

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

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

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 which 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

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

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,

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

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 estimate 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.

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

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.

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

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

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.

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. BT1 0 0 1 230.81 459.07 T.18 288.4c427.1maxre d these effects in appeal.

Taken together, our results suggest that, while

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.

References

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Ma, C. T. A., & Manove, M. (1993). Bargaining with deadlines and imperfect player control. *Econometrica: Journal of the Econometric Society*, 1313-

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

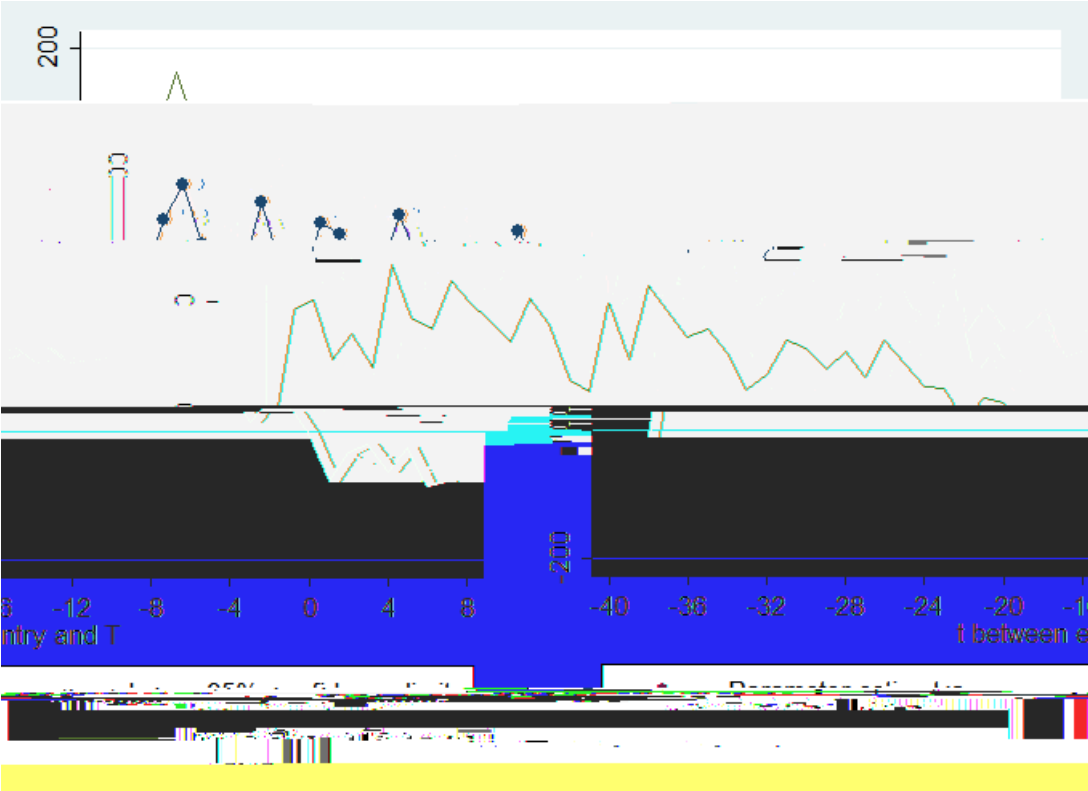


Figure 2. Impact on the likelihood to complete the pre-trial in 4 month

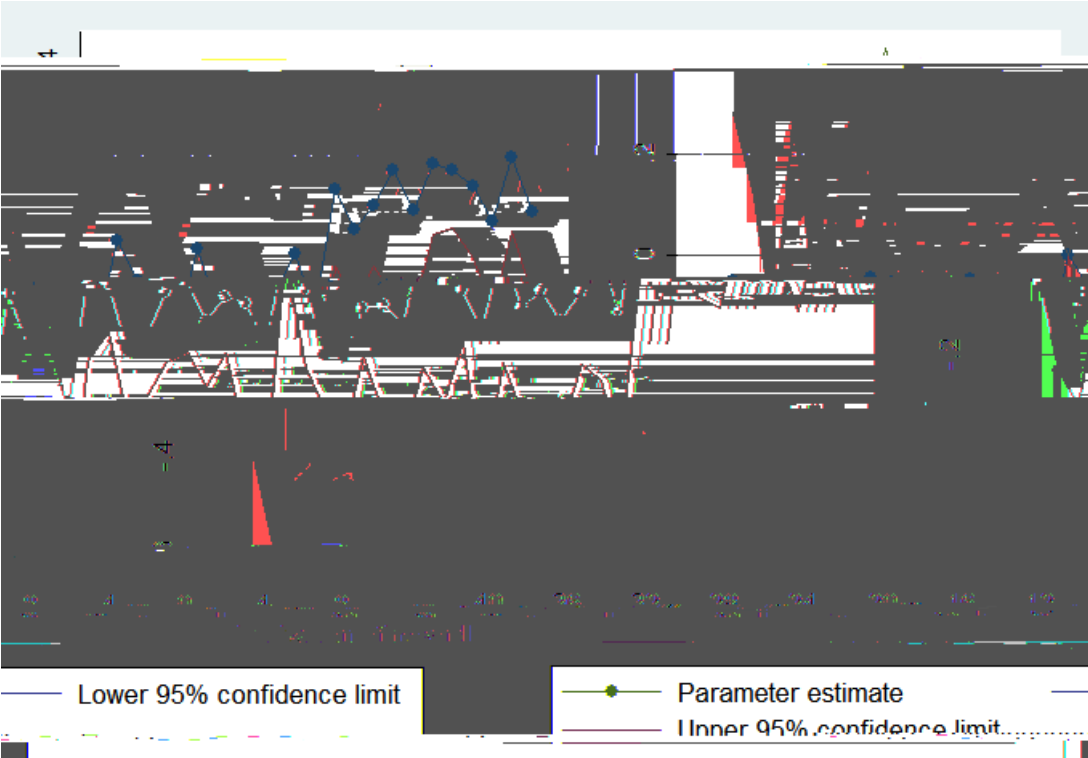


Figure 3. Impact on the duration of the decision stage (number of days)

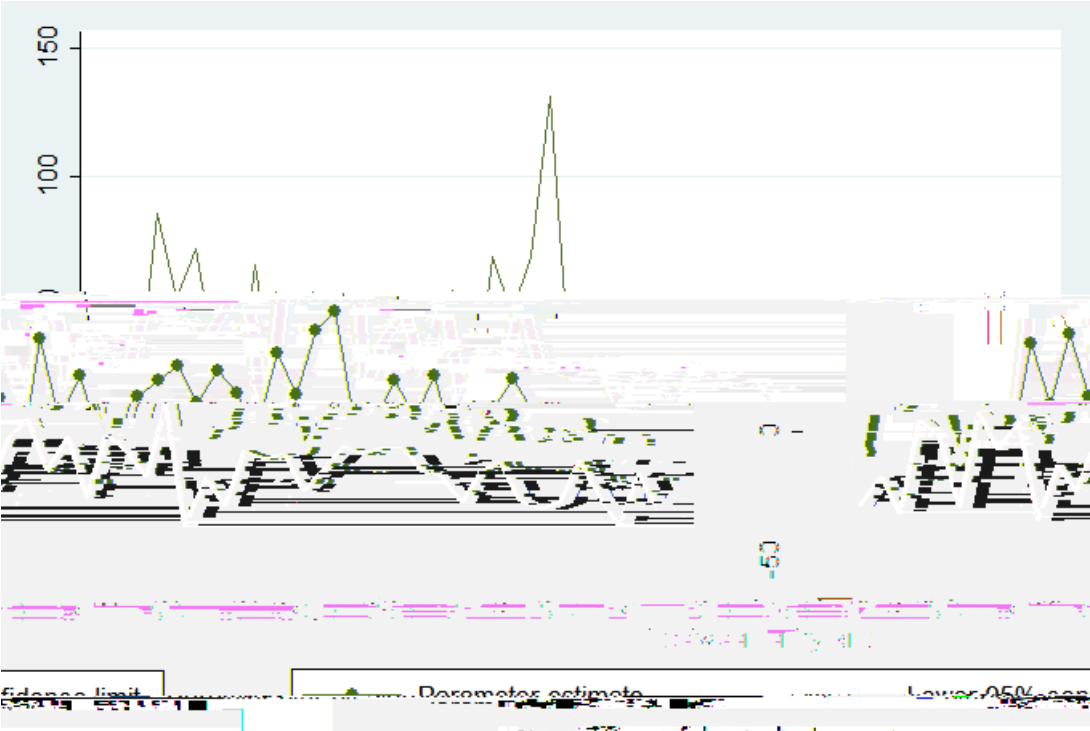


Figure 4. Impact on the likelihood to complete the decision stage in 1 month



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

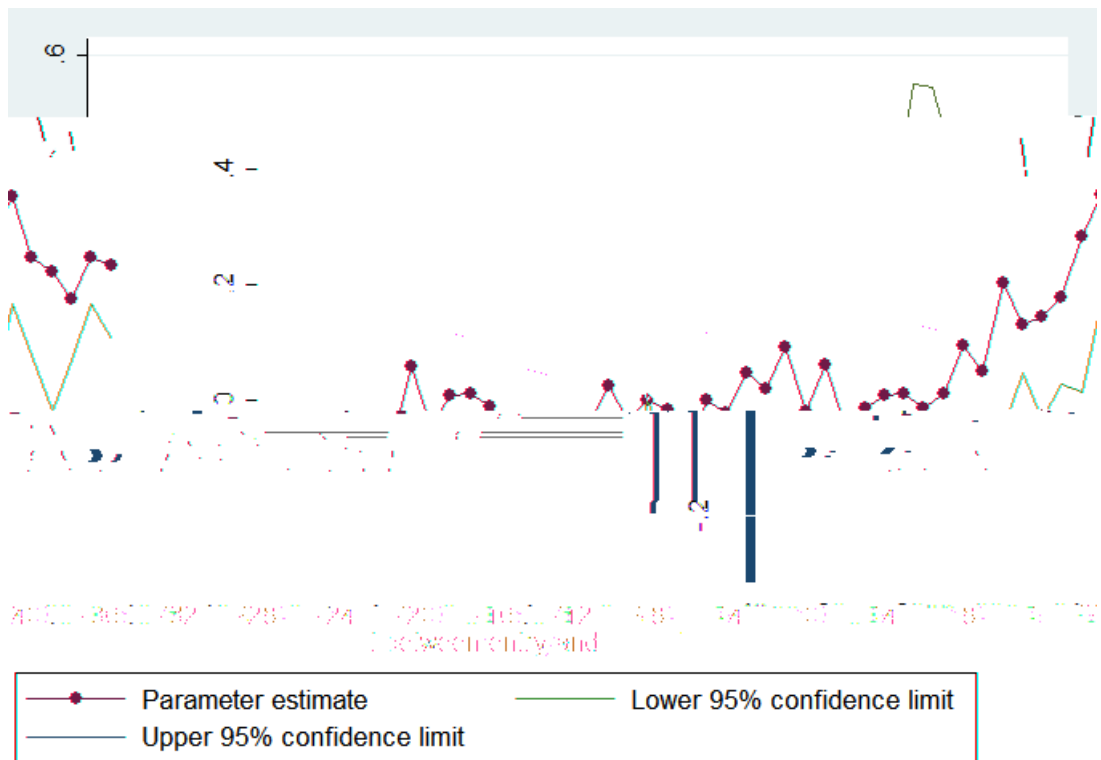


Figure 6. Impact on the number of pre-trial hearings

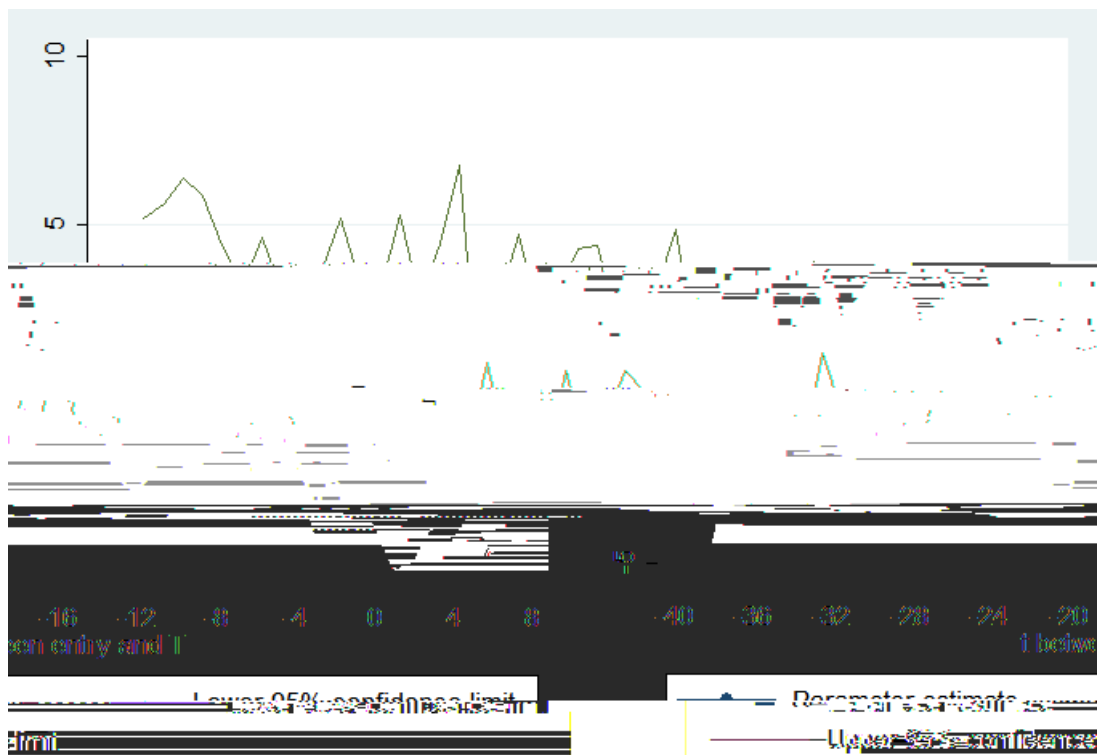


Figure 7. Impact on the pre-trial likelihood of being heard

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

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

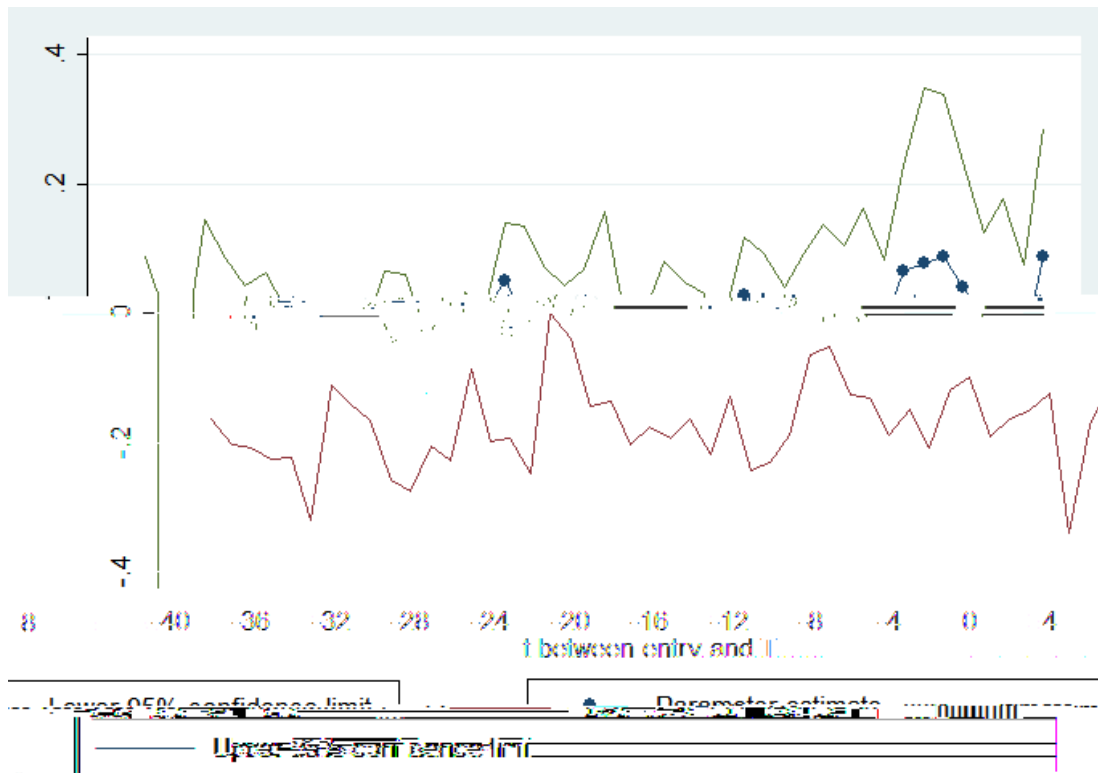


Figure 12. Impact on the likelihood of decision postponement

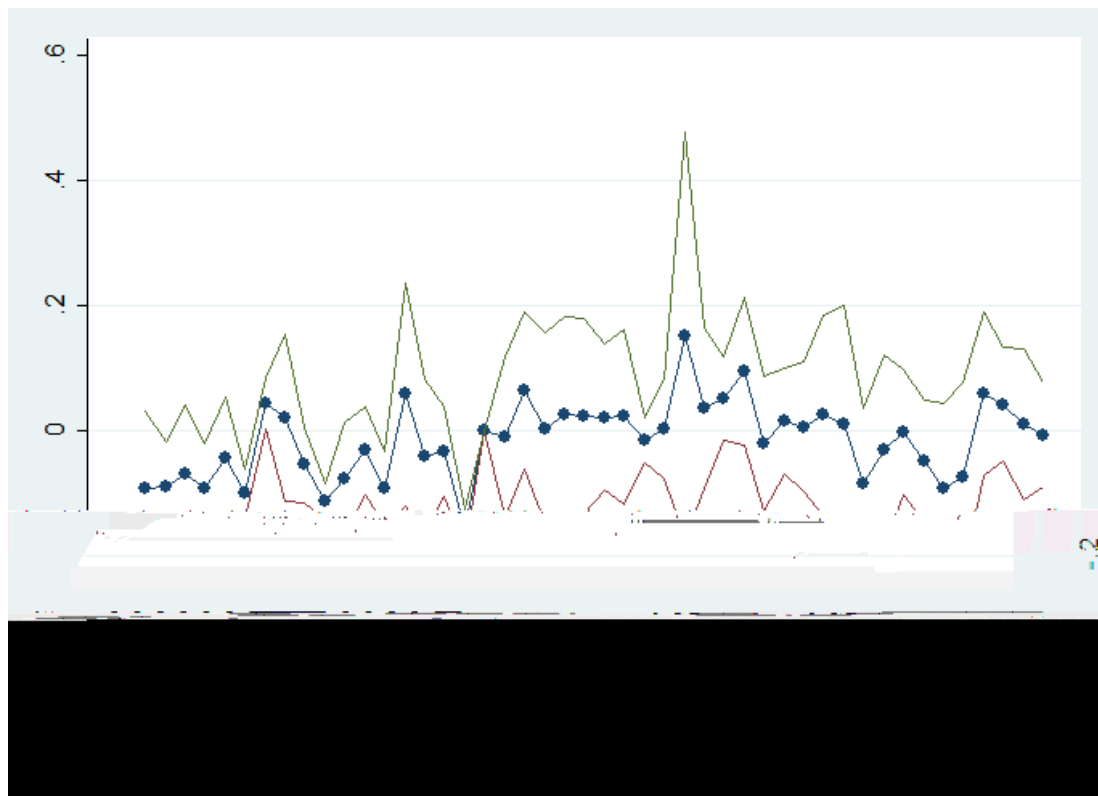


Table 1: **Impact of the speed of pre-trial procedures**

	Large window	Small window
Duration of	(1) (2)	(3) (4)

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 completion in 1 month	Duration of decision stage (in days)	Likelihood of 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.

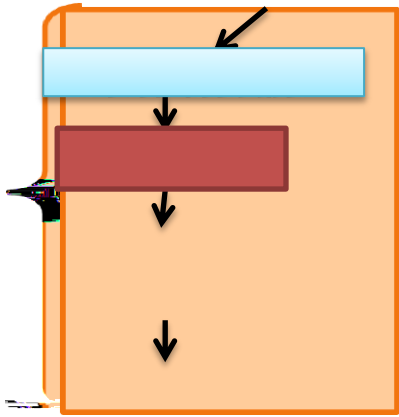
Table 3: Impact on the pre-trial stage: Channels

	Large window				Small window			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number of pretrial hearings	No pre-trial hearings	Pre-trial likelihood of being heard	Judge more strict	Number of pretrial 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. (the) - 180

Table 4: Impact on the decision stage: Channels

Large window		Small window	
(1)	(2)	(3)	(4)
Number of decision stage hearings	Decision stage likelihood of being heard	Pre-trial insu cient	Decision postponed
		(5)	(6)
		Number of decision stage hearings	Decision stage likelihood of being heard
		(7)	(8)
		Pre-trial insu cient	Decision stage likelihood of being heard



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