

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

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Understanding to extent to which policy reforms can increase the celerity of legal dispute resolution is instrumental in formulating viable policies to improve the investment climate. We conduct an event study of a reform aiming to shorten the length of civil and commercial pre-trial procedures in Senegal. For identification, we exploit the staggered rollout across the seven civil and commercial chambers of the regional court of Dakar and high-frequency data on the treatment of the 2010/15 caseload. We find a large reduction in the length of the pre-trial stage of 100 days (0.7 SD). 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 pp.), case-level pre-trial hearings are reduced (0.6 SD), while judges are 46% more likely to set hard deadlines. While our results support a marginal reduction in quality, the overall efficiency gains dominates.

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I. Introduction

The speed of justice is typically referred to as a key indicator of a country's business climate and figures at the core of the *Doing Business* indicators (The World Bank Group, 2011).²

Whether to start or close a business, register property (including intellectual), protect investors or enforce contracts, firms need to rely on the legal system. 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). Consequently, slow justice delivery is associated with a poorer business climate.

Economic governance policies in developing countries often aim to increase the speed of commercial justice. Yet, court-level interventions susceptible of cutting delays are rarely rigorously evaluated (Chemin, 2009). Most legal reforms are rolled out non-randomly across courts, judges or cases. Coupled with aggregated, annual data, the evidence linking faster justice to investment often fails to establish causality (Aboala et al., 2014).

We present case-level evidence on the causal impact of a legal reform designed to increase efficiency and reduce delays in court. Using rich, high-frequency administrative data on the 2010/15 caseload in the court of first instance in Dakar, Senegal, we show that the reform had large, positive, and significant effects on the speed of civil and commercial justice. We exploit high-frequency data on the civil and commercial caseload of the first-instance court of Dakar, Senegal to isolate the mechanisms underlying these effects, such as the channels through which judges intensified the procedure and quality vs. quantity tradeoffs. This study is part of a larger ongoing research agenda that aims to establish a causal link

²For a more exhaustive review of the indicators of quality for the justice system, see Dankov et al.(2003).

between the speed of justice and firms' perceptions of the legal system, investment behavior and firm health.

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, suggesting a significant margin of improvement in the speed of commercial dispute resolution.

Second, the Ministry of Justice introduced a decree, in 2013, aiming to accelerate the speed of civil and commercial disputes process. The decree changed the civil and commercial procedural code so as to empower judges to reduce the formalism and enforce submission of supporting evidence from the outset, apply pressure on the parties along the process, and enforce 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. It also empowered the parties to request supporting documents and set a schedule of hearings with attached milestones at the onset of the pre-trial procedure. While the decree was passed in July/August 2013 by ministerial vote and published in the *Journal Officiel* in October 2013, its application was staggered across the various chambers of the regional court between November 2013 and April 2014. We exploit this gradual rollout for our identification.

Third, we have full access to six years of high-frequency data on the civil and commercial caseload in the regional court of Dakar.³ These data allows us to make two contributions to the current literature on the impact of legal reforms on the speed of commercial justice. First, the high-frequency nature of these data combined with the gradual roll out of the decree across the seven civil and commercial chambers offer the opportunity of using an *event study* design to measure to causal impact of the introduction of the decree on the speed of justice. Second, while the policy experiment we study does not allow us, by design, to identify the channels through which the decree affected the speed of justice, we use these rich court data to shed light on the merits of various mechanisms in linking a change in the legal text to an increase in the speed of dispute resolution.

This study contributes to two strands of the literature. First, it builds on a nascent literature on public service reform and, in particular, the determinants of judicial efficiency. There, we innovate first by collecting rich, high-frequency court-level data in a developing country context. Indeed, court-level studies tend to be limited to richer economies (Chang and Schoar, 2006; Coviello et al., 2015). Recent works that scrutinize the impact of judicial reforms in developing countries' contexts tend to have limited access to court-level data, the most disaggregate data being judge-month caseload statistics (Chemin, 2009; Lichand and Soares, 2014; Ponticelli). In contrast, we have full access to audience and case-level data from the Regional Court of Dakar. We use these data to build a high-frequency panel of all cases that entered the court between 2010/2015, and retrace a full record of all procedures and hearings they underwent from entry to final judgment. This allows us not only to

³ In this version of the paper, data collection at the court was still ongoing, and data on hearings were only available to us for the 2012/14 period. In a subsequent version, we will expand the coverage to 2010, 2011, and 2015.

document the impact of the reform on the overall speed of justice, but also provide evidence on the underlying mechanisms.

Second, we add to the literature by formally documenting the impact of a national reform in civil and commercial procedure. Ponticelli (2014) uses judge-level monthly data to document the impact of court enforcement on the effectiveness of a bankruptcy reform in Brazil. He finds that court-level efficiency is a strong complement to this financial reform, as the impact is insignificant in districts where the speed of commercial justice is low. This suggests that, in the presence of inefficient commercial dispute resolution, legal reform should accompany financial reforms. Visaria (2009) and Lichand and Soares (2014) evaluate the impact of court creation on the efficiency of dispute resolution and the investment climate in Brazil and India, respectively. Yet, few studies causally look at the effectiveness of legal reforms in securing faster dispute resolution. Chemin (2009) uses yearly court-level data to identify the impact of a legal reform in Pakistan, exploiting district-level variations in coverage. Our identification strategy innovates on the existing judicial reform literature. We use within-court variation 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 civil and commercial justice.

We also provide new evidence on the effect of imposing deadlines on workers' performance. While a lot of the literature focuses on supply-driven tasks (e.g., one worker needs to complete a given task in isolation of external factor, see the case of FDA drug review deadlines in Carpenter et al., 2012), we look at the impact of deadlines in a situation where a judge's output is conditional on others' (the parties) effort level. In our setting, judges and parties reach an agreement through a bargaining with imperfect control, as theoretically

describes in Ma and Manove (1993). We build on this literature by documenting the mechanisms through judges impose control and achieve higher speed of dispute resolution. We also follow Carpenter et al (2012) and explore potential quality/efficiency tradeoff on a complex task, preparing a trial.

Placed in the context of its larger research agenda, this study will also contribute firm-level evidence on the role of institutions, and a more efficient legal system, on investment behavior, perception of the justice system, and demand for formal dispute resolution.

We find the reform positively affects the speed of justice by both reducing the formalism of the civil law system, and increasing the efficiency of overall procedure. We find a large reduction in the length of the pre-trial stage of 100 days (0.7 SD). 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 pp.), case-level pre-trial hearings are reduced (0.6 SD), while judges are 46% more likely to set hard deadlines. We provide some evidence of speed-quality tradeoffs, while we find no evidence of judges' effort displacement from deliberations to pre-trial stages. Overall, we find that, in the context of this reform, the efficiency gains dominate the reduction in quality of the pre-trial procedure.

The remainder of the paper is organized as follows. We place the decree in the context of the Senegalese civil and commercial code of procedure, and provide background on Senegal's justice system in Section 2. Section 3 details the data and the event study design central to our identification. Section 4 presents the main empirical results. Section 5 concludes.

II. Background and motivation of the reform

A. Civil and Commercial law in Senegal

Work in progress: Annex A presents a schedule of Senegal’s civil and commercial procedure.

B. Decree n°2013-1071

The text of the decree stipulates explicitly its goal of speeding up dispute resolutions in order 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 three main ways: first, it enforced a four-month limit on the duration of the pre-trial procedure; second, it assigned new powers to pre-trial judges; third, it required the parties to with take active part in the procedure. First, it imposed a four-month limit on the length of the procedure. This maximum delay was put in application for all ongoing cases in a given chamber at time of application, although the text recognized that it could not be retroactively applied where cases were close to, or over, the four-month deadline.

Second, judges have more leverage to speed up pre-trial hearings. Specifically, it allows judges to exert pressures on the parties to avoid dilatory actions, by imposing stricter delays on pre-trial hearings, managing more closely additional expert reports and inquiries he may have requested from the parties, and allows judges to declare a case *inacceptable* in the very beginning of the pre-trial.⁴ Second, additional “circuits” are created, allowing urgent affairs to be judged at the outset, without undergoing pre-trial hearings. Again, the decree required that these measures be applied to the ongoing caseload at time of

⁴ In the previous version of the code, pre-trial judges could not declare a case brought forward without sufficient supporting evidence *dismissed for lack of evidence*, and instead would accept them into the pre-trial where a large number of hearings would have to be scheduled to assemble the supporting evidence without necessarily succeeding.

application, recognizing that it would not be applicable to cases “further along” the procedure.

Finally, defendant and plaintiff sides are asked to cooperate and be active participants throughout the pre-trial procedure. First, both parties are invited, at the first pre-trial hearing, to sign a procedural contract and agree, *ex ante*, to a calendar of hearings for the duration of the pre-trial procedure. This is akin to measures taken in the French commercial and civil law,⁵ and corresponds to a trend of predetermination of procedural hearings to cut delays. Second, both parties are empowered to demand that the opposing side present supporting documents on an ongoing basis over the course of the procedure. The judge would set a reasonable deadline for presentation of the evidence, and a case could be nullified should the party fail to provide the evidence within this delay. Finally, the reform grants each party direct access to the opposing party’s witnesses. This is a break from the previous text, whereby judges were sole responsible for witness interrogation.

C. Expected impact of the decree on the investment climate

More efficient dispute resolution is first expected to directly benefit the firms involved in an ongoing court case, and, second, to improve the general business climate, thereby benefitting other firms – existing and potential – that do not have pending cases (cf. Figure 1).

For firms with an ongoing court case, lengthy judicial processes will tie down resources, both financial (money to be put aside for lawyer fees, frozen corporate accounts, etc.) and human (time spent by the business owner and others gathering the required documents, appearing before court etc.). In the worst case, firm survival is threatened. Cutting down

⁵ Decree n° 2005-1678, December 25, 2005.

case processing delays will free up these financial and human resources more quickly, and can thus increase survival rates, and lead to an increase in investment in profitable business activities and foster innovation. These, in turn, would lead to improved business outcomes in terms revenues and profits, as well as open the potential for business expansion and increased employment generation.

Furthermore, firms involved in court cases may have a harder time accessing credit due to the uncertainty on the lenders' side regarding the length of the judicial process, and hence regarding the costs involved for their client (and therefore regarding the probability of loan recovery). Cutting down treatment delays – and reducing their variability – can then improve firm's access to credit, again fostering investment and innovation, which may lead to improved firm level revenues, profits, and employment.

Finally, the first-hand experience of a more efficient court process would improve the firm's perception of the justice system as a viable way to resolve commercial disputes. If firms are confident their future commercial disputes will be resolved efficiently, their willingness to enter into contractual agreements increases. This may have a variety of benefits for the firm, from reduced expenses, to increased quality of inputs, as they are able to contract the cheapest, most innovative, etc. suppliers, regardless of whether they already have trust-increasing long-term relationships or family ties with them. Hence, increased confidence in formal dispute resolution is another path through which the reduction of case treatment delays can affect the fundamental business outcomes for firms involved in them.

At the economy level, efficient commercial dispute resolution improves the business climate, as it improves the de-facto enforceability of contracts, increasing the willingness of economic actors to enter into contracts. This is firstly expected to enhance investment levels

and access to finance for firms, as investors and banks adjust to the fact that they can more quickly access collaterals in case of loan default. Secondly, it is expected to lead to more contracting of firms among each other, leading to higher levels of economic activity overall. More efficient dispute resolution furthermore reduces the importance of alternative methods of ensuring compliance, such as relying on long-term business relationships and family ties, thereby creating more equal opportunities for disadvantaged and less well connected groups to thrive in business. It is however much harder to ascertain causality of this results chain with respect to the general business climate or for the average firm in the economy, than for the firm involved in a court case discussed above.

III. Data and empirical strategy

A. Data

We have full access to administrative data on civil and commercial caseload in the first-instance court of Dakar, Senegal, over the 2010/15 period. This is at the core of our contribution, as court data was only available in paper form at the onset of the project. In the context of the World Bank's Economic Governance Project, we worked with a team of court-based enumerators to digitize all archives going back to 2010 and set up a real-time data entry for the ongoing caseload. This thorough data capture effort allows us to observe steps in the legal chain along two dimensions. First, we observe all hearings held by civil and commercial judges, with a full record of which cases were heard in each hearing, at which stage of the procedure, and the corresponding decision taken during the hearing. These include pre-trial as well as other hearings. 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.⁶ All pre-trial 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 the intensity of the procedure across cases.

Second, we have access to the full caseload for the 2010-2015 period.⁷ For each case, we have a full record of when it entered in the court, when it was transferred to a chamber to start the pre-trial procedure, when it finished the pre-trial procedure, which type of final decision was taken and when, and the judge in charge of the case at every hearing, as well as a set of case characteristics including the contested amount.

We use these two sources of data to retrace case-level and audience-level history for the entire caseload that entered the court over the 2010/15 period. This yields a sample of over 8,800 cases, of which 5,300 we use in the current version of the paper. We run our analysis at two levels. First, we construct case-level outcomes, collapsing hearing data at the level of the case. We compute different outcome variables for both pre-trial and decision stages: total duration, total number of hearings for a given case, probability to complete the stage within the legally set delay, probability of a case being heard at any audience over the course of the procedure. Second, we build hearing-level outcomes, collapsing all chamber-hearing-case level outcomes at the chamber-hearing level. This yields a sample of 21 observations per chamber per year.

B. Identification

⁶ 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.

⁷ In this preliminary version, we only utilize the 2012/14 caseload, as data for the 2010/11 and 2015 years were still being entered and reconciled.

We employ an event study design to capture the causal impact of a reform in the civil and commercial procedure code on the speed of justice in the regional first-instance court of Dakar.⁸ We exploit the fact that, while the decree was ratified in July/August 2013 and published in October 2013, it was applied at different times across the 7 civil and commercial chambers of the regional court. The timing of the introduction across chambers is likely endogenous to chamber characteristics. We use high-frequency data around these multiple cut-offs to identify the causal effect of the reform, net of all other contemporaneous factors, in a flexible difference-in-differences framework. Combining the staggered introduction of the reform across chambers with 3 years of pre-intervention data allows us to purge or estimates of seasonal effects, while controlling for chamber-level heterogeneity. Hence, we exploit variations across chambers,

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, there would have been no differential trends in the speed of justice across chambers. There are three main threats to our identification: chamber-level endogeneity of the application with respect to trends in size of the caseload, caseload and court level structural changes occurring in within that two-year window. First, our identification is threatened by the possibility that the different chambers decided on the timing of application of the decree as a reaction to chamber-specific shocks. For instance, a sudden increase in the caseload may have led the president of a chamber to speed up application. We show that this is not the case, and that chambers do not experience any particular spike in structure or size of their caseload in the periods preceding the application of the decree.

⁸ This approach is akin to that used by Jensen (2007), Guidolin and La Ferrara (2007), and Atkins et al. (2015).

Second, we rely on the assumption that the profile of the caseload is unaffected by the introduction of the decree. We run a number of robustness checks to establish the validity of this assumption. First, we show that the number of cases that enter the court over time follows a smooth trend around the date of application of the decree.

Finally, we check that a number of case characteristics (size of the claims; number of plaintiffs and of defendants; gender of the parties) are also unaffected by the introduction of the reform. Second, we review court-level changes in the structure of the chamber over the period, and do not find any evidence of structural changes other than the introduction of the decree.⁹ These checks corroborate the idea that the reform did not lead to any systematic changes in the profile of the caseload.

IV. 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.

A. Duration

We estimate two main models to measure the impact of the decree on the speed and nature of court procedure. First, we document the effect of the decree by period of entry of a given case in court, around its chamber's cutoff. The intuition is that the decree was applied to the younger part of a judge's portfolio. Therefore, we should see a clear jump in the speed of resolution for the cases having entered the court close to the application threshold. In

⁹ In a separate study, we exploit a case-level random assignment to measure the impact of computer-based reminders on judges' performance. We control for the corresponding assignment dummy in all our regressions.

practice, we estimate a flexible functional form that assigns one treatment effect per case entry period, as follows

$$y_{ij} = \sum_{\tau=-42}^{21} \alpha_{\tau} 11(t_of_application_since_entry_i == \tau) + D_m + D_j + \varepsilon_{ij} \quad (1)$$

y_{ij} is outcome of case i , in chamber j ; $t_of_application_since_entry_i$ indicates the number of hearing / half-month periods between the entry of case i in court and the application of the decree in chamber j , where 0 is indexed to be the date of application of the decree in all chambers (negative values indicates the a case entered before the application of the decree, while positive values refer entry after application); $11(condition)$ is an indicator function taking value one if $condition$ is met, zero otherwise; $11(t_of_application_since_entry_i == \tau)$ is an indicator function that takes value one if case i entered τ periods away from chamber j 's application of the decree.¹⁰ D_m and D_j are calendar month and chamber dummies. Standard errors are clustered at the (*chamber x period of entry*) level.¹¹ Second, we document the average effect of the decree across the cutoff, using one overall treatment dummy. For this, we estimate the following model

$$y_{ij} = \alpha \sum_{\tau=-42}^{21} 11(t_of_application_since_entry_i > \tau) + D_m + D_j + \varepsilon_{ij} \quad (2)$$

In the next version of the paper, we will use a framework similar to Coviello et al. (2015) to explore spillovers across the application cutoff, exploiting within-judge exogenous variations in the size of the portfolio across periods.

¹⁰ In the current version of the paper we restrict our analysis to a window of 38 pre-decree application and 8 post-decree application hearing periods t , as the 2012-2014 data used allow for this window around each of the chamber-level decree application dates, plus four months' time to complete the pre-trial stage. Future versions will include two full years pre-decree application (42 t) and one year post-decree application (21 t). We estimate the second model both using the full window of 38 pre and 8 post periods, and a smaller window around the cutoff of 8 pre and 8 post hearing periods.

¹¹ Our results are robust to a more stringent clustering at the chamber level.

We find evidence of a clear jump in pre-trial duration for cases that entered the chamber close to the application of the decree (see Figure 1).¹² The average effect indicates a reduction in the pre-trial duration by 102 days or 58 days, depending on the window chosen (see Table 1, Columns 1 and 3). This effect is large (0.7 sd and 0.6 sd respectively), and while the estimate of the average effect is biased downwards due to inevitable data censoring¹³ (evidenced in Figure 1 by an overall downwards trend in pre-trial duration), the censoring cannot account for the observed jump in pre-trial duration.

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 2), an outcome that is not affected by censoring.¹⁴ Recall that one of the innovations of the decree was to introduce a fixed four month delay for the pre-trial hearings. On average, the likelihood of meeting this deadline was increased by about 20 percentage points, a sizeable (0.4/0.5 sd) and highly significant impact (see Table 1, Columns 2 and 4).

The decree explicitly targets inefficiencies in the pre-trial stage of commercial and civil cases, and hence we would not expect to see any impact on the duration of the “decision stage”¹⁵ unless judges shift effort from the decision stage to the now deadline-enforced pre-trial stage. Indeed, no clear jump can be observed in the duration (see Figure 3) or the likelihood of completing this stage within one month (see Figure 4). Consistently Table 2 shows no clear evidence of average effects.

¹² All figures and tables presented in this version of the paper use the more stringent chamber-level clustering.

¹³ Of any entry cohort, the longest-lasting cases are still ongoing and hence dropped from any analysis using trial duration.

¹⁴ The window of analysis (up to 8 post-decree application hearings) was chosen such that we observe four months of post-decree application data for all cases.

¹⁵ The final phase of the trial, deliberation, where a team of judges rules on the merits of the case.

B. 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 mechanisms underlying these effects on duration in the two main phases of the trial: pre-trial and decision stages.

a. Pre-trial stage

First, we look at the number of pre-trial hearings cases undergo around the application of the decree. Figure 6 reports period-of-entry specific treatment effects, as estimated through (1). 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. The effect is large (col 1, Table 3), as cases entering a chamber after the decree experienced on average 3.7 fewer pre-trial hearings (equivalent to 0.6 sd). This effect is robust to estimation within a smaller window around the application (col 5, Table 3). 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. This is corroborated by the fact that the decree had no significant impact on a case's likelihood to be heard at any hearing scheduled in its chamber over the pre-trial procedure (Figure 7; cols 3 and 7, Table 3).

Second, we measure the impact of the reform on the extent to which judges started fast-tracking cases out of the pre-trial stages. 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 judgment) increasing sharply for cases entering the chamber just before the cut-off (Figure 5). The average effect is large, with a 23pp. increase from a pre-decree mean of 10% (col 2, Table 3), and is robust to estimation in a smaller window around the application (col 6, Table 3). It is important to note that the sharp decline in duration presented in IV.A. is not attributable to an increase in fast-tracking, as results are robust to excluding fast-tracked cases from the sample.

These results lead us to wonder, how did judges elicit the parties' cooperation in meeting the new deadline? We use hearing-level outcomes to retrace how many times a judge imposed a strict deadline on parties in non-decisive pre-trial hearings. Again, we find a break away from the trend at the application of the decree (Figure 8). 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 associated with a large effect, as judges are 6.5 pp. more likely to apply a strict deadline on one or both of the parties at the end of a non-decisive hearing, or a 46% increase from a baseline of 14.1% (col 4, Table 3). The effect is robust to estimation within a smaller window around the introduction of the decree in the chamber (col. 8, Table 3). 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.

b. Decision stage

Next, we examine channels through which the decree may have reduced the duration of the decision stage. Recall that the decree did not modify the civil and commercial procedure beyond pre-trial stage. Yet, we find evidence of a positive impact of the decree on the

duration of the decision stage. Interestingly, we find no evidence of displacement of judges' effort from decision to pre-trial hearings. Instead, we find that cases that entered a chamber after the decree experience on average 0.9 fewer decision hearings (0.29 sd) than those that entered the chamber earlier. Yet, the jump is not as clear as in the pre-trial phase, perhaps for lack of post-decree data in the current version (Figure 9). Similar to our pre-trial results, we see a decline in the probability of a case being heard at any hearing scheduled in its chamber over the course of the decision procedure (Figure 10; cols 2 and 6, Table 4), corroborating the idea that judges did not intensify the schedule of hearings.

c. Quality of the pre-trial hearings

Finally, we examine potential quality-celerity tradeoffs in the pre-trial phase. As discussed above, the pre-trial procedure aims to prepare a case for judgment in the decision phase of the trial. We have access to two simple indicators of quality of a pre-trial process, marking different gradients of quality of pre-trial proceedings. First, quality can be expressed as the likelihood that a deliberation is broken, and the case is sent back to pre-trial ("pre-trial insufficient"). This relates to a very low quality of pre-trial proceedings. Instead, relatively less prepared cases are likely to have their decision postponed at a hearing ("decision postponed"), instead of being marked with a judgment. (In the current draft, we only use the portion of post-decree data available to us, which necessarily implies that our observations are severely truncated on the right-hand side. This is something we will address, with more data, in the next version.)

We estimate (1) and (2) using these two measures as outcome variables. While Figure 11 indicates a sharp increase the probability that a case gets sent back to pre-trial after the introduction of the decree, this swing is within confidence interval of the long-term average.

Pooling all post-decree periods of entry together and estimating (2), however, turns up a positive and significant effect of the decree on the likelihood of being sent back to pre-trial. The effect is large, at 8.4 pp. relative to a baseline means of 12.9% (col 3, Table 4). However, the effect is not robustness to estimation within a narrow window around the application (col 7, Table 4), and the truncated nature of our current dataset warrants caution in interpreting this effect.

Finally, looking at a milder sign of poor quality of the pre-trial proceedings shows no impact of the decree (Figure 12; cols 4 and 8, Table 4). This further corroborates the idea that judges' effort is not reallocated to pre-trial hearings and away from decision stage: the number of decision hearings per case is reduced, and the decisiveness of each hearing remains constant across the decree application threshold.

Finally, an important measure of quality of a decision (in first instance) is the probability that a decision gets appealed (Coviello et al, 2014). Our team is in the process of collecting these data and linking them to our first instance caseload, and we will present these results in a future version.

V. Discussion

We formally document the impact of a legal reform on the speed and process of civil and commercial justice in Dakar, Senegal. The application of the decree was staggered over a 6-month period across the seven 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 2010/15 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 no change 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. Again, we see no intensification of the hearings at decision stage, and rather a decline in the total number of hearings a case has to go through to reach a final decision.

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 23.3 pp. 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 (relative to 10% at baseline). We also find that cases that entered after application were 50% more likely to complete the pre-trial proceedings relative to the baseline.

When searching for additional cues in the data on the mechanisms through which delays were cut and deadlines adhered to, we find that judges were 46% more likely to apply strict deadlines on the parties in non-decisive hearings. Looking at markers of quality of the pre-trial proceedings, we find little to no effect. Overall, the reduction in delays dominates a potential decline in quality, at least in the first instance. More data will be added to explore these effects in appeal.

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 resolution. This suggests that, contrary to the

model proposed by Coviello et al (2014), the *decisiveness* of legal proceedings offers a non-trivial margin at which legal reform can impact the speed of justice.

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Figure 1. Impact on the pre-trial duration (number of days)

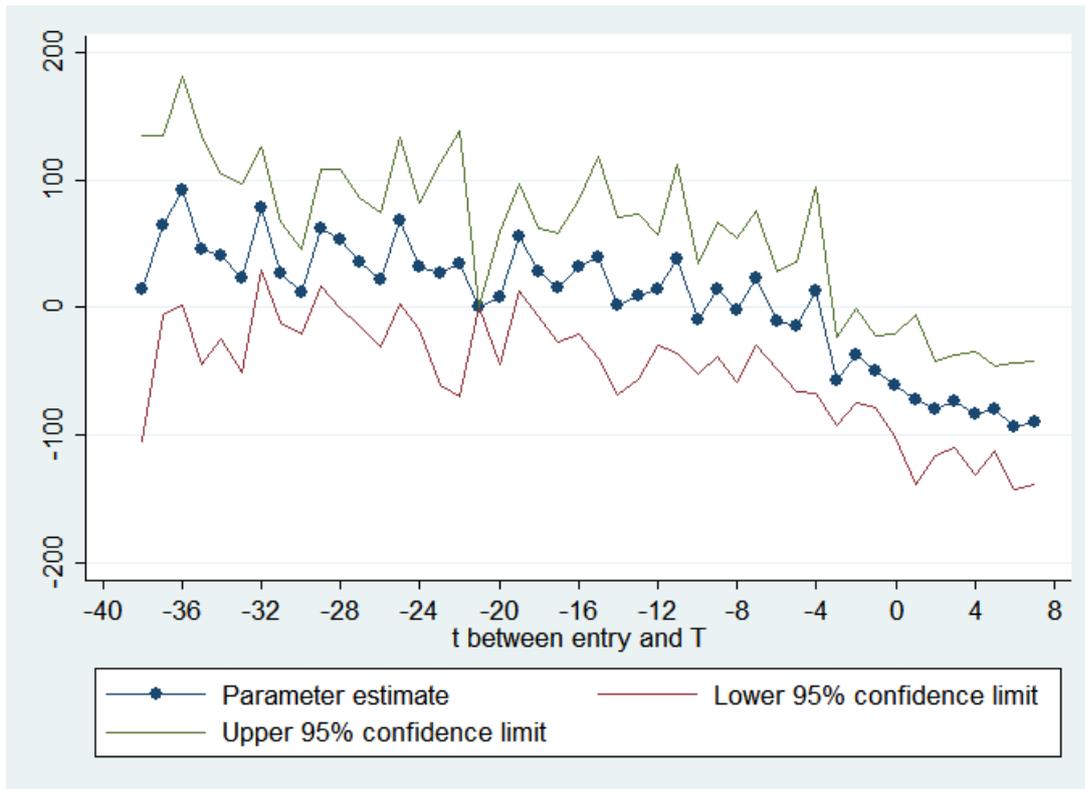


Figure 2. Impact on the likelihood to complete the pre-trial in 4 month

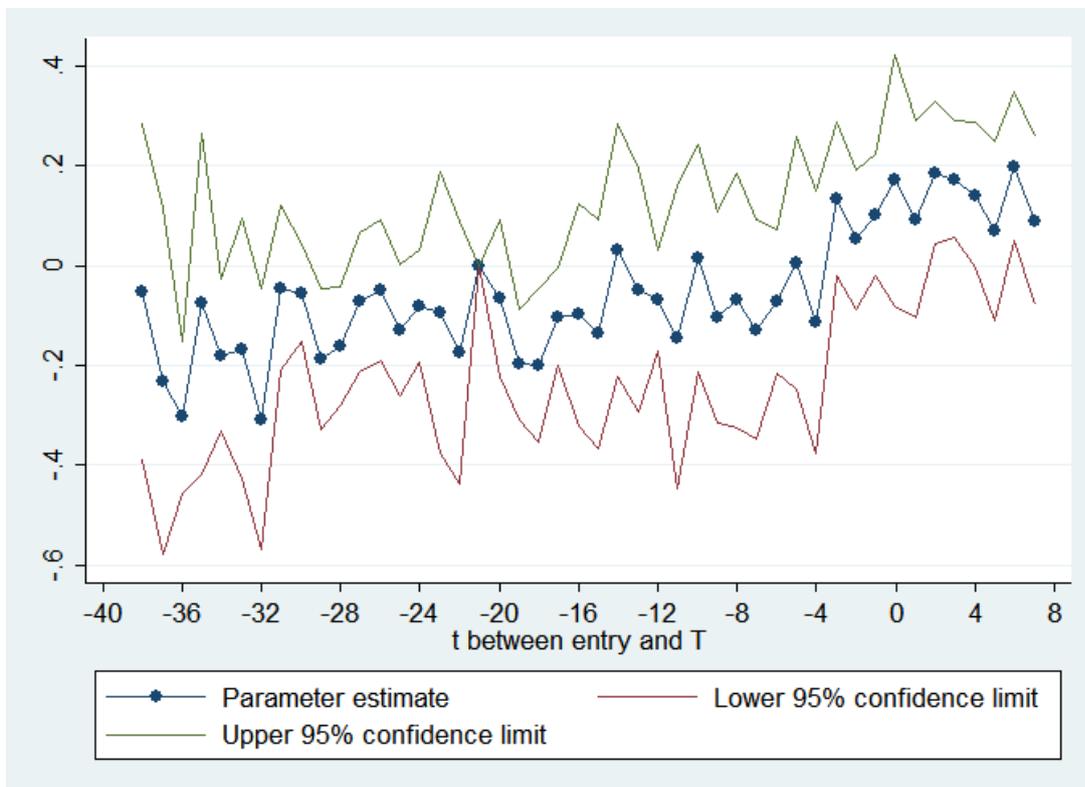


Figure 3. Impact on the duration of the decision stage (number of days)

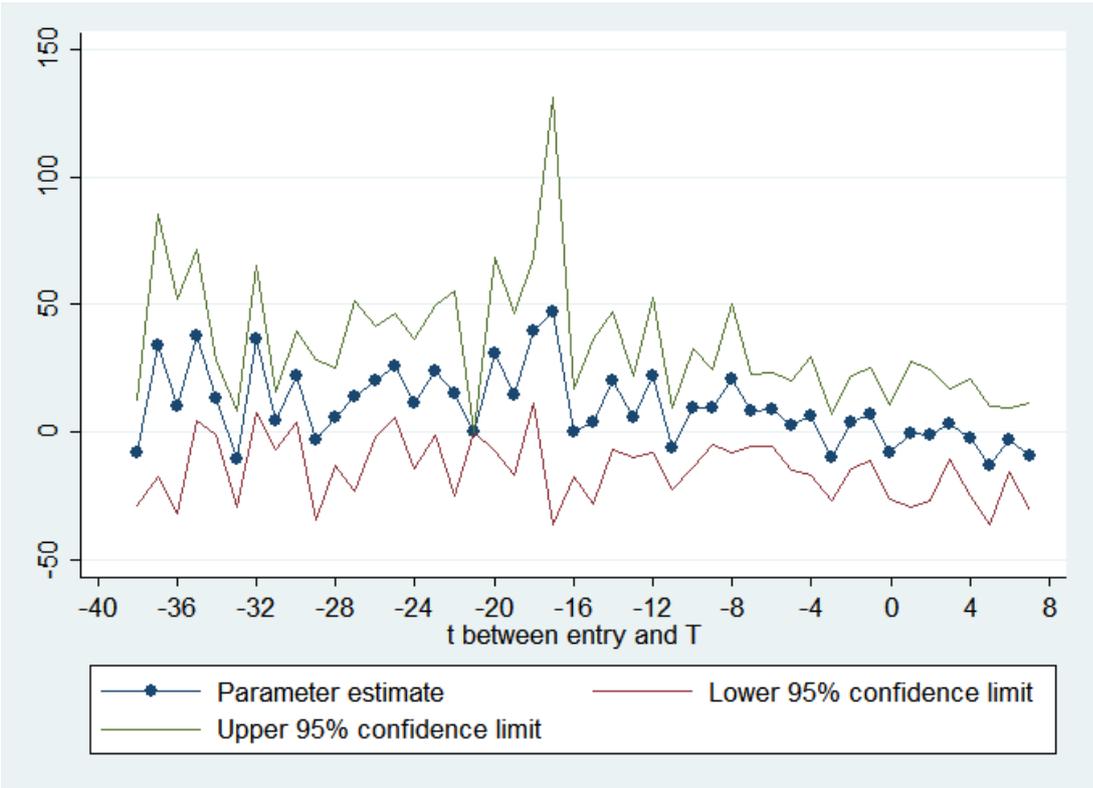


Figure 4. Impact on the likelihood to complete the decision stage in 1 month

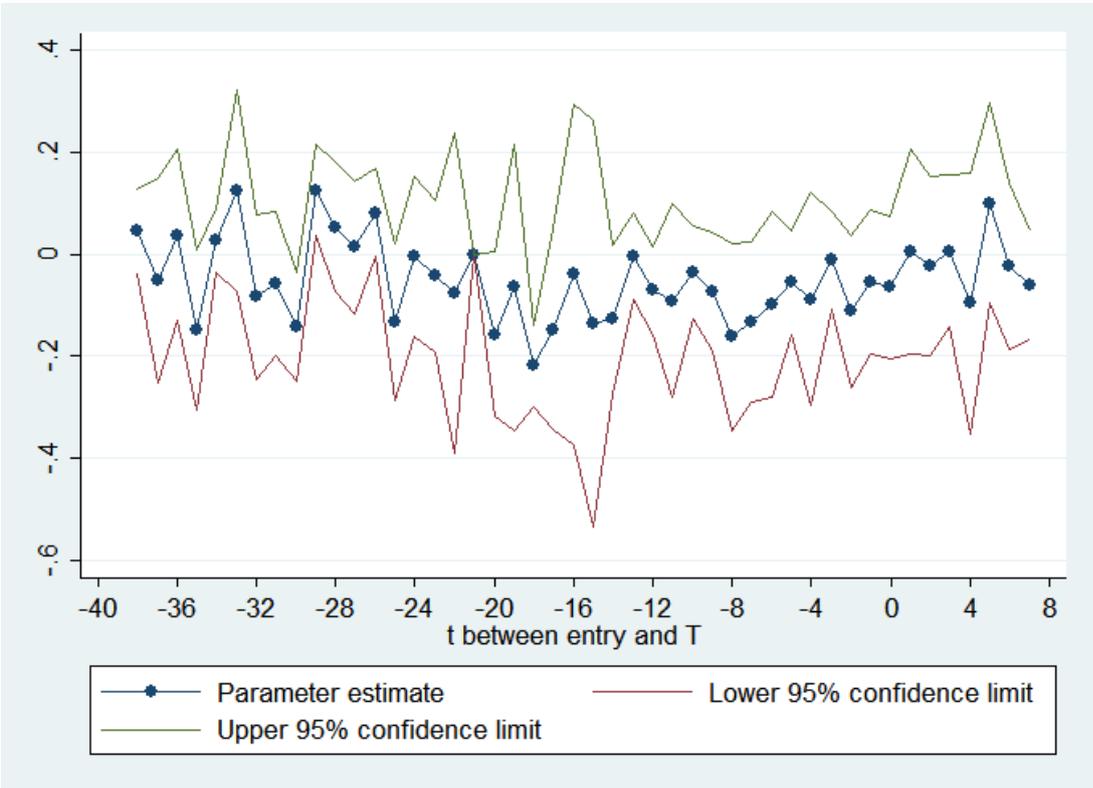


Figure 5. Impact on immediate decision likelihood (fast-tracked or inadmissible)

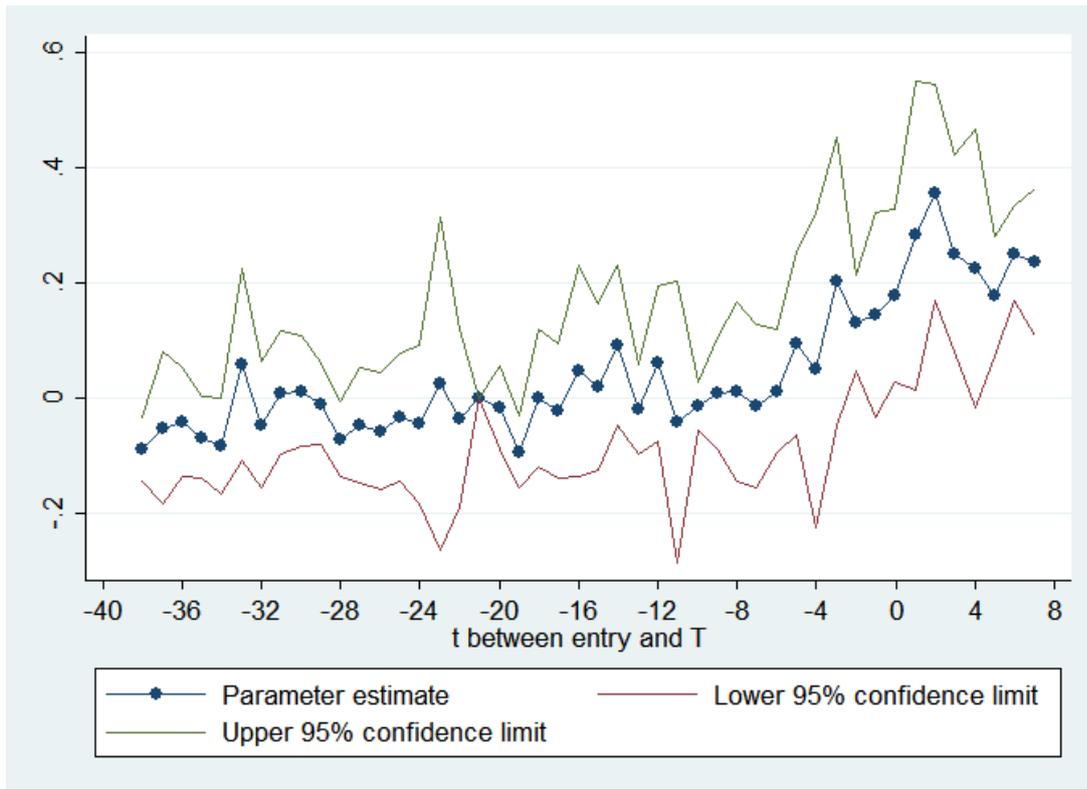


Figure 6. Impact on the number of pre-trial hearings

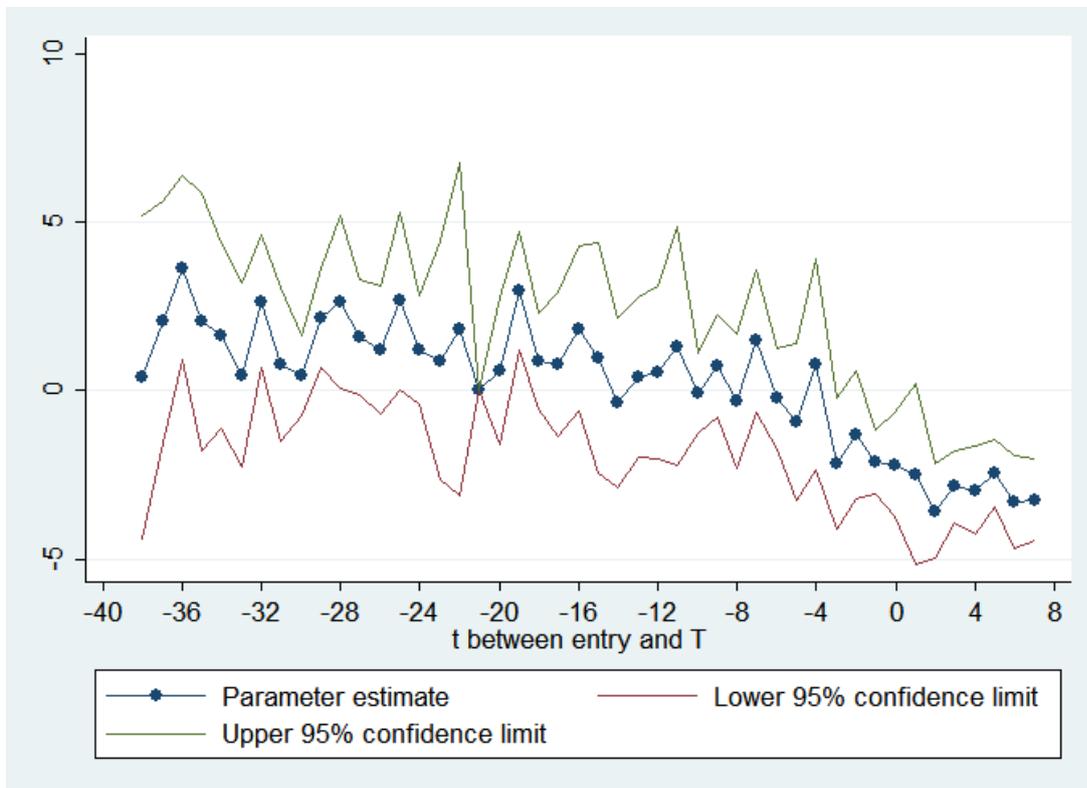


Figure 7. Impact on the pre-trial likelihood of being heard

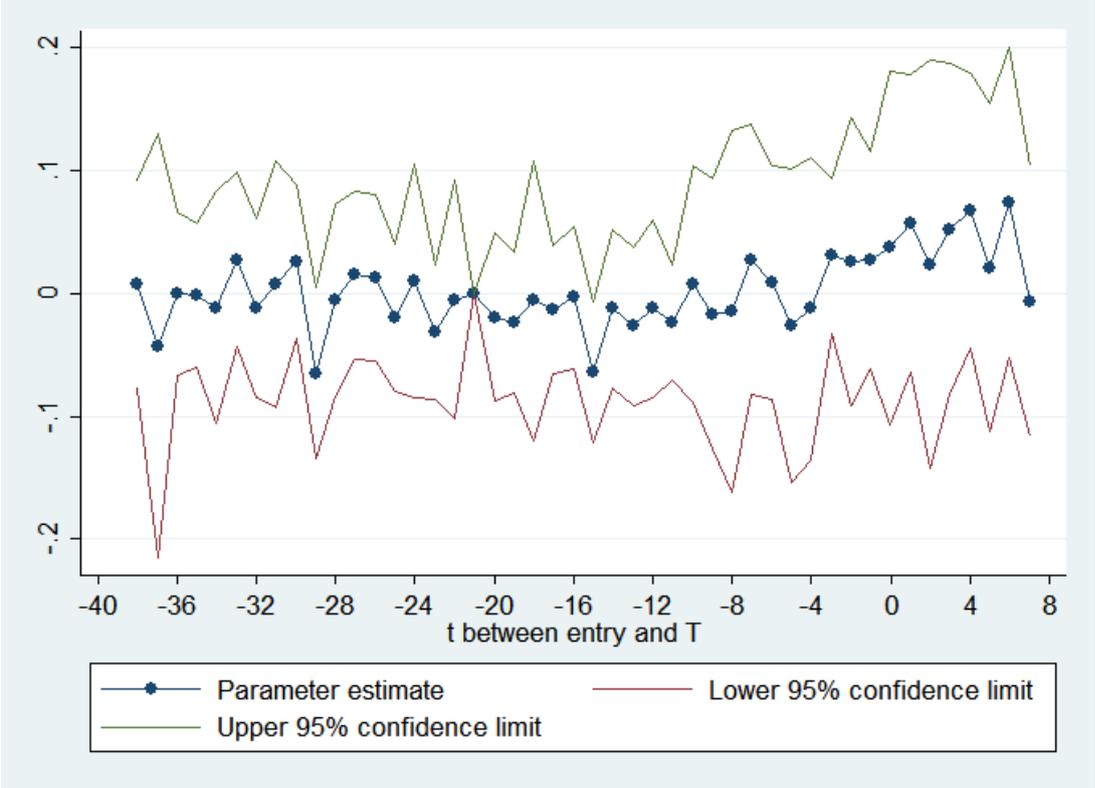


Figure 8. Impact on the judge being stricter in the pre-trial

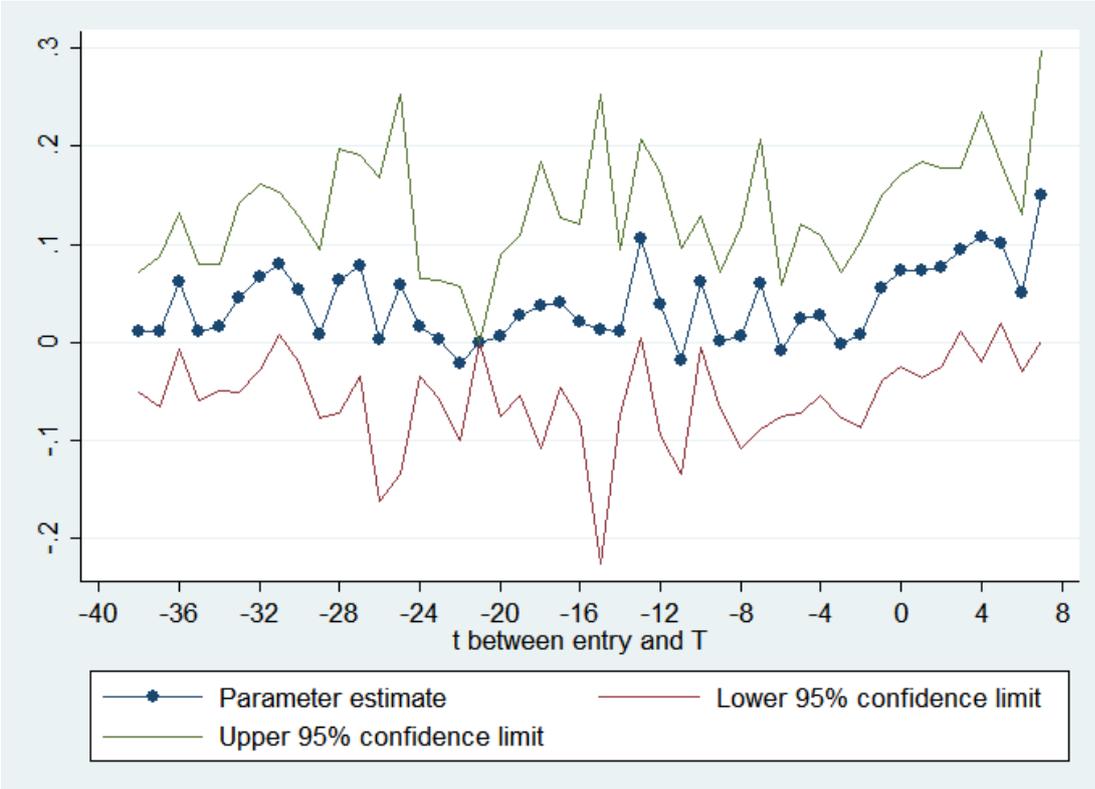


Figure 9. Impact on the number of decision stage hearings

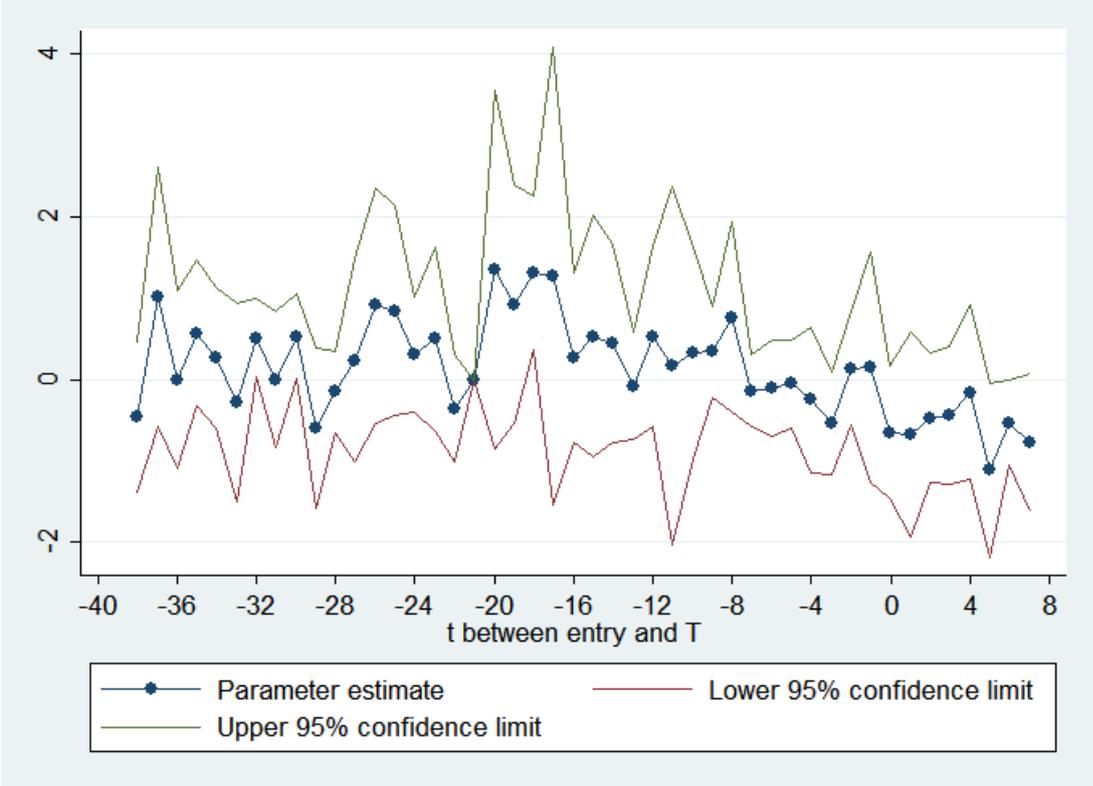


Figure 10. Impact on the decision stage likelihood of being heard

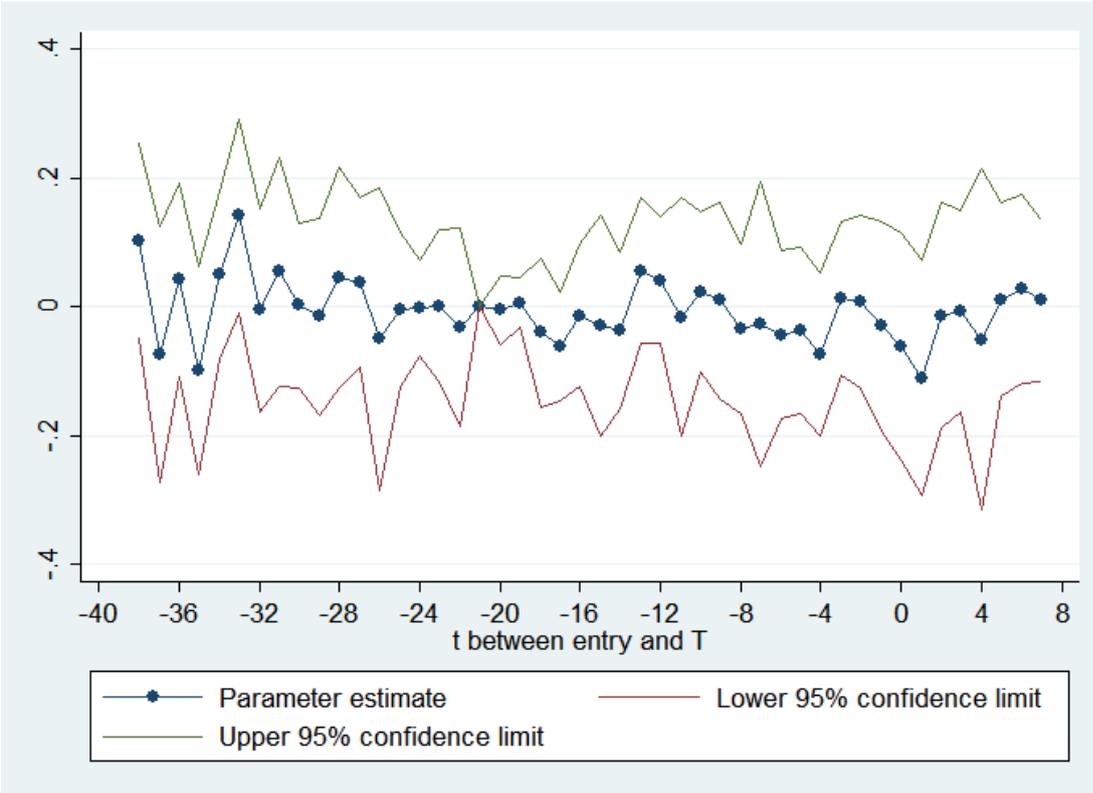


Figure 11. Impact on the likelihood of pre-trial failure

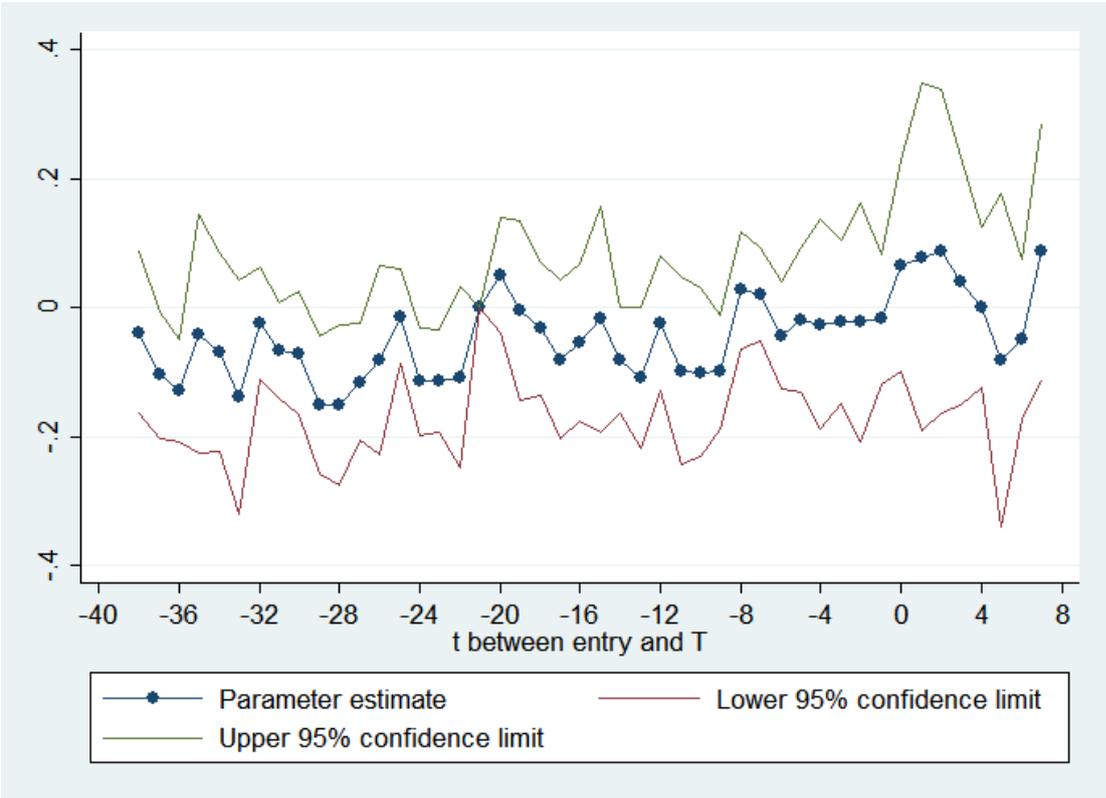


Figure 12. Impact on the likelihood of decision postponement

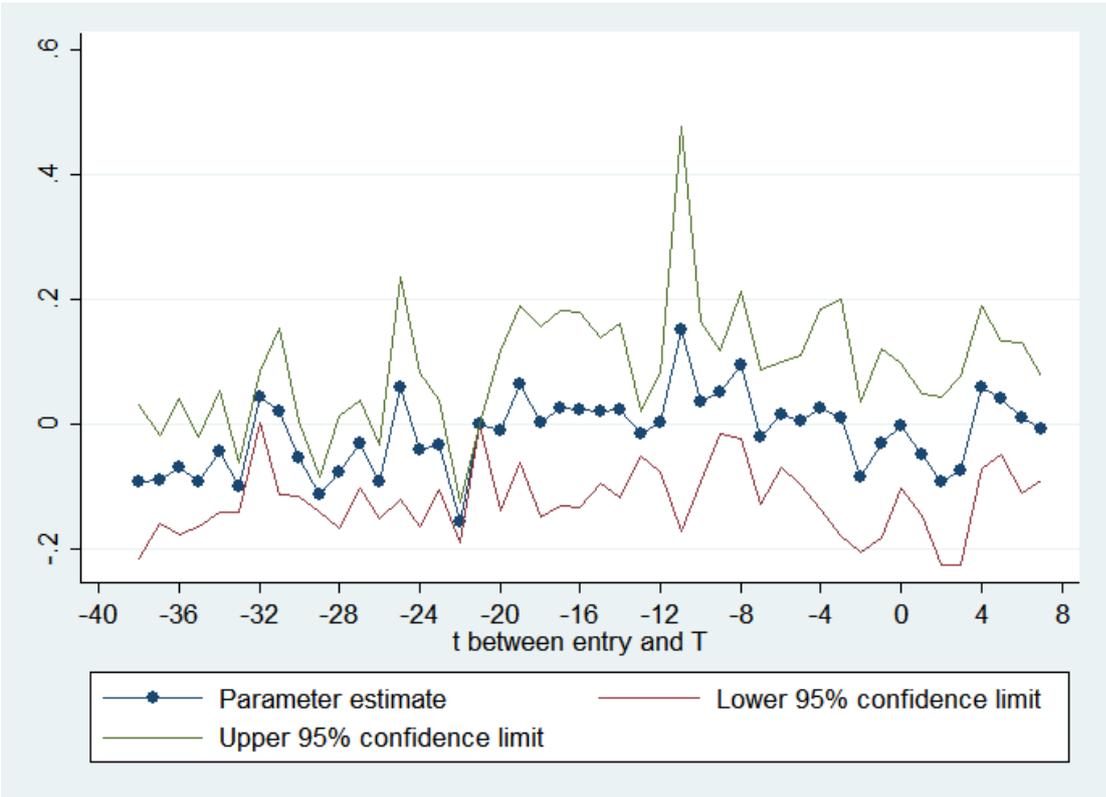


Table 1: **Impact on the speed of pre-trial procedures**

	Large window		Small window	
	(1)	(2)	(3)	(4)
	Duration of pre-trial hearings (in days)	Likelihood of pre-trial completion in 4 months	Duration of pre-trial hearings (in days)	Likelihood of pre-trial completion in 4 months
Entered after decree application	-101.596*** (15.312)	0.232*** (0.051)	-57.663*** (11.005)	0.183*** (0.043)
Constant	81.469** (26.444)	0.404** (0.110)	98.912*** (9.060)	0.526*** (0.100)
Chamber FEs	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes
Pre-mean	140.851	0.525	100.870	0.615
Pre-sd	138.463	0.499	102.749	0.487
R-Squared	0.187	0.134	0.176	0.131
Observations	3392	3524	1408	1526

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber. Large window: includes cases entering between 38 audiences before and 8 audiences after decree application; Small window: 8 audiences before and 8 after. Pre-mean and pre-sd: average and standard deviation of the outcome in the 38 (large window) / 8 (small window) audiences before decree application.

Table 2: Impact on the speed of the decision stage

	Large window		Small window	
	(1)	(2)	(3)	(4)
	Duration of decision stage (in days)	Likelihood of decision completion in 1 month	Duration of decision stage (in days)	Likelihood of decision completion in 1 month
Entered after decree application	-17.605** (4.757)	0.022 (0.037)	-3.783 (4.954)	0.015 (0.027)
Constant	63.901*** (5.782)	0.548*** (0.035)	59.490*** (1.433)	0.405*** (0.014)
Chamber FEs	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes
Pre-mean	51.769	0.581	45.282	0.551
Pre-sd	79.730	0.493	61.923	0.498
R-Squared	0.038	0.085	0.061	0.105
Observations	3201	3392	1287	1408

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber. Large window: includes cases entering between 38 audiences before and 8 audiences after decree application; Small window: 8 audiences before and 8 after. Pre-mean and pre-sd: average and standard deviation of the outcome in the 38 (large window) / 8 (small window) audiences before decree application.

Table 3: Impact on the pre-trial stage: Channels

	Large window			Small window				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number of pre-trial hearings	No pre-trial hearings	Pre-trial likelihood of being heard	Judge more strict	Number of pre-trial hearings	No pre-trial hearings	Pre-trial likelihood of being heard	Judge more strict
Entered after decree application	-3.745*** (0.473)	0.233*** (0.027)	0.048 (0.037)	0.065*** (0.015)	-2.039*** (0.473)	0.148*** (0.034)	0.048** (0.016)	0.051*** (0.006)
Constant	7.429*** (0.700)	0.052 (0.046)	0.901*** (0.043)	0.159*** (0.013)	5.764*** (0.382)	0.095 (0.048)	0.957*** (0.028)	0.151*** (0.020)
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-mean	7.336	0.101	0.870	0.141	5.861	0.168	0.879	0.133
Pre-sd	6.266	0.302	0.144	0.174	5.207	0.374	0.134	0.180
R-Squared	0.140	0.098	0.210	0.033	0.131	0.082	0.176	0.074
Observations	3524	3524	2955	2585	1526	1526	1154	927

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber. Large window: includes cases entering between 38 audiences before and 8 audiences after decree application; Small window: 8 audiences before and 8 after. Pre-mean and pre-sd: average and standard deviation of the outcome in the 38 (large window) / 8 (small window) audiences before decree application.

Table 4: **Impact on the decision stage: Channels**

	Large window				Small window			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number of decision stage hearings	Decision stage likelihood of being heard	Pre-trial insufficient likelihood of being heard	Decision postponed	Number of decision stage hearings	Decision stage likelihood of being heard	Pre-trial insufficient likelihood of being heard	Decision postponed
Entered after decree application	-0.901*** (0.141)	-0.030 (0.038)	0.084** (0.023)	0.010 (0.018)	-0.325** (0.105)	-0.012 (0.022)	0.051 (0.028)	-0.013 (0.012)
Constant	1.749*** (0.266)	0.878*** (0.022)	0.126*** (0.027)	0.369*** (0.028)	1.513** (0.445)	0.492*** (0.014)	0.188 (0.110)	0.407*** (0.056)
Chamber FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-mean	2.294	0.799	0.129	0.173	2.017	0.775	0.164	0.190
Pre-sd	3.144	0.250	0.335	0.379	2.537	0.262	0.371	0.392
R-Squared	0.028	0.230	0.020	0.054	0.040	0.338	0.024	0.049
Observations	3524	2950	2948	2948	1526	1113	1110	1110

*** p<0.01, ** p<0.05, * p<0.1. All models estimated by OLS. Standard errors in parentheses, clustered by chamber. Large window: includes cases entering between 38 audiences before and 8 audiences after decree application; Small window: 8 audiences before and 8 after. Pre-mean and pre-sd: average and standard deviation of the outcome in the 38 (large window) / 8 (small window) audiences before decree application.

Annex A: Schedule of the Civil and Commercial Procedure

