days]). These large gains in speed are not due to procedural intensification and do not affect quality. Using tax filings, we show that bringing pre-trial duration under four months has substantial economic impacts on firms involved in court disputes. Firms with longer pre-trials experience an average 11% decline in monthly revenues over the six months following the start of the pre-trial, while shorter pre-trials are associated with a statistically insignificant 4% decline. Using the decree to instrument for speed of treatment we show that every 100 days a case spends in pre-trial causes a significant 3.2-5.0% average monthly decrease in firm revenues. Evidence from our firm survey reinforces these findings: firms are willing to pay higher legal fees in order to secure the speed gains realized by the reform, and perception of the justice system may have improved after the decree, suggesting net positive benefits of the reform on firms.

These results contribute direct, micro-level evidence on the link between legal reform, court efficiency, and economic performance, complementing a literature that has relied on more aggregate variation (Djankov et al., 2003; La Porta et al., 2008; Chemin, 2018; Alencar & Ponticelli, 2016; Lilienfeld-Toal et al., 2012). There are two central contributions. First, this study shows that simple procedural changes can meaningfully combat court inefficiencies in contexts of high legal formalism, such as Senegal. The richness of our court data allows us to establish the direction of causality and to detail the mechanisms at play, namely a reduction in formalism. Second, this is the first study to formally document a direct, causal link between case-level judicial efficiency and firm-level outcomes.

Finally, this paper motivates a focus on bureaucratic efficiency as a development outcome. Inefficient bureaucracies withhold services from the poor (Finan et al., 2017) and increase trade costs (Djankov et al., 2010). Slow, backlogged courts prevent the application of financial reforms such as bankruptcy law reforms (Alencar & Ponticelli, 2016). Combining detailed, high-frequency administrative records to evaluate the causal impact of reforms on bureaucratic efficiency is a fruitful area for future research.

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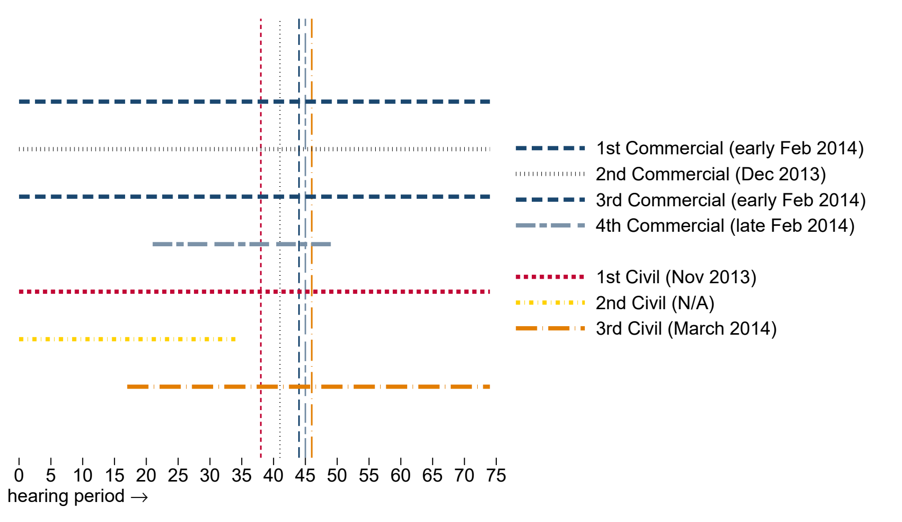
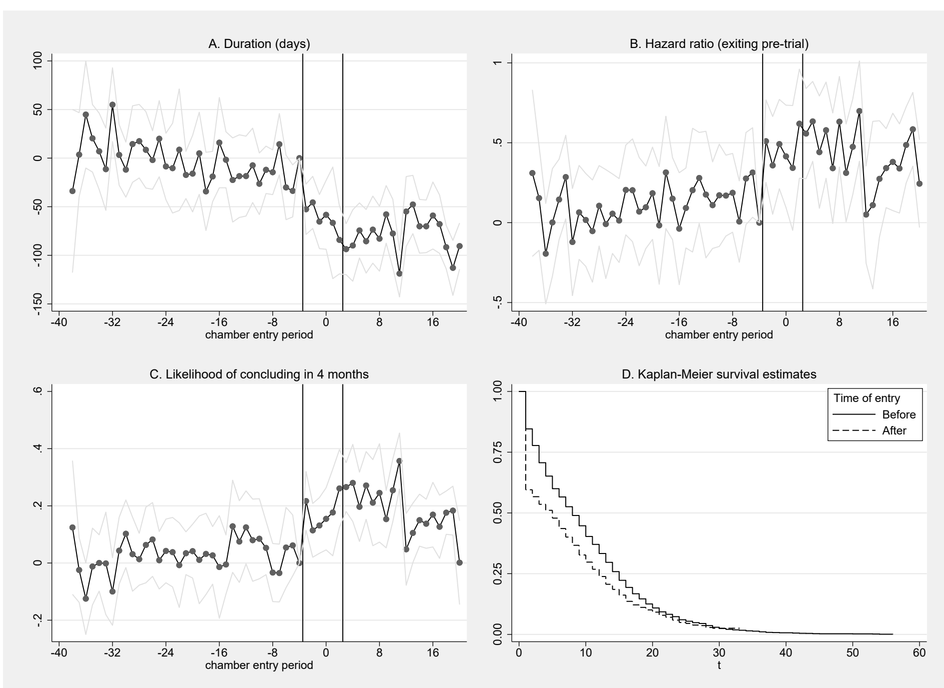
Figures

Figure 1: Timeline of decree introduction and chamber dynamics over the study period



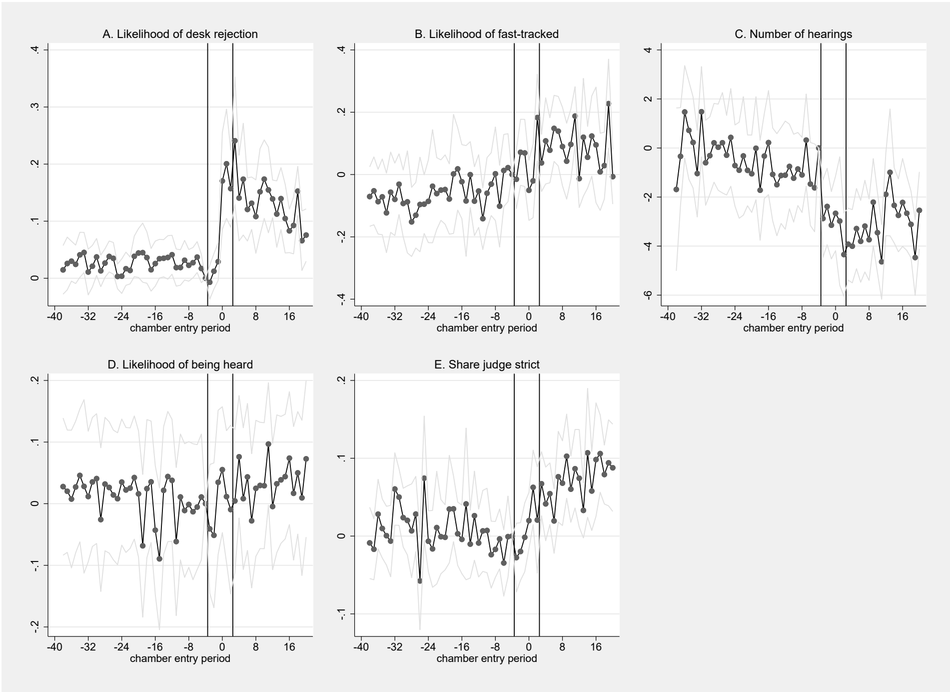
Notes: The x-axis presents hearing periods, 0 being the first hearing in January 2012, with hearings scheduled twice a month. Vertical lines indicate the decree application timing in the different chambers (cf. legend, dates in brackets). Horizontal lines show the chamber operating timelines. The 1st to 3rd commercial and the 1st civil chamber were operational through the study period (January 2012 to June 2015); the 4th commercial chamber opened in January 2013 and closed at the end of April 2014; the 2nd civil was operational at beginning of the study period and closed at the end of July 2013; the 3rd civil was opened in November 2012 and remained operational at the end of the study period.

Figure 2: Impact on pre-trial delays



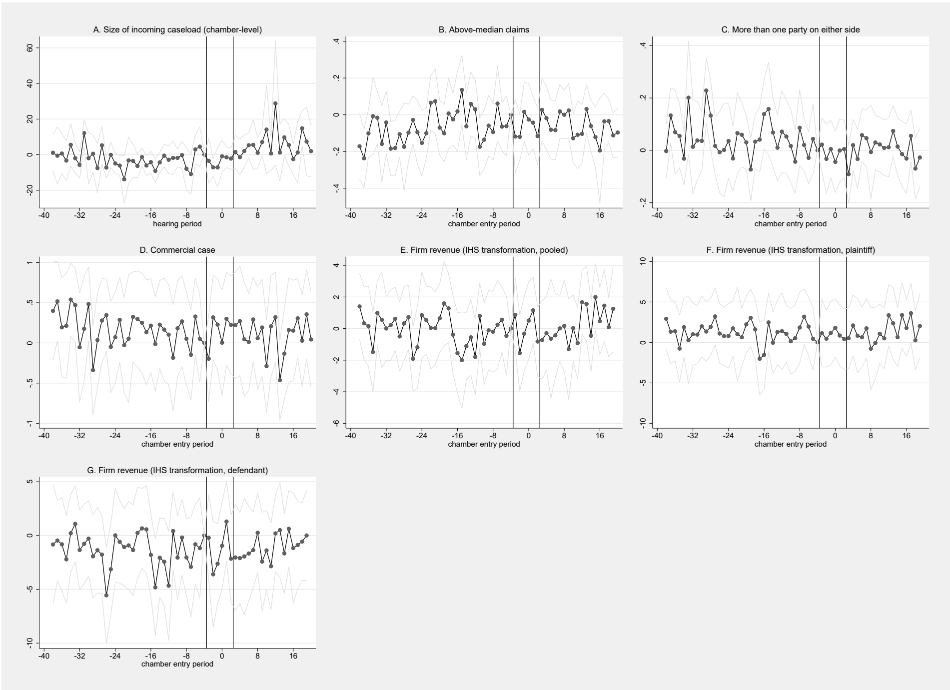
Notes: For x-axes in panels A-C, period is indexed in relation to chamber-level decree application (zero-centered), and the gray lines represent the 95% confidence interval.

Figure 3: Channels of impact on pre-trial delays



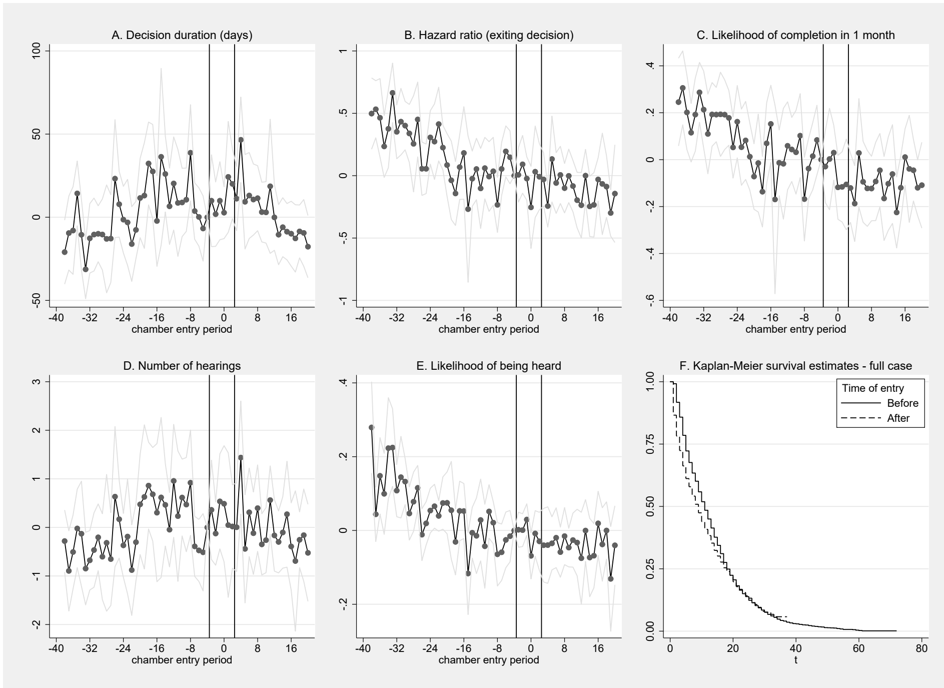
Notes: For all x-axes, entry period is indexed in relation to chamber-level decree application (zero-centered). The gray lines represent the 95% confidence interval.

Figure 4: Volume and composition of the incoming caseload



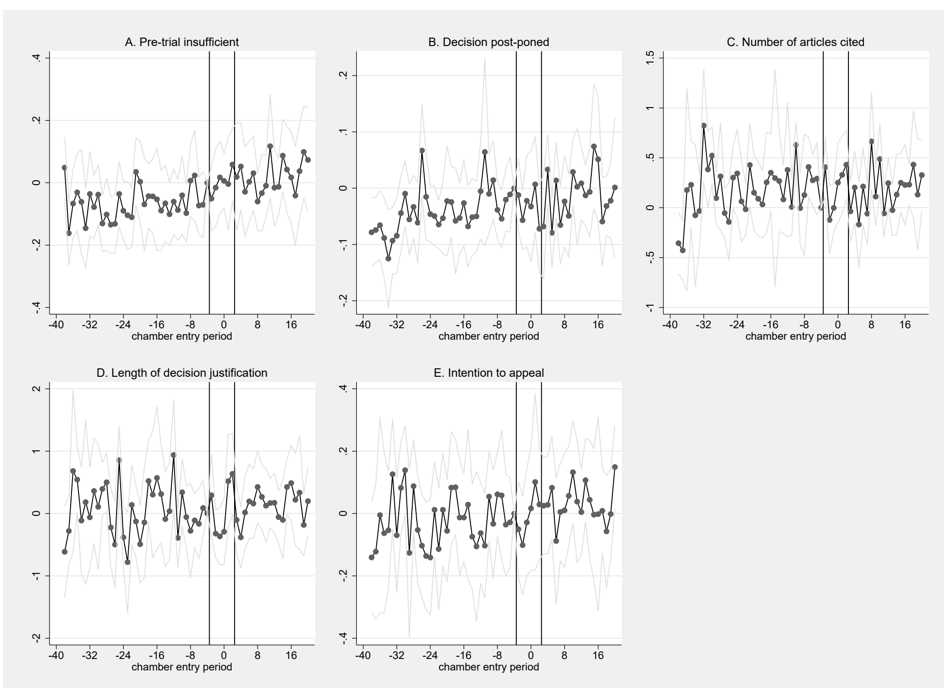
Notes: For all x-axes, period is indexed in relation to chamber-level decree application (zero-centered). The gray lines represent the 95% confidence interval.

Figure 5: Impact on decision stage



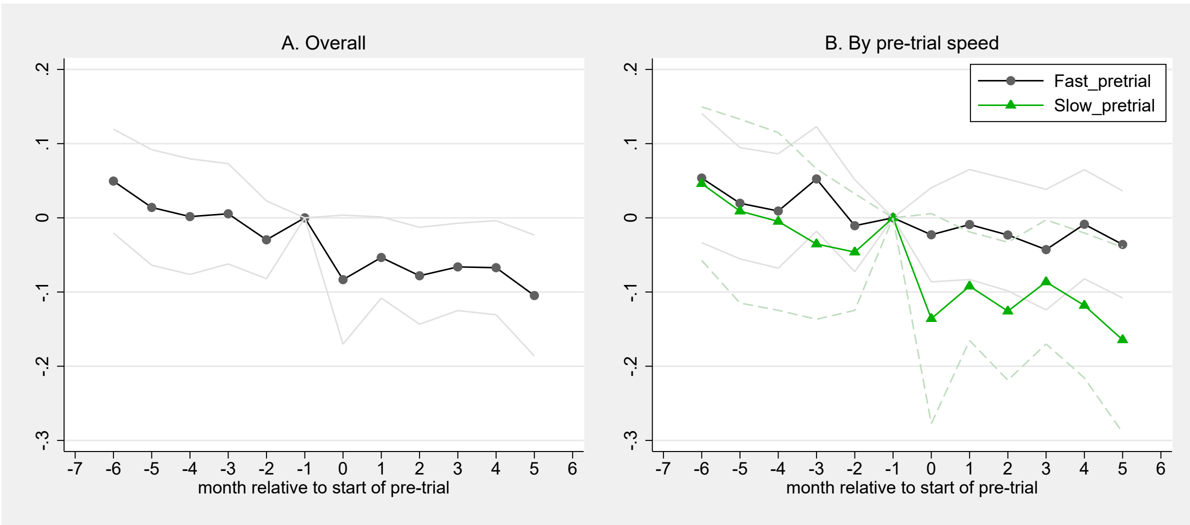
Notes: For x-axes in panels A-E, period is indexed in relation to chamber-level decree application (zero-centered), and the gray lines represent the 95% confidence interval.

Figure 6: Impact on quality



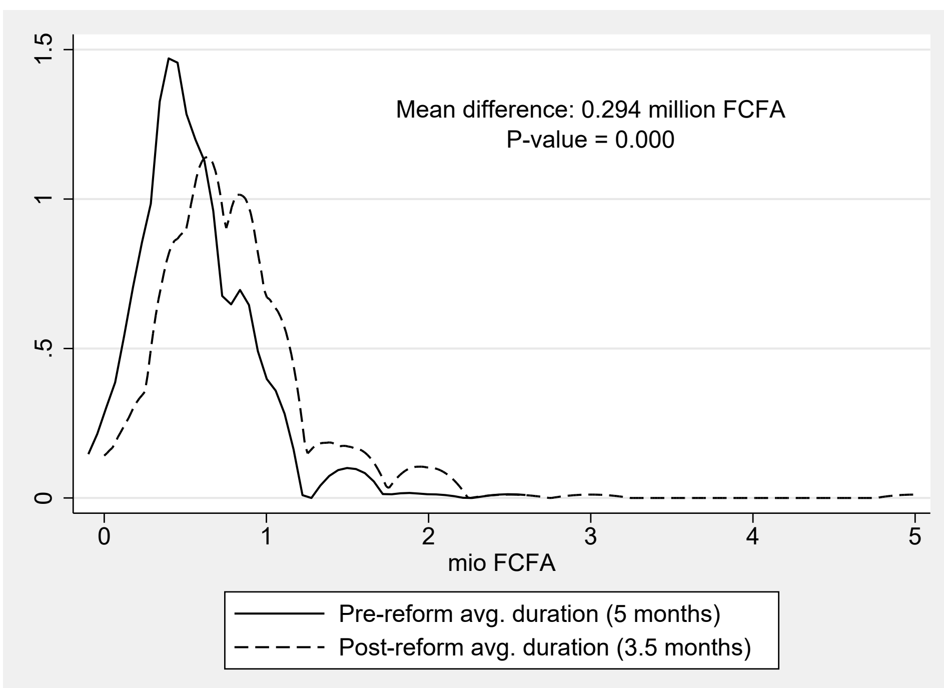
Notes: For all x-axes, period is indexed in relation to chamber-level decree application (zero-centered). The gray lines represent the 95% confidence interval.

Figure 7: Case parties' log revenues around the start of the pre-trial, overall and by speed of pre-trial.



Notes: For all x-axes, month is indexed in relation to the case-level pre-trial start month (zero-centered). In panel B, Fast-pretrial and Slow-pretrial log revenue time series are each displayed relative to their reference period (period -1). The gray lines represent the 95% confidence interval for the Fast-pretrial time series and the dashed lines represent the 95% confidence interval for the Slow-pretrial time series.

Figure 8: Firm's willingness to pay for lawyer fees to achieve pre- vs. post-decree speed (densities)



Notes: kernel = epanechnikov, bandwidth= 0.0990

Tables

Table 1: Chamber-level caseload summary statistics

		1st Commercial	2nd Commercial	3rd Commercial	4th Commercial	1st Civil	2nd Civil	3rd Civil
Average number of incoming cases per hearing	2012	11.0	13.5	18.7	.	13.3	13.7	12.3
	2013	11.5	13.4	12.0	13.2	14.6	4.9	15.7
	2014	21.2	19.2	24.4	9.1	19.0	.	23.9
	2015	19.5	21.8	26.8	.	15.1	.	25.8
Average number of ongoing cases in pre-trial	2012	142.9	188.9	149.2	.	228.0	166.7	37.0
	2013	116.0	208.7	109.3	63.2	195.8	85.4	89.7
	2014	151.8	200.8	140.0	69.3	156.3	.	119.1
	2015	178.0	269.6	163.8	.	154.0	.	136.0
Average number of ongoing cases in decision stage	2012	25.5	26.8	46.9	.	52.9	50.1	3.0
	2013	26.8	49.1	44.6	16.8	69.0	33.8	31.1
	2014	50.3	97.5	86.0	28.0	99.4	.	49.9
	2015	62.8	118.7	127.0	.	96.7	.	72.5

Notes: The table shows yearly descriptive statistics at the chamber level over the 2012/15 period. The first panel reports the average incoming number of cases per hearing. The second panel reports the average number of cases undergoing pre-trial stage per hearing. The third panel reports the average number of cases undergoing decision stage per hearing.

Table 2: Pre-decree summary statistics of civil and commercial caseload

	N	Mean	StD	Median	Min	Max
PANEL A: Case-level characteristics and outcomes						
Duration of pre-trial hearings (in days)	2665	156.94	146.02	126.00	0.00	980.00
Likelihood of pre-trial completion in 4 months	2671	0.49	0.50	0.00	0.00	1.00
Duration of decision stage (in days)	2380	63.12	82.70	29.00	14.00	761.00
Likelihood of decision completion in 1 month	2405	0.50	0.50	0.00	0.00	1.00
Final outcome: Judgment	2639	0.88	0.32	1.00	0.00	1.00
Final outcome: Settlement	2639	0.04	0.18	0.00	0.00	1.00
Final outcome: Other	2639	0.08	0.27	0.00	0.00	1.00
Case fast-tracked to decision stage	2671	0.14	0.35	0.00	0.00	1.00
Judge more strict (share)	2287	0.12	0.15	0.06	0.00	1.00
Number of pretrial hearings	2671	8.26	6.47	7.00	0.00	42.00
Number of decision stage hearings	2405	2.60	3.40	1.00	1.00	36.00
Pre-trial likelihood of being heard	2287	0.85	0.15	0.88	0.17	1.00
Decision stage likelihood of being heard	2405	0.77	0.25	0.88	0.17	1.00
Pre-trial insufficient	2405	0.12	0.32	0.00	0.00	1.00
Decision postponed	2405	0.05	0.23	0.00	0.00	1.00
Claim amount (in million FCFA)	1675	71.54	339.34	8.00	0.08	7,400.00
Number of plaintiffs	2541	1.23	1.54	1.00	0.00	38.00
Number of plaintiffs which are firms	2541	0.54	0.51	1.00	0.00	3.00
Number of plaintiffs which are private individuals	2541	0.69	1.68	0.00	0.00	38.00
Number of defendants	2541	1.32	1.06	1.00	0.00	22.00
Number of defendants which are firms	2541	0.58	0.63	1.00	0.00	11.00
Number of defendants which are private individuals	2541	0.65	1.07	1.00	0.00	21.00
More than one party on either side	2541	0.25	0.43	0.00	0.00	1.00
PANEL B: Party-level characteristics						
2012 revenues (in billion FCFA)	1992	21.81	81.05	2.52	0.00	720.06
2012 revenues (IHS transformation)	1992	20.44	6.35	22.34	0.00	28.00

Notes: Baseline summary statistics, for cases entering between 38 and 4 hearings before decree application. 2671 baseline observations for pre-trial and overall outcomes, except for rows 1, 5-7 (censoring), 9 (only for cases that have any adjournments), 12 (only for cases that have more than one hearing). 2405 baseline observations for decision stage outcomes, except for row 3 (censoring). Fewer observations for case characteristics 16-22 (not available for all cases). IHS transformation: Inverse hyperbolic sine transformation.

Table 3: Impact of the decree on pre-trial stage (with flexible trends)

	(1) Duration of pre-trial hearings (in days)	(2) Hazard ratio - finishing pre-trial	(3) Likelihood of pre-trial completion in 4 months	(4) Desk rejection	(5) Fast-tracked	(6) Number of pretrial hearings	(7) Pre-trial likelihood of being heard	(8) Judge more strict (share)
Entered after decree application (β)	-46.09***	1.34***	0.24***	0.17***	0.09**	-1.99***	0.03	0.06***
	(11.18)	(0.13)	(0.04)	(0.02)	(0.04)	(0.49)	(0.02)	(0.01)
P-value 6pWCBoot	0.07		0.03	0.02	0.16	0.04	0.42	0.05
Chamber FE x Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Without adj. period	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-mean	156.94		0.49	0.00	0.14	8.26	0.85	0.12
Pre-sd	146.02		0.50	0.00	0.35	6.47	0.15	0.15
R-Squared	0.22		0.14	0.08	0.08	0.15	0.21	0.08
Observations	4568	4795	4795	4795	4795	4795	3533	3537

Notes: Estimates of case-level impacts of the reform on pre-trial proceedings. Entered after decree application is a dummy that takes value 1 when a case entered its assigned chamber after the application of the decree. All models include chamber-specific linear trends allowed to change after the decree (full interaction of chamber FEs, trend, and Entered after decree application), calendar month fixed effects and a constant. All models estimated by OLS. Standard errors in parentheses, clustered at the (chamber x hearing of entry) level. Window includes cases entering between 38 and 4 hearings before and between 4 and 21 hearings after decree application. 4795 observations, except for col 1 (censoring), col 7 (only for cases that have more than one hearing), col 8 (only for cases that have any adjournments). Significance levels are denoted as follows: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 4: Differential impact of the decree on pre-trial stage by case difficulty (claim amount, fully flexible trends)

	(1) Duration of pre-trial hearings (in days)	(2) Likelihood of pre-trial completion in 4 months	(3) Desk rejection	(4) Fast-tracked	(5) Number of pretrial hearings	(6) Pre-trial likelihood of being heard	(7) Judge more strict (share)
Above median claim	33.05**	-0.14***	0.02**	-0.10*	1.75***	0.01	-0.01
	(13.06)	(0.05)	(0.01)	(0.05)	(0.61)	(0.02)	(0.02)
Entered after decree	-37.31**	0.20***	0.15***	0.15**	-1.87**	0.00	0.03
application	(16.23)	(0.06)	(0.04)	(0.07)	(0.76)	(0.03)	(0.03)
Above median claim X	27.45	-0.09	-0.06	-0.09	1.26	-0.01	0.07**
Entered after decree appl.	(17.91)	(0.07)	(0.04)	(0.08)	(0.87)	(0.03)	(0.03)
Above median claim X	-0.91	0.00	0.00*	-0.00	-0.03	0.00	-0.00
Trend	(0.58)	(0.00)	(0.00)	(0.00)	(0.03)	(0.00)	(0.00)
Triple interaction	-1.97*	0.01	-0.00	0.01**	-0.09*	-0.00	-0.00
	(1.05)	(0.00)	(0.00)	(0.00)	(0.05)	(0.00)	(0.00)
P-value 6pWCBoot: entered after	0.11	0.04	0.06	0.23	0.08	0.98	0.24
Effect for large cases	-9.86	0.11	0.09	0.06	-0.61	-0.01	0.10
P-value: effect for large cases	0.51	0.05	0.00	0.25	0.39	0.64	0.00
P-value 6pWCBoot: large cases	0.67	0.29	0.01	0.14	0.54	0.86	0.09
Chamber FEs x Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Without adjustment period	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-mean	105.46	0.65	0.00	0.23	6.17	0.89	0.11
Pre-sd	120.36	0.48	0.00	0.42	5.51	0.14	0.15
R-Squared	0.25	0.15	0.08	0.10	0.18	0.21	0.13
Observations	3319	3555	3555	3555	3555	2505	2508

Notes: Estimates of case-level impacts of the reform on pre-trial proceedings. Entered after decree application is a dummy that takes value 1 when a case entered its assigned chamber after the application of the decree; above median claim is a dummy that takes value 1 when a case's claim amount is above the median claim amount recorded over the study period. All models include chamber-specific linear trends allowed to change after the decree (full interaction of chamber FEs, trend, and Entered after decree application), calendar month fixed effects, and a constant. All models estimated by OLS. Standard errors in parentheses, clustered at the (chamber x hearing of entry) level. Window includes cases entering between 38 and 4 hearings before and between 4 and 21 hearings after decree application. Difference in number of observations compared to Table 3 due to missing claim amounts. Significance levels are denoted as follows: * p<0.10, ** p<0.05, *** p<0.01.

Table 5: Differential impact of the decree on pre-trial stage by baseline judge speed (flexible linear trends by speed)

	(1) Duration of pre-trial hearings (in days)	(2) Likelihood of pre-trial completion in 4 months	(3) Desk rejection	(4) Fast-tracked	(5) Number of pretrial hearings	(6) Pre-trial likelihood of being heard	(7) Judge more strict (share)
Fast judge	-49.29***	0.21***	-0.00	0.18***	-2.02***	0.04	0.04**
	(11.20)	(0.04)	(0.01)	(0.03)	(0.53)	(0.02)	(0.02)
Entered after decree application	-21.60	0.20***	0.11***	0.03	-0.68	0.04	0.05**
	(14.92)	(0.06)	(0.03)	(0.03)	(0.68)	(0.03)	(0.02)
Fast judge X Entered after decree application	14.08	-0.09	0.04	0.03	0.04	-0.08**	0.01
	(17.44)	(0.08)	(0.05)	(0.06)	(0.87)	(0.04)	(0.04)
Trend	-1.76***	0.00**	-0.00	0.00	-0.07***	-0.00	-0.00***
	(0.38)	(0.00)	(0.00)	(0.00)	(0.02)	(0.00)	(0.00)
Fast judge X Trend	0.61	-0.00	0.00	0.01***	0.02	-0.00	0.00***
	(0.46)	(0.00)	(0.00)	(0.00)	(0.02)	(0.00)	(0.00)
Entered after decree application X Trend	-0.93	-0.01**	-0.00	-0.00	0.02	0.00	0.00***
	(0.86)	(0.00)	(0.00)	(0.00)	(0.04)	(0.00)	(0.00)
Triple interaction	2.15**	-0.00	0.00	-0.01***	0.08	0.00	-0.00
	(1.01)	(0.00)	(0.00)	(0.00)	(0.05)	(0.00)	(0.00)
Collegial pre-trial	-119.74***	0.44***	0.07***	0.36***	-5.76***	0.03***	-0.04***
	(4.90)	(0.02)	(0.01)	(0.02)	(0.22)	(0.01)	(0.01)
P-value 6pWCBoot: entered after	0.52	0.31	0.20	0.67	0.60	0.62	0.13
Effect for fast judges	-7.52	0.11	0.15	0.06	-0.64	-0.04	0.06
P-value: effect for fast judges	0.54	0.04	0.00	0.28	0.29	0.13	0.05
P-value 6pWCBoot: fast judges	0.74	0.21	0.08	0.58	0.55	0.19	0.09
Chamber FEs	No	No	No	No	No	No	No
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Amount category controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Without adjustment period	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Comparison mean	208.83	0.31	0.00	0.07	10.36	0.83	0.13
Comparison sd	152.10	0.46	0.00	0.25	6.63	0.14	0.15
R-Squared	0.39	0.33	0.12	0.30	0.37	0.07	0.08
Observations	4558	4837	4837	4837	4837	3632	3635

Notes: Estimates of case-level impacts of the reform on pre-trial proceedings. Entered after decree application is a dummy that takes value 1 when a case entered its assigned chamber after the application of the decree; fast judge is a dummy that takes value 1 when a judge's baseline pre-trial speed is above the median baseline pre-trial speed. All models include a linear trend - allowed to change after the decree (Interaction), calendar month fixed effects, amount category dummies, a collegiate pre-trial dummy, and a constant. All models estimated by OLS. Standard errors in parentheses, clustered at the (chamber x hearing of entry) level. Window includes cases entering between 38 and 4 hearings before and between 4 and 21 hearings after decree application. Difference in number of observations compared to Table 3 due to missing baseline speed of case judge. Significance levels are denoted as follows: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 6: Impact of the decree on decision phase (flexible trends)

	(1) Duration of decision stage (in days)	(2) Hazard ratio - finishing decision stage	(3) Likelihood of decision completion in 1 month	(4) Number of decision stage hearings	(5) Decision stage likelihood of being heard
Entered after decree application (β)	8.63	1.09	-0.04	0.03	0.04
	(9.49)	(0.10)	(0.05)	(0.38)	(0.03)
P-value 6pWCBoot	0.59		0.70	0.93	0.33
Chamber FE x Trend	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes
Without adj. period	Yes	Yes	Yes	Yes	Yes
Pre-mean	63.12		0.50	2.60	0.77
Pre-sd	82.70		0.50	3.40	0.25
R-Squared	0.07		0.17	0.03	0.39
Observations	3608	3844	3844	3844	3844

Notes: Estimates of case-level impacts of the reform on decision-stage proceedings. See Notes on Table 3. 3844 observations for decision stage outcomes, except col 1 (censoring).

Table 7: Impact of the decree on quality of pre-trial and decisions (flexible trends)

	(1) Pre-trial insufficient	(2) Decision postponed	(3) Number of articles	(4) Decision length	(5) Appeal
Entered after decree application (β)	0.01	-0.01	-0.19	-0.09	0.02
	(0.04)	(0.03)	(0.13)	(0.20)	(0.05)
P-value 6pWCBoot	0.89	0.81	0.46	0.81	0.56
Chamber FE x Trend	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes
Without adj. period	Yes	Yes	Yes	Yes	Yes
Pre-mean	0.12	0.05	2.84	5.54	0.54
Pre-sd	0.32	0.23	1.59	2.56	0.50
R-Squared	0.02	0.05	0.01	0.04	0.05
Observations	3832	3832	2742	2741	2742

Notes: Estimates of case-level impacts of the reform on the quality of pre-trial and deliberation proceedings. See Notes on Table 3. 3844 observations for decisoin stage outcomes, except cols 1-2 (censoring as only for cases with at least one decision stage hearing), and cols 3-5 (missing outcomes).

Table 8: Effect of entering pre-trial and of pre-trial speed on parties' log revenues (diff-in-diff model)

	Model 1	Model 2				Model 3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	OLS	IV	FS	RF	OLS	IV	FS
After pre-trial starts	-0.08***	-0.12***	-0.11	0.42***	-0.09***			1.84***
	(0.02)	(0.03)	(0.20)	(0.02)	(0.02)			(0.06)
After pre-trial starts		0.07*	0.06					
X Fast pre-trial		(0.04)	(0.44)					
After pre-trial starts						-0.03***	-0.05***	
X Pre-trial duration (in 100 days)						(0.01)	(0.01)	
After pre-trial starts				0.11***	0.01			-0.78***
X After decree (γ_1)				(0.04)	(0.05)			(0.09)
Effect if fast pre-trial		-0.04	-0.05					
P-value: effect if fast pre-trial		0.13	0.85					
FEs	partyid	partyid	partyid	partyid	partyid	partyid	partyid	partyid
Time FEs	yrmth	yrmth	yrmth	yrmth	yrmth	yrmth	yrmth	yrmth
Pre-mean	18.74			0.46		18.70		152.86
Pre-sd	2.70			0.50		2.71		149.87
Fstat excl inst				7				533
Observations	302835	302835	302835	302835	302835	301611	301611	301611

Notes: Model 1 is the base specification. In Model 2 the pre-trial speed variable is the fast pre-trial dummy, in Model 3 it is pre-trial duration (in 100 days). In cols 1-3, 5-7 the dependant variable is monthly log revenues; col 4 is the first stage for the IV of col 3, and col 8 is the first stage for the IV of col 7; col 5 is the reduced form. After case started is a dummy that takes value 1 for a case parties' observations after its pre-trial has started, and value 0 before pre-trial start and for firms in the sample that do not have a court case in the study period. Fast pre-trial is a dummy that takes value 1 when a case party's pre-trial was completed in four months or less; Pre-trial duration is its duration (in 100 days). After decree is a dummy that takes value 1 when a party's case started its pre-trial after the application of the decree. All models include case party and month fixed effects and are estimated by OLS; cols 1-5 include 12 observations for each of the 1622 case parties (6 months before pre-trial start and 6 months after) as well as uninterrupted spells of log revenue observations of at least 12 months (average: 33.046 months) within the study period for 8,575 firms that did not have a court case; the same holds for cols 6-8 but with only 1520 case parties due to the censoring of the pre-trial duration variable. Standard errors in parentheses, clustered at the firm-level. Significance levels are denoted as follows: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Pre-mean and pre-sd refer to sample average and standard deviation respectively of case parties' log revenues in the month before pre-trial start.

Table 9: Changes in firms' perceptions of the justice system

	(1) Duration	(2) Duration	(3) Costs (in 1000 FCFA)	(4) Costs (in 1000 FCFA)	(5) Hypothetical	(6) Hypothetical	(7) Perception index	(8) Perception index
Case(s) after decree applic.	1.50		260.67		0.05		0.06**	
	(2.17)		(222.76)		(0.05)		(0.03)	
Case after decree applic.		-0.92		-8.05		0.14**		0.07
		(2.72)		(238.31)		(0.07)		(0.04)
Number of cases	-0.02		6.49		0.00**		-0.00	
	(0.09)		(8.24)		(0.00)		(0.00)	
Pre-mean	21.51	21.51	1140.05	1140.05	0.75	0.75	0.49	0.49
R-Squared	0.00	0.05	0.08	0.14	0.02	0.03	0.02	0.02
Observations	275	152	272	151	251	139	277	153

Notes: Estimates of firm-level impacts of the reform on perceived duration and costs, hypothetical use, and perception of the justice system. Uneven cols: sample is all firms, Case(s) after decree applic. is a dummy that takes value one if a firm has had at least one case after decree application. controls include the number of cases the firm had. Even cols: sample restricted to firms that had only one case, Case after decree applic. is a dummy that indicates the case was after the introduction of the decree. All models control for baseline number of employees, include a constant, and are estimated by OLS. Significance levels are denoted as follows: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

A For online publication: Supplementary Figures and Tables

Figure A1: Digitizing the archives of the Regional Court of Dakar

Figure A2: Court-level caseload (number of cases)

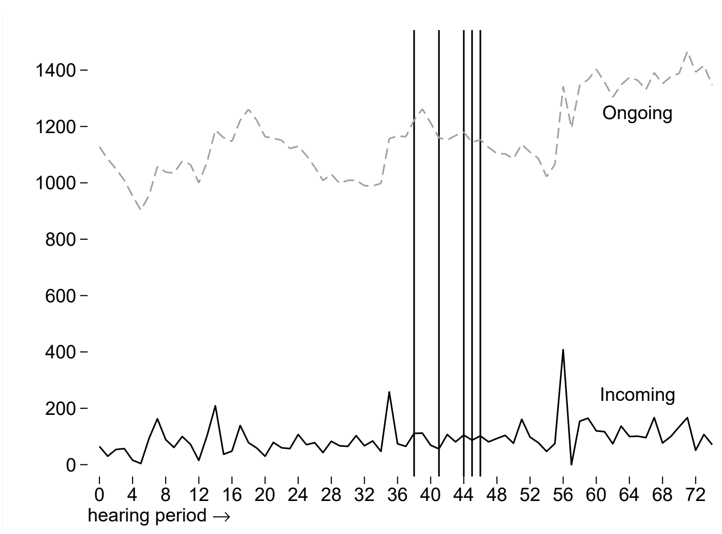
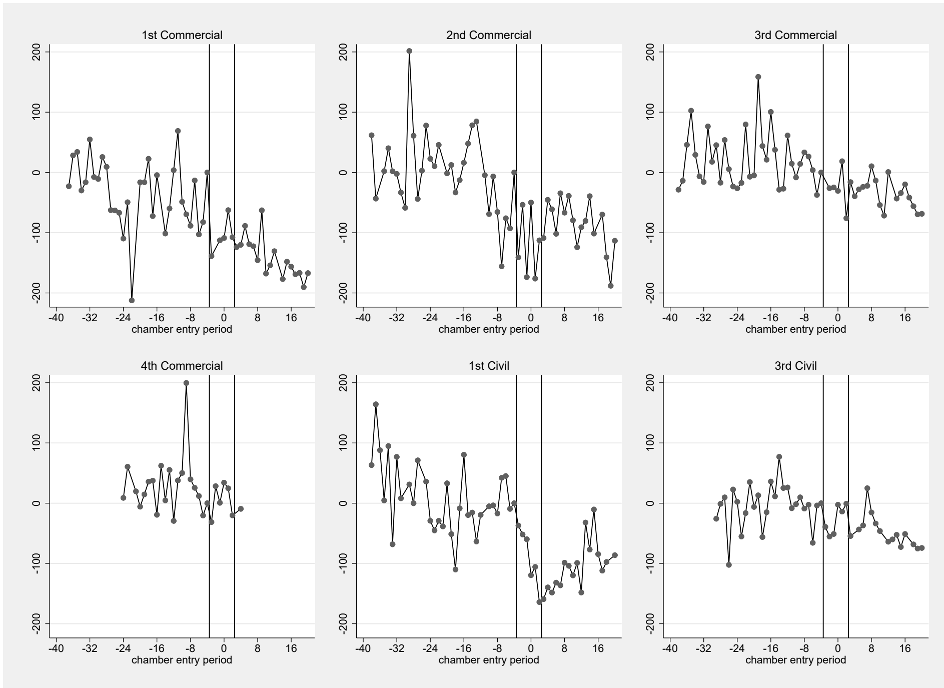
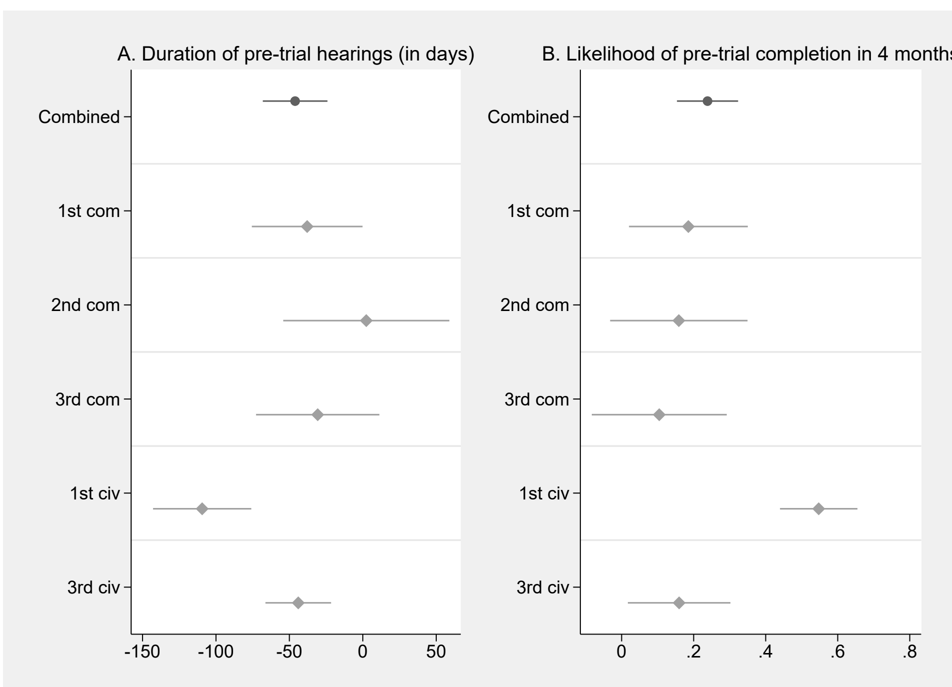


Figure A3: Pre-trial duration, across chambers



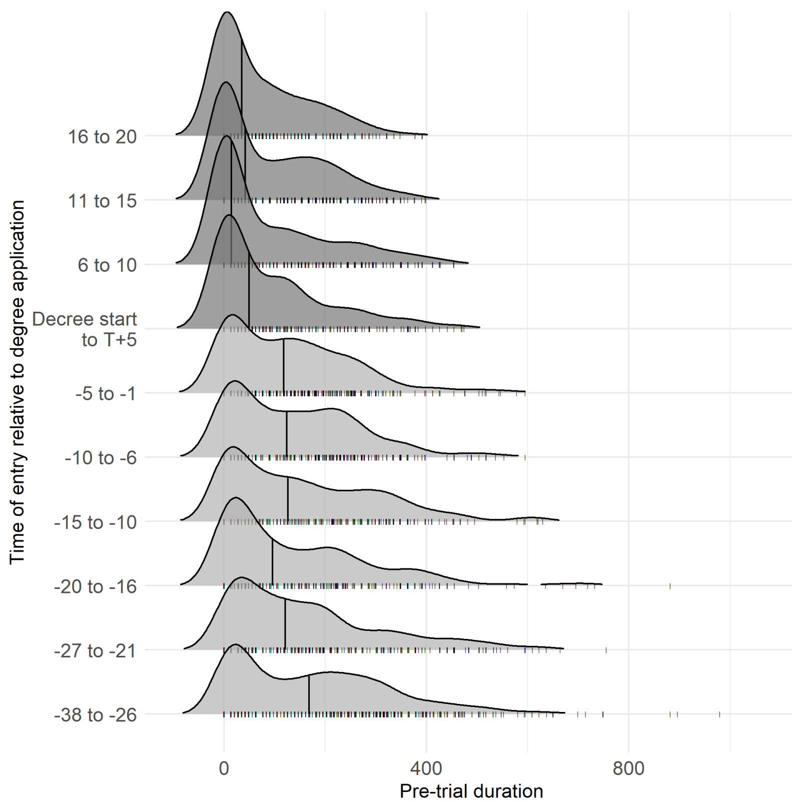
Notes: For all x-axes, period is indexed in relation to chamber-level decree application (zero-centered).

Figure A4: Average effects across chambers and on aggregate



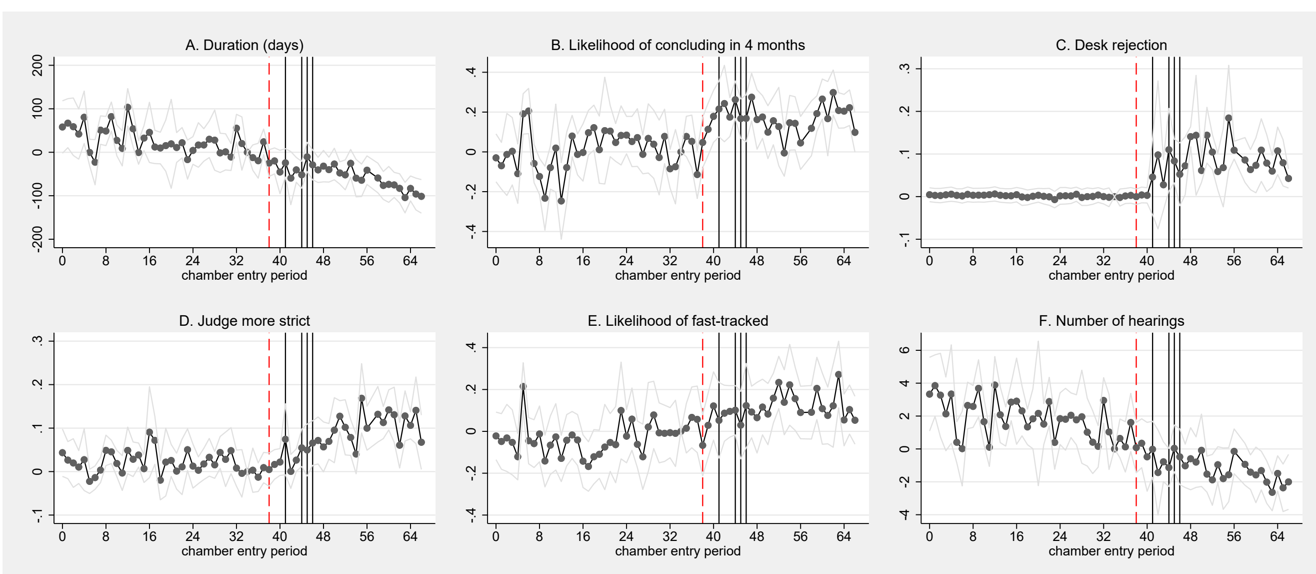
Notes: 4^{th} commercial chamber not displayed as it has too few post-adjustment period observations (see Figure A1).

Figure A5: Distributions of pre-trial duration



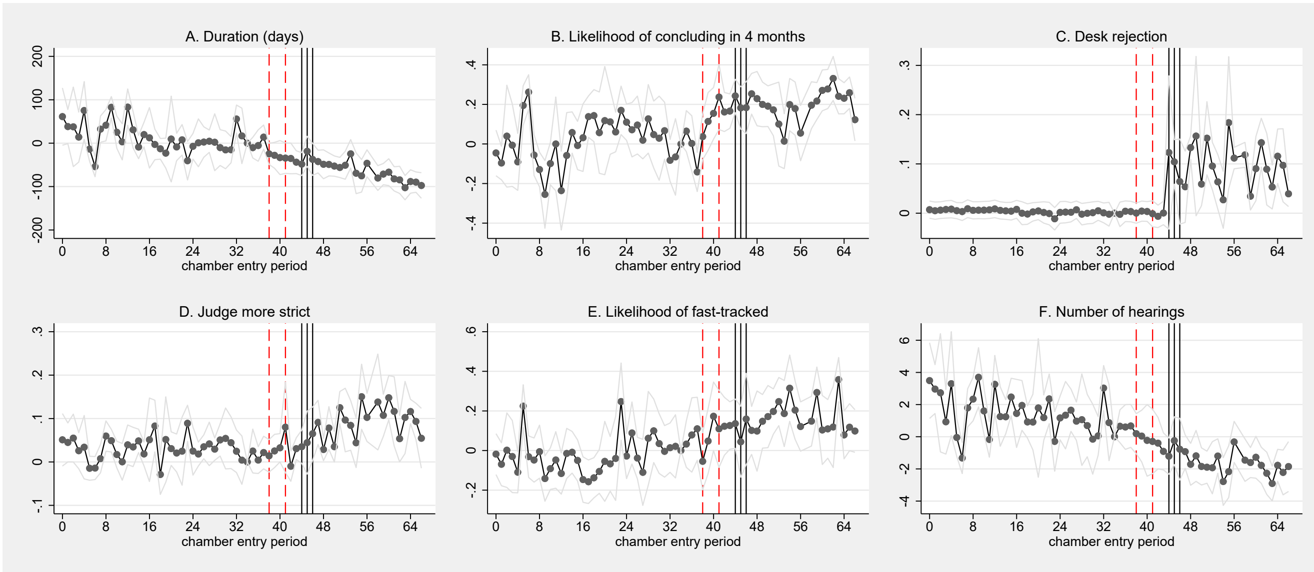
Notes: The distribution below 0 is the effect of smoothing. The bottom ticks represents the true values.

Figure A6: Single cutoff placebo check – leaving out first chamber



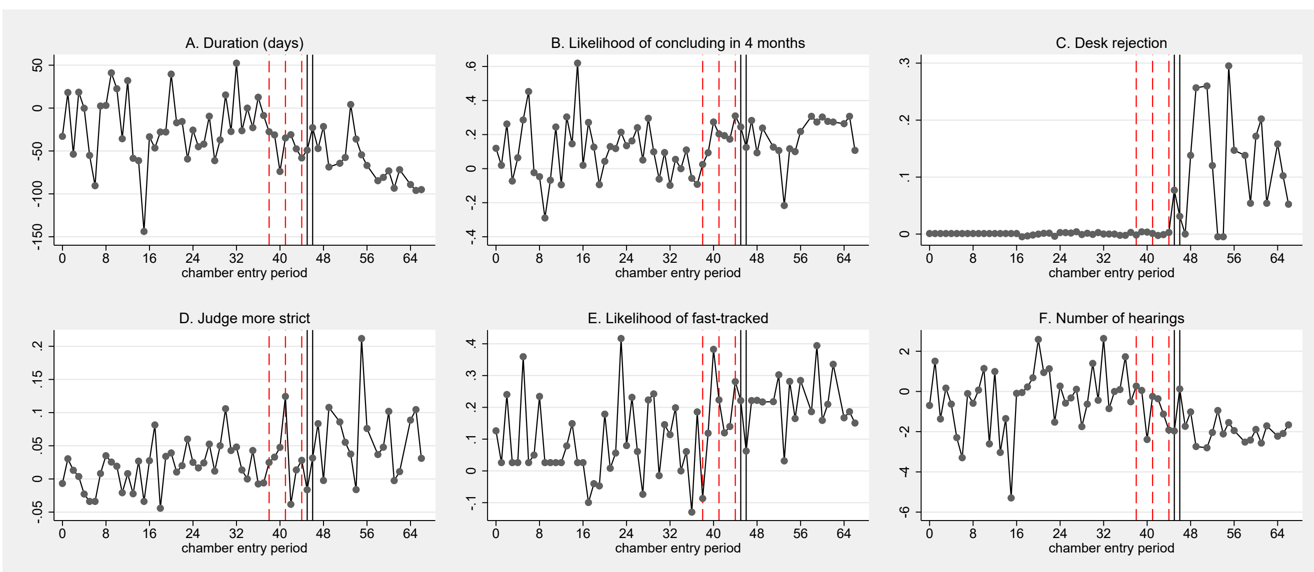
Notes: 4^{th} commercial chamber not displayed as it has too few post-adjustment period observations (see Figure A-2). The gray lines represent the 95% confidence interval.

Figure A7: Single cutoff placebo check – leaving out first and second chambers



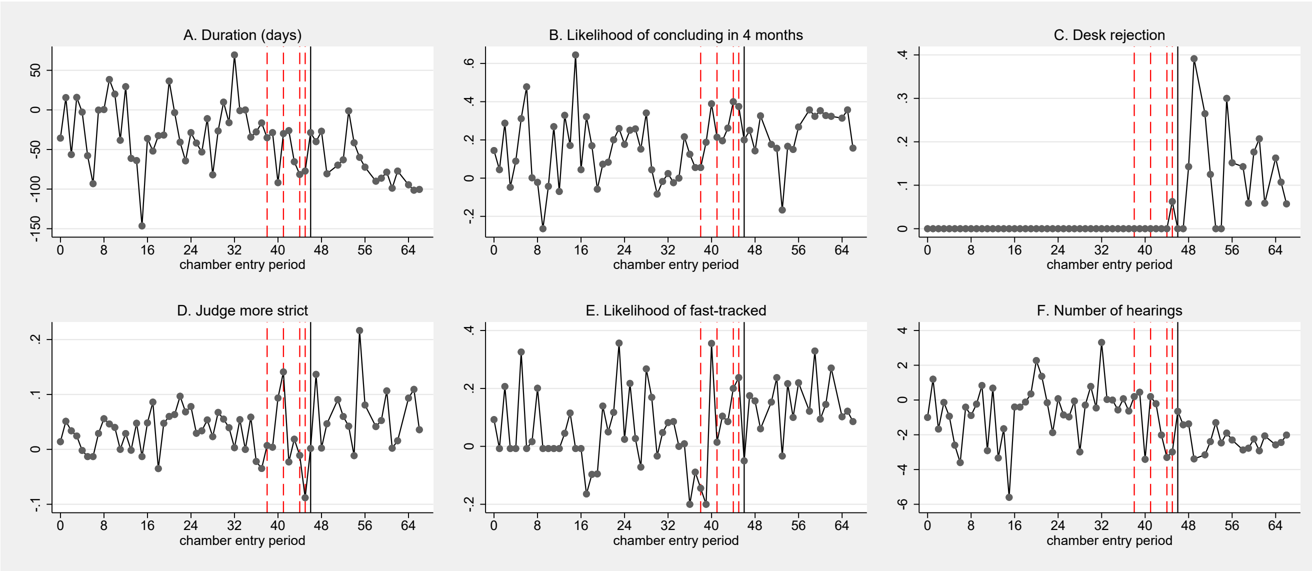
Notes: 4^{th} commercial chamber not displayed as it has too few post-adjustment period observations (see Figure A-2). The gray lines represent the 95% confidence interval.

Figure A8: Single cutoff placebo check – leaving out first, second and third chambers



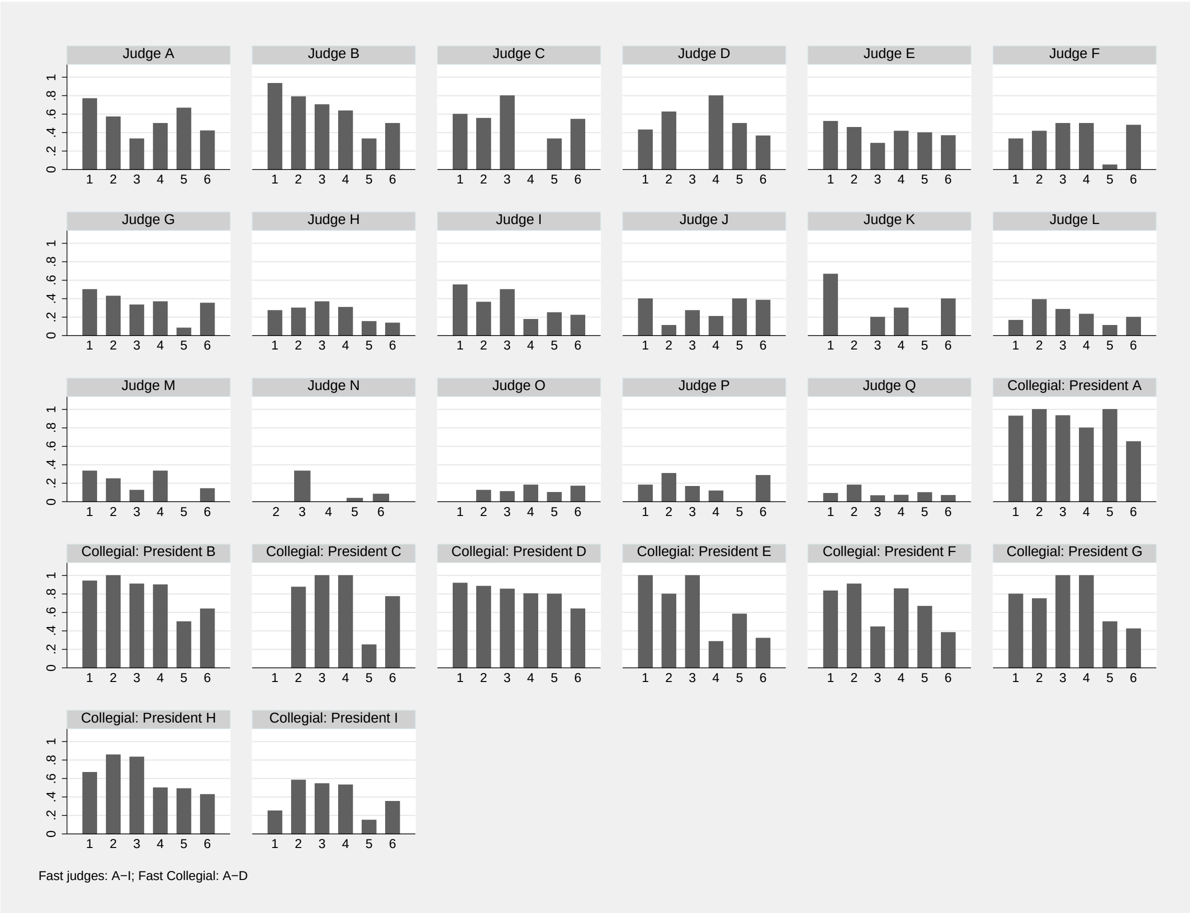
Notes: 4^{th} commercial chamber not displayed as it has too few post-adjustment period observations (see Figure A-2).

Figure A9: Single cutoff placebo check – leaving out first, second, third and fourth chambers



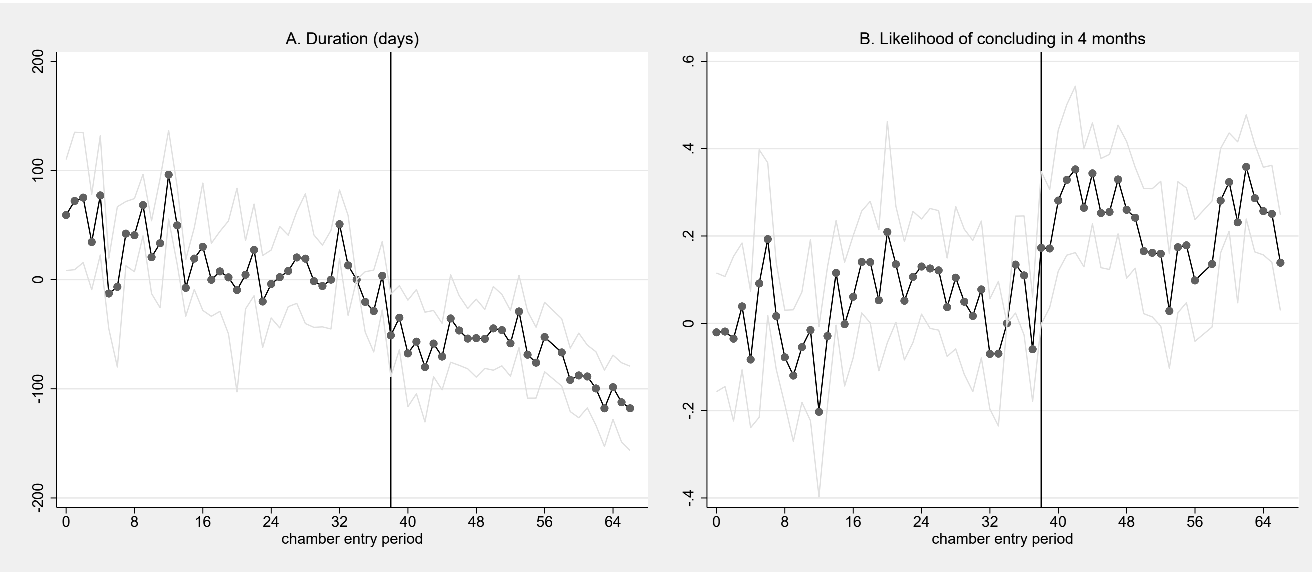
Notes: 4^{th} commercial chamber not displayed as it has too few post-adjustment period observations (see Figure A-2).

Figure A10: Pre-decree judge-level variation in speed by claim amount



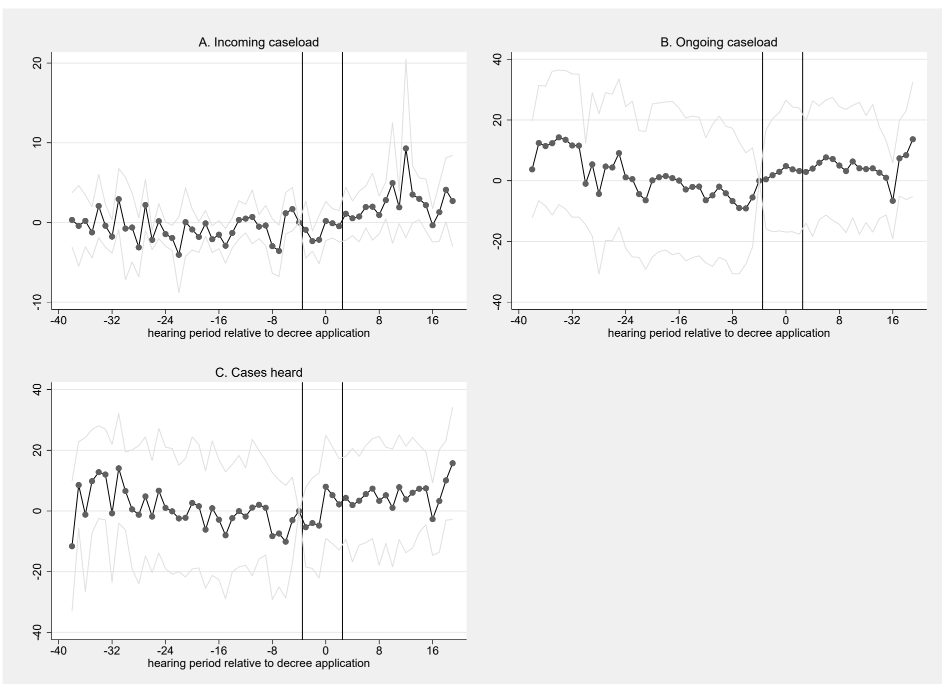
Notes: Figure A10 displays the judge-level variations in baseline speed (y-axis) by claim amount quintiles (x-axis), with a sixth category indicating cases without a claim amount.

Figure A11: Impact on pre-trial delays assuming the same application cutoff for all chambers (first application cutoff, first civil chamber)



Notes: The gray lines represent the 95% confidence interval.

Figure A12: Judges' workload



Notes: For all x-axes, period is indexed in relation to chamber-level decree application (zero-centered). The gray lines represent the 95% confidence interval.

Figure A13: Case parties' log revenues around the start of the pre-trial, by side

Notes: For the x-axis, month is indexed in relation to the case-level pre-trial start month (zero-centered). Plaintiff and defendant log revenue time series are each displayed relative to their reference period (period -1). The gray lines represent the 95% confidence interval for the Plaintiff time series and the dashed lines represent the 95% confidence interval for the Defendant time series.

Table A1: Impact of the decree on pre-trial stage (including adjustment period)

	(1) Duration of pre-trial hearings (in days)	(2) Hazard ratio - finishing pre-trial	(3) Likelihood of pre-trial completion in 4 months	(4) Desk rejection	(5) Fast-tracked	(6) Number of pretrial hearings	(7) Pre-trial likelihood of being heard	(8) Judge more strict (share)
Entered after decree	-33.56***	1.20**	0.16***	0.17***	0.03	-1.48***	0.03*	0.05***
application (β)	(10.45)	(0.09)	(0.04)	(0.02)	(0.03)	(0.40)	(0.02)	(0.01)
Trend	-1.40***	1.01***	0.00**	-0.00	0.00***	-0.06***	-0.00**	-0.00***
	(0.33)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)
Interaction	0.70	0.99***	-0.01***	-0.00***	-0.00	0.09***	0.00	0.00***
	(0.67)	(0.00)	(0.00)	(0.00)	(0.00)	(0.03)	(0.00)	(0.00)
P-value 6pWCBoot	0.10		0.04	0.00	0.47	0.04	0.36	0.03
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Without adj. period	No	No	No	No	No	No	No	No
Pre-mean	152.46		0.50	0.00	0.15	8.06	0.85	0.12
Pre-sd	144.24		0.50	0.02	0.36	6.41	0.15	0.15
R-Squared	0.19		0.13	0.08	0.07	0.14	0.17	0.06
Observations	5064	5297	5297	5297	5297	5297	3879	3883

Notes: Estimates of case-level impacts of the reform on pre-trial proceedings. See notes for cols 2-4, Table A-1. 5297 observations, except for col 1 (censoring), col 7 (only for cases that have more than one hearing), col 8 (only for cases that have any adjournments).

Table A2: Impact of the decree on pre-trial stage (common linear trends)

	(1) Duration of pre-trial hearings (in days)	(2) Hazard ratio - finishing pre-trial	(3) Likelihood of pre-trial completion in 4 months	(4) Desk rejection	(5) Fast-tracked	(6) Number of pretrial hearings	(7) Pre-trial likelihood of being heard	(8) Judge more strict (share)
Entered after decree application (β)	-42.93***	1.32***	0.23***	0.17***	0.09**	-2.00***	0.02	0.06***
	(12.94)	(0.13)	(0.05)	(0.02)	(0.04)	(0.49)	(0.03)	(0.02)
Trend	-1.12***	1.00**	0.00	-0.00	0.00***	-0.04***	-0.00**	-0.00**
	(0.38)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)
Interaction	0.45	0.99**	-0.01***	-0.00***	-0.00	0.07**	0.00	0.00***
	(0.81)	(0.01)	(0.00)	(0.00)	(0.00)	(0.03)	(0.00)	(0.00)
P-value 6pWCBoot	0.09		0.03	0.02	0.09	0.04	0.48	0.03
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Without adj. period	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-mean	156.94		0.49	0.00	0.14	8.26	0.85	0.12
Pre-sd	146.02		0.50	0.00	0.35	6.47	0.15	0.15
R-Squared	0.20		0.13	0.08	0.07	0.14	0.18	0.06
Observations	4568	4795	4795	4795	4795	4795	3533	3537

Notes: Estimates of case-level impacts of the reform on pre-trial proceedings. Entered after decree application is a dummy that takes value 1 when a case entered its assigned chamber after the application of the decree. All models include a linear trend - allowed to change after the decree (Interaction)-, chamber and calendar month fixed effects, and a constant. All models estimated by OLS. Standard errors in parentheses, clustered at the (chamber x hearing of entry) level. Window includes cases entering between 38 and 4 hearings before and between 4 and 21 hearings after decree application. 4795 observations, except for col 1 (censoring), col 7 (only for cases that have more than one hearing), col 8 (only for cases that have any adjournments). Significance levels are denoted as follows: * p<0.10, ** p<0.05, *** p<0.01.

Table A3: Differential impact of the decree on pre-trial stage by case difficulty (claim amount; common linear trends)

	(1) Duration of pre-trial hearings (in days)	(2) Likelihood of pre-trial completion in 4 months	(3) Desk rejection	(4) Fast-tracked	(5) Number of pretrial hearings	(6) Pre-trial likelihood of being heard	(7) Judge more strict (share)
Above median claim	35.21***	-0.13**	0.02*	-0.11**	1.85***	0.01	-0.02
	(12.94)	(0.05)	(0.01)	(0.05)	(0.60)	(0.02)	(0.02)
Entered after decree	-29.33*	0.18***	0.16***	0.13*	-1.84**	-0.02	0.03
application	(16.47)	(0.06)	(0.04)	(0.07)	(0.73)	(0.03)	(0.03)
Above median claim X	22.81	-0.07	-0.07*	-0.06	1.18	-0.01	0.07**
Entered after decree	(18.33)	(0.07)	(0.04)	(0.08)	(0.87)	(0.03)	(0.03)
Trend	-0.39	0.00	-0.00	0.00**	-0.02	-0.00	-0.00
	(0.50)	(0.00)	(0.00)	(0.00)	(0.02)	(0.00)	(0.00)
Above median claim X	-1.21**	0.00	0.00	-0.00	-0.04	0.00	-0.00
Trend	(0.58)	(0.00)	(0.00)	(0.00)	(0.03)	(0.00)	(0.00)
Entered after decree	-0.84	-0.01*	-0.00	-0.01***	0.04	0.01***	0.00***
application X Trend	(0.89)	(0.00)	(0.00)	(0.00)	(0.04)	(0.00)	(0.00)
Triple interaction	-2.12**	0.00	-0.00	0.01**	-0.10**	-0.00	-0.00
	(1.02)	(0.00)	(0.00)	(0.00)	(0.05)	(0.00)	(0.00)
P-value 6pWCBoot: entered after	0.08	0.04	0.07	0.33	0.05	0.67	0.38
Effect for large cases	-6.52	0.11	0.08	0.07	-0.66	-0.03	0.09
P-value: effect for large cases	0.68	0.07	0.00	0.17	0.33	0.23	0.00
P-value 6pWCB: large cases	0.80	0.31	0.02	0.16	0.55	0.29	0.06
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Without adjustment period	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-mean	105.46	0.65	0.00	0.23	6.17	0.89	0.11
Pre-sd	120.36	0.48	0.00	0.42	5.51	0.14	0.15
R-Squared	0.24	0.14	0.07	0.09	0.18	0.17	0.10
Observations	3319	3555	3555	3555	3555	2505	2508

Notes: Estimates of case-level impacts of the reform on pre-trial proceedings. Entered after decree application is a dummy that takes value 1 when a case entered its assigned chamber after the application of the decree; above median claim is a dummy that takes value 1 when a case's claim amount is above the median claim amount recorded over the study period. All models include a linear trend - allowed to change after the decree (Interaction), chamber and calendar month fixed effects, and a constant. All models estimated by OLS. Standard errors in parentheses, clustered at the (chamber x hearing of entry) level. Window includes cases entering between 38 and 4 hearings before and between 4 and 21 hearings after decree application. Difference in number of observations compared to Table 3 due to missing claim amounts. Significance levels are denoted as follows: * p<0.10, ** p<0.05, *** p<0.01.

Table A4: Differential impact of the decree on pre-trial stage by baseline judge speed (flexible trends by speed and chamber)

	(1) Duration of pre-trial hearings (in days)	(2) Likelihood of pre-trial completion in 4 months	(3) Desk rejection	(4) Fast-tracked	(5) Number of pretrial hearings	(6) Pre-trial likelihood of being heard	(7) Judge more strict (share)
Fast judge	-29.36***	0.16***	-0.00	0.16***	-1.58***	-0.00	0.03*
	(10.45)	(0.04)	(0.01)	(0.03)	(0.51)	(0.02)	(0.02)
Entered after decree application	-13.92	0.18***	0.12***	0.04	-0.67	0.02	0.06***
	(14.82)	(0.06)	(0.03)	(0.03)	(0.69)	(0.02)	(0.02)
Fast judge X Entered after decree appl.	-7.16	-0.04	0.05	-0.01	-0.05	-0.06	-0.01
	(16.89)	(0.07)	(0.04)	(0.06)	(0.86)	(0.04)	(0.03)
Fast judge X Trend	-0.04	0.00	0.00	0.01***	-0.01	-0.00	0.00***
	(0.52)	(0.00)	(0.00)	(0.00)	(0.02)	(0.00)	(0.00)
Triple interaction	3.31***	-0.01	-0.00	-0.01**	0.08	0.00*	-0.00
	(1.10)	(0.00)	(0.00)	(0.00)	(0.05)	(0.00)	(0.00)
Collegial pre-trial	-120.96***	0.45***	0.07***	0.36***	-5.94***	0.02***	-0.03***
	(4.77)	(0.02)	(0.01)	(0.02)	(0.23)	(0.01)	(0.01)
P-value 6pWCBoot: entered after	0.75	0.30	0.07	0.59	0.61	0.82	0.10
Effect for fast judges	-21.08	0.14	0.17	0.04	-0.72	-0.04	0.05
P-value: effect for fast judges	0.08	0.00	0.00	0.49	0.22	0.18	0.09
P-value 6pWCBoot: fast judges	0.25	0.08	0.05	0.84	0.35	0.13	0.21
Chamber FEs x Trend	No	No	No	No	No	No	No
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Amount category controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Without adjustment period	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Comparison mean	208.83	0.31	0.00	0.07	10.36	0.83	0.13
Comparison sd	152.10	0.46	0.00	0.25	6.63	0.14	0.15
R-Squared	0.41	0.34	0.12	0.31	0.39	0.20	0.11
Observations	4558	4837	4837	4837	4837	3632	3635

Notes: Estimates of case-level impacts of the reform on pre-trial proceedings. Entered after decree application is a dummy that takes value 1 when a case entered its assigned chamber after the application of the decree; fast judge is a dummy that takes value 1 when a judge's baseline pre-trial speed is above the median baseline pre-trial speed. All models include chamber-specific linear trends allowed to change after the decree (full interaction of chamber FEs, trend, and Entered after decree application), calendar month fixed effects, amount category dummies, a collegiate pre-trial dummy, and a constant. All models estimated by OLS. Standard errors in parentheses, clustered at the (chamber x hearing of entry) level. Window includes cases entering between 38 and 4 hearings before and between 4 and 21 hearings after decree application. Difference in number of observations compared to Table 3 due to missing baseline speed of case judge. Significance levels are denoted as follows: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A5: Impact of the decree on decision phase (including adjustment period)

	(1) Duration of decision stage (in days)	(2) Hazard ratio - finishing decision stage	(3) Likelihood of decision completion in 1 month	(4) Number of decision stage hearings	(5) Decision stage likelihood of being heard
Entered after decree	3.78	1.07	-0.04	-0.13	0.02
application (β)	(6.58)	(0.08)	(0.04)	(0.29)	(0.03)
Trend	0.75***	0.99***	-0.01***	0.02***	-0.01***
	(0.17)	(0.00)	(0.00)	(0.01)	(0.00)
Interaction	-2.33***	1.00	0.01**	-0.05**	0.00*
	(0.40)	(0.01)	(0.00)	(0.02)	(0.00)
P-value 6pWCBoot	0.70		0.64	0.69	0.55
Chamber FEs	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes
Without adj. period	No	No	No	No	No
Pre-mean	63.44		0.50	2.63	0.77
Pre-sd	82.63		0.50	3.45	0.25
R-Squared	0.06		0.15	0.03	0.33
Observations	3963	4214	4214	4214	4214

Notes: Estimates of case-level impacts of the reform on decision-stage proceedings. See Notes for cols 2-4, Table A-1. 4214 observations for decision stage outcomes, except col 1 (censoring).

Table A6: Impact of the decree on decision phase (common linear trends)

	(1) Duration of decision stage (in days)	(2) Hazard ratio - finishing decision stage	(3) Likelihood of decision completion in 1 month	(4) Number of decision stage hearings	(5) Decision stage likelihood of being heard
Entered after decree	5.76	1.15	-0.02	-0.07	0.05
application (β)	(8.73)	(0.10)	(0.06)	(0.37)	(0.04)
Trend	0.83***	0.98***	-0.01***	0.02***	-0.01***
	(0.20)	(0.00)	(0.00)	(0.01)	(0.00)
Interaction	-2.73***	1.00	0.01**	-0.06**	0.00*
	(0.55)	(0.01)	(0.00)	(0.02)	(0.00)
P-value 6pWCBoot	0.75		0.87	0.85	0.35
Chamber FEs	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes
Without adj. period	Yes	Yes	Yes	Yes	Yes
Pre-mean	63.12		0.50	2.60	0.77
Pre-sd	82.70		0.50	3.40	0.25
R-Squared	0.07		0.15	0.03	0.32
Observations	3608	3844	3844	3844	3844

Notes: Estimates of case-level impacts of the reform on decision-stage proceedings. See Notes on Table 3. 3844 observations for decision stage outcomes, except col 1 (censoring).

Table A7: Impact of the decree on quality (common linear trends)

	(1) Pre-trial insufficient	(2) Decision postponed	(3) Number of articles	(4) Decision length	(5) Appeal
Entered after decree	0.01	-0.01	-0.17	-0.14	0.03
application (β)	(0.04)	(0.03)	(0.16)	(0.22)	(0.05)
Trend	0.00	0.00***	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Interaction	0.00	-0.00	0.01	0.02	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
P-value 6pWCBoot	0.87	0.81	0.40	0.72	0.54
Chamber FEs	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes
Without adj. period	Yes	Yes	Yes	Yes	Yes
Pre-mean	0.12	0.05	2.84	5.54	0.54
Pre-sd	0.32	0.23	1.59	2.56	0.50
R-Squared	0.02	0.04	0.01	0.04	0.04
Observations	3832	3832	2742	2741	2742

Notes: Estimates of case-level impacts of the reform on the quality of pre-trial and deliberation proceedings. See Notes on Table 3. 3844 observations for decisoin stage outcomes, except cols 1-2 (censoring as only for cases with at least one decision stage hearing), and cols 3-5 (missing outcomes).

Table A8: Descriptive statistics on negative implications for firms of having a judicial conflict

	N	Mean	StD	Median	Min	Max
PANEL A: Firms with case(s) as plaintiff						
Total amount spent (in million FCFA)	131	1.76	3.65	0.65	0.00	35.00
Work hours were lost due to judicial conflict	122	0.98	0.16	1.00	0.00	1.00
Monetary value of work hours lost (in million FCFA)	46	1.39	3.23	0.25	0.00	18.00
Had to take on debt due to the judicial conflict	141	0.24	0.43	0.00	0.00	1.00
Judicial conflict stopped firm from making investments	141	0.40	0.49	0.00	0.00	1.00
Same type of case and situation, would go to court again?	131	0.86	0.35	1.00	0.00	1.00
PANEL B Firms with case(s) as defendant:						
Total amount spent (in million FCFA)	89	3.39	8.447	0.70	0.00	50.00
Work hours were lost due to judicial conflict	93	0.96	0.204	1.00	0.00	1.00
Monetary value of work hours lost (in million FCFA)	28	1.29	2.258	0.45	0.00	10.00
Had to take on debt due to the judicial conflict	101	0.20	0.400	0.00	0.00	1.00
Judicial conflict stopped firm from making investments	101	0.33	0.471	0.00	0.00	1.00

Notes: In panels A and B all questions after the first refer to the most recent case. Total amount spent includes lawyer fees, court fees, and informal payments. Respondants who indicated positive numbers of hours lost without giving a number said it was impossible to quantify but large.

Table A9: Effect of pre-trial speed on parties' average log revenues after pre-trial start (sample with non-missing duration)

	Model 1	Model 2			
	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	IV	FS	RF
After pre-trial starts	-0.08***	-0.11***	-0.12	0.42***	-0.08***
	(0.02)	(0.03)	(0.12)	(0.02)	(0.02)
After pre-trial starts		0.07*	0.09		
X Fast pre-trial		(0.04)	(0.25)		
After pre-trial starts				0.18***	0.02
X After decree (γ_1)				(0.04)	(0.05)
Effect if fast pre-trial		-0.04	-0.04		
P-value: effect if fast pre-trial		0.13	0.79		
FEs	partyid	partyid	partyid	partyid	partyid
Time FEs	yrmth	yrmth	yrmth	yrmth	yrmth
Pre-mean	18.70			0.50	
Pre-sd	2.71			0.50	
Fstat excl inst				21	
Observations	301611	301611	301611	301611	301611

Notes: Models 1 and 2 of Table 8 restricted to the sample with non-missing pre-trial duration (as for Model 3 of Table 8). The dependant variable in cols 1-4 is monthly log revenues; col 4 is the first stage for the IV of col 3; col 5 is the reduced form. After case started is a dummy that takes value 1 for a case parties' observations after its pre-trial has started, and value 0 before pre-trial start and for firms in the sample that do not have a court case in the study period. Fast pre-trial is a dummy that takes value 1 when a case party's pre-trial was completed in four months or less. After decree is a dummy that takes value 1 when a party's case started its pre-trial after the application of the decree. All models include case party and month fixed effects and are estimated by OLS; they include 12 observations for each of the 1520 case parties with non-missing pre-trial duration (6 months before pre-trial start and 6 months after) as well as uninterrupted spells of log revenue observations of at least 12 months (average: 33.046 months) within the study period for 8,575 firms that did not have a court case. Standard errors in parentheses, clustered at the firm-level. Significance levels are denoted as follows: * p<0.10, ** p<0.05, *** p<0.01. Pre-mean and pre-sd refer to sample average and standard deviation respectively of case parties' log revenues in the month before pre-trial start.

Table A10: Effect of pre-trial speed on parties' average log revenues after pre-trial start (ANCOVA specification)

	Model 2				Model 3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	IV	FS	RF	OLS	IV	FS
Fast pre-trial	0.08**	0.03					
	(0.03)	(0.21)					
Pre-trial duration (in 100 days)					-0.01	-0.01	
					(0.01)	(0.03)	
After decree			0.12***	0.00			-0.79***
			(0.03)	(0.03)			(0.10)
Log revenue, minus 1	0.29***	0.29***	-0.02	0.29***	0.29***	0.29***	0.06
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.05)
Log revenue, minus 2	0.27***	0.27***	0.01	0.27***	0.25***	0.25***	-0.04
	(0.04)	(0.05)	(0.02)	(0.05)	(0.05)	(0.05)	(0.06)
Log revenue, minus 3	0.16***	0.16***	0.03*	0.16***	0.17***	0.17***	-0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.05)
Log revenue, minus 4	0.04	0.04	-0.01	0.04	0.06*	0.06*	0.03
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.05)
Log revenue, minus 5	0.09***	0.09***	-0.01	0.09***	0.09***	0.09***	0.01
	(0.03)	(0.03)	(0.01)	(0.03)	(0.03)	(0.03)	(0.04)
Log revenue, minus 6	0.15***	0.15***	-0.01	0.15***	0.14***	0.14***	0.02
	(0.03)	(0.03)	(0.01)	(0.03)	(0.03)	(0.03)	(0.04)
FEs	chamber	chamber	chamber	chamber	chamber	chamber	chamber
Time FEs	calmth	calmth	calmth	calmth	calmth	calmth	calmth
Pre-mean	18.74				18.70		
Pre-sd	2.70				2.71		
Fstat excl inst			14				67
Observations	1622	1622	1622	1622	1520	1520	1520

Notes: Ancova versions of Models 2 and 3 of Table 8. In Model 2 the pre-trial speed variable is the fast pre-trial dummy, in Model 3 it is pre-trial duration (in 100 days). In cols 1-2, 4-6 the dependent variable is average monthly log revenues after the start of the pre-trial; col 3 is the first stage for the IV of col 2; col 7 is the first stage for the IV of col 6; col 4 is the reduced form. Fast pre-trial is a dummy that takes value 1 when a case party's pre-trial was completed in four months or less; Pre-trial duration (in 100 days) is its duration. After decree is a dummy that takes value 1 when a party's case started its pre-trial after the application of the decree. All models include chamber and calendar month fixed effects, control for six baseline values of log monthly revenues (the six months before the start of the pre-trial), and include a constant. All models are estimated by OLS; for cols 1-4 they include one observations for each of the 1622 case parties; for cols 5-7 this number is 1520 due to the censoring of the pre-trial duration variable. Standard errors in parentheses, clustered at the (chamber x hearing of entry) level. Significance levels are denoted as follows: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Pre-mean and pre-sd refer to sample average and standard deviation respectively of case parties' log revenues in the month before pre-trial start.

Table A11: Balance on baseline characteristics of observations included in the tax record and firm survey samples

Variable	(1) Court-only	(2) In ... sample	(3) Difference (2)-(1)	(4) Observations
PANEL A: Tax record sample (party level)				
Entered after decree application	0.43	0.43	-0.00	5,401
	(0.49)	(0.50)	(0.02)	
Party is plaintiff	0.47	0.50	0.02	5,401
	(0.50)	(0.50)	(0.02)	
Claim amount (in million FCFA)	103.88	80.12	-25.65*	3,926
	(496.24)	(318.28)	(14.69)	
Above-median claim amount	0.60	0.59	-0.03*	3,926
	(0.49)	(0.49)	(0.01)	
2012 revenues (IHS transformation)	19.43	22.08	2.63***	3,785
	(7.79)	(2.83)	(0.26)	
PANEL B: Tax record sample (firm level)				
Share of firm's cases that entered after decree	0.42	0.42	-0.00	2,154
	(0.46)	(0.41)	(0.02)	
Share of firm's cases that are as plaintiff	0.44	0.53	0.09***	2,154
	(0.47)	(0.42)	(0.02)	
Average claim amount of cases firm is involved in	128.83	90.31	-39.25	1,740
	(605.82)	(318.63)	(28.34)	
2012 revenues (IHS transformation)	13.98	20.83	6.94***	993
	(9.07)	(3.01)	(0.45)	
Number of cases	2.29	3.09	0.76***	2,154
	(7.53)	(4.62)	(0.29)	
PANEL C: Firm survey sample (firm level)				
Share of firm's cases that entered after decree	0.42	0.41	-0.00	2,154
	(0.45)	(0.43)	(0.03)	
Share of firm's cases that are as plaintiff	0.46	0.50	0.04	2,154
	(0.46)	(0.43)	(0.03)	
Average claim amount of cases firm is involved in	121.90	92.52	-26.82	1,740
	(558.67)	(408.15)	(34.33)	
2012 revenues (IHS transformation)	17.64	18.94	1.31**	993
	(7.44)	(5.98)	(0.51)	
Number of cases	2.38	3.34	0.97*	2,154
	(6.46)	(9.19)	(0.53)	

Notes: Balance on baseline characteristics, Panel A: comparing case parties across court-only and tax record sample; Panel B: comparing court-only firms with those that have at least one party in the tax record sample; Panel C: comparing firms across court-only and firm survey sample. Number of observations in sample (Column 2): Panel A 1622; Panel B 582; Panel C 277. Columns (1) and (2) show raw means and standard deviations (in parentheses); Column (3) shows the regression adjusted difference including chamber and calendar month fixed effects, standard errors in parentheses, clustered at the (chamber x hearing of entry) level. Significance levels are denoted as follows: * p<0.10, ** p<0.05, *** p<0.01.

Table A12: Effect size balance of observations included in the tax record and firm survey samples

	Panel A: Tax record sample (party level)			Panel B: Firm survey sample (firm level)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Likelihood of pre-trial completion in 4 months	Duration of pre-trial hearings (in days)	Intention to appeal	Share of firm's cases done in 4 months	Average duration of firm's cases	Share of firm's cases with intention to appeal
In tax record sample	-0.02	15.32***	0.02			
	(0.02)	(5.53)	(0.02)			
Entered after decree application	0.14***	-74.01***	0.04			
	(0.02)	(6.73)	(0.03)			
In tax record sample X Entered after	-0.03	-2.84	-0.03			
	(0.03)	(7.88)	(0.04)			
In survey sample				-0.02	-0.84	0.01
				(0.04)	(13.31)	(0.05)
Case(s) after decree application				0.12***	-63.34***	-0.01
				(0.02)	(6.01)	(0.03)
In survey sample X Case(s) after				0.01	9.57	0.01
				(0.06)	(16.07)	(0.07)
Effect if in sample	0.11	-76.85	0.01	0.13	-53.77	0.00
P-value: effect if in sample	0.00	0.00	0.89	0.02	0.00	0.97
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.12	0.19	0.04	0.02	0.06	0.00
Observations	5401	5112	3093	2154	2081	1480

Notes: Balance on decree effect size. Panel A: Comparing decree effect size for case parties across court-only and tax record sample; as in the tax record analysis using ANCOVA, controls include chamber fixed effects and calendar month fixed effects, and standard errors (in parentheses) are clustered at the (chamber x hearing of entry) level. Panel B: Comparing decree effect size for firms across court-only and firm survey sample; as in the firm survey analysis, we control for the number of cases the firm had; as the baseline number of employees (also controlled for in that analysis) stems from the firm survey, here we use baseline (2012) revenues (inverse hyperbolic sine transformation) to control for firm size instead; robust standard errors in parentheses. All models estimated by OLS. Significance levels are denoted as follows: * p<0.10, ** p<0.05, *** p<0.01.

Table A13: Robustness checks of the court event study design

	(1) Number of incoming cases	(2) Above median claim	(3) More parties	(4) Commercial cases	(5) 2012 revenues (IHS transf.)	(6) 2012 revenues (IHST, plaintiff)	(7) 2012 revenues (IHST, defendant)
Hearing after decree application	7.14						
	(4.69)						
Trend	-0.09						
	(0.11)						
Interaction	0.34						
	(0.29)						
Entered after decree		-0.04	-0.01	0.03	-0.15	0.84	-0.74
application (β)		(0.05)	(0.03)	(0.21)	(0.66)	(0.81)	(1.08)
Trend		0.00***	-0.00*	-0.01	-0.03*	-0.04*	-0.01
		(0.00)	(0.00)	(0.00)	(0.01)	(0.02)	(0.02)
Interaction		-0.01**	0.00	0.01	0.12***	0.07	0.12*
		(0.00)	(0.00)	(0.01)	(0.04)	(0.05)	(0.07)
P-value 6pWCBoot	0.04	0.56	0.84	0.85	0.84	0.10	0.68
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Without adj. period	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-mean	14.28	0.51	0.25	0.68	20.44	21.05	19.85
Pre-sd	11.72	0.50	0.43	0.47	6.35	5.61	6.94
R-Squared	0.42	0.19	0.08	0.04	0.01	0.04	0.02
Observations	279	3286	4534	4795	3437	1692	1745

Notes: Structural break diagnostic at the chamber-hearing (col 1) and case (cols 2-4) levels. Col 1: Hearing after decree application is a dummy that takes value 1 if the hearing is taking place after the chamber's application of the decree. Cols 2-6: Entered after decree application is a dummy that takes value 1 when a case entered its assigned chamber after the application of the decree. Cols 1-6: All models include a linear trend - allowed to change after the decree (Interaction), chamber and calendar month fixed effects, and a constant. Col 1: standard errors clustered at the hearing level. Cols 2-6: standard errors are clustered at the (chamber x hearing of entry) level. Window includes cases entering between 38 and 4 hearings before and between 4 and 21 hearings after decree application. Significance levels are denoted as follows: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

B For online publication only: Additional Robustness

B.1 Additional robustness of the court event study design

In this Appendix we perform two additional classes of robustness checks on the event study design presented in Section 5.2.3. First, we analyze changes in volume and composition of the incoming caseload as potential sources of bias. Second, we also discuss bias in the event study estimates coming from selection on trend shock in unobserved case characteristics.

We first investigate the possibility that the timing of the introduction across chambers may be endogenous to chamber characteristics. Given that all estimating equations control for chamber fixed effects and allow for different linear trends across chambers, this would only pose a threat to our identification if either the order of application of the decree was decided based on (expected) differential shocks, or if chambers that were already applying the reform received different treatment compared to those not yet applying it. For instance, differential changes in (expected) chamber caseloads around the reform could have motivated the president of the court to target different chambers for applying the decree at different times, with the expectation that the decree would put these chambers on different trajectories. Alternatively, she could have assigned fewer (or, inversely, more) cases to the chambers that were about to apply the decree.

We first test the hypothesis of a smooth trend in the volume of incoming caseload at the chamber level.[34] We run a structural break diagnostic, akin to our main specifications but at the chamber-hearing level. We adapt equation (1) and regress the number of incoming cases in a given chamber-hearing on t-since-application dummies and calendar month and chamber fixed effects. In this modified event study specification, the dummies of interest now indicate the number of hearing periods between a given chamber-hearing and the period in which the assigned chamber applied the decree. We find no evidence of a structural break in the trend for the chamber incoming

[34]As noted in Section 2, the size of the incoming caseload varies across chambers. This is attributable to a certain degree of specialization in each chamber. We additionally verify that there were no changes in delays between a case entering court and being distributed into chambers, and between a case's distribution hearing and first chamber hearing (not reported).

caseload around these multiple cutoffs in the event study specification (Panel A, Figure 4). We obtain similar results estimating (3), as reported in col 1, Table A13.

Second, we look for changes in the composition of the caseload across the implementation cutoffs. Even though we verify that the president of the court did not assign fewer cases to the chambers that just started applying the reform, she could have assigned different types. Alternatively, plaintiffs may have decided to use the court for different types of cases as a result of the decree. We estimate (1) on the size of the claims, number of parties (having more than one party involved on either side of the dispute), and the type of dispute (commercial case). The results corroborate the notion that those case characteristics are not affected by the introduction of the reform (Panels B, C, and D, Figure 4). Again, estimating (3) lead to the same conclusion (cols 2–4, Table A13).

Another potential source of bias is certain types of plaintiffs may have anticipated the enactment of the decree and may have fast-tracked/delayed their case submission just before/after the application in any of the chambers. Inversely, certain types of plaintiffs may have waited for the decree to be applied in all chambers to file their cases. Alternatively, plaintiffs may have delayed or fast tracked certain types of cases (e.g., against certain kind of defendants). We note that such a behavior would likely have resulted in bunching in the chamber-level incoming caseload either over the months before the decree application cutoffs or over the months following it. The event study graph on chamber-level incoming caseload shows no such pattern (Panel A, Figure 4). We use tax administration data to show that there is no jump in the size of firms (measured by 2012 revenues) involved in cases at the cutoff, neither overall, nor for plaintiffs and defendants separately (Panels E, F, and G, Figure 4). This indicates that the effect is neither driven by different firms bringing cases to court after the reform, nor by different firms being sued. Running (3) on these variables returns the same conclusions (cols 5–7, Table A13).

One scenario that our robustness checks cannot fully rule out is that anticipation effects may have caused sorting along unobserved case characteristics across the decree introduction cutoffs. For instance, plaintiffs whose cases have weak or incomplete evidence may have rushed them to court just before the decree introduction to avoid the threat of desk-rejection under decree application. Typically, such cases would tend to take longer in pre-trial and demand more hearings, which would lead us to observe an increase in pre-trial duration just before the introduction of the decree in the event study. This is not what Figure A3 suggests: if anything, we see a decline in duration for cases entering court just before the decree application.

Similarly, the start of decree application in the first chamber may have led those plaintiffs who want a fast resolution but are expecting a long pre-trial for their case (e.g., because they are facing a defendant known for resorting to dilatory tactics) to delay their entry into court to match the decree application in their expected chamber. First, it is worth noting that postponing the filing of a case to cut court delays is not an obvious strategy in this context: total time to resolution of the dispute may not change much.[35] Second, should this have been the case, we would see a reduction in pre-trial duration in all chambers right after application in the first chamber, which, for that chamber, would be a true reform impact, but for all other chambers would be an artifact of the change in case composition (the cases with a longer expected delay are missing from the pool); this initial effect would be followed by an increase in pre-trial duration back to a higher level once these cases re-enter the pool of incoming cases. As a check, we estimate the event study specification (1) on pre-trial duration and likelihood of completing in four months, if all chambers were simultaneously treated at the first cutoff (t=38, first civil chamber; Panels A and B, Figure A11). As expected, this dampens the drop in pre-trial duration. However, we reassuringly observe a decline in duration after the first introduction cutoff, and a smooth downward slope thereafter, indicating no bunching of slow cases in the aftermath of the decree application.

Finally, we return to the placebo checks discussed in Section 5.2.3, and note that these placebo checks confirm that judges at least partially apply the reform to their ongoing caseload at time of decree application, generating fuzziness at the discontinuity. First, we do not see a larger anticipation window for cases entering chambers in which the decree was applied later. This is consistent with the fact that we do not find any pre-reform increase in the likelihood that judges desk-rejected cases or imposed strict deadlines on parties requesting an adjournment (Panels C and

[35] From thorough interviews with civil and commercial law firms we learned that lawyer's fees are typically composed of both a pre-agreed rate and a premium proportional to the awarded amount and they are both unrelated to realized case duration. In addition, by the time the last chamber was treated (March 2014), the first chamber had only been treated for five months; it is hard to imagine that actors had time to form clear expectations of post-decree duration within the decree application window.

D, Figures A6 to A9). Second, we see a modest pre-jump in the probability of fast-tracking cases in all placebo tests (Panel E, Figures A6 to A9). Similarly, we find that the number of hearings starts to decline for cases that entered just before the decree was applied (Panel F, Figures A6 to A9). This suggests that some judges apply the new rules retroactively to their ongoing caseload. Similarly, estimating the event study design forcing the same decree application date for all cases does not change the magnitudes of impact we report (Figure A11). By describing anticipation effects on our main outcome of interest (pre-trial duration) chamber by chamber, these placebo checks indicate that, if anything, anticipation effects would bias our estimates of reform impact downwards.

These robustness checks unanimously corroborate that the event study design likely produces unbiased estimates of the causal impact of the reform on the speed of justice.

B.2 Robustness of results to desk rejections

To what extent do desk-rejected cases return to court? In procedural terms, a re-submitted desk-reject will look like an entirely new case, and there is no identifier linking original and re-submitted cases. The scant case characteristics we have access to only allow us to imprecisely distinguish re-submitted cases (concerning a matter as previously filed) from new cases (concerning a different subject matter) between the same parties. Nevertheless, we try to get a sense of the issue, and look at desk-rejected cases involving at least two firms, the subset for which the precision of the match is highest. Out of 54 desk-rejections involving at least two firms, only about one third appears to have returned to the court. Unfortunately, our data do not allow us to identify changes in the case file submission, and therefore we cannot tell whether a case was re-submitted with the same case file or whether supporting documents were added. However, the fact that two thirds of these returning desk-rejections are re-submitted over a month after the desk-rejection suggests some additional case preparation from the plaintiffs (the average time to re-submission is two months, and the maximum, six).

Among these identified re-submitted desk-rejections, 14% are still ongoing at the end of the study period, while for cases submitted for the first time in the same period this share is 32%. Of the re-submitted desk-rejections that are completed, only 56% ended with a judgment, compared to 74% for first-time submissions. Interestingly, this reduction in judgments as the final outcome is driven by an increased likelihood that the plaintiff lifts their claim: this happens for 28% of completed re-submitted cases, while this number is only 9% for first-time submissions. Eleven percent of these completed re-submissions, or two cases, were struck with a second desk-rejection (similar to the share among first-time submissions, which is 13%); both returned again, and their second re-submission ended with a judgment. Together with the fact that only about one third of desk-rejected cases return at all, and that most do not do so immediately, this finding suggests that desk rejections are indeed used by judges to prevent baseless and poorly prepared claims from entering the pre-trial phase.

C For online publication only: Judges' workload

We adapt the event study specification (1) to document changes in judges' workload and report overall changes in judges' incoming and ongoing caseload around the decree introduction cutoffs (Figure A12). While aggregating our data to the judge level weakens the precision of our estimates, we observe that the number of cases heard at each hearing increases in line with the upward trend in judge-level incoming caseload (Panels A and C, Figure A12). Interestingly, judges' ongoing caseload remains relatively flat (Panel B, Figure A12). Although we lack statistical power to precisely measure these effects, the patterns indicate that the reform did not lead to a meaningful increase in judges' backlogs, corroborating the notion that the reform improved judicial efficiency.