

Reforming the Speed of Justice: Evidence from an Event Study in Senegal

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Preliminary & Incomplete

This Version: June, 2017

Can changing the rules of the game affect judges' performance? We study the effect of a simple procedural reform on the celerity of civil and commercial adjudication in Senegal. The reform gave judges the duty and powers to meet a clear deadline. We combine the staggered rollout across the six civil and commercial chambers of the court of Dakar and high-frequency caseload data to construct an event study. We find a large reduction in the length of the pre-trial stage of 35-46 days (0.24-0.32 SD). The effect is similar for small and large cases, and is attributable to an increase in the *decisiveness* of each hearing: the number of case-level pre-trial hearings is reduced, as judges are more likely to set hard deadlines. These gains in speed do not come at the cost of quality, while we document positive firm-level welfare impacts.

Keywords: Judiciary, Litigation Process, Efficiency, Bureaucracy, Public Organization, Public Administration, Economic Development

JEL Classification: K41, D73, O12

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I. Introduction

Stronger institutions lead to higher levels of investments (Pande and Udry, 2006; Le, 2004; Rodrik, 2000 and 2005), and capital accumulation drives a higher growth rate (Barro, 1991; Mankiw, Romer, & Weil, 1992; Solow, 1956). Courts play a central role in strengthening institutions, and the speed of justice is typically referred to as a key indicator of a country's institutional efficiency (Djankov et al., 2003; Lichand and Soares 2014; Alencar and Ponticelli 2016; Visaria 2009). Whether to start or close a business, register property (including intellectual), protect investors or enforce contracts, firms need to rely on the legal system. Slow justice delivery is associated with a poorer business climate.

Yet, the evidence on policy options to cut legal delays is scant (Chemin, 2009b). Most legal reforms are rolled out non-randomly across courts, judges or cases. Coupled with aggregate, annual data, the evidence linking faster justice to investment often fails to establish causality (Aboal et al., 2014). Perhaps more problematic, the quality tradeoffs and welfare implications of speeding up adjudication have not, to this day, been empirically documented.

Can changing the rules of the game affect judges' performance? We combine micro data on court cases and firms to document the causal impact of a legal reform on delays, quality of adjudication, and firm-level welfare impacts. In 2013, Senegal's Ministry of Justice introduced a decree aiming to increase the celerity of civil and commercial adjudications. The reform gave first-instance judges the duty and administrative powers to meet a procedural deadline during the pre-trial stage of a trial. First, the decree set a four-month limit on the duration of pre-trial hearings—which historically accounted for over two thirds of the total duration of a case in first instance. Second, the reform imparted new

administrative powers to judges. Specifically, it encouraged judges to enforce submission of supporting evidence from the outset and impose strict deadlines to the parties along the pre-trial process. We combine a staggered administrative rollout across chambers of the regional court with high-frequency case-level data to construct an event study and identify the causal impact of the reform on the speed of justice.

This study makes four central contributions. First, we bring new evidence on the determinants of judicial efficiency. Court-level studies tend to be circumscribed to richer economies (Chang and Schoar, 2006), and have limited case-level data (Coviello et al., 2015). In fact, the most granular data is typically judge-month statistics (Chemin, 2009a&b; Lichand and Soares, 2014; Alencar and Ponticelli, 2016). In contrast, we have full access to bi-monthly audience and case-level data from the Regional Court of Dakar. We build a high-frequency panel of all cases that entered the court between 2012/2015.² For each case, we retrace all procedures and hearings from entry to final judgment. This allows us not only to document the impact of the reform on the overall speed of justice, but also provide evidence on the underlying mechanisms and differentiate *intensification* from increased *decisiveness* of the procedure.

Second, we innovate by proposing causal estimates of the impact of a judicial reform. Chemin (2009a) uses yearly court-level data to identify the impact of a legal reform in Pakistan, exploiting district-level variations in coverage. We use within-court variations in coverage and high-frequency case and hearing-level data to construct an event study around a change in legal procedure. This allows us to isolate the causal impact of the reform on the speed of adjudication as well as quality and welfare impacts.

² As the data becomes available, we will extend the analysis to include 2016 and investigate longer-term effects.

Third, we add to the literature on public service reform by formally documenting the case-level impact of a national change in civil and commercial procedure. We exploit a legal reform that imposed deadlines on the duration of the purely administrative part of the trial (pre-trial hearings) to study judges' incentives and the drivers of procedural delays. We follow Bandiera, Pratt and Valenti (2009) and apply a *passive* vs. *active* delays framework. of pre-reform delays. Our main innovation is the richness of data we have access to in documenting judges' decisions along the judicial chain. This allows us to exploit both changes in the distribution of delays and rich caseload data to identify the margin at which procedural waste is reduced.

Last, we build evidence on the behavioral effects of deadlines. Delays in court may result from strategic behavior on the judges' part, whereby additional procedural time yields more precise evidence or higher likelihood to extract rents. Alternatively, they may just be a manifestation of *irrational* procrastination (Akerlof 1991). The reform we study is akin to the deadline experiment proposed by Chetty et al. (2014) in which they manipulate the delay for journal referees to complete their review. An important difference is that, in our set-up judges are not explicitly reminded of the deadline at any point—hence not “nudged” into action close to the deadline through the use of reminders. Instead, our results come from a change in the default delay within which judges are expected to complete their pre-trial hearings. Finally, we test for “tunnel vision” (Mullainathan and Shafir 2013), whereby setting tight deadlines on one activity may increase the quality of the output subject to the deadline, while reducing performance on other tasks.

We find the reform positively affected the speed of justice by both reducing formalism and increasing the efficiency of the procedure. We find a large reduction in the length of the pre-trial stage of 35-46 days (0.24-0.32 SD), as judges are 30.6-43.5% more likely to apply the

four-month deadline (an increase of 15.2-21.6 pp. from a baseline of 49.7%). We show that this effect is attributable to an increase in the *decisiveness* of each hearing, as the number of fast-tracked cases increases (23.8 percentage points), case-level pre-trial hearings are reduced (0.21 SD), while judges are 40% more likely to set hard deadlines. We investigate possible speed-quality tradeoffs, and find no evidence of judges' effort displacement from deliberations to pre-trial stages across all available measures: decision hearings are scheduled at the same speed, the overall number of hearings does not increase, and the quality of the evidence and the decision do not appear to be affected by the reform. We also show that the decree does not affect parties' intentions to appeal court decisions. Finally, interviewing firms who used the court in our study period suggests positive welfare impacts of the decree, both in a stated preference approach and comparing firm perceptions and economic activity across the decree cutoff.

The remainder of the paper is organized as follows. We provide some element of background on Senegal's justice system and the legal civil and commercial procedure in Section 2. Section 3 places the reform in the context of the Senegalese civil and commercial code of procedure. Section 4 details the data; Section 5 presents the theoretical framework and Section 6 the empirical strategy. Section 7 lays out our main empirical results, and Section 8 concludes.

II. Civil and commercial justice in Senegal

Senegal offers a good context to study the effect of a reform in court procedures, for three reasons. First, Senegal is a civil law country, which implies a relatively a high degree of formalism and, therefore, lengthy procedures (Djankov et al., 2003). Senegal ranked 142

out of 189 economies in the “contract enforcement” category of the 2014 Doing Business Report, suggesting a significant margin of improvement in the speed of commercial dispute resolution. We now detail the architecture of the court and legal procedure that make the context of our study.

1. The court

Our study takes place in the regional court of first instance of Dakar, Senegal. Senegal is a civil law country, and judges are organized in chambers, consisting of a president and two additional judges (*collegiality*). While the court of Dakar adjudicates all types of affairs, we focus on civil and commercial justice. At the beginning of our study period, in 2012, there were 4 commercial and 3 civil chambers in the tribunal of Dakar. Tables 1 and 2 describes the variations we have access to at the chamber and case levels, respectively.

Commercial and civil procedures in the tribunal of first instance consist of the following general steps (see Annex A for a full schedule of the procedure): referral (*saisine*), enrollment (*enrôlement*), distribution (*répartition*), pre-trial hearings (*mise en état*), decision (*délibération*), and judgment (*jugement*). Referral and enrollment are purely administrative steps that do not require the involvement of the judges. In 2012, 1546 new civil and commercial cases were enrolled. Distribution consists in the assignment of the new caseload to the chambers by the president of the court; it is notionally made on the basis of existing caseload and, to a limited extent, the specialization of each chamber. Chambers follow a schedule of hearings. Each chamber disposes of two dates per month on which hearings can be scheduled. Each hearing opens with the assignment of new cases to pre-trial judges, chaired by the president of the chamber. Next, each pre-trial judge chairs her scheduled pre-trial hearings. Finally, the president of a chamber chairs decision

hearing, attended by the parties and all judges serving in that chamber (*collégiale*). On average, a chamber takes in 16.4 new cases at each hearing (bi-monthly), ranging from 9.1 to 26.8 across chambers and years (Table 1).

Over the 2012/15 study period, two chambers closed: the 4th commercial in 2015, and the 2nd civil in 2014. These closures led to increases in the size of the ongoing portfolio in other chambers, as their ongoing cases were redistributed across the tribunal by the court president. These changes in portfolio are uneven across chambers, due to a certain degree of specialization of each chamber (Table 1).

Commercial and civil disputes vary widely in their nature and complexity. Commercial cases include mostly payment and other contract disputes, including sale and rent contracts involving a moral person (firm). Similarly, civil cases include contract and payment disputes between individuals (e.g. landlord and tenant), as well as other civil issues like divorces. 63% of civil and commercial disputes in our sample include a payment claim. Among these, the average claim amount is of CFA 75,604,000 (or about 157,000 dollars), ranging from CFA 75,000 to CFA 8,700,838,000 (about 160 dollars to 18,098,000 dollars; Table 2).

2. Procedure

We now provide a simplified overview of the first instance civil and commercial procedure in Senegal, focusing on the pre-trial hearing and decision stages leading up to the publication of a judgment.

Pre-trial phase. At its first hearing, a case is assigned to a pre-trial judge by the president of the chamber, starting the pre-trial phase. During this phase, the parties are invited to

build up their case. Pre-trial hearings are chaired by the assigned pre-trial judge. Throughout this phase, the judge serves an administrative role. She is required to show impartiality while the parties are expected to build their case and, therefore, does not directly influence the quality of the parties' arguments. Consequently, the parties present their arguments, supporting documents, and procedural pleas, and additional expert reports may be ordered.³ On average, a chamber handles 146 ongoing pre-trial cases per hearing, ranging from 37 to 225 (Table 1). The outcome of a pre-trial hearing is either the referral of the case to an additional pre-trial hearing, or the conclusion of the pre-trial phase and beginning of the decision phase.⁴ The judge can mark a referral as "strict" or "final" to communicate to the parties the urgency to conclude the pre-trial hearings. The pre-trial phase ends when the judge declares it closed and sends the case to the decision stage by scheduling a decision hearing.⁵ Before the reform, a case underwent on average 8.08 pre-trial hearings over a 153.03-day period (Table 2).

Decision phase. In the period preceding the decision hearing, the three judges of the chamber individually review the case file and meet in closed sessions to discuss the arguments put forward by the two parties. There are three possible outcomes to the closed-session deliberations of the judges. If a conclusion is reached, the judgment is pronounced. If the judges need more time to come to a conclusion, the decision is postponed and the date of the next hearing is announced. If the review of the case file reveals that the pre-trial failed to collect the evidence necessary to come to a conclusion, the case is referred back to

³ In practice, the documentation is presented in written form by handing one copy to the opposing party and the other to the pre-trial judge for inclusion in the case file.

⁴ Other, rare, hearing outcomes (both in the pre-trial and the decision stage) are a nullification of the case at the request of the plaintiff and an amicable adjudication at the request of the parties.

⁵ During the pre-trial phase, the case file is kept at the enrollment office and is only seen by the pre-trial judge briefly during the hearings when adding to it written arguments and evidence brought forth by the parties. The first time the pre-trial judge assesses the case file in its entirety is when he/she verifies its completeness at the end of the pre-trial (*vérification*). In contrast, the case file stays with the judge throughout the decision stage.

the pre-trial stage (“pre-trial insufficient”). These outcomes are announced in the presence of the parties during the decision hearing, and documented in the decision hearing minutes. On average, a chamber has 54 ongoing decision cases per hearing, ranging from 3 to 107.3 across chambers and years (Table 1). Before the reform, an average case was deliberated and judged in 2.35 hearings over a 57.15-day period (Table 2), and a median duration of one month.

Shortly after a judgment is pronounced it is made available to the parties, ending the first-instance proceedings.

III. The 2013 reform of the pre-trial phase

The legal reform at the center of our study explicitly stipulates the goal of speeding up dispute resolutions to attract investors and private equity funds (Ministère de la Justice, 2013). The decree (n°2013-1071, dated August 6, 2013) was adopted by ministerial council on July 18, 2013. It modified the civil procedural code to address both supply and demand-side bottlenecks in the pre-trial procedure, in two main ways: first, it set a four-month limit on the duration of the pre-trial procedure; and second, it assigned new powers to pre-trial judges.

First, it imposed a four-month limit on the length of the pre-trial procedure. Before the application of the decree, only half of all cases completed the pre-trial procedure in four months or less (Table 2). Once the four-month period ends, a judge can move a case to decision *as is* (“*en l’état*”).

Second, judges were given more control over the speed of pre-trial hearings. Specifically, it allowed judges to exert pressures on the parties to avoid dilatory actions, by managing

additional expert reports and inquiries he may have requested from the parties more closely, and allows judges to declare a case *inacceptable* in the very beginning of the pre-trial.⁶ Furthermore, additional “circuits” are created, allowing urgent cases to be judged at the outset, without undergoing pre-trial hearings.

An important feature of the decree application that we use in our empirical analysis is that the new deadline was not enforced by court management. This is for both practical and legal reasons. In practice, the court did not dispose of a case-management system to track adherence to the decree at the case level. In legal terms, judges benefit from full independence in Senegal, making enforcement of procedural deadlines unfeasible.⁷

IV. Data

We measure the impact of the reform using two sources of data: administrative caseload data and firm-level data.

1. Court data

We have access to administrative data on the full caseload across all seven civil and commercial chambers of the first-instance court of Dakar, Senegal, over the 2012/15 period.⁸ Digitizing these records is at the core of our contribution, as court data were only available in paper form at the onset of the project. In the context of the World Bank’s

⁶ In the previous version of the code, pre-trial judges could not dismiss a case brought forward without sufficient supporting evidence. Instead, such cases would undergo the pre-trial procedure for a duration not specified in the code, during which the supporting evidence would either materialize or fail to be assembled, going forward to the deliberations as is. An incomplete case sent to deliberations would either be sent back to pre-trial (declaring the evidence insufficient for a decision to be made by the collegiality), or the decision would be made on the incomplete evidence.

⁷ One common criticism of the decree, in this respect, is that it did not involve court clerks who, unlike judges, could be subject to deadlines.

⁸ In the current version, we use data up to mid-2015; we are in the process of adding the remainder of 2015 and up to mid-2016 data, yielding two full years of post-reform observations for all chambers.

Economic Governance Project, we put in place a team of court-based enumerators to digitize all archives going back to 2011 and set up a real-time data entry for the ongoing caseload. This thorough data capture allows us to observe steps in the legal chain along two dimensions.

First, we collect case-level information on the full civil and commercial caseload over the 2012/15 study period. For each case, we have a record of when it entered the court, which chamber it was transferred to for the pre-trial procedure (first hearing), when, and which judge presided over its pre-trial hearings, the date of the judgement and the text of the decision itself (minutes), as well as some case characteristics (civil or commercial, contested amount, number of parties on each side).

Second, we have case-hearing-level data from all pre-trial and decision hearings held by the seven civil and commercial chambers. These data record of which cases were heard in each hearing and the corresponding outcome of the hearing. Hearings are scheduled on a bi-monthly basis, on a chamber-specific schedule that is set every 6 months by the president of the court; this yields 21 hearings per chamber per year, after removing the summer break.⁹ All judges in a given chamber must hold hearings at the dates set in the schedule. Yet, not all ongoing cases must be heard at every hearing, yielding variations in both length and intensity of the procedure across cases. To capture case-level outcomes, we collapse this case-hearing-level data to the case-level to retrace the complete history of the entire caseload that entered the court over the 2012/15 period. This yields an analysis sample of 5,169 cases. We obtain case-level outcomes that allow us to gauge both the celerity and the quality of the procedure, at pre-trial and decision stages separately. We construct the main

⁹ A six-week summer break is established at the chamber level over the three-month period August-October, on a rotating basis across chambers, and all judges in a given chamber must take leave during this period.

outcomes of interest, the duration of the pre-trial phase and a binary variable indicating whether a case completed the pre-trial phase within four months (the new deadline imposed by the reform). We also compute the duration of the decision stage and a construct a binary indicator of whether a decision was reached within a month (pre-reform median duration), to check for positive or negative spillovers of the reform on to that phase of the trial.

We then construct additional case-level outcomes to shed some light on the channels the reform may have worked through: the number of pre-trial and decision stage hearings, the share of hearings in which a case was heard,¹⁰ and whether the judge pronounced a strict or last referral at any time during the case's pre-trial ("judge more strict"). Next, we derive a basic indicator of pre-trial quality: whether the case was sent back to pre-trial from the decision phase ("pre-trial failure"); and a measure of judges' effort at the decision stage: whether the collegiate pronounced in the decision phase that they could not deliver the decision at the hearing they had planned to ("decision postponement"). Finally, we use the minutes of the decision hearing to document the quality of the decision itself: number of articles cited in the judgement; length and relative length of the decision justification; and parties' intention to appeal the decision.

Table 2 provides baseline summary statistics for these outcomes. Before the reform, the pre-trial lasted on average 153 days, and had 8.1 hearings in that period. 49.7% of cases completed the pre-trial in four months or less, and 12.1% had no pre-trial but went straight to decision phase. Cases had on average 2.6 hearings over the duration of the decision-stage which lasted on average 64 days, but 49.4% of cases completed it in a month or less. While a

¹⁰ This outcome is computed as a ratio of the number of hearings in which a case was heard over the number of hearings that took place in the chamber while the case was ongoing.

case was ongoing in the pre-trial phase, there was a high likelihood it would be heard at a scheduled hearing (88.7%); this likelihood was somewhat lower in the decision phase (77%). Judges issued stricter referrals for only 15.4% of the sample pre-reform. The pre-trial was declared insufficient for 11.5% of cases and the decision postponed for 5.5% of cases.

Second, we build chamber-hearing-level outcomes, collapsing all case-hearing-level outcomes at the chamber-hearing level. This yields a sample of 21 hearings per chamber per year. We use these data in our robustness checks, to describe the inflow of cases (volume & type) in each chamber and in the court over time.

2. Firm data

Ultimately, we are interested in capturing the welfare implications of the reform on firms involved in legal disputes. From August 2016 to February 2017, we tracked and interviewed firms who had cases in the court of Dakar over our 2012/15 study period. In total, 2,209 firms were involved in 2,688 distinct cases that involve firms in our study sample. We recover addresses and/or phone numbers for 1934 out of these 2209 firms, through a combination of court records, name merging firms and a national registry of firms operating in Senegal which contains contact information fields (the Répertoire National des Entreprises et Associations, RNEA), and searches in public address books and a web search engine. Out of the remaining 275 firms, 91 were located outside of the survey area (abroad or in a different region of Senegal) and for 184 no contact information could be obtained. Conditional on being located, our response rate is 31%.¹¹

¹¹ These are preliminary figures since cleaning and verification, as well as final tracking were still ongoing as we wrote this version of the paper. This non-response rate is composed as follows: 35% refused to answer our survey (shared almost equally among those who refused outright and those who postponed meetings), while 39% could not be located despite having contact information on record and 7% or were found not to exist anymore (as corroborated by neighbors). This level of response rate in firm surveys is common: the World Bank-led 2014 Enterprise Survey in Senegal obtained a 44.71% response rate.

We interview the owner, CEO, or legal counsel of each firm, by order of preference. We survey a range of economic outcomes and perceptions of the justice system, and record stated preferences for faster pre-trial proceedings.

V. Conceptual framework

We offer a conceptual framework in which we outline judge-level determinants of procedural efficiency. Describing judges' incentives allows us to formulate simple predictions on their response to the introduction to the decree and characterize the class of delays the reform helped address.

Judges are career bureaucrats competing for promotion to the higher levels of the judiciary. As such, they expend effort to convince their peers and superiors of their talent and, possibly, extract other private benefits from their position (Dewatripont et al 1999a&b). Judges' incentive to delay a procedure vary across phases of the trial. At pre-trial, a judge's speed (throughput) is the main signal a judge can send to her management about her effort level. Yet, speed influences the ability of a judge to increase the precision of the evidence, or engage in rent-seeking. At decision, the quality of the justification is the main signal, and is a function of the precision of the evidence. As such, increasing pre-trial delays may yield higher payoff for larger or more complex cases, whether in terms of bribes of precision of the evidence. More complex cases may send a stronger quality signal than simpler ones; bigger litigations may attract larger bribes.

Passive delays occur when judges do not extract private benefits from longer pre-trial hearings. In this setup, judges simply procrastinate and fail to set firm deadlines in pre-trial hearings. This procrastination comes at a cost to the judges, as it multiplies the number of hearings and, therefore, time they spend on each case. Hence, this is akin to

what Akerlof (1991) describes as irrational procrastination. In this case, the reform simply nudges judges to adopt a new delay by solving a coordination problem.

We adapt Bandiera et al (2009) to characterize the effect of the decree on procedural delays.

Judges' objective function is

$$\Omega_{ijk} = -\varphi_{ijk} + \beta_{ij}b_{ik}$$

where b_{ijk} is the personal benefit accruing from case k to judge i ; β_{ij} is the active delay parameter (which normalizes the cost of delays to 1); $\varphi_{ik} = f(b_{ik}, \mu_{ij})$ characterizes procedural delays, with $\frac{\partial f}{\partial b} > 0$, and μ_{ij} is the passive delay parameter, $\frac{\partial f}{\partial \mu} > 0$.

The active delay parameter β_{ij} captures norms and rules that a judge faces that make it costly to delay procedures, or the risk of "being caught".

The passive delay parameter μ_{ij} captures the level of procedural formalism that prevent a judge from increasing the speed of adjudication. One way to interpret this parameter in our setting is that, pre-reform, lawyers and parties are aware of the judges' limited power to reject poor evidence. As a result, a plaintiff has no incentive to bring forward a complete case. Cases are brought forward too early, with incomplete evidence, which increases delays. The extreme version of this problem are cases brought forward without any substantive evidence.

Another expression of this passive waste parameter is that judges may procrastinate in managing pre-trial hearings, and fail to set deadlines to parties. Pre-reform, there is no explicit time limit on pre-hearing delays. Judges operate within socially acceptable norms, or what court management values.

Did the reform affect mostly passive or active delays? By setting deadlines, the reform lowers μ_i , as it reduces the level of procedural formalism judges face in setting tight timelines to parties. Since delays increase in both parameters μ_{ij} and β_{ij} , estimating the average effect of the reform on procedural delays will not be enough to characterize their nature. Instead, we use the fact that, in equilibrium, judges with different preferences for active and passive delays should pick different levels of private benefit b_{ijk} across small or simple cases, and large or complex cases.

If the reform affected delays mostly through judges with a stronger preference for private benefits, we should observe that judges respond by specializing in extracting private benefits from larger or more complex cases. In this case, they would reduce delays but increase the hearing intensity on larger and more complex cases. Instead, hearing intensity would remain constant on smaller and simpler cases.

If the impact of the reform operated mostly through judges with a lower preference for private benefits, we should instead observe that judges respond by decreasing the duration of all cases and reducing the number of hearings across all types of cases. Instead, judges would have to resort to their new powers to increase the *decisiveness* of pre-trial hearings and move closer to the enforcement frontier. In both cases, the effect of the reform on the quality of the evidence and, therefore, of the decisions, is a priori ambiguous.

VI. Empirical strategy

1. Empirical specifications

We employ an event study design with multiple cutoffs to capture the causal impact of the reform on the speed of justice in the regional first-instance court of Dakar.¹² We exploit the fact that, while the decree was ratified in July/August 2013 and published in October 2013,

¹² The event study approach is akin to that used by Jensen (2007), Guidolin and La Ferrara (2007), and Atkin et al. (2015).

it was applied at different times across the 7 civil and commercial chambers of the regional court, reaching full coverage only in March 2014.

Using high-frequency data around these multiple cut-offs and two years of pre-intervention data, we are able to identify the causal effect of the reform, net of all other contemporaneous factors, in a flexible event study framework. The intuition is that if the reform had an effect on the outcomes of interest, we expect to see a structural change in that outcome at the time of the reform's application. For example, we should see a sharp increase in the speed of adjudication for the cases having entered the court close to the application threshold, relative to those that entered earlier. The high-frequency multi-year nature of our data, together with the staggered introduction of the reform across chambers, allows us to attribute this change to the reform, as we can exclude as causes seasonality or other events, and structural changes external to the court. In fact, for an external event to be responsible for the observed structural change in the outcome of interest, it would have had to affect each chamber at the precise time the latter introduced the reform, which is unlikely.¹³ We estimate three main models to measure the impact of the decree on the speed and nature of court procedures. The first is our main event study model. In practice, we estimate a flexible functional form that assigns one treatment effect per case entry period, as follows:

$$y_{ij} = \alpha + \sum_{\tau=-38}^{20} \beta_{\tau} \mathbb{1}(tAE_{ij} == \tau) + D_m + D_j + \varepsilon_{ij} \quad (1)$$

y_{ij} is an outcome of case i , in chamber j ; tAE_{ij} indicates the number of hearings (half-month periods) case i entered in chamber j after the application of the decree in that chamber at

¹³ Events and actions *internal* to the court are a more plausible source of endogeneity, which we will address below among the robustness checks.

hearing T_j . Hence, 0 is indexed to be the first hearing of application of the decree in a given case's chamber (regardless of the actual application date), and negative values indicate that a case entered before the application of the decree, while 0 and positive values refer to entry after application.; $\mathbb{1}(tAE_{ij} == \tau)$ is an indicator function that takes value one if case i entered τ periods away from chamber j 's application of the decree.¹⁴ In other words, we include one dummy per period of entry relative to the decree application in the chamber, estimating one treatment effect per case entry period. If the reform had an effect, we expect to see significant a jump in these dummy coefficients around $\tau = 0$. D_m and D_j are calendar month and chamber fixed effects. Standard errors are clustered at the (*chamber x period of entry*) level.¹⁵

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 current data extraction, and for a given period of entry it is the duration of the longest cases that is missing. While this censoring should only cause a negative trend in our dummy coefficients, and not a jump, we nevertheless estimate a second model that takes duration censoring into account: We combine the event study approach with survival analysis to estimate the effect of the reform on the outcome case duration. In practice, we use a Cox proportional hazard model to 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 adds to the simple OLS estimation proposed in (1) in that it corrects for censoring without being subject to selection bias, conditional on

¹⁴ In the current version of the paper we restrict our analysis to a window of 38 pre-decree application and 20 post-decree application hearing periods. We use the January 2012-June 2015 data to construct the same time window around each of the chamber-level decree application dates, allowing for four months' time to complete the pre-trial stage. Hence, entry is restricted to February 2015. In a future version, we will extend the window to 1.5 years post-decree application (30 t).

¹⁵ Our results are robust to a more stringent clustering at the chamber level.

baseline hazard rate $h_0(t)$. Here, failure corresponds to exiting the pre-trial stage. We estimate the following Cox proportional hazard model

$$h_{ij}(t|D_m, D_j) = h_0(t) \exp\left[\sum_{\tau=-38}^{20} \beta_\tau \mathbb{1}(tAE_{ij} == \tau) + D_m + D_j\right] \quad (2)$$

$\hat{\beta}_\tau$ 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. Hence, coefficients below 1 imply a lower probability of exiting, and above 1, a higher probability.

Finally, we flexibly document the average effect of the decree across the cutoff, using one overall treatment dummy and allowing for different slopes in a sharp regression discontinuity framework. For this, we estimate the following model

$$y_{ij} = \alpha + \beta \mathbb{1}(tAE_{ij} \geq 0) + \eta tAE_{ij} + \gamma \mathbb{1}(tAE_{ij} \geq 0) * tAE_{ij} + D_m + D_j + \varepsilon_{ij} \quad (3)$$

where $\beta \mathbb{1}(tAE_{ij} \geq 0)$ is an indicator function that takes value one if the case entered after decree application in chamber j , tAE_{ij} is a linear trend in entry after application, and D_m and D_j are calendar month and chamber fixed effects as before.

We run the analysis on two samples: a full sample, and excluding an adjustment period of three hearings on either side of the cutoff to purge our estimates of short-term adjustments.

2. Robustness

Our identifying assumption is that the introduction of the decree is the main source of variations in the speed of justice in the two years following the application of reform and that, in the absence of the reform, the speed of justice would have followed a steady trend both within and across chambers. As mentioned above, because of the high-frequency multi-year nature of the data and the staggered reform introduction, seasonality and events

outside of the court are unlikely to pose a threat to our identification. However, case assignment to chambers *inside* the court is nonrandom and the timing of the introduction across chambers is likely endogenous to chamber characteristics. This implies that the main threats to our identification are chamber and court-level structural changes that may have occurred around the introduction of the decree. We therefore run the following checks.

First, we test the assumption that the volume of the incoming caseload at the court level is unaffected by the introduction of the decree. Plaintiffs may have anticipated the enactment of the decree and have fast tracked their cases through court just before the application in any of the chambers or, inversely, may have waited for application of the decree in all chambers to file their cases. We show that the number of cases that enter the court over time follows a smooth trend once we abstract from seasonality (Figure 3).¹⁶

Second, we test this assumption of a smooth trend in the volume of incoming caseload at the *chamber* level.¹⁷ This is a relevant check as the court could have assigned fewer (or, inversely, more) cases to the chambers that were about to start decree application. We run a structural break diagnostic, akin to our main specifications but at the chamber level: In a sharp RDD, we regress the number of incoming cases at each chamber hearing on a post-application dummy (treatment), a linear trend, and their interaction. The coefficients on the treatment variable are insignificant, whether or not we allow for an adjustment period (cols 1-2, Table 3). These results show no significant break in trend around these multiple cutoffs (see Figure 1 for a graphic representation, using the chamber-level equivalent of our event study specification).

¹⁶ Note that a spike in incoming caseload is observed every year after the summer break, which we are controlling for by including calendar month fixed effects in all specifications.

¹⁷ 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.

Next, we verify that there is no change in composition of the caseload. This is because even if the court did not assign *fewer* cases to the chambers that just started applying the reform, they could have assigned the *easier* ones. For this, we show that the size of the claims cannot predict the introduction of the reform in a given chamber (cols 3-4, Table 3; Figure 22). Finally, we find no record of court-level changes in the structure of the chambers over our study period, other than the introduction of the decree.¹⁸ These checks unanimously corroborate the validity of our event study design in capturing the causal impact of the reform on the speed of justice.

There is one potential source of bias that our design cannot address: chamber-level endogeneity of the application with respect to anticipated post-reform chamber-level structural breaks.¹⁹ In this scenario, the different chambers decided on the timing of application of the decree in reaction to anticipated chamber-specific shocks. For instance, a predicted increase in the caseload specific to a given chamber may have led the president of that chamber to speed up application. Chamber-level structural changes are unlikely, since the caseload is evenly distributed across chambers by the president of the court twice a month during the distribution hearing. Again, finding that the inflow of cases into each chamber remains constant across the different cutoffs, both on average and individually, indicates this was not the case (Figures 1 and 22).

¹⁸ The only change in the court is the closing of two chambers, as mentioned in Section 2. These closures do not coincide with any of our cutoffs. Since a reduction in the number of chambers implies a cut in the number of judges, these closures should dampen the effect of the decree on the speed of treatment.

¹⁹ We should note that the size of the caseload varies by chamber. This is due to the degree of specialization of each chamber within the broad areas of civil and commercial justice.

VII. Results

In this section, we examine the causal impact of the reform on the length and structure of the pre-trial procedure. We first present results on the overall effect on duration of the pre-trial procedure. Next, we use rich procedure data to document the channels through which the reform affected celerity, and evidence on quality vs. efficiency tradeoffs. Finally, we use firm-level data to gauge the welfare impacts of faster adjudication.

1. Court delays

Pre-trial phase

Did the reform affect the speed of treatment in the pre-trial phase? We find evidence of a clear jump in pre-trial duration for cases that entered a given chamber close to the application of the decree in that chamber (Figure 4). Note that this figure graphs the results from our event study specification in Equation (1) in Section 6, that is, the coefficients of the dummies for the number of hearings a case entered relative to the chamber's decree application date T_j .²⁰ The average effect given by our sharp RDD specification (equation (3) in Section 6) indicates a reduction in the pre-trial duration by 34.77-46.01 days, depending on the inclusion of an adjustment period around the cutoff (cols 1 and 2, Table 4). This is a large effect, on the order of 0.24-0.32 of a pre-reform standard deviation.

Note that our duration variable is censored²¹ (evidenced in Figure 4 by an overall downwards trend in the effect of the entry-period dummies on pre-trial duration). However,

²⁰ Recall that 0 is the first hearing in a chamber under decree application, while negative values indicate pre-reform hearings.

²¹ This is because for any late entry cohort, the longest-lasting cases are still ongoing and hence omitted from this sample.

the event study results in Figure 4 indicate that there is a significant break from this pre-trend at the cutoff; similarly, the RDD results in Table 4 (cols 1 and 2) show a large and significant treatment effect despite controlling for a linear pre-trend (and allowing this trend to be affected by the reform). Hence the censoring cannot account for the observed jump in pre-trial duration. To further support our conclusion that censoring in our measure of duration is not driving the result, we estimate a Cox proportional hazard model as expressed in Equation (2) in Section 6. Our results indicate that the introduction of the decree significantly increased the hazard rate of a case finishing pre-trial by 18.7-30% (Table 4, cols 3-4; see Figure 5 for the event study specification equivalent).

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 four months (see Figure 6),²² an outcome that is not affected by censoring.²³ Recall that one of the decree's innovations was to introduce a fixed four-month delay for the pre-trial hearings. On average, the likelihood of meeting this deadline significantly increases by about 15.2-21.6 percentage points, a 22-43.5% increase (cols 5 and 6, Table 4).

Our conceptual framework indicates that comparing the distribution of pre-trial durations across the application of the reform will shed light on the nature of the delays. We plot kernel distributions of procedural delays pre- and post-reform (Figure 7). The results are stark: after the decree is applied, the bulk of cases see their pre-trial shift to the left. This applies to all ranges of the pre-reform distribution. This is confirmed by a juxtaposition of

²² Just as for these pre-trial duration results, the results of our event study specification (Equation (1) in Section 6) will be presented in the form of graphs (Figures 5, 8-19) while the results from the RDD specification (equation (3) in Section 6) will be presented in table form (Tables 4-8).

²³ Recall that sample and the window of analysis (up to 20 post-decree application hearings) were chosen such that we observe four months of post-decree application data for all cases in the sample.

densities across case cohorts (Figure 8).²⁴ This hints that pre-trial delays were mostly *passive*, and that judges uniformly apply shorter timelines to all types of cases.

Decision phase

The decree explicitly targeted inefficiencies in the pre-trial stage of commercial and civil cases. We look into potential unintended adverse effects of the reform on court efficiency, whereby judges may have shifted effort from the decision stage to the now deadline-enforced pre-trial stage. Our results do not corroborate this notion. First, we do not estimate a significant jump in the duration of deliberations (Figure 9, and cols 1 and 2, Table 5), the hazard rate of completing deliberations (Figure 10, and cols 3 and 4, Table 5),²⁵ nor the likelihood of completing this stage within one month (Figure 11, and cols 5 and 6, Table 5). This confirms that the reform did not immediately affect deliberations. However, we observe that the introduction of the decree induced a significant change in the (linear) trend directing the speed of deliberation (cols 1 and 2, Table 5), towards a reduction in duration, which is corroborated by a possible trend change in the hazard ratio to exit the decision stage (Figure 10) and in the likelihood to complete the decision stage in one month (Figure 11).

This positive effect of the pre-trial reform on the decision stage is all the more surprising as increasing the speed of the pre-trial seems to have led to an increase in the size of the decision caseload (Figure 21), as an increase in judges' workload should be linked to an

²⁴ We include this check because of the censoring our duration measure that induces a mechanical trend towards shorter durations, see above. While we do see evidence of the mechanical trend in Figure 8, a clear jump remains apparent.

²⁵ While computing the hazard rate at pre-trial stage allowed us to fully account for right-hand censoring of the duration outcome, this is not true at decision stage. This is because our sample of decision cases is itself censored: it is restricted to cases that have made it out of the pre-trial phase but the time our data was last extracted (July 2015).

increase in delays (Coviello et al, 2014). These results indicate that the reform did not adversely affect the speed of deliberations. Instead, they suggest that an exogenously induced increase in judges' efficiency at pre-trial stages may have had positive spillovers on the decision phase.

2. Mechanisms

Our policy experiment does not allow us to causally unpack the mechanisms underlying the changes in the speed of justice. Instead, we use our rich case and hearing-level court data to shed light on the channels through which the decree affected duration in pre-trial and decision stages.

Pre-trial stage

First, we look at the number of pre-trial hearings cases undergo around the application of the decree. Figure 12 reports period-of-entry specific treatment effects, as estimated through equation (1) in Section 6. Similar to the effects on duration, we observe a significant and sudden decline in the number of pre-trial hearings undergone by cases that entered the chamber close to the application of the decree. Cases entering a chamber after the decree experienced on average 1.42-1.91 fewer pre-trial hearings, equivalent to 0.22-0.30 SD (cols 1 and 2, Table 6). We also find a modest though significant jump in a case's likelihood to be heard at any hearing scheduled in its chamber over the pre-trial procedure, a 5.1-6.2 percentage point increase from a mean of 88.7% (Figure 14; cols 5 and 6, Table 6). Overall, these results suggest that the decree did not cut delays through intensification in the placement of hearings across a chamber's calendar, but rather by increasing the *decisiveness* of each hearing.

Second, we measure the impact of the reform on the extent to which judges started fast-tracking cases out of the pre-trial stages and into deliberations. Recall that the decree empowered judges to fast-track or dismiss a case for lack of evidence from the onset of the pre-trial procedure. We construct a case-level binary variable that takes value 1 if a case that entered a chamber altogether avoided the pre-trial procedure. We find that pre-trial judges made use of this new power, with a jump in the likelihood of cases experiencing an immediate decision (dismissal or transferal into deliberations) increasing sharply for cases entering the chamber just before the cut-off (Figure 13). The average effect is large, a 18.3-23.8 pp. increase from a pre-decree mean of 13.1% (cols 3 and 4, Table 6). It is important to note that the sharp decline in duration presented in Section 7.1 is not attributable to an increase in fast-tracking, as results are robust to excluding fast-tracked cases from the sample.

Finally, we use hearing-level outcomes to retrace whether a judge ever imposed a strict deadline on parties in non-decisive pre-trial hearings. Again, we find a sharp break away from the trend at the application of the decree (Figure 15). Since we now use a hearing-level outcome, the break appears at 0—the first hearing after the application of the decree in a given chamber. This is a large effect, as judges are 5.5-6.2 pp. more likely to apply a strict deadline on one or both of the parties at the end of a non-decisive hearing, from a baseline of 15.4% (cols 7 and 8, Table 6). This result corroborates the idea that efficiency gains were made not through an intensification of the schedule, but an increased decisiveness at each step of the pre-trial procedure.

Decision stage

Recall that we find no evidence of displacement of judges' effort from decision to pre-trial hearings. Instead, we show a positive impact of the decree on the speed of the decision stage in the form of a shift in the linear trend governing duration. We now examine channels through which the decree may have reduced the duration of the decision stage. Cases that entered a chamber after the decree did not, on average, experience a different number of decision stage hearings, but, as for the decision duration, there is change in the trend (cols 1 and 2, Table 7; also see Figure 16). Similarly, we see no jump in the probability of a case being heard at any scheduled hearing over the course of the decision procedure, but we do observe a change in the trend (cols 3 and 4, Table 7; also see Figure 17). This signals a slowly realizing increase in judge's effort at deliberation stage: after the decree introduction, cases are progressively deliberated faster, in fewer, less spread out hearings.

3. Quality

Finally, we examine potential quality-celerity tradeoffs. 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 a decision along three dimensions: completeness of the evidence brought forward, judges' documentation of the decision, and parties' intention to appeal the decision.

First, we assess completeness of the evidence by looking at the incidence of two decision hearing outcomes: pre-trial failure and decision postponement. Figure 18 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 an insignificant and small average effect (cols 1 and 2, Table 8). Similarly, we find no significant change in the likelihood that a

decision is postponed (Figure 19; cols 3 and 4, Table 8). For both outcomes, there is no change in trend around the introduction of the reform.

Second, we estimate the impact of the reform on the length of and number of articles cited in judges' decision justifications. Again, we do not detect a discernible jump in these outcomes (Figures 23 and 24; cols 1-4, Table 9).

Finally, an important measure of quality of a decision (in first instance) is the probability that a decision gets appealed (Coviello et al, 2014). Similarly, we fail to detect any impact of the reform on parties' intention to appeal, both in the event study design and on average across the introduction cutoff (Figure 25, cols 5 and 6, Table 9). While more work is needed to establish potential composition effects, this result corroborates the idea that the reform did not affect the quality of decisions.

Together with the results on decision duration and decision stage channels, these results suggest that accelerating the pace of the pre-trial procedure did not displace judge's effort away from deliberations, and did not lead to a decline in the quality of neither the evidence nor the legal justification. This corroborates the notion that pre-reform procedural waste was passive: judges did not extract any additional benefit, in the form of increased precision of the evidence, from running longer pre-trial procedures.

4. Heterogeneity

Our conceptual framework predicts that showing a differential effect of the reform across small/large or simple/complex cases would support the notion that court delays are predominantly actively set by judges. We test this prediction by allowing the effect of the decree to vary with the size of the dispute (claim amount). We run an interacted version of

equation (1) in Section 6, allowing for different treatment effects and trends across cases with above and below median claim amount (Tables 10 (including adjustment period) and 11 (excluding adjustment period)).

First, our results confirm the idea that larger claim size is associated with longer procedure delays, on average. Second, we find that the decree equally increased the speed of both small and large-claim cases (col 1, Tables 10 and 11).²⁶ More specifically, the impact of the decree on the likelihood of completing pre-trial in four months is identical across types of cases (col 2, Tables 10 and 11). We fail to detect a differential intensification of the hearing schedule for larger cases (col 4, Tables 10 and 11). Turning to the channels, we find that judges are 10.3 to 12.1 percentage points more likely to apply pressure on larger cases after the decree, while the effect on smaller cases is not significant (difference significant at 10% and 1% level respectively; col 5, Tables 10 and 11). These results lend some support to the idea that judges applied the decree equally to all types of cases. In order to do so, they had to apply relatively more pressure on the parties for large cases. Specifically, the absence of intensification of the procedure for large cases goes against theoretical predictions in the presence of an active manipulation of delays. Taken together, these results lend support to the notion that the origins of pre-reform delays were mostly passive.

²⁶ Here, we run Equation (1), Section 6 with and without adjustment period, without significant changes in results (col 1, Tables 10 and 11). Notice, however, that our results on the differential effect for small and large cases on duration are *quantitatively* sensitive to the exclusion of the adjustment period (-1.8 with adjustment period, 26.7 without, both coefficients insignificant). This is due to a change in the post-trend for larger cases: the coefficient on the triple interaction changes sign across samples. This is attributable to the small post-reform sample size (Figure 23). However, this quantitative discrepancy does not affect our interpretation, as the effects remain significant for both types of cases (the p-values of the effect for large cases are 0.029 and 0.074 respectively with and without adjustment period), and do not differ in magnitude (both coefficients are insignificant).

5. Welfare impacts

We now use firm-level outcomes document the welfare implications of the reform in two ways. First, we elicit stated preferences for shorter delays. We present two scenarios of pre-trial delays, using our empirical estimates. The firm has to contract a lawyer to resolve a dispute of a median amount.²⁷ The choice is between two lawyers: one that can reliably complete pre-trial proceedings at the average pre-reform speed (5 months); and one that can reliably complete pre-trial proceedings at the average post-reform speed (3.5 months) and post-reform durations. Firms are asked how much they would be willing to pay each lawyer, in an open-ended manner.²⁸ We find that firms unanimously report being willing to pay more for a faster lawyer, an average of FCFA 848,784 (about USD 1626), relative to a lawyer performing at pre-reform speed, for which they would pay FCFA 561,833 (about USD 1076). The mean difference of FCFA 290,000 (about USD 550) is significant at the 1% level.²⁹ The kernel densities corresponding to each response are shown in Figure 26.

Second, we exploit the fact that some firms used the court either before or after the decree was applied to track the impact of the reform on firms' perceptions of the justice system as well as on economic outcomes. Since we interview the firms in 2016/17, years after the introduction of the reform, these effects cannot be interpreted causally. Instead, we interpret them as descriptive evidence of welfare implications. We report results on the sample of firms that either only had a case before or after the decree, omitting all firms that had cases before and after.³⁰ In addition, we report three specifications for each outcome of

²⁷ The median dispute amount in our caseload is FCFA 7,500,000, or about USD 12,180.

²⁸ We are aware of the limitations of this method (Diamond and Hausman 1994). The idea is to use the answers as an "opinion poll" to assess that firms see a positive value in shorter disputes, and not to establish the true value of the reform (Chetty 2015).

²⁹ Qualitatively similar results are obtained when the question is phrased as an increase in administrative court fees.

³⁰ Our results are robust to the inclusion of firms that had cases before and after in the treatment group.

interest: controlling for number of employees at baseline (recalled), and adding controls for type of respondents.

We make three central observations. First, the effect of the reform on firms' perceived lawyer costs is remarkably similar to the values returned in the stated preference exercise (an effect of USD 433-455, relative to a baseline of USD 976). This provides all the more assurance in the validity of these responses that stated preferences were performed on all firms, regardless of their exposure to the decree. Taken together, these results suggest that, although lawyer fees have increased substantially as a result of the reform, an average firm finds it welfare improving to pay these higher fees conditional on securing speed gains equivalent to those realized by the reform. Second, firms who underwent legal disputes after the reform have, on average, higher perception of the justice system according to an index measure used by the World Bank Enterprise Survey (Table 12). However, we do not discern any change in hypothetical future use of the court for commercial disputes, nor any precisely estimated change in perceived speed. Third, firms who had legal disputes after the reform experience more robust economic outcomes (Table 13). These differences are large, as measured in terms of firm size (imprecisely estimated), probability of entering public tender bids (67%, p -value <0.1), and probability of having an ongoing loan (62%, p -value <0.05). Again, while these estimates do not allow us to formally document the economic impact of the reform, it suggests that firms that experienced court cases in the year after the reform have more robust economic outcomes relative to those that had cases in the two years prior.

VIII. Conclusion

We formally document the impact of a 2013 legal reform that changed the rules of the game of civil and commercial justice in Dakar, Senegal. The application of the decree was staggered over a 5-month period across six civil and commercial chambers of the court. We exploit this gradual rollout as well as rich, high-frequency hearing and case-level data over the 2012/15 period to construct an event study around each chamber's application date.

We find large effect on duration, and document that these efficiency gains were not made through intensification of hearings over shorter periods of time. Instead, cases that entered a chamber after the decree was applied experienced fewer hearings, with only a modest increase in frequency. While, *de jure*, the decree affected the procedural code only at the pre-trial stage, we find that the efficiency gains spill over to the next (decision) stage in the trial by affecting the post-reform trend.

The reform aimed to give judges more power to fast track cases out of the pre-trial phase, and to apply firm delay on the parties in order to meet a maximum 4-month pre-trial duration. We show that judges are more likely to use their newfound powers and fast-track cases out of pre-trial either for immediate decision or to dismiss them for lack of evidence. Searching for additional cues in the data on the mechanisms through which delays were cut and deadlines adhered to, we find that judges were 35.7% to 40.3% more likely to apply strict deadlines on the parties in non-decisive hearings. Taken together, our results suggest that the reform reduced delays mostly by eliciting judges with lower taste for private gains to reduce (passive) delays.

Looking across a number of markers of quality of the pre-trial proceedings and deliberations, we fail to detect any adverse effect. Overall, efficiency gains dominate, with

positive spillovers to the deliberation phase of the trial. In addition, we find no effects on the quality of neither the evidence nor the justification accompanying legal decisions, and decisions to appeal are not affected.

We track firms that had ongoing court cases over our 2012/15 study period and find evidence of positive welfare impacts of the reform, as measured by eliciting stated preferences as well as perceptions of the justice system and a range of economic outcomes.

Can changing the rules of the game affect judges' performance? Taken together, our results suggest that, while judges in developing, civil law countries may face many constraints to productivity (Djankov et al, 2003; Chemin, 2009), simple changes in the procedure, such as a reduction in formalism and the application of deadlines, can be effective in increasing the speed of dispute resolution. This suggests that, unlike previously proposed by Coviello et al (2014), the *decisiveness* of legal proceedings offers a non-trivial margin at which simple legal reforms can impact the speed of justice.

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Figure 1: Chamber-level incoming caseload

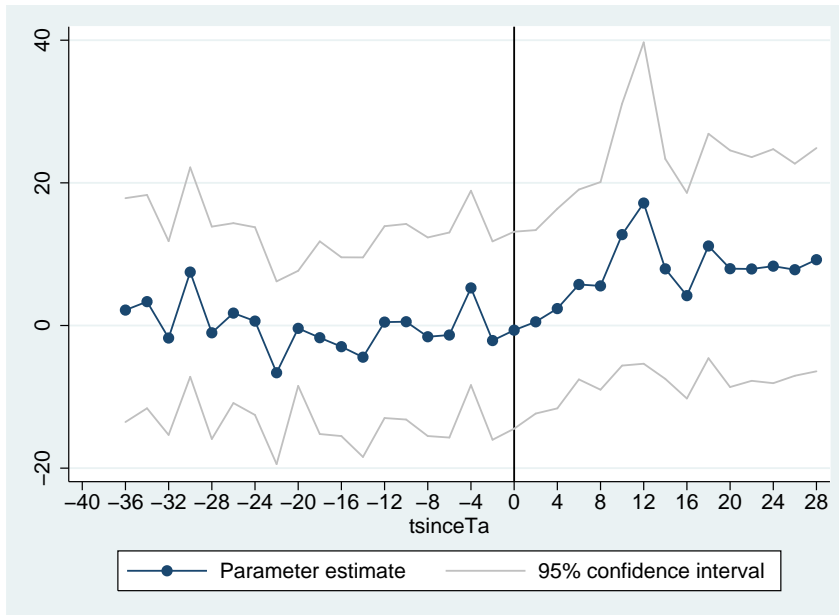


Figure 2: Type of incoming cases (above median claim amount)

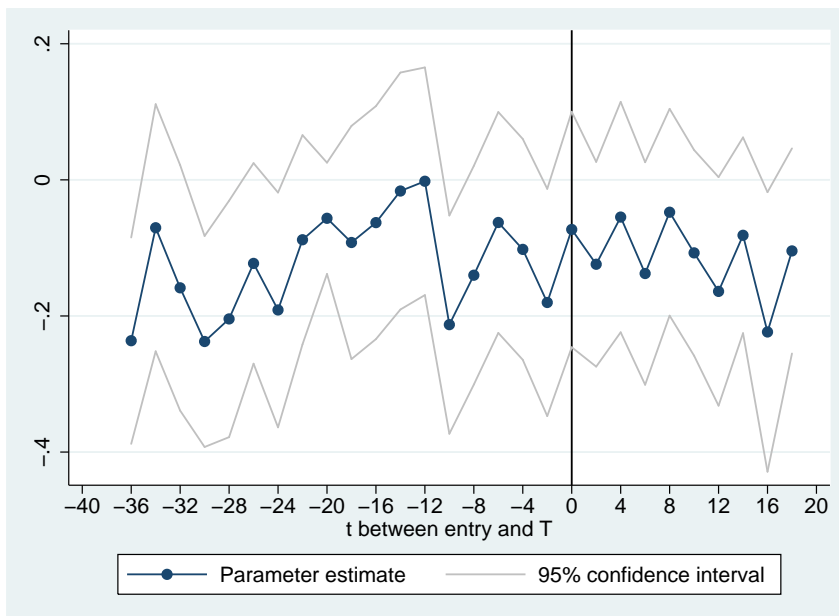


Figure 3: Courtwide incoming and ongoing caseload

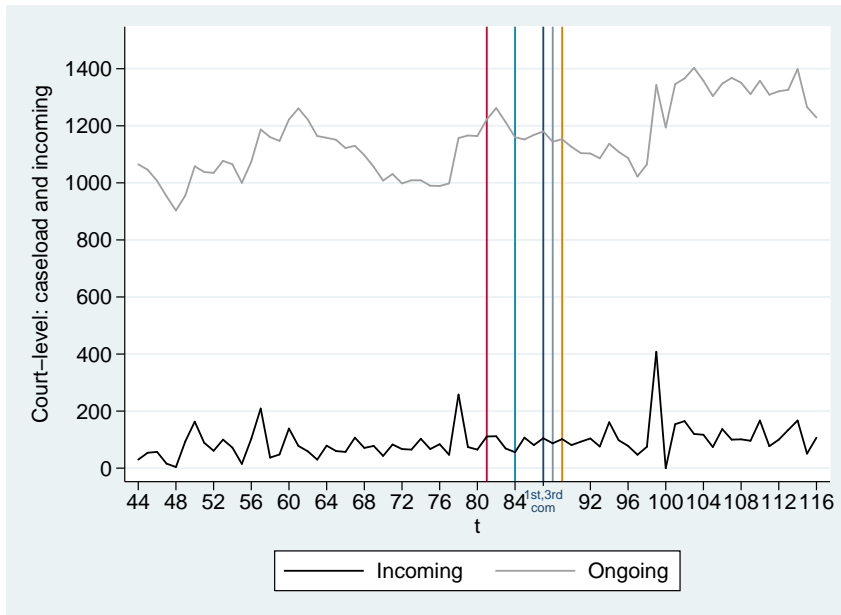


Figure 4: Impact on the pre-trial duration (number of days)

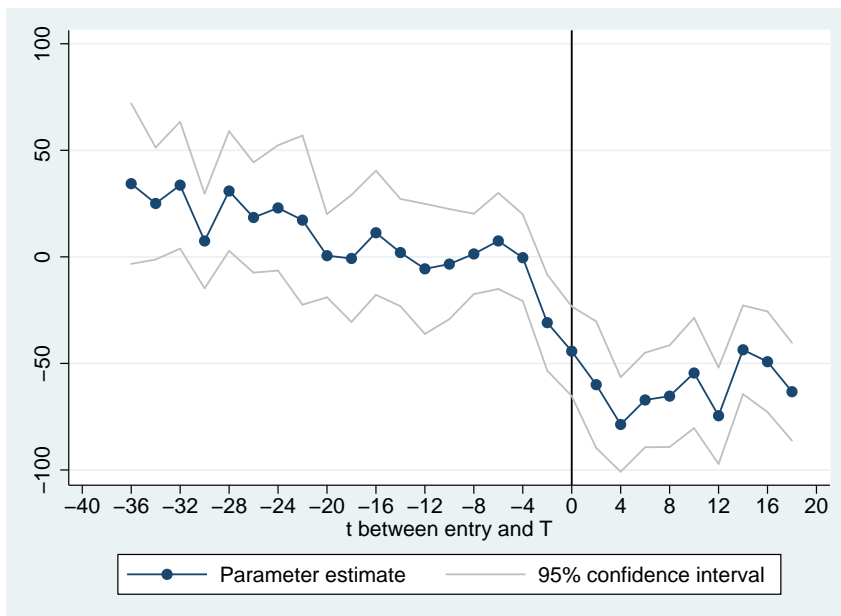


Figure 5: Impact on the hazard ratio to exit the pre-trial

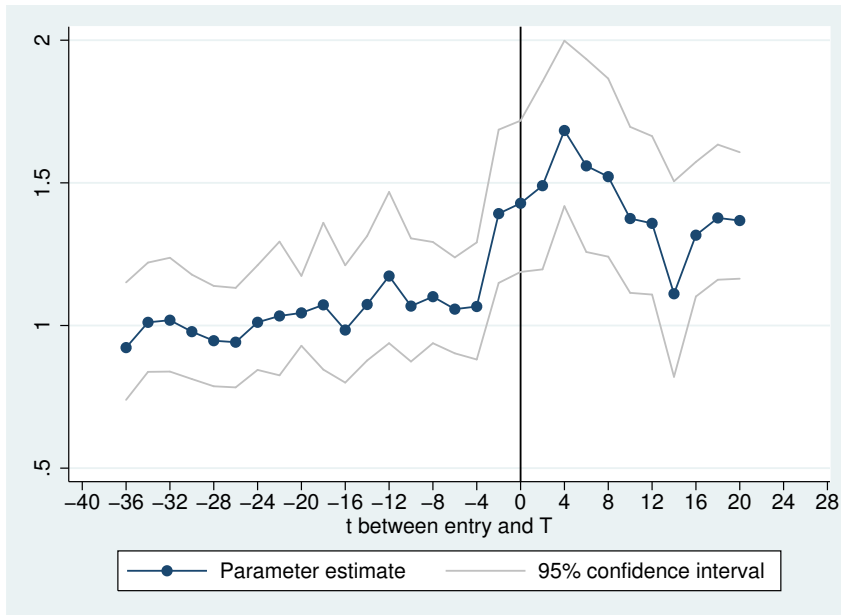


Figure 6: Impact on the likelihood to complete the pre-trial in 4 month

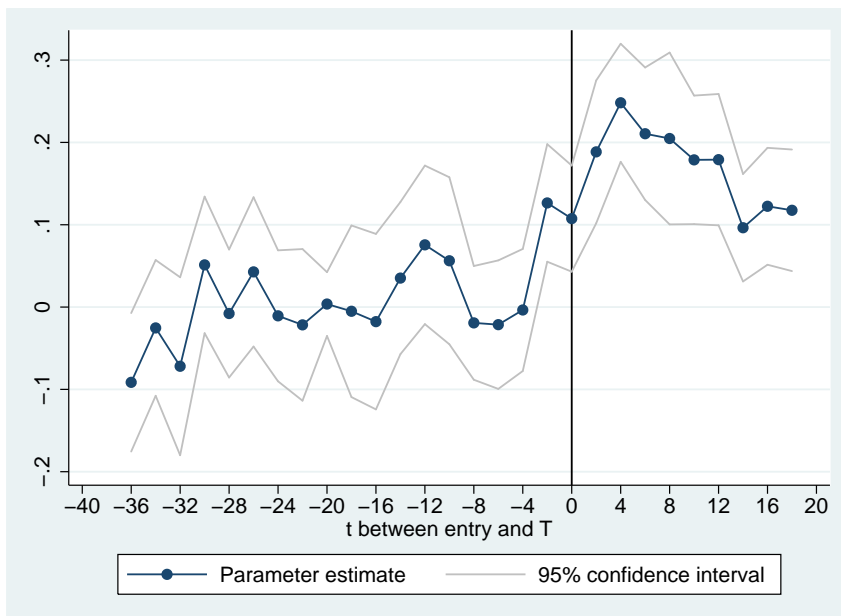


Figure 7: Pretrial duration: distribution

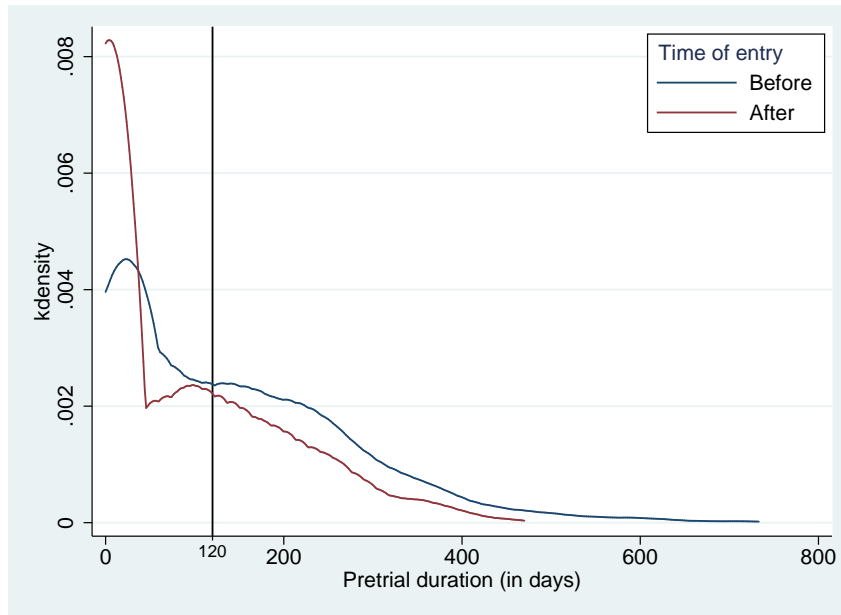


Figure 8: Pretrial duration: distribution by cohort of entry

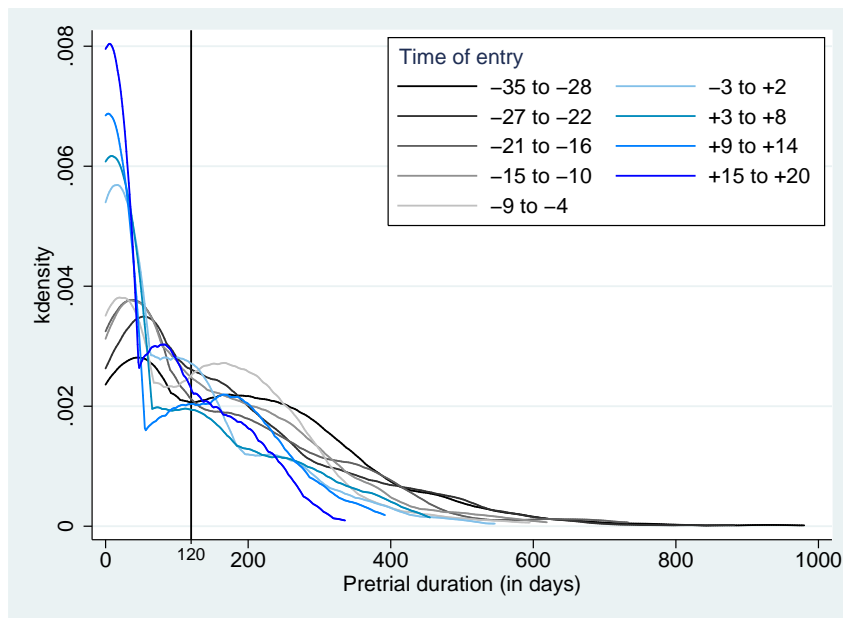


Figure 9: Impact on the duration of the decision stage (number of days)

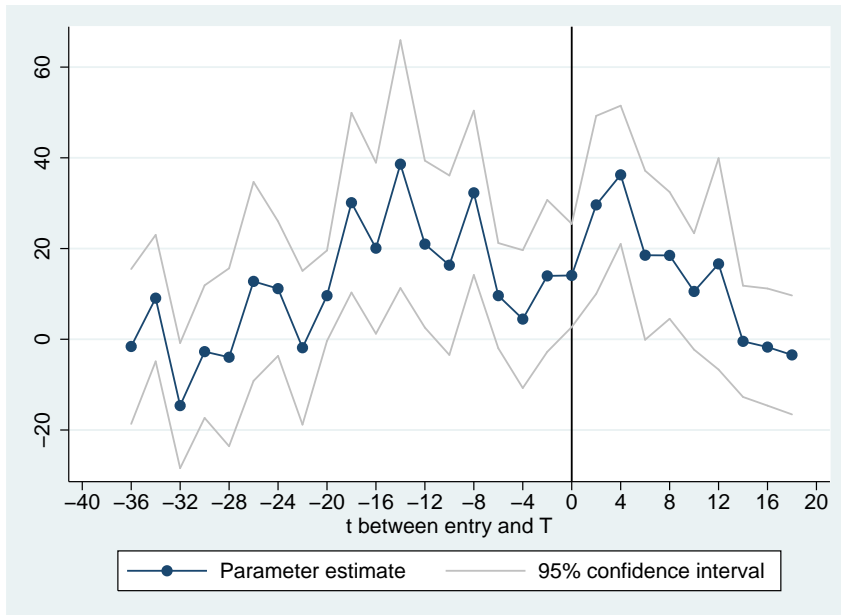


Figure 10: Impact on the hazard ratio to exit the decision stage

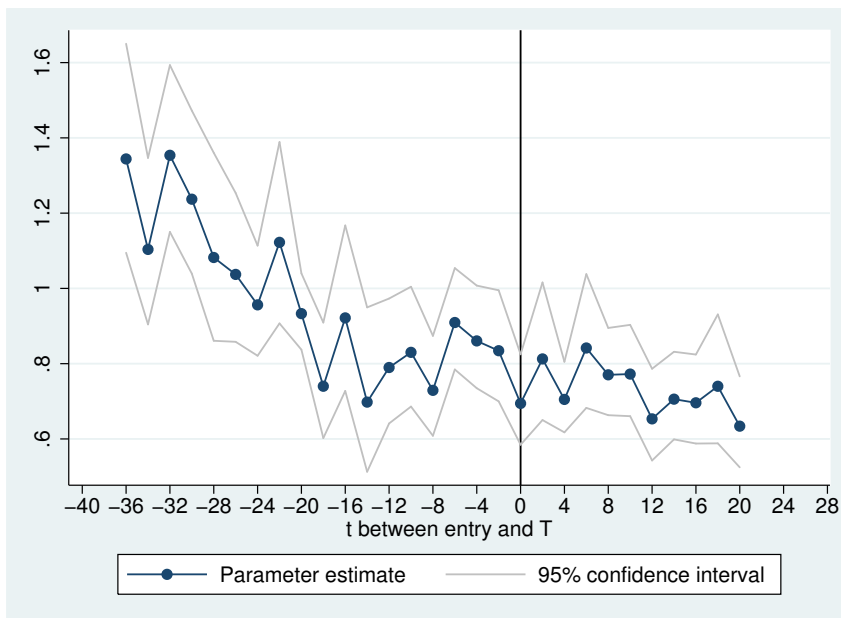


Figure 11: Impact on the likelihood to complete the decision stage in 1 month

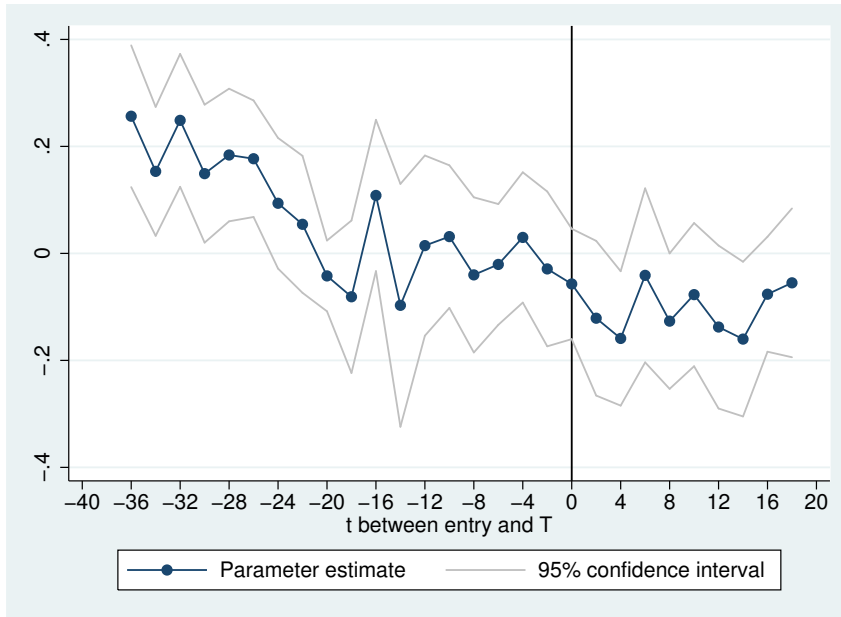


Figure 12: Impact on the number of pre-trial hearings

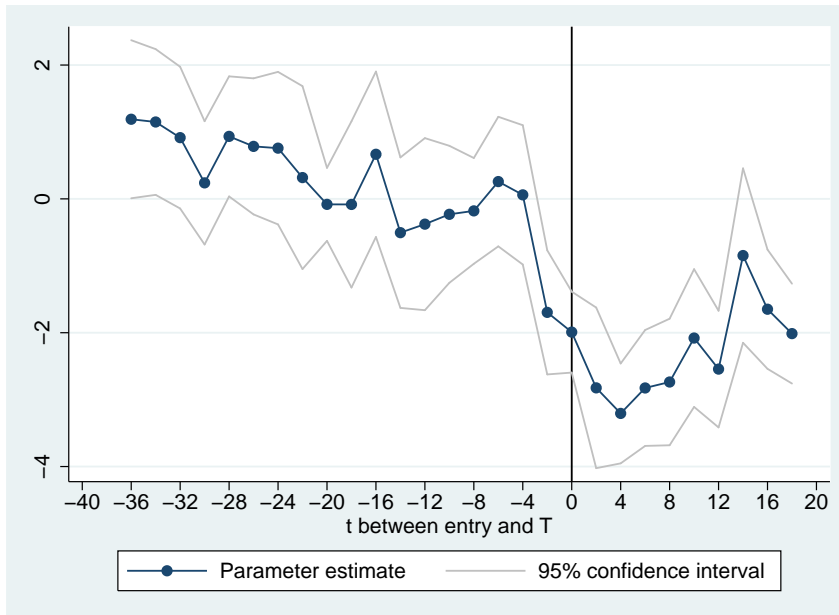


Figure 13: Impact on immediate decision likelihood (fast-tracked or inadmissible)

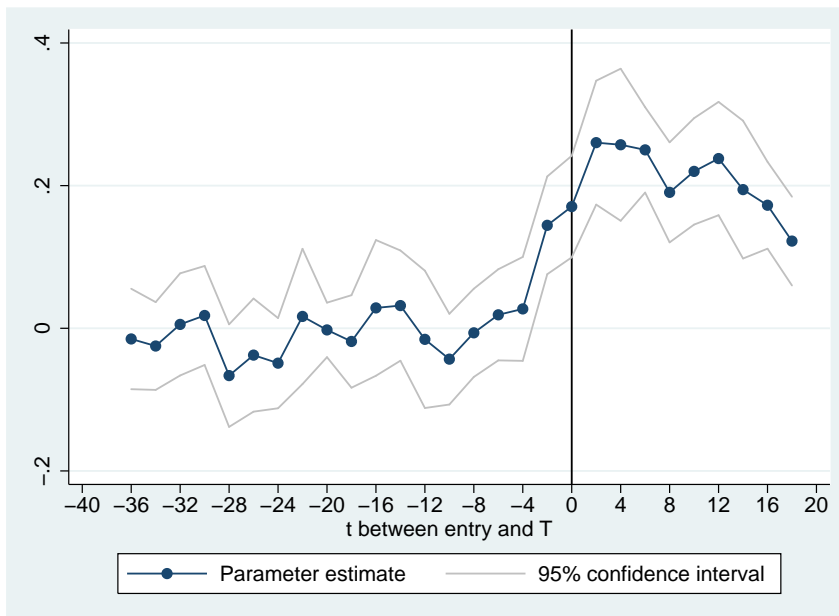


Figure 14: Impact on the pre-trial likelihood of being heard

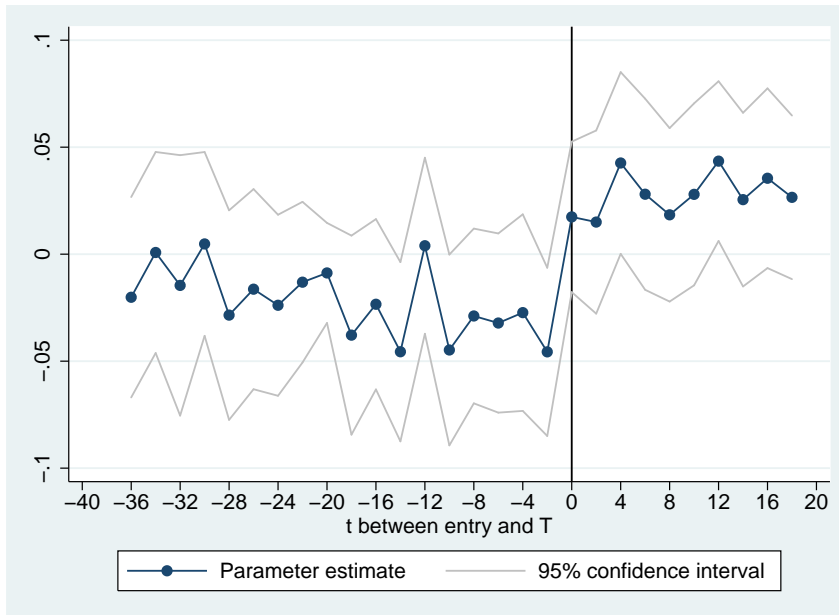


Figure 15: Impact on the judge being stricter in the pre-trial

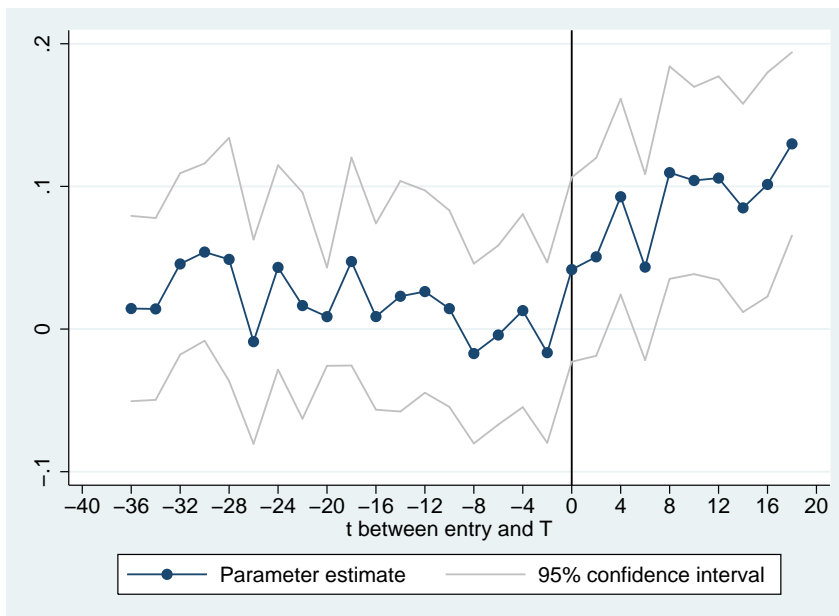


Figure 16: Impact on the number of decision stage hearings

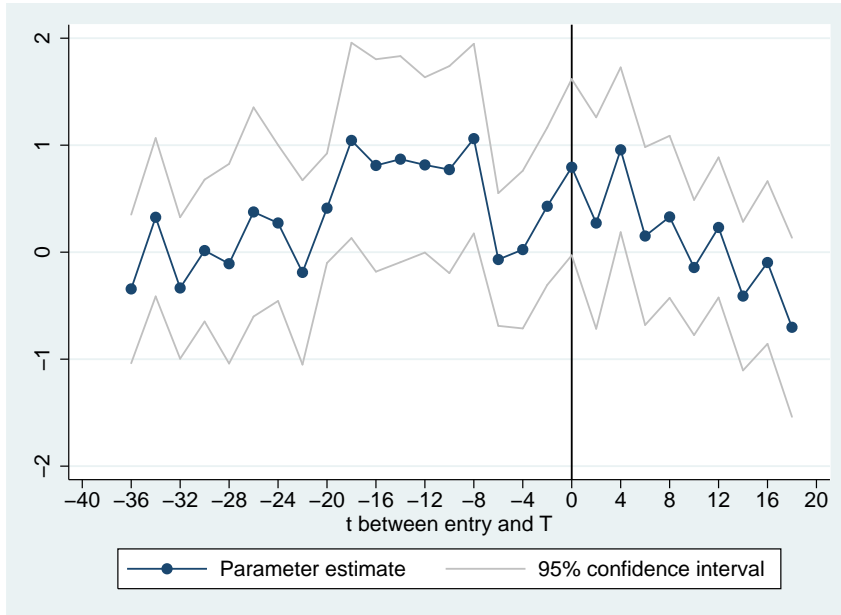


Figure 17: Impact on the decision stage likelihood of being heard

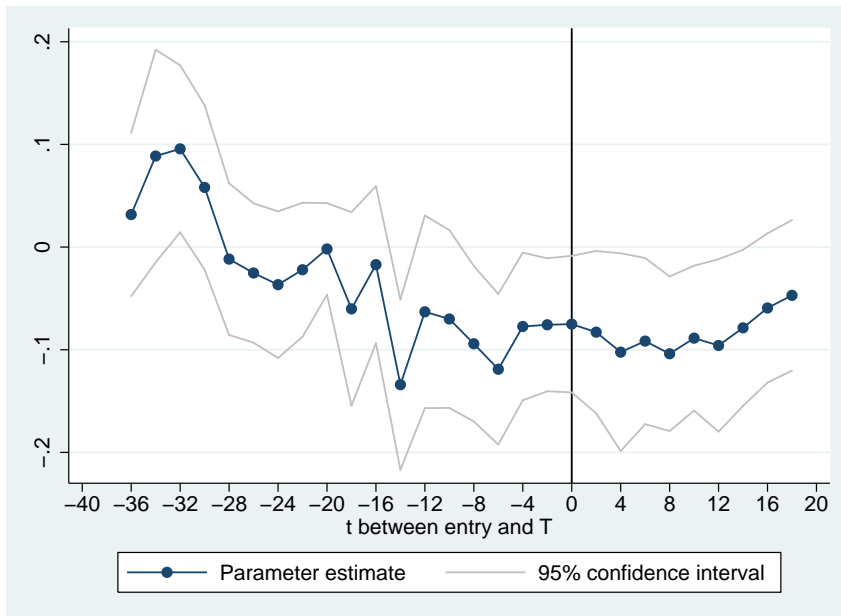


Figure 18: Impact on the likelihood of pre-trial failure

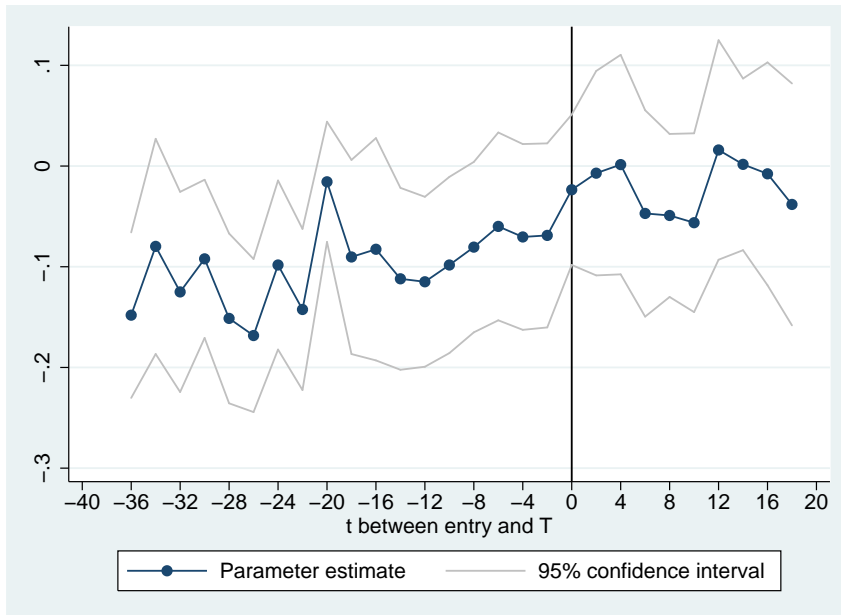


Figure 19: Impact on the likelihood of decision postponement

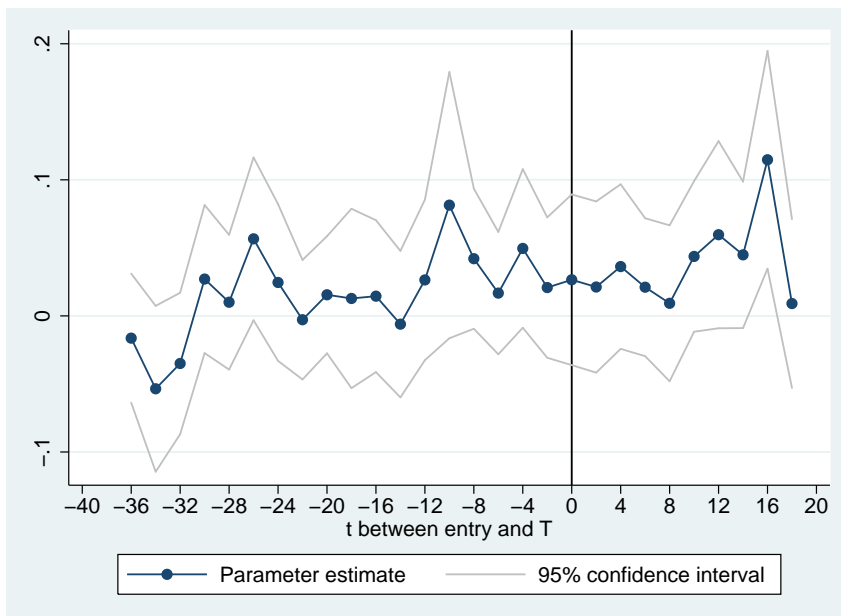


Figure 20: Kaplan-Meier survival estimates

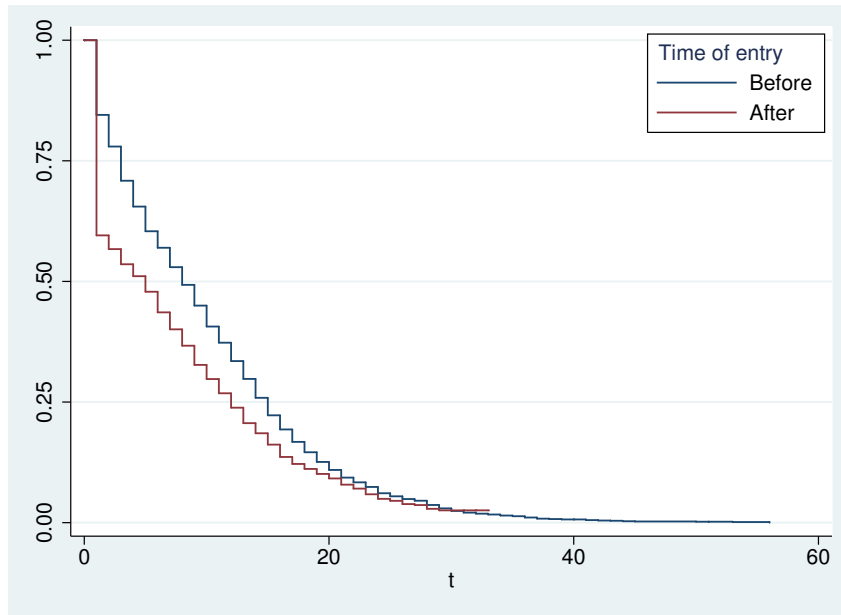


Figure 21: Ongoing cases in pre-trial and decision stages

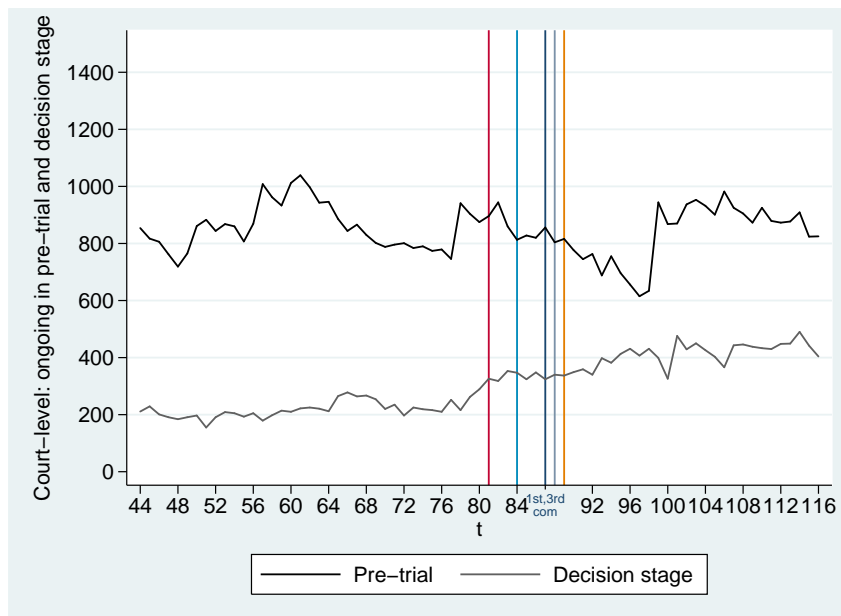


Figure 22: Differential effects - pre-trial duration

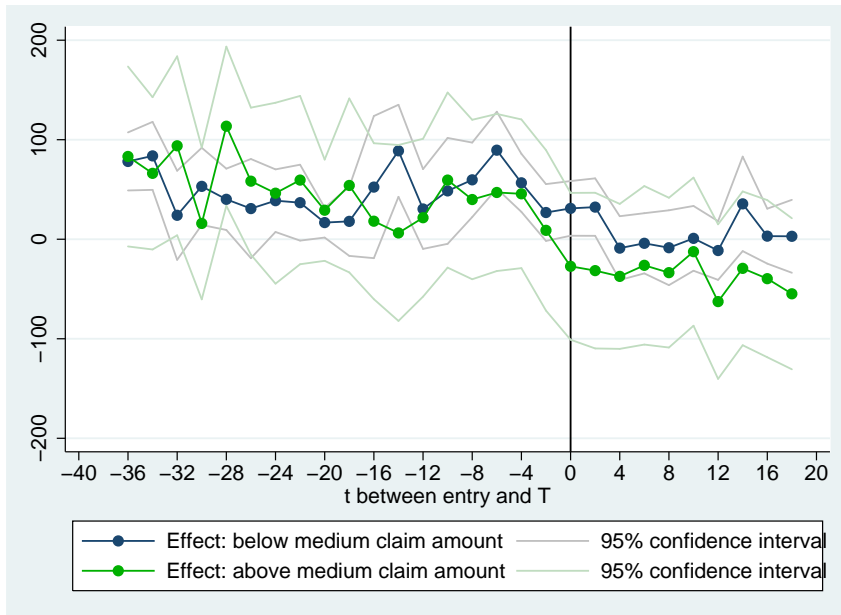


Figure 23: Number of articles cited

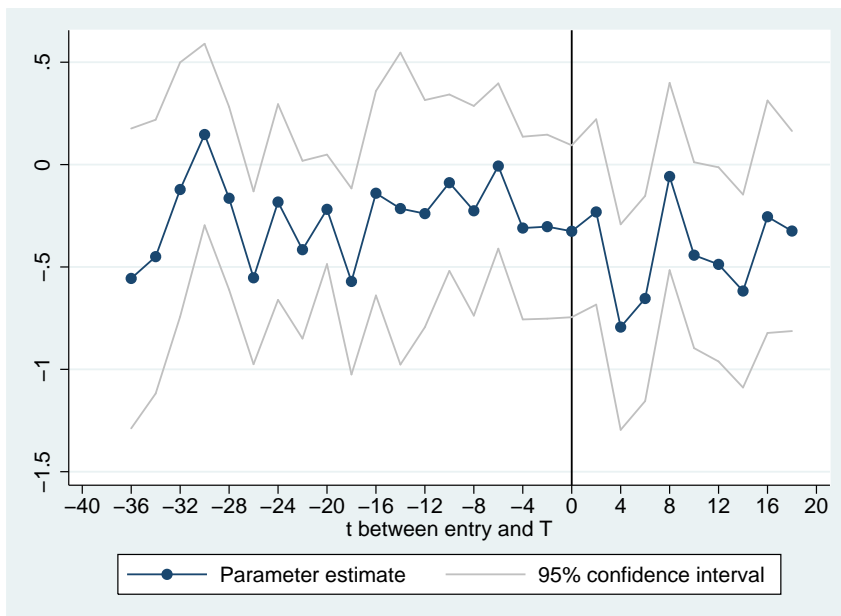


Figure 24: Relative length of decision

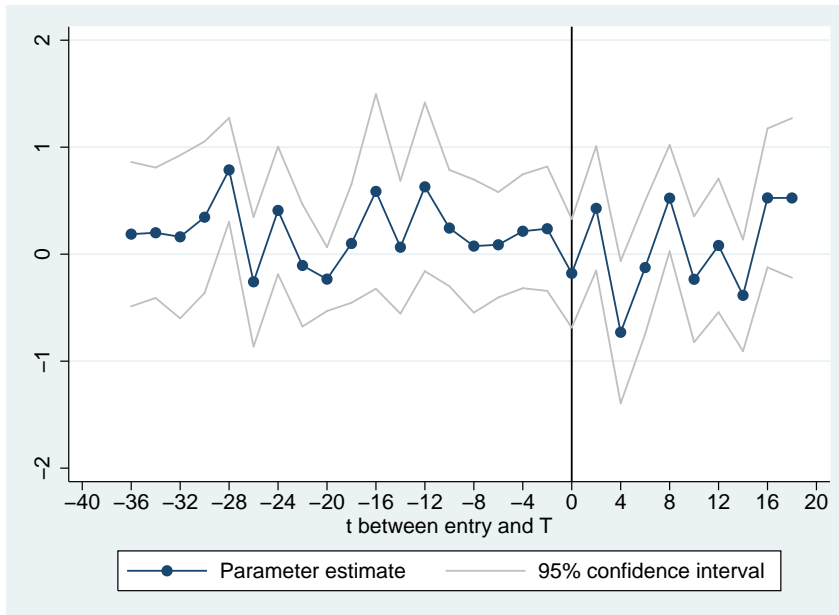


Figure 25: Intention to appeal

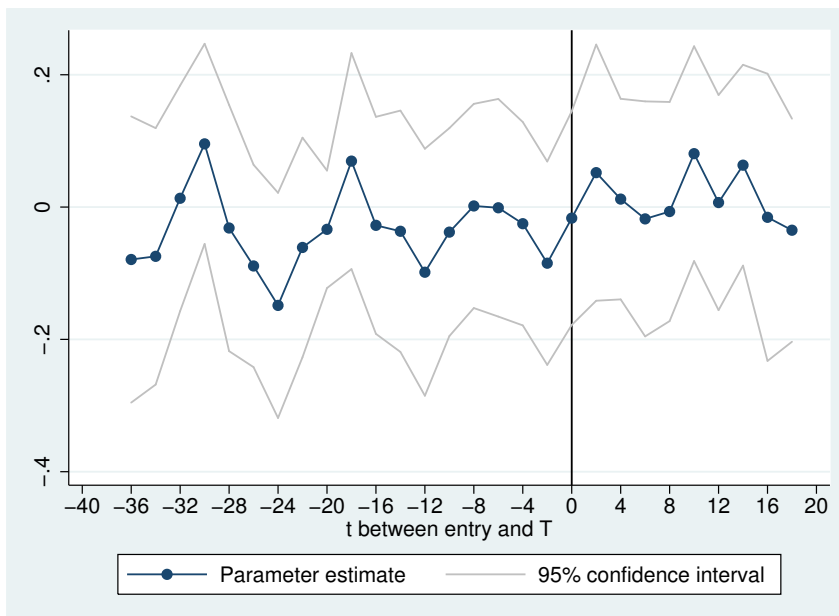


Figure 26: Relative length of decision

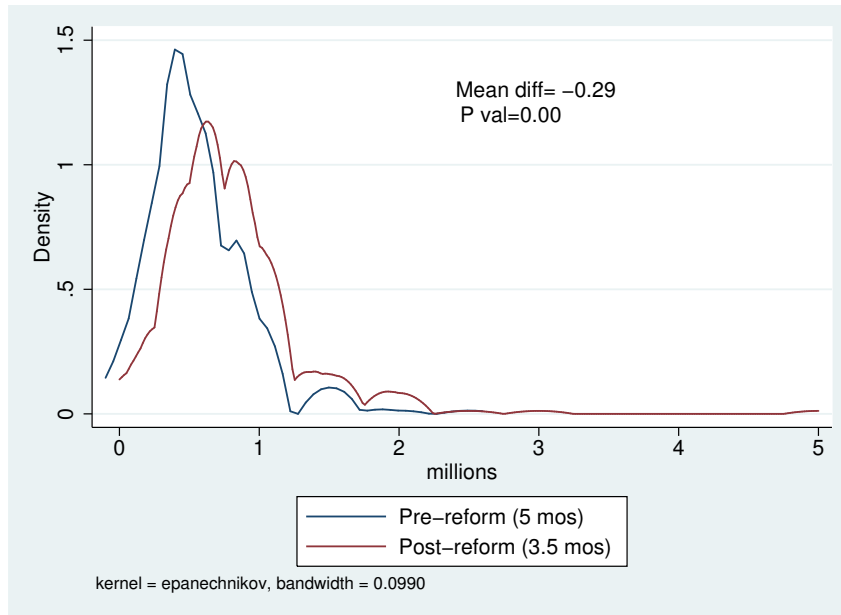


Table 1: **Summary statistics - chamber-level**

	1st Com- mercial	2nd Com- mercial	3rd Com- mercial	4th Com- mercial	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 140.6	188.1	149.1	.	225.1	165.0	37.0
	2013 116.0	209.1	109.3	63.2	196.2	86.4	89.7
	2014 153.4	201.2	142.0	70.3	156.3	.	123.1
	2015 174.1	262.1	155.5	.	144.8	.	134.3
Average number of ongoing cases in decision stage	2012 24.7	26.1	46.9	.	50.5	48.6	3.0
	2013 26.8	48.7	44.6	16.8	68.6	32.7	31.1
	2014 48.7	97.1	84.0	27.0	99.4	.	45.9
	2015 60.5	107.3	101.4	.	85.6	.	64.9

Table 2: Summary statistics - pre-reform period

	N	Mean	StD	Median	Min	Max
Duration of pre-trial hearings (in days)	2827	153.034	143.753	124.000	0.000	980.000
Likelihood of pre-trial completion in 4 months	2836	0.497	0.500	0.000	0.000	1.000
Duration of decision stage (in days)	2498	63.972	83.380	30.000	14.000	761.000
Likelihood of decision completion in 1 month	2529	0.494	0.500	0.000	0.000	1.000
Number of pretrial hearings	2836	8.081	6.402	7.000	0.000	41.000
Number of decision stage hearings	2538	2.622	3.470	1.000	0.000	36.000
No pre-trial	2836	0.121	0.326	0.000	0.000	1.000
Pre-trial likelihood of being heard	2814	0.887	0.131	0.923	0.286	1.000
Decision stage likelihood of being heard	2527	0.770	0.248	0.857	0.167	1.000
Judge more strict	2157	0.154	0.183	0.105	0.000	1.000
Pre-trial insufficient	2527	0.118	0.322	0.000	0.000	1.000
Decision postponed	2527	0.055	0.229	0.000	0.000	1.000
Claim amount (in 1,000,000 FCFA)	1773	75.604	390.238	8.000	0.075	8,700.838

Table 3: Robustness checks

	(1)	(2)	(3)	(4)
	numin	numin	Above median claim amount	Above median claim amount
Hearing after decree application	4.958 (3.075)	6.140 (4.203)		
Trend	-0.088 (0.104)	-0.086 (0.113)		
Interaction	0.551** (0.255)	0.460 (0.295)		
Entered after decree application			-0.037 (0.041)	-0.038 (0.050)
Trend			0.003*** (0.001)	0.004*** (0.001)
Interaction			-0.005* (0.003)	-0.006* (0.003)
Constant	12.779** (5.695)	6.572 (4.992)	0.526*** (0.063)	0.296*** (0.061)
Chamber FEs	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes
Without adj. period	No	Yes	No	Yes
Pre-mean	14.190	14.190	0.501	0.501
Pre-sd	11.522	11.522	0.500	0.500
R-Squared	0.403	0.433	0.201	0.193
Observations	310	274	3532	3198

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 4: Impact on the speed of pre-trial procedures

	(1)	(2)	(3)	(4)	(5)	(6)
	Duration of pre-trial hearings (in days)	Duration of pre-trial hearings (in days)	Hazard ratio - finishing pre-trial	Hazard ratio - finishing pre-trial	Likelihood of pre-trial completion in 4 months	Likelihood of pre-trial completion in 4 months
Entered after decree application	-34.772*** (10.315)	-46.011*** (12.927)	1.187** (0.090)	1.300*** (0.128)	0.152*** (0.034)	0.216*** (0.045)
Trend	-1.490*** (0.294)	-1.212*** (0.344)	1.008*** (0.002)	1.006** (0.002)	0.002** (0.001)	0.001 (0.001)
Interaction	0.956 (0.672)	0.909 (0.803)	0.986*** (0.005)	0.987** (0.006)	-0.008*** (0.002)	-0.009*** (0.003)
Constant	69.568*** (12.250)	167.044*** (13.173)			0.649*** (0.047)	0.407*** (0.038)
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month	Yes	Yes	Yes	Yes	Yes	Yes
Without adj. period	No	Yes	No	Yes	No	Yes
Pre-mean	153.034	153.034			0.497	0.497
Pre-sd	143.753	143.753			0.500	0.500
R-Squared	0.191	0.201			0.127	0.130
Observations	4901	4405	5096	4594	5096	4594

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 5: Impact on the speed of the decision stage

	(1)	(2)	(3)	(4)	(5)	(6)
	Duration of decision stage (in days)	Duration of decision stage (in days)	Hazard ratio - finishing pre-trial	Hazard ratio - finishing pre-trial	Likelihood of decision completion in 1 month	Likelihood of decision completion in 1 month
Entered after decree application	3.426 (6.778)	5.217 (9.298)	0.079 (0.072)	0.164* (0.088)	-0.039 (0.040)	-0.009 (0.055)
Trend	0.706*** (0.176)	0.798*** (0.201)	-0.014*** (0.002)	-0.016*** (0.002)	-0.007*** (0.001)	-0.008*** (0.001)
Interaction	-2.203*** (0.422)	-2.609*** (0.599)	0.003 (0.005)	0.001 (0.006)	0.006** (0.003)	0.006 (0.003)
Constant	104.072*** (8.565)	65.708*** (8.104)			0.440*** (0.077)	0.474*** (0.071)
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Without adj. period	No	Yes	No	Yes	No	Yes
Pre-mean	63.972	63.972			0.494	0.494
Pre-sd	83.380	83.380			0.500	0.500
R-Squared	0.061	0.064			0.155	0.154
Observations	3851	3496	4109	3738	4116	3745

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 6: Impact on the pre-trial stage: Channels

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number of pretrial hearings	Number of pretrial hearings	No pre-trial	No pre-trial	Pre-trial likelihood of being heard	Pre-trial likelihood of being heard	Judge more strict	Judge more strict
Entered after decree application	-1.423*** (0.419)	-1.913*** (0.520)	0.183*** (0.030)	0.238*** (0.038)	0.051*** (0.012)	0.062*** (0.018)	0.055*** (0.015)	0.062*** (0.023)
Trend	-0.059*** (0.013)	-0.043*** (0.015)	0.003*** (0.001)	0.002** (0.001)	-0.000 (0.000)	-0.001 (0.000)	-0.001** (0.000)	-0.001** (0.001)
Interaction	0.078*** (0.028)	0.065* (0.035)	-0.008*** (0.002)	-0.008*** (0.003)	0.001 (0.001)	0.000 (0.001)	0.005*** (0.001)	0.005*** (0.002)
Constant	7.322*** (0.411)	5.548*** (0.571)	0.137*** (0.033)	0.210*** (0.037)	0.859*** (0.014)	0.780*** (0.016)	0.159*** (0.016)	0.139*** (0.018)
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	Yes	No	Yes	No	Yes	No	Yes
Pre-mean	8.057	8.057	0.120	0.120	0.887	0.887	0.154	0.154
Pre-sd	6.417	6.417	0.325	0.325	0.131	0.131	0.182	0.182
R-Squared	0.135	0.142	0.096	0.104	0.206	0.217	0.067	0.069
Observations	5169	4667	5169	4667	5137	4638	3434	3141

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 7: Impact on the decision stage: Channels

	(1)	(2)	(3)	(4)
	Number of decision stage hearings	Number of decision stage hearings	Decision stage likelihood of being heard	Decision stage likelihood of being heard
Entered after decree application	-0.087 (0.296)	0.012 (0.379)	0.022 (0.026)	0.046 (0.039)
Trend	0.020*** (0.007)	0.022*** (0.008)	-0.005*** (0.001)	-0.006*** (0.001)
Interaction	-0.079*** (0.019)	-0.089*** (0.025)	0.006*** (0.002)	0.007** (0.003)
Constant	2.998*** (0.326)	2.414*** (0.340)	0.769*** (0.024)	0.791*** (0.030)
Chamber FEs	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes
Without adj. period	No	Yes	No	Yes
Pre-mean	2.615	2.615	0.773	0.773
Pre-sd	3.442	3.442	0.247	0.247
R-Squared	0.029	0.030	0.307	0.298
Observations	4377	4000	4137	3768

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 8: Impact on pre-trial quality

	(1)	(2)	(3)	(4)
	Pre-trial insufficient	Pre-trial insufficient	Decision postponed	Decision postponed
Entered after decree application	0.026 (0.029)	0.017 (0.042)	-0.009 (0.022)	-0.004 (0.029)
Trend	0.001** (0.001)	0.001 (0.001)	0.001*** (0.001)	0.002*** (0.001)
Interaction	0.001 (0.002)	0.003 (0.003)	-0.001 (0.002)	-0.002 (0.002)
Constant	0.098*** (0.033)	0.166*** (0.040)	0.019 (0.017)	0.056*** (0.021)
Chamber FEs	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes
Without adj. period	No	Yes	No	Yes
Pre-mean	0.120	0.120	0.054	0.054
Pre-sd	0.325	0.325	0.227	0.227
R-Squared	0.018	0.020	0.043	0.045
Observations	4137	3768	4137	3768

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 9: Impact on decision quality

	(1)	(2)	(3)	(4)	(5)	(6)
	Number of articles	Number of articles	Decision length	Decision length	Appeal	Appeal
Entered after decree application	0.052 (0.120)	-0.142 (0.169)	0.071 (0.184)	-0.160 (0.228)	0.065 (0.047)	0.051 (0.055)
Trend	-0.000 (0.004)	0.002 (0.004)	-0.004 (0.006)	-0.003 (0.006)	0.000 (0.001)	0.000 (0.001)
Interaction	0.000 (0.009)	0.009 (0.012)	0.014 (0.014)	0.029* (0.016)	-0.001 (0.003)	-0.001 (0.004)
Constant	2.946*** (0.203)	2.883*** (0.239)	6.349*** (0.245)	5.306*** (0.200)	0.588*** (0.059)	0.593*** (0.068)
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes
Without adj. period	No	Yes	No	Yes	No	Yes
Pre-mean	2.835	2.846	5.533	5.547	0.535	0.537
Pre-sd	1.565	1.595	2.520	2.568	0.499	0.499
R-Squared	0.007	0.005	0.035	0.038	0.037	0.040
Observations	2985	2672	2984	2671	2985	2672

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 10: Differential impacts by case difficulty: claim amount

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Duration of pre-trial hearings (in days)	Likelihood of pre-trial completion in 4 months	Number of pre-trial hearings	Pre-trial likelihood of being heard	Judge more strict	Pre-trial insufficient	Decision postponed
Above median claim amount	30.252*** (10.089)	-0.118*** (0.039)	1.704*** (0.473)	-0.005 (0.012)	-0.006 (0.023)	0.062* (0.034)	0.089*** (0.023)
Entered after decree application	-26.538** (13.480)	0.107** (0.052)	-1.136* (0.633)	0.039*** (0.014)	0.042 (0.029)	-0.034 (0.040)	-0.007 (0.027)
Abm claim amount X	-1.824 (15.904)	-0.011 (0.060)	-0.187 (0.732)	-0.010 (0.015)	0.061* (0.033)	0.096* (0.054)	-0.047 (0.040)
Entered after Trend	-0.063 (0.425)	-0.001 (0.002)	-0.010 (0.020)	-0.001** (0.000)	-0.001 (0.001)	0.000 (0.001)	0.001* (0.001)
Entered after decree application X	-1.065 (0.845)	0.001 (0.003)	-0.006 (0.039)	0.002** (0.001)	0.007*** (0.002)	0.009*** (0.003)	0.002 (0.002)
Abm claim amount X Trend	-1.525*** (0.524)	0.003 (0.002)	-0.049** (0.024)	0.001 (0.000)	-0.000 (0.001)	0.002 (0.002)	0.002*** (0.001)
Trend	0.426 (1.134)	-0.003 (0.004)	0.023 (0.048)	-0.001 (0.001)	-0.003 (0.002)	-0.012*** (0.004)	-0.006** (0.003)
Triple interaction	79.005*** (14.421)	0.336*** (0.047)	7.729*** (0.630)	0.786*** (0.018)	0.148*** (0.024)	0.135*** (0.048)	0.050 (0.031)
Constant	-28.362 (14.421)	.096 (0.047)	-1.323 (0.630)	.029 (0.018)	.103 (0.024)	.062 (0.048)	-0.053 (0.031)
Effect for large cases	.0285	.0488	.02	.0437	0	.2072	.1282
P-value: effect for large cases	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	No	No	No	No	No	No	No
Without adjustment period	100.071	0.669	5.878	0.927	0.149	0.093	0.024
Pre-mean	117.008	0.471	5.338	0.117	0.189	0.291	0.153
Pre-sd	0.224	0.153	0.177	0.221	0.090	0.031	0.053
R-Squared	3380	3532	3532	3522	2208	2851	2851

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 11: Differential impacts by case difficulty: claim amount (without adjustment period)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Duration of pre-trial hearings (in days)	Likelihood of pre-trial completion in 4 months	Number of pre-trial hearings	Pre-trial likelihood of being heard	Judge strictness	Pre-trial insufficient	Decision postponed
Above median claim amount	27.530** (13.121)	-0.089* (0.052)	1.477** (0.612)	-0.008 (0.013)	-0.030 (0.026)	0.097** (0.040)	0.103*** (0.028)
Entered after decree application	-55.798*** (17.159)	0.229*** (0.065)	-2.678*** (0.794)	0.049*** (0.019)	-0.002 (0.043)	-0.021 (0.054)	0.005 (0.037)
Abm claim amount X	26.678 (19.781)	-0.116 (0.076)	1.405 (0.916)	-0.011 (0.018)	0.123*** (0.045)	0.073 (0.070)	-0.046 (0.057)
Entered after Trend	0.304 (0.515)	-0.002 (0.002)	0.011 (0.024)	-0.001* (0.001)	-0.001 (0.001)	-0.000 (0.001)	0.001 (0.001)
Entered after decree application X	-0.082 (0.961)	-0.004 (0.004)	0.042 (0.045)	0.001 (0.001)	0.009*** (0.003)	0.010** (0.004)	0.002 (0.003)
Abm claim amount X Trend	-1.596** (0.618)	0.004* (0.002)	-0.057** (0.028)	0.001 (0.001)	-0.001 (0.001)	0.003* (0.002)	0.003*** (0.001)
Trend Triple interaction	-1.397 (1.330)	0.002 (0.005)	-0.068 (0.056)	-0.000 (0.001)	-0.005* (0.003)	-0.014** (0.005)	-0.008* (0.004)
Constant	178.399*** (18.361)	0.627*** (0.063)	5.080*** (0.791)	0.788*** (0.022)	0.133*** (0.025)	0.102* (0.059)	0.003 (0.022)
Effect for large cases	-29.12	.113	-1.273	.038	.121	.052	-.041
P-value: effect for large cases	.0735	.0771	.0797	.0439	.0001	.42	.3738
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	100.071	0.669	5.878	0.927	0.149	0.093	0.024
Pre-sd	117.008	0.471	5.338	0.117	0.189	0.291	0.153
R-Squared	0.233	0.151	0.183	0.225	0.096	0.032	0.057
Observations	3051	3198	3198	3188	2019	2596	2596

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber-entry-t. Window includes cases entering between 38 hearings before and 20 hearings after decree application.

Table 12: Perceptions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Duration	Duration	Cost	Cost	Hypoth. 1	Hypoth. 1	Index 1	Index 1
Treat	3.937* (2.293)	3.758 (2.325)	455.396* (246.465)	433.453* (249.474)	0.047 (0.057)	0.042 (0.058)	0.063* (0.034)	0.063* (0.034)
BL number of employees	-0.000 (0.004)	-0.001 (0.004)	1.798*** (0.394)	1.672*** (0.416)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Resp.: jur		2.102 (3.935)		390.117 (426.233)		0.032 (0.096)		-0.043 (0.058)
Resp.: oth		0.692 (2.426)		-57.873 (260.759)		0.036 (0.061)		0.055 (0.036)
Constant	20.552*** (1.701)	20.170*** (1.995)	992.947*** (182.674)	993.445*** (211.793)	0.767*** (0.042)	0.752*** (0.049)	0.491*** (0.025)	0.472*** (0.029)
Pre-mean	20.591	20.591	976.421	976.421	0.779	0.779	0.503	0.503
R-Squared	0.013	0.015	0.111	0.116	0.008	0.010	0.017	0.035
Observations	223	223	220	220	204	204	226	226

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses.

Table 13: Firm-level

	(1)	(2)	(3)	(4)	(5)	(6)
	Number of employees		Public lender	Public lender	Ongoing loan	Ongoing loan
Treat	19.560*	15.003	0.120**	0.117*	0.193***	0.153**
	(11.021)	(11.008)	(0.059)	(0.059)	(0.064)	(0.062)
BL number of employees	0.311***	0.297***	0.000	0.000	0.000*	0.000
	(0.018)	(0.019)	(0.000)	(0.000)	(0.000)	(0.000)
Resp.: jur		49.552***		-0.006		0.432***
		(18.726)		(0.101)		(0.105)
Resp.: oth		18.236		0.067		0.174***
		(11.489)		(0.062)		(0.065)
Constant	23.872***	14.339	0.180***	0.155***	0.245***	0.156***
	(8.161)	(9.370)	(0.044)	(0.050)	(0.047)	(0.053)
Pre-mean	30.248	30.248	0.174	0.174	0.245	0.245
R-Squared	0.590	0.604	0.025	0.031	0.062	0.136
Observations	226	226	225	225	226	226

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses.