DISCUSSION PAPER SERIES

DP14856

DELAYS AT THE BORDER: COURT EFFICIENCY AND DELAYS IN PUBLIC CONTRACTS

Francesco Decarolis, Gianpiero Mattera and Carlo Menon

INDUSTRIAL ORGANIZATION

ISSN 0265-8003

DELAYS AT THE BORDER: COURT EFFICIENCY AND DELAYS IN PUBLIC CONTRACTS

Francesco Decarolis, Gianpiero Mattera and Carlo Menon

Discussion Paper DP14856
Published 08 June 2020
Submitted 01 June 2020

Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

DELAYS AT THE BORDER: COURT EFFICIENCY AND DELAYS IN PUBLIC CONTRACTS

Abstract

The inefficiency of the judicial system might affect the extent of delays in the execution of public contracts. We leverage on the large variation in the average length of civil proceedings across Italian jurisdictions and a granular dataset of public contracts to apply a border-discontinuity design strategy. Using a quantile regression approach, we uncover a non-linear, causal effect of court inefficiency: slower courts decrease delays at the lowest two deciles of the delay distribution, and increase delays in the top three deciles of the distribution. These findings fit a framework where contract enforcement is a key driver of contract performance.

JEL Classification: H11, H57, K41

Keywords: Court efficiency, public procurement, spatial discontinuity, Quantile regression

Francesco Decarolis - francesco.decarolis@unibocconi.it
Bocconi and CEPR

Gianpiero Mattera - gianpiero.mattera@oecd.org
OECD

Carlo Menon - carlomenon@gmail.com
Laterite Ltd

Delays at the Border:
Court Efficiency and Delays in Public Contracts

Francesco Decarolis, Gianpiero Mattera, Carlo Menon*

April 2020

Abstract

The inefficiency of the judicial system might affect the extent of delays in the execution of public contracts. We leverage on the large variation in the average length of civil proceedings across Italian jurisdictions and a granular dataset of public contracts to apply a border-discontinuity design strategy. Using a quantile regression approach, we uncover a non-linear, causal effect of court inefficiency: slower courts decrease delays at the lowest two deciles of the delay distribution, and increase delays in the top three deciles of the distribution. These findings fit a framework where contract enforcement is a key driver of contract performance.

JEL Codes: H11, H57, K41.

Keywords: court efficiency, public procurement, spatial discontinuity, quantile regression.

*Decarolis, Department of Economics and IGIER, Bocconi University, Italy; email: francesco.decarolis@unibocconi.it. Mattera, Directorate for Science, Technology and Innovation, OECD, France; email: gianpiero.mattera@oecd.org. Menon, Laterite, Rwanda; email: carlomenon@gmail.com. We thank Dario Lunardon and Olmo Silva for comments and suggestions, and Chiara Motta for excellent research assistance. Decarolis gratefully acknowledges financial support from the European Research Council (ERC-2015-StG-679217). The opinions expressed in the paper are those of the authors and do not necessarily reflect the views of the OECD and its member countries.

1 Introduction

Foreign aid agencies spent a total of \$5.4 billion USD between 1996 and 2016 on various judicial reform programs (Chemin, 2020). The fundamental role that courts play in effective contract enforcement is the main reason behind why a well-functioning judicial system is considered to be a pillar of economic development (North, 1990; Djankov et al., 2003; Acemoglu and Robinson, 2012).[1] With \$11 trillion (12 percent of global GDP) spent on them in 2018, public procurement contracts represent a class of contracts that is economically and socially fundamental (Bosio et al., 2020). Nearly all of the activities where the public sector is involved, from defence to transportation, from education to healthcare, require the public sector to procure works or goods from private contractors. It is therefore of primary importance to quantify the extent to which inefficiencies in the judicial system might worsen procurement outcomes.

In this study, we provide the first causal evidence of the effects of the average length of first instance civil proceedings at the court and year level (our proxy for court inefficiency) on the delays in public contract execution (our main measure of procurement outcomes). Speed, along with quality and access, is one of the three dimensions along which judicial systems are typically evaluated. Similarly, delays in contract execution are an often-used measure of the effectiveness of public (and private) procurement. To estimate the effects of court speed on contract delays, we exploit a series of key features of the setting analyzed: the procurement of public works in Italy. First, courts are essentially the only institution entitled to resolve disputes over public contracts. This ensures the salience of court efficiency, which would be reduced if the parties could apply alternative dispute resolution systems or other informal institutions or social norms (Acemoglu and Johnson, 2005). Second, the allocation of contractual rights is such that only a private supplier would act as plaintiff in a court litigation. By law, various types of guarantees exist in favor of the public contracting authority (CA), so that essentially all court cases involve private contractors attempting to earn back part of the penalties applied to their last installment. Third, the allocation of cases to courts follows a transparent rule: the relevant jurisdiction depends on the location of the CA.

This latter feature, coupled with the huge differences in court speed across jurisdictions, implies that a firm's decision to delay contract completion might be influenced by the perceived enforcement strength. However, court inefficiency may have diverging effects on delays. We present a simple conceptual framework to illustrate this insight. For smaller delays, the relatively low level of penalties makes a private supplier less willing to sue a CA in the case of inefficient courts, thus marginally reducing the time delay to deliver the contract. In case of larger delays, the expected gains for a firm from suing a CA grows monotonically with the size of the delays. As court inefficiency grows, both direct and strategic effects come into play and the supplier has an incentive to delay contract delivery.

Because of the potential heterogeneous effects on the delay distribution, we adopt a quantile regression approach. The implementation of this approach rests on three features, which are the founding elements of our empirical strategy. The first is a large dataset that combines together data on the universe of public work contracts awarded between 2002 and 2012, and the yearly average speed of the relevant court. The second is a border-discontinuity design that exploits the large variation in the average length of civil proceedings across courts, and the exogeneity of the borders of local courts. As detailed below, the key to our strategy is the possibility of comparing contracts awarded by municipalities located on opposite sides of jurisdiction borders, and the substantial variation in the court speed of the 165 courts in the data.[2] The third is a quantile regression estimator that allows us to investigate for

[1] Indeed, strong contract enforcement has been found to impact significantly on the availability and cost of credit (Qian and Strahan, 2007; Fabbri, 2010; Ponticelli and Alencar, 2016), the development of financial credits (Djankov et al., 2008), trade volumes (Berkowitz et al., 2006) and competition in markets (Johnson et al., 2002).

[2] Italian courts take on average more than twice the number of days needed on average in OECD countries to resolve a commercial dispute (World Bank, 2020). On average, 1,120 days are needed to resolve a commercial dispute through courts in Italy. Italy also ranks 122nd out of 189 countries in the World bank's *enforcing contracts* indicator, largely because of the extreme length of judicial proceedings.

a large number of fixed effects. Fixed effects are needed to implement the border-discontinuity design, but also to account for any residual unobserved heterogeneity. Thanks to the granularity of our data, we can include firm-level fixed effects accounting for the identity of the private contractor. Hence, the identifying variation comes from observing the delay in the completion of contracts for the same firm winning contracts across different jurisdictions. Crucially, this helps to account for border-spillover effects that might result from firms sorting across different jurisdictions.

We find that slower courts reduce delays at the lowest two deciles of the delay distribution, while they increase delays in the top three deciles of the distribution. This reveals the heterogeneous effects of court inefficiency: the delay distribution is flatter and has fatter tails where contract enforcement is weaker. The estimates are always highly statistically significant and large in terms of economic magnitude. They indicate that an increase from the 25^{th} to 75^{th} percentile in the average length of trials results in a decrease of 6.6 percent in time delays at the 20^{th} percentile of its distribution, while an equal increase in the length of judicial procedures leads to an increase in time delays of about 4 percent at the 80^{th} percentile of delay distribution. For a typical contract, which lasts one year, these estimates imply a reduction of delays of 24.1 days and an increase in delays of 14.7 days respectively. These findings pass a number of robustness checks involving different model specifications and analysis samples, that are reported both in the text and in the web appendix.

This study contributes in two ways to the nascent empirical literature on the causal impacts of judicial inefficiency. First, we provide novel evidence on the non-monotonic effect of court inefficiency on the demand for justice. Given the importance of the public procurement setting that we study, this is a fundamental finding that should inform the ongoing policy evaluations of the court system reforms. The second contribution is to offer one of the few instances in the literature of a causal assessment of the effects of court inefficiency. Chemin (2020) offers the first cross-country evidence of this type, by evaluating 10 years of judicial reforms, and reviews the state of the existing literature on causal analyses. He groups these into studies involving either randomized experiments (Grenier and Pattanayak, 2011; Aberra and Chemin, 2017; Sandefur and Siddiqi, 2013) or natural experiments (Chemin, 2009a,b, 2012; Kondylis and Stein, 2018; Lilienfeld-Toal et al., 2012; Lichand and Soares, 2014).[3] Our study belongs to the latter group. Chemin (2020) also stresses how different sectors of the economy are differently exposed to the consequences of court inefficiency. In our setting, courts behavior is particularly salient due to the near absence of any alternative to the courts as dispute resolution systems.

The research on public procurement is the second strand of the literature to which this study contributes. Multiple studies have dealt with the causes and consequences of the waste associated with time and cost renegotiations in public contracts (Guasch et al., 2008; Lewis and Bajari, 2011; Decarolis, 2014; Bajari et al., 2014; De Silva et al., 2017; Coviello et al., 2018; Ryan, 2020; Bosio et al., 2020). Of all of these studies, Coviello et al. (2018) is the only one that seeks to uncover the relationship between court inefficiency and procurement outcomes. Like our study, they look at Italian public work procurement. Our empirical analysis, however, differs in that we address heterogeneous effects through a quantile regression approach. While their theoretical model allows for heterogeneous effects, their linear regression approach does not seek to evaluate this heterogeneity.

Finally, from a methodological perspective, this study combines the spatial discontinuity design developed for the Italian courts by Giacomelli and Menon (2017) with a quantile regression framework with fixed effects (Koenker, 2004). Quantile regressions are a natural choice for estimating the heterogeneous effects on delays that an analysis of firm incentives uncovers. The inclusion of firm fixed effects, however, is particularly important in our setting because it is an established fact that court inefficiency might impact the growth and productivity of firms, both at the extensive and intensive margin (Disney et al., 2003; Bartelsman et al., 2013; García-Santana and Ramos, 2015; Giacomelli and Menon, 2017).

[3] Interestingly, Chemin (2020) finds that reforms to improve court speed were not successful when not combined with improvements also on the access and quality dimensions that jointly determine the court systems performance. In our setting, access is by law identical over the whole country. Quality is harder to assess and we do not have a measure with a coverage comparable to the measure of speed used in the analysis.

2 Institutions and Incentives

2.1 Institutions I: The geography of judicial efficiency in Italy

The geography of civil justice in Italy is a remarkable case of historical legacy and institutional inertia that introduces exogenous sharp discontinuities in court inefficiency across the national territory (Giacomelli and Menon, 2017). In this section, we illustrate the specific institutional setting, while in section 3 we explain how it supports our identification strategy.

In the time span of our analysis (2002-2012), the functioning of the Italian civil justice system was assigned to 165 courts, covering the whole national territory. The geography of these courts was established in 1865, straight after the unification of Italy, taking into account the judicial systems of the previously independent states. Since then and until 2013, no existing court has been removed, and a few new courts were established in the 1960s and 1990s.[4] As a result of this historical inertia, court jurisdictions do not systematically match any other administrative boundaries. However, they never cross regional borders and, in some cases, they overlap with provinces.[5]

Another remarkable characteristic of the Italian judicial geography relates to the large variation in the degree of inefficiency across courts. This is visible in Figure 1, which maps the average length of civil proceedings (measured in days) by court jurisdiction in the period of our analysis, 2002-2012. This variable represents our proxy for the strength of contract enforcement at the court level: longer civil proceedings point to a slow court, characterized by weak contract enforcement. From the map, we can observe a systematic geographical pattern (Southern courts on average are twice as slow as Northern ones), even though there is still a fair amount of variation within regions and between neighboring courts. As an example, consider the case of the court of Varese in Lombardy which has an average duration that is in the 1,400-1,500 days range (among the worst). It neighbours with 4 courts: 2 in Lombardy and 2 in Piedmont, and these 4 neighbouring courts are among the fastest, with an average duration in the 445-630 days range.

As discussed in Giacomelli and Menon (2017) and Bianco et al. (2007), several factors may explain such a large variation. First, the difference across macro-regions (North, Centre, and South) may partly be explained by a variation in the litigation rate (expressed as the ratio of the number of disputes brought to courts, to the population), which is higher in Southern regions. Second, the national government is in charge of allocating the (scarce) resources among the courts, which makes this decision highly centralized, with limited flexibility to adjust for changes in local demand. Thirdly, the supply side shows significant variation as well, due to the impossibility for local courts to select and appoint judges, as well as to the absence of effective court management systems and of incentive mechanisms for judges to expedite proceedings. As a result, there is substantial randomness in the distribution of the ability of judges and effort among courts.

Civil proceedings are assigned to courts on a geographic basis. For public procurement contracts, unless some special circumstances apply, the court in charge will be that overseeing the geographic area where the CA is located. Since the CAs in our samples are municipalities, it is straightforward to associate each procurement contract to the relevant court.

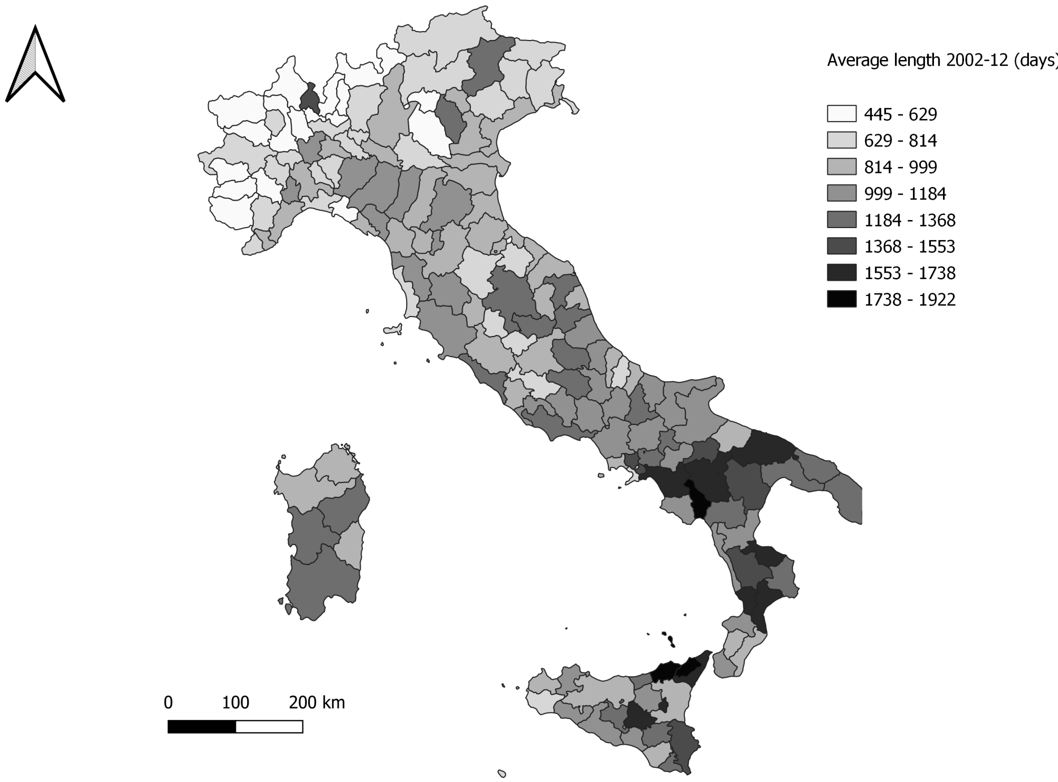
2.2 Institutions II: The public procurement system

The procurement of contracts for the construction and maintenance of public infrastructure follows detailed rules. Depending on the contract value and other characteristics, these rules derive from

[4] In the last 50 years, 11 new small courts have been established (5 in the 1960s and 6 in the 1990s). Our findings are unaffected by excluding these courts.

[5] Regions and provinces are the administrative units which correspond to level 2 and level 3 in the Eurostat NUTS classification, respectively.

Figure 1: Average length of civil proceedings, 2002-12



Notes: The figure reports the yearly average length (in days) of civil proceedings across the 165 courts in Italy.
Source: ISTAT and Italian Ministry of Justice.

the EU Directives on procurement, national legislation, local authority regulations, or, typically, a combination of all of the above (see Decarolis and Giorgiantonio (2005)). Frequently, the system entails a CA - for instance, the procurement office of a municipality - setting up a specific document (namely, a call for tenders). This is made publicly available in advance of the bidding stage and contains information on the job description, the reserve price (i.e., the maximum amount a CA is willing to pay), the awarding criterion to select the winning supplier (for instance, price-only or an index accounting for both price and other technical elements), and the contract duration (i.e., the delivery time to complete the job). Moreover, it specifies also the procedure to solve any contractual dispute, with civil courts being the default contract enforcement institutions.[6]

If a dispute emerges during the contract execution, both the CA and the supplier can act as plaintiffs in court. However, various mechanisms are in place to help solve disputes and avoid the opening of a court case. The exact nature of these mechanisms changes depending on whether the dispute regards in-scope work (i.e., features of the job as originally planned) or out-of-scope work (i.e., modifications of the originally planned job). For both types, both the CAs and the suppliers are entitled to negotiate if the dispute involves a relatively "minor share" of the contract value, otherwise contract resolution is the only possibility (typically up to one-fifth of the initial contract value). The distribution of cost overruns

[6]Any alternative dispute resolution system that is common in the US procurement system (such as arbitration) is hardly ever adopted.

in the data has multiple discontinuity points due to thresholds set by the law and the local regulations for the allowed cost renegotiation.[7] For delays, instead, there are no pre-specified thresholds in the law and, accordingly, the distribution observed in the data is rather smooth. For this reason, we will focus our analysis on delays, leaving the discussion on cost overruns in the web appendix.[8]

Finally, CAs can take advantage of a few tools to induce suppliers to comply with contract specifications. The first tool relies on the timing of payments, distributed over the contract duration to allow a CA to delay them if not satisfied with the work's progress. The second tool is to apply penalties to the last payment installment. These penalties are per-day of delay and their amount is proportional to the contract value.[9] The third tool is the execution of a supplier's financial guarantees which, by law, take the form of an irrevocable letter of credit to be paid immediately at the CA's request. With all these tools available, a CA will act as the plaintiff before a court only in those rare circumstances where the behavior of a supplier caused damages for a CA going beyond those that the penalties and the financial guarantees are meant to restore. For this reason, nearly all court cases involve firms suing CAs and not vice versa. Hence, we shall focus on a firm's incentives to a sue CA when thinking of the interaction between court efficiency and delays in procurement contracts.

2.3 Incentives: A basic conceptual framework

The institutional framework determines a precise sequence of the actions to be taken by a CA and its suppliers when delays occur in contract execution. Firstly, the CA applies the penalty to the last payment due to its supplier; then, this latter either accepts the penalty or sues the CA before a court. If the firm opts for undertaking a judicial action, the CA can either defend itself before the court or remit the penalty.

Court cases generate for the plaintiff both fixed costs, sustained when opening the case, and variable costs, which are commensurate with the duration of the trial. Moreover, we assume that the delay in executing the contracted work impacts the payoffs of the parties in opposite directions: positive for the firm, negative for the CA.[10] This assumption, coupled with the cost structure and the action sequencing described above, implies there should be a non-linear relationship between contract delays and court inefficiency. When delays are low, a CA will impose a small penalty on a firm, thus making the cost of starting a court case for a firm likely larger than the benefit that a firm might obtain by having the penalty restored. Such an incentive not to sue a CA is stronger in slower (inefficient) courts, where the expected length of the trial would discourage firms to pay high variable costs to restore a moderate penalty. Hence, a firm cannot credibly commit to sue a CA for low delays in presence of less efficient courts, therefore the only way for a firm to reduce the penalty is to deliver on time.

As the delay grows, so does the penalty and, hence, the expected value of what a firm could get restored if successfully suing a CA. In particular, if the marginal cost of going to trial is sufficiently low,[11] there

[7] In fact, what a "minor share" is depends on both the national law and a plethora of regional regulations: in most regions, modifications implying a cost overrun of up to 5 percent of the contract value are subject to a straightforward authorization process; for the next 15 percent of the contract value the authorization process is more involved but still relatively straightforward (all the rights are allocated to the CA which can ask modifications up to this threshold); finally, if the modifications involve one fifth of the original contract value or more, contract resolution is typically unavoidable. See Decarolis and Giorgiantonio (2005) and Decarolis and Palumbo (2015).

[8] In the web appendix, we show that, relative to the findings about delays, those about cost overruns are qualitatively similar, although less sharply defined.

[9] In more detail, penalties are commensurate with the days of delay and proportional to the amount of the contract or the performance of the contract. The penalties due for delayed fulfillment are calculated daily between 0.3 per thousand and 1 per thousand of the net contractual amount, to be determined considering the extent of the consequences related to the delay, and in any case, cannot overall exceed 10 percent of said net contractual amount.

[10] This is often justified by arguing that CAs suffer monetary and reputational damages from not having the infrastructure available for the citizenship, while firms benefit from not having to exert the costly effort required to complete the contract promptly.

[11] We assume that variable costs are linear to the number of days of trial, i.e. marginal cost is constant.

will be some cut-off such that, when the delay exceeds this value, a firm will have a positive expected payoff by choosing to bring the case to a court. Hence, the firm can credibly commit to suing the CA. Moreover, the inefficiency of the court becomes a force exacerbating delays due to both a direct and a strategic effect. On the one hand, the direct effect derives from the fact that, by starting a trial, the contract completion is interrupted, accumulating further delays until the case is settled. On the other hand, the strategic effect is a consequence of the direct effect: envisaging a long court case impeding the completion of the public work, a firm anticipates that a CA would accommodate its delay without imposing any penalty. The firm will strategically take advantage of this possibility, increasing its profits by delaying the job.[12]

Figure 2 offers a graphical illustration of the relationship between the strength of contract enforcement and delay in public procurement. From a firm's perspective, suing becomes profitable if the net expected gains from going to trial are greater than the losses associated with litigation. Thus, denote by A the net contract value, f the fixed cost of suing a CA, ct the variable cost associated with the court case, with c the marginal cost of suing the CA, and t the length of the judicial process, our proxy for weak contract enforcement (inefficiency). The penalty amount will increasing by the delay d, and be proportional to the contract value, with pA being the daily penalty (recall that, by law, p has to be set between 0.03% and 0.1%). On top of that, the overall penalty must not exceed 10% of the net contract value. Hence, for a given probability of winning in court, the expected gains from suing the CA can be represented as a function by parts:

$$\text{Expected gains from suing} = \begin{cases} -f - ct + Apd & if \quad d \leq \frac{0.1}{p} \\ -f - ct + 0.1A & if \quad d > \frac{0.1}{p} \end{cases}$$

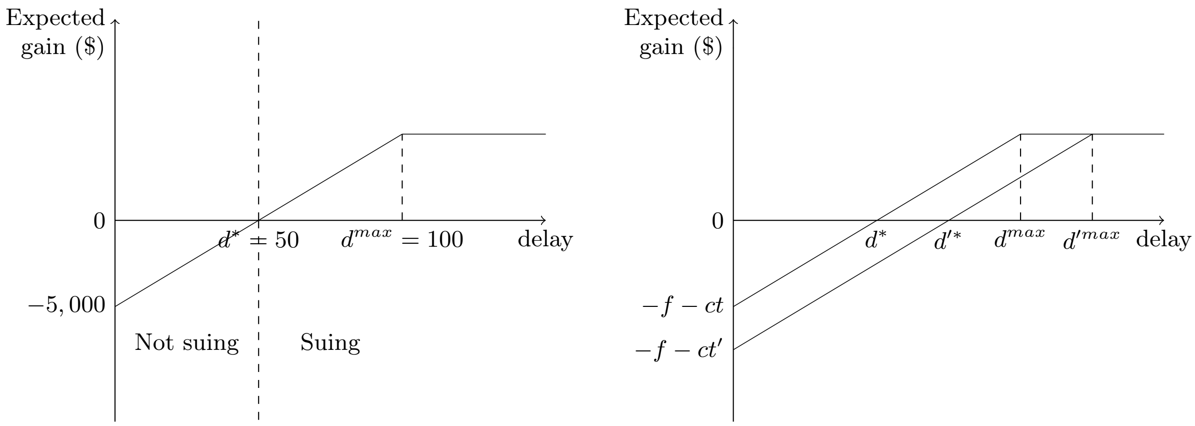
The left panel in Figure 2 provides a numerical example of how a firm's incentive to sue varies. Suppose the contract value A is 100,000, whereas the sum of fixed and marginal cost of going to court ($f + ct$) adds up to 5,000. Assuming the upper bound for p, namely 0.1%, going on trial will provide a positive expected gain with a delay of 50 days ($d^* = \frac{f+ct}{Ap}$). Such a gain will increase until the 100^{th} day of delay (d^{max}), when the penalty amount would have reached its maximum, the decimal part of the contract value ($Apd^{max} = A0.1$).

On the other hand, the right panel of Figure 2 describes how a firm's suing incentives vary with court inefficiency: in a more inefficient court, the variable cost of suing will be greater due to the increased length of judicial procedures ($t' > t$). In such a way, a firm will require a longer delay to find it profitable to go to trial ($d'^* > d^*$), and for smaller delays the lengthy procedures will make going to trial unattractive.

A different channel might be also at play if firms display heterogeneous marginal costs of suing. In this case, a firm with a lower marginal cost of suing would compete more aggressively in bidding for public procurement contracts in jurisdictions characterized by slower courts. At the bidding stage, such a firm would indeed anticipate its advantageous position in the case of future delays, thus deciding to offer a lower price. The slower the court, the more this selection effect would be pronounced. Hence, both selection and moral hazard might be at play and induce a non-linear association between delay and court inefficiency. The combination of our identification strategy and detailed data allows us to separately evaluate these two forces.

[12]There are many legal technicalities we are glossing over. For instance, following this accommodating the strategic introduction of some out-of-scope modifications in the contract to justify why a longer execution is not a delay to which penalties are applicable. Otherwise, the public administrator acting on behalf of a CA which decides not to impose any penalty on a belated firm would expose himself to a serious threat of being sued for damages. They can indeed be prosecuted by the Court of Auditors for not having applied the penalty to which a CA was entitled.

Figure 2: Incentives and court efficiency



3 Identification and econometric specification

3.1 Data

We consider a large dataset that merges data on average length of first instance civil proceedings at the court and year level, and data on procurement performance at the contract level. The average length of civil proceedings is commonly used as a proxy for the strength of contract enforcement, e.g., in the World Bank "Doing Business" ranking (World Bank, 2020), as well as in the economic literature (Fabbri, 2010; Giacomelli and Menon, 2017). Such data are provided by ISTAT, the Italian national statistical office, and the Italian Ministry of Justice. Public procurement data come from a large database maintained by the Italian Anti-corruption Authority (ANAC), which comprises information on all public work contracts awarded between 2002 and 2012. These procurement auctions include the universe of public works auctioned off by Italian public contracting authorities (CA) over the sampling period. Most jobs involve the maintenance or construction of roads and (non-military) buildings, or more specialized works on public infrastructure. To focus on an homogeneous sample of CA, we restrict the attention only to contracts that were issued by municipalities. Following Decarolis (2014), time delay is measured as the ratio between the length of delay in delivering the contract and the length of the contract, both measured in number of days.[13]

Descriptive statistics for the main variables used in the analysis are reported in the web appendix. The statistics are reported separately for both the full sample (roughly 45,000 contracts) and the sample of municipalities located along a jurisdiction border (roughly 24,000 contracts). Our border-discontinuity design described below requires restricting the analysis to the latter sample. The two samples, however, are close along most of the relevant observable characteristics. As an example, the average contract value in the full sample is 402,00 €(SD 559,000), and 409,000 €(SD 590,000) in the border sample; in the full sample, the average delay amounts to 87.7 percent of the duration originally stated in the contract (SD 102 percent), while it is 86.2 percent (SD 100 percent) in the border sample.

3.2 Empirical Strategy: spatial discontinuity design

Our theoretical considerations suggest that a firm's behavior may change depending on the perceived court efficiency, and the related contract enforcement level. When delay and expected penalties are

[13] As mentioned earlier, regressions involving cost overruns (i.e. the ratio between the extra cost and the contract value) are reported in the web appendix.

limited, a more inefficient court (i.e. where civil proceedings are particularly long) discourages suing, making the firm's threat of going to trial against the CA less and less credible. On the other hand, for higher values of delays, a slower and inefficient court increases the benefits of suing a CA. The expected penalties are indeed greater than the expected total cost connected to civil proceedings, increasing the likelihood of firms opting for challenging the CA before a court.

If we were to assume that the effect of the court inefficiency on procurement performance does not change at different points of the procurement performance distribution, we would have considered the following empirical specification:

$$Y_{ipt} = L_{kt}\beta + \varepsilon_{ipt} \tag{1}$$

where subscripts i, p, t denote contract, place (i.e., the municipality issuing the contract), and year, respectively. Y_{ipt} is the measure of procurement performance of interest (i.e., delays) while L_{kt} corresponds to the average length of judicial civil procedures in court k at time t. However, a key insight of our contribution is the emphasis on the heterogeneous effects that court efficiency might have at different points of the delay distribution. For this reason, the empirical approach that we follow is based on quantile regressions. In particular, we model the τ^{th} quantile of the distribution of the procurement outcome Y as a conditional function of L as follows:

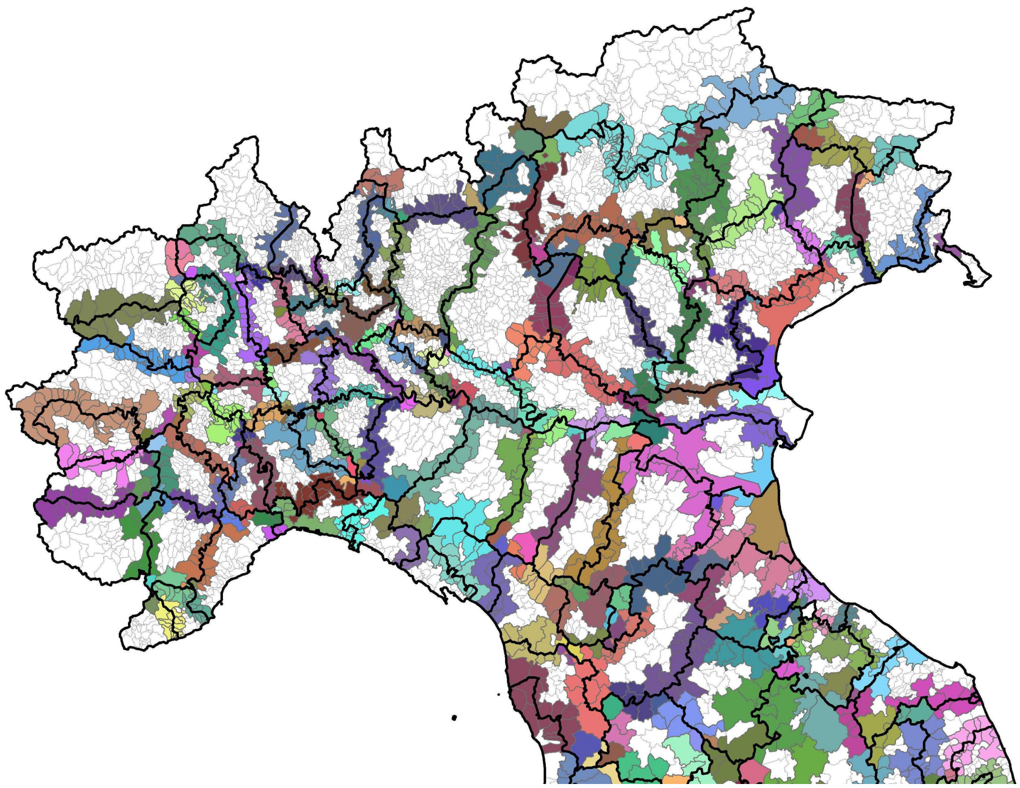
$$Q_{y_{ipt}}(\tau \,| L_{kt}) = L'_{kt}\beta(\tau) \tag{2}$$

where the coefficient of the main regressor, $\beta(\tau)$, is conditional upon the quantile of interest τ.

However, the model is likely to deliver inconsistent estimates due to a systematic omitted variable bias. Delays in contract execution are likely driven by multiple factors, some of which might well be also associated with court efficiency. To address this issue, we take advantage of a spatial discontinuity design (Duranton et al., 2011; Black, 1999; Holmes, 1998), which was first applied to the case of civil justice inefficiency in Italy by Giacomelli and Menon (2017). The empirical strategy consists of restricting the sample to observations that are located near a spatial discontinuity that affects only the variable of interest (i.e., judicial inefficiency), and in mean-differentiating all the variables within the group of observations that share the same discontinuity. The latter is operationalized by augmenting the model in equation (2) with a set of border dummy variables δ_b, which identify municipalities located at either side of a given jurisdiction border. For the sample selection step, instead, we follow Giacomelli and Menon (2017) by identifying groups of municipalities located on either side of a jurisdiction border. In our data, the border groups comprise on average 12.5 municipalities each, and we will use them to implement the spatial discontinuity design. Figure 3 provides a graphic representation limited to the Italian northern regions for the sake of readability: among all of the municipalities (thin gray lines), we select only those along jurisdiction borders (darker bold lines) and each border defines a border group.

Two conditions are required at the discontinuity (i.e. jurisdiction borders) to produce consistent estimates: *i)* court inefficiency displays a discrete variation; and *ii)* all other unobserved factors that may affect the outcome vary smoothly. The first condition is evident in Figure 1 in the Appendix: large discontinuities are present for most border groups. The second assumption cannot be validated empirically. However, we recall that the peculiarity of our institutional setting lies in the fact that jurisdiction borders do not match any other type of administrative boundary. Giacomelli and Menon (2017) provide supporting evidence on the exogenous nature of jurisdiction borders by showing that a series of variables unlikely to be affected by the length of civil proceedings (ranging from the crime rate to proxies for social capital) have indeed the same value at both sides of the discontinuity, and display the same distribution in efficient and inefficient courts. Although their analysis reveals causal effects of court efficiency on the size and growth of firms incorporated in different jurisdictions, our analysis will control for firm heterogeneity by including fixed effects for the identity of the firm performing the contract. Our main estimates thus exploit within-firm variation in court efficiency resulting from firms winning contracts across municipalities located on opposite sides of jurisdiction borders.[14]

Figure 3: Border groups



Note: the darker bold lines in the map correspond to jurisdiction borders, while the thinner gray lines correspond to municipality borders. The map shows the Italian northern regions. Different colours correspond to different border groups.

3.3 Quantile estimator with multi-way fixed effects

The presence of border dummies entails the introduction of a large number of fixed effects. This, however, implies that standard quantile methods may provide inconsistent estimates of the main regressor due to its inflated variability. Koenker (2004) suggests a solution to this problem by employing a penalized fixed effects estimator for longitudinal data, with LASSO penalty to shrink fixed effects to zero. Hence, similar to standard quantile regression models, the effects of covariates are permitted to be dependent upon the quantile of interest. However, individual effects are treated as pure location shifts and are not quantile dependent.[15]

Therefore, we estimate the following quantile function:

$$Q_{y_{itb}}(\tau \,|L_{kt}, \delta_b, \gamma_r, \vartheta_t, \zeta_i) \;= \delta_b + L'_{kt}\beta(\tau) + \gamma_r + \vartheta_t + \zeta_i + \phi_f \tag{3}$$

[14]Firm fixed effects thus account for border-spillover effects associated with agents sorting themselves into the geography as response to change of the variable under analysis (i.e. the independent variable). In our case, this would occur if firms decide to bid for contracts in municipalities associated with courts that are systematically more or less efficient.

[15]See also II.1 for a brief summary of the estimation approach used here

where the border dummies δ_b are instrumental to exploit judicial spatial discontinuity for identification. The model is further augmented with a set of year (ϑ_t), region (γ_r) and contract value (ζ_c) fixed effects, which help to control, respectively, for unobserved heterogeneity over time and across regions, as well as for contract characteristics such as the amount awarded. As mentioned above, the inclusion of firm fixed effects, ϕ_f, is particularly important as it allows us to control for any unobserved, firm-specific heterogeneity which could be particularly salient as firms might self-select into contracts, in part based on their different attitudes toward court efficiency.[16]

4 Results

In this section, we present the quantile estimates through a series of plots. Tables with the corresponding estimates and additional statistics are reported in the web appendix. In Figure 4, the top three panels present three model specifications not including firm fixed effects, while the bottom panels augment these same three models with firm fixed effects. Each panel specifies the exact set of fixed effects that are included. From left to right, these are: border, region and year (left panels); border and region-year (middle panels); border-year and region-year (right panel).

All models deliver qualitatively similar results. Court inefficiency significantly impacts the tails of the delay distribution: an increase from the 25^{th} to 75^{th} percentile in the average length of trials results in a decrease of 6.6 percent in time delays at the 20^{th} percentile of its distribution (coefficient -0.151 and significant at 1 percent).[17] Estimates also indicate that an equal increase in the length of judicial procedures leads to an increase in time delays of about 4 percent at the 80^{th} percentile of delay distribution (coefficient 0.092, significant at 1 percent).[18]

In line with the basic theoretical framework described earlier, the estimates suggest that an increase in court inefficiency leads to a diverging effect on procurement delays. For smaller delays, the relatively low level of penalties makes a firm less willing to sue a CA, thus marginally reducing the time delay to deliver the contract. In case of relatively larger delays (e.g. a situation in which the delay virtually overcomes the cut-off level d^*), the expected gains for a firm to sue a CA grow monotonically with the size of delays. As court inefficiency grows, both the direct and strategic effect come in to play and a firm has an incentive to further delay the delivery of the contract.

This heterogeneous effect on the delay distribution is confirmed by controlling for unobserved regional trends over time (center-upper panel) and for border trends and regional trends over time (right-upper panel). In particular, border-year effects represent a useful refinement of our empirical strategy, allowing us to compare contracts that are performed in the same period and issued in municipalities at the two sides of a border. The inclusion of such dummies does not produce a sizeable effect on the enforcement coefficients along the whole distribution of time delay. In particular, the inclusion of region-year dummies does not alter the magnitude and significance of coefficients (center-upper panel).

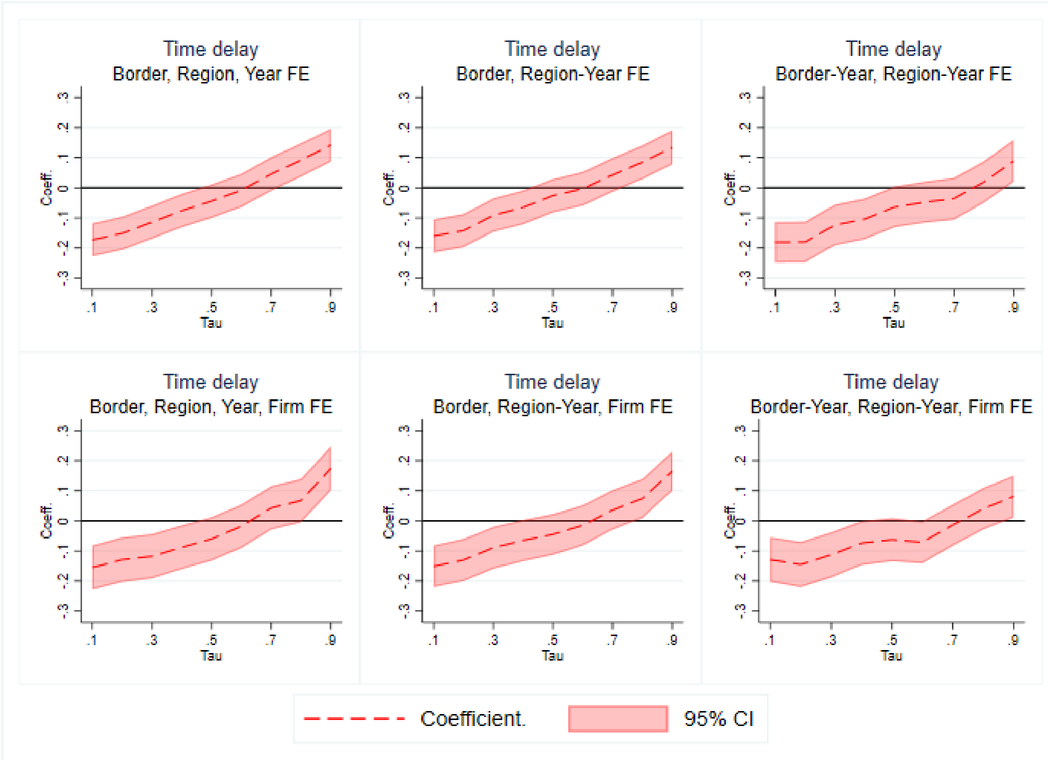
Adding also border-year dummies (right-upper panel) slightly reduces the size of coefficients related to top quantiles (the only significant coefficient among top quantiles is the one attached to the 90^{th} quantile, which amounts to 0.089, while the coefficients related to the first two specifications are respectively 0.142 and 0.134) of the distribution, while significantly lowering coefficients at the bottom (the coefficient associated with the 40^{th} percentile reduces to -0.104).

[16] For inference, we estimate robust standard errors using the generalized bootstrap method proposed by Bose and Chatterjee (2003). This procedure requires us to define the factor variable that identifies the data panel structure, and jurisdiction borders b play this role in our analysis. To each border, we assign a weight randomly drawn from a unit-exponential distribution. Note that weights are not assigned to observations, implying that different contracts awarded in the same border will be assigned the same random-weight.

[17] As an example, for a 365-days contract, this effect amounts to a reduction in delays of about 24.1 days.

[18] Again, taking into account a 365-days contract, this effect corresponds to an increase in time delays of about 14.7 days.

Figure 4: Baseline estimates and Firm Fixed Effect Quantile Regressions



Note: The figure displays quantile estimates and 95 percent confidence intervals (red area) based on equation 3. Upper panels report estimates without firm fixed effects, while lower panels report estimates augmented with firm fixed effects. Dependent variable: time delay is a contract level measure, calculated as the difference between the actual and the contractual time as a percentage of the contractual time. Values below -0.5 and above 5 were dropped. Negative values correspond to cases where the contract has been delivered in advance of contractual terms. Independent variable: Length of judicial procedures varies at court-year level, measured as 1 plus the natural logarithm of the sum of pending cases at the beginning of the year t and the end of the year t, over the sum of the new cases filed during the year and the cases that ended with a judicial decision or were withdrawn by the parties during the year.

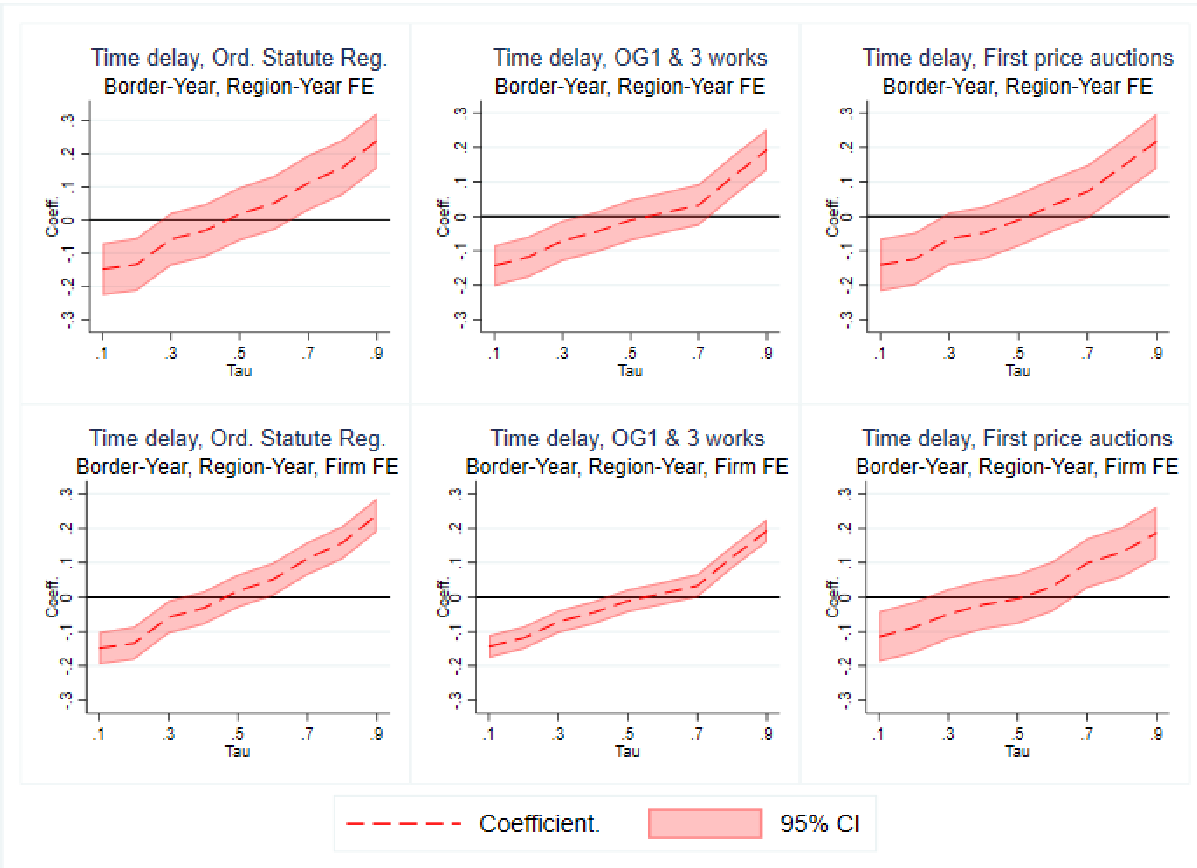
The comparison of the estimates reported in the top and lower panels of Figure 4 suggest that the inclusion of firm fixed effects has a minor impact on the estimates. With firm fixed effects included, the size of coefficients for the top 90^{th} percentile is either higher (left and middle panels) or indistinguishable (right panels) from the case without these fixed effects.

4.1 Robustness: sample splits

To further assess the robustness of our estimates, we present next the results obtained by restricting the sample in different ways. Figure 5 reports the quantile estimates for three subsamples and, as before, separating the models between those with and without firm fixed effects. The first sample, corresponding to the plots on the left, includes only "Ordinary Statute" regions. In fact, regional laws sometimes deviate from the national regulation in complex ways, as is the case for the threshold defining the admissible cost overruns. "Ordinary Statute" regions, however, tend to adhere more closely to the national law than the remaining regions.

The second check limits the observations to contracts involving the construction or maintenance of either roads or (non-military) buildings (namely, OG1 and OG3 contracts). These two are the most frequent job types in the data and, by restricting the attention to them, we ensure greater homogeneity of the resulting procurement procedures. The third sample focuses on contracts awarded through first price auctions, where the winner is the firm offering the highest discount over the reserve price announced in the call for tenders. Decarolis (2014) shows that the first price criterion (when compared to the other awarding criteria employed in Italy for public works) significantly impacts both price and performance: the winning price is lower, but ex-post both cost overruns and time delays are higher.

Figure 5: Sample splits



Notes: The figure plots estimated coefficients (dotted line) and 95% confidence intervals (red area). Left panel refers to a sample restricted only "Ordinary Statute" regions. Center panel considers only contracts on civil/industrial building repair or road, highways and bridge construction/maintenance works (OG1 or OG3). Right panel reports results for a sample restricted to only first price auctions. Upper panels report baseline estimates, while lower panels report baseline estimates augmented with firm fixed effects. Dependent variable: time delay is a contract level measure, calculated as the difference between the actual and the contractual time as a percentage of the contractual time. Values below -0.5 and above 5 were dropped. Negative values correspond to cases in which the contract has been delivered in advance to contractual terms. Independent variable: Length of judicial procedures varies at court-year level, measured as 1 plus the natural logarithm of the sum of pending cases at the beginning of the year t and the end of the year t, over the sum of the new cases filed during the year and the cases that ended with a judicial decision or were withdrawn by the parties during the year.

The qualitative results from our baseline estimates are confirmed across all the subsamples. Furthermore, coefficient magnitudes in all the three cases are higher than the corresponding baseline ones. For instance, the coefficient associated with the top 80^{th} percentile for the sample restricted to only

"Ordinary Statute" regions is 0.158, which implies that an increase from the 25^{th} to 75^{th} percentile in the average length of trials leads to an increase by 6.9% in delay. At the 20^{th} percentile, an increase in court inefficiency of a similar amount corresponds to a reduction in delay of about 5.9%. Results for OG1 and OG3 contracts also display qualitatively similar results, with coefficients for the top 80^{th} percentile ranging from 0.116 and 0.179. Higher coefficients are also obtained at lower percentiles, although the level of significance appears to be slightly lower than baseline estimates across all specifications. Regressions for first price auctions display significantly higher coefficients for top quantiles, although the inclusion of border-year fixed effects produces a moderate upward shift in coefficient levels at bottom quantiles, which results in lower significance of estimated coefficients at the lower side of the delay distribution.

5 Conclusions

We test the effect of weak contract enforcement on ex-post contract performance in the context of public work procurement. When courts are slower in settling cases, we find that small delays in contract completion decline due to court inefficiency, while large delays are magnified by it. This non-linear effect is interpreted within a framework where private contractors strategically decide how much to delay contract completion depending on their perception of the duration of court cases and hence the likelihood that public buyers will enforce penalties if a delay occurs.

Uncovering this heterogeneous effect is the main contribution of this study. By presenting it, we complement the emerging empirical literature on the causal impacts of judicial systems (Chemin, 2020) by offering an important insight for policy evaluation: average effects which are small in magnitude and imprecisely estimated might result from a non-linear relationship between court inefficiency and a strategic agent's demand for justice. Hence, ongoing policy evaluation of the judicial system in both developed and developing countries should account for such an effect to implement effective reforms.

Besides, we complement this literature by proposing the estimation of quantile regressions within a border-discontinuity identification strategy. This approach has the potential of being broadly applicable as boundaries across jurisdictions exist in nearly all countries. However, this spatial identification strategy requires assumptions to be satisfied and granular data to be implemented.

Based on our findings, an interesting question relates to which justice reforms would be desirable, and the sheer size of public procurement (Bosio et al., 2020) makes this a question of primary importance. Delays in the construction of public infrastructures hurt citizens and businesses relying on this infrastructure and, in aggregate, might constitute a significant impediment to economic growth. Inefficient courts, by worsening delays for those contracts experiencing larger delays, can cause significant social welfare losses. Therefore, without aiming to answer this complex problem, we conclude by mentioning the next two potential remedies that would be interesting to explore in future research.

We think of these two remedies as addressing the two channels through which court inefficiency affects delays, that is through what we defined as *direct* and *strategic* effects. Speeding up court cases involving public contracts would address the *direct* effect. A range of solutions exists to achieve this goal. One straightforward solution would be to create a fast-track within courts for cases involving procurement contracts. This could prioritize those cases involving infrastructure where delays are more substantial in terms of their length or economic impact. A second solution could be to assign to specialized courts the jurisdiction over disputes involving public procurement. This would likely be highly effective, given the similarities across disputes over penalties for delays applied by public contractors. A more proactive solution could entail enhancing alternative dispute resolution (ADR) systems. In the US, the major reform of the Federal Acquisition Regulation under the Clinton presidency in the 1990s saw numerous legislative enactments and executive orders encouraging the use of ADR techniques to resolve disputes involving the Government, including procurement-related cases (Schooner, 2001).

The second set of remedies involves curbing the *strategic* exploitation of court inefficiency by private suppliers. There is a long and well-established literature in public procurement dating back to at least Spulber (1990) arguing why this is a daunting task: the intrinsic uncertainty existing in contract procurement leads to the risk of selecting those contractors most likely to seek ex post renegotiations. This risk is especially pronounced in public procurement due to the mandatory use of open competition auctions to select suppliers, as competition fosters adverse selection and moral hazard. Solutions offered in the literature to ease this problem range from the use of performance bonding (Calveras et al., 2004; Ganuza, 2007), to the introduction of vendor rating systems (Decarolis et al., 2016), to changes in the auction format (Burguet et al., 2012). The complexity of the problem entails the impossibility of identifying an always optimal solution: for instance, performance bonding is known to substantially increase a supplier's price bids, while a vendor rating system only makes sense with frequently procured contracts. Nevertheless, it would be important for future research to better link the analysis of these procurement reforms to their interactions with the issue of the efficiency of the court systems as they jointly contribute to the definition of procurement outcomes.

References

Aberra, A. and Chemin, M. (2017). Does legal representation increase investment? evidence from a field experiment in kenya. Technical report, working paper.

Acemoglu, D. and Johnson, S. (2005). Unbundling institutions. *Journal of political Economy*, 113(5):949–995.

Acemoglu, D. and Robinson, J. A. (2012). *Why nations fail: The origins of power, prosperity, and poverty.* Crown Books.

Bajari, P., Houghton, S., and Tadelis, S. (2014). Bidding for incomplete contracts: An empirical analysis of adaptation costs. *American Economic Review*, 104(4):1288–1319.

Bartelsman, E., Haltiwanger, J., and Scarpetta, S. (2013). Cross-country differences in productivity: The role of allocation and selection. *American Economic Review*, 103(1):305–334.

Berkowitz, D., Moenius, J., and Pistor, K. (2006). Trade, law, and product complexity. *Review of Economics and Statistics*, 88(2):363 – 373.

Bianco, M., Giacomelli, S., Giorgiantonio, C., Palumbo, G., and Szego, B. (2007). La durata (eccessiva) dei procedimenti civili in italia: offerta, domanda o rito? *Rivista di Politica Economica*, 47(9-10):3 – 53.

Black, S. E. (1999). Do better schools matter? Parental valuation of elementary education. *Quarterly Journal of Economics*, 114(2):577 – 599.

Bose, A. and Chatterjee, S. (2003). Generalized bootstrap for estimators of minimizers of convex functions. *Journal of Statistical Planning and Inference*, 117(2):225 – 239.

Bosio, E., Djankov, S., Glaeser, E. L., and Shleifer, A. (2020). Public procurement in law and practice. Working Paper 27188, National Bureau of Economic Research.

Burguet, R., Ganuza, J., and Hauk, E. (2012). Limited liability and mechanism design in procurement. *Games and Economic Behavior*, 76(1):15 – 25.

Calveras, A., Ganuza, J., and Hauk, E. (2004). Wild bids. gambling for resurrection in procurement contracts. *Journal of Regulatory Economics*, 26:41–68.

Chemin, M. (2009a). Do judiciaries matter for development? evidence from india. *Journal of Comparative Economics*, 37(2):230–250.

Chemin, M. (2009b). The impact of the judiciary on entrepreneurship: Evaluation of pakistan's âaccess to justice programmeâ. *Journal of Public Economics*, 93(1-2):114–125.

Chemin, M. (2012). Does court speed shape economic activity? evidence from a court reform in india. *The Journal of Law, Economics, & Organization*, 28(3):460–485.

Chemin, M. (2020). Judicial Efficiency and Firm Productivity: Evidence from a World Database of Judicial Reforms. *The Review of Economics and Statistics*, 102(1):49–64.

Clark, D. and Merryman, J. (1976). Measuring the duration of judicial and administrative proceedings. *Michigan Law Review*, 75:89 – 99.

Coviello, D., Moretti, L., Spagnolo, G., and Valbonesi, P. (2018). Court efficiency and procurement performance. *The Scandinavian Journal of Economics*, 120(3):826–858.

De Silva, D. G., Dunne, T., Kosmopoulou, G., and Lamarche, C. (2017). Contract Modification and Bidding in Highway Procurement Auctions. *mimeo*.

Decarolis, F. (2014). Awarding price, contract performance, and bids screening: Evidence from procurement auctions. *American Economic Journal: Applied Economics*, 6(1):108–32.

Decarolis, F. and Giorgiantonio, C. (2005). Local public procurement regulations: The case of italy. *International Review of Law and Economics*, 43:209 – 226.

Decarolis, F. and Palumbo, G. (2015). Renegotiation of public contracts: An empirical analysis. *Economics Letters*, 132:77–81.

Decarolis, F., Spagnolo, G., and Pacini, R. (2016). Past performance and procurement outcomes. Working Paper 22814, National Bureau of Economic Research.

Disney, R., Haskel, J., and Heden, Y. (2003). Restructuring and productivity growth in UK manufacturing. *Economic Journal*, 113(489):666–694.

Djankov, S., Hart, O., McLiesh, C., and Shleifer, A. (2008). Debt enforcement around the world. *Journal of Political Economy*, 116(6):1105 – 1149.

Djankov, S., La Porta, R., Lopez-de Silanes, F., and Shleifer, A. (2003). Courts. *The Quarterly Journal of Economics*, 118(2):453–517.

Duranton, G., Gobillon, L., and Overman, H. G. (2011). Assessing the effects of local taxation using microgeographic data. *Economic Journal*, 121(555):1017 – 1046.

Fabbri, D. (2010). Law enforcement and firm financing: Theory and evidence. *Journal of the European Economic Association*, 8(4):776 – 816.

Ganuza, J.-J. (2007). Competition and cost overruns in procurement. *The Journal of Industrial Economics*, 55(4):633–660.

García-Santana, M. and Ramos, R. (2015). Distortions and the size distribution of plants: evidence from cross-country data. *SERIEs*, 6(3):279–312.

Giacomelli, S. and Menon, C. (2017). Does weak contract enforcement affect firm size? evidence from the neighbourâs court. *Journal of Economic Geography*, 17(6):1251–1282.

Grenier, J. and Pattanayak, C. W. (2011). Randomized evaluation in legal assistance: What difference does representation (offer and actual use) make. *Yale LJ*, 121:2118.

Guasch, J. L., Laffont, J.-J., and Straub, S. (2008). Renegotiation of concession contracts in latin america: Evidence from the water and transport sectors. *International Journal of Industrial Organization*, 22(7):1267–1294.

Holmes, T. J. (1998). The effect of state policies on the location of manufacturing: Evidence from state borders. *Journal of Political Economy*, 106(4):667 – 705.

Johnson, S., McMillan, J., and Woodruff, C. (2002). Courts and relational contracts. *Journal of Law, Economics, and Organization*, 18(1):221 – 277.

Koenker, R. (2004). Quantile regression for longitudinal data. *Journal of Multivariate Analysis*, 91(1):74–89.

Koenker, R. W. and Bassett, G. (1978). Regression quantiles. *Econometrica*, 46(1):33–50.

Kondylis, F. and Stein, M. (2018). *The speed of justice*. The World Bank.

Lewis, G. and Bajari, P. (2011). Procurement with Time Incentives: Theory and Evidence. *Quarterly Journal of Economics*, 126(3):1173–1211.

Lichand, G. and Soares, R. R. (2014). Access to justice and entrepreneurship: Evidence from brazilâs special civil tribunals. *The Journal of Law and Economics*, 57(2):459–499.

Lilienfeld-Toal, U. v., Mookherjee, D., and Visaria, S. (2012). The distributive impact of reforms in credit enforcement: Evidence from indian debt recovery tribunals. *Econometrica*, 80(2):497–558.

North, D. (1990). Institutions and their consequences for economic performance. *The limits of rationality*, pages 383–401.

Palumbo, G., Giupponi, G., Nunziata, L., and Sanguinetti, J. S. M. (2013). The economics of civil justice: new cross-country data and empirics. Economics department working paper 1060, OECD.

Ponticelli, J. and Alencar, L. S. (2016). Court enforcement, bank loans, and firm investment: evidence from a bankruptcy reform in brazil. *The Quarterly Journal of Economics*, 131(3):1365–1413.

Qian, J. and Strahan, P. E. (2007). How laws and institutions shape financial contracts: The case of bank loans. *Journal of Finance*, 62(6):2803 – 2834.

Ryan, N. (2020). Contract Enforcement and Productive Efficiency: Evidence from the Bidding and Renegotiation of Power Contracts in India. *Econometrica*, 88(2):383–424.

Sandefur, J. and Siddiqi, B. (2013). Delivering justice to the poor: theory and experimental evidence from liberia. In *World Bank Workshop on African Political Economy, Washington, DC, May*, volume 20.

Schooner, S. L. (2001). Limited liability and mechanism design in procurement. *American University Law Review*, 50(3).

Spulber, D. F. (1990). Auctions and contract enforcement. *Journal of Law, Economics and Organization*, 6(2):325–44.

World Bank (2020). *Doing Business 2020*. World Bank Group, Washington, D.C.

Online Appendix

I Data construction

I.1 Data on procurement

We use data on the universe of public procurements for works occurring at the municipal level in Italy and maintained by the Anti Corruption Authority (ANAC). The sample chosen includes contracts issued within the period 2002-12. The dataset includes information on the identity of the municipality that issues the contract along with the municipality fiscal code, identity of the awarding firm, starting date as well as awarding and delivery date. Contract level information includes the reserve price, auction format, type of works covered by the contract. The dataset covers the contract from its implementation period until its actual delivery date, thus measuring delays. Other variables include the contract value (i.e., the price at which the CA initially awarded the contract) and the final value, which might differ from the contract value in case of renegotiation between the firm and the CA.

The primary dependent variable is the delay in contract completion. This latter is the difference between the actual delivery date and the delivery date foreseen in the contract, calculated as a share of the contract length (the difference between the delivery date and starting date). This delay ratio is bounded within the interval [-0.5, 5] to remove possible outliers. It follows that the variable delay ratio's negative values represent contracts that firms delivered before the foreseen delivery time. In this appendix, we also analyze cost overruns as an alternative outcome. This variable represents the difference between the final value and the contract value, as a share of the contract value. In the analysis, the variable is left and right truncated within the values [-0.5,2.5] to drop possible outliers.

I.2 Data on court efficiency

Court efficiency is proxied by the average length of the first instance civil proceedings in each court (more detail in the paper). We use case flow data provided by the Italian Ministry of Justice to construct our primary independent variable (length of judicial proceedings), an estimate of the average lifetime of court proceedings (in days). For any court k:

$$L_t = \frac{P_t + P_{t+1}}{E_t + F_t} * 365 \tag{4}$$

where P_t and P_{t+1} reflect, respectively, pending cases registered in the court k at the beginning of the year t and year $t+1$, F_t the new cases filed during the year, and E_t the cases that ended with a judicial decision or were withdrawn by the parties during the year.[19]

I.3 Descriptive statistics

Descriptive statistics are presented below for both the full and border sample (i.e., the sample of municipalities located at a jurisdiction border, used in the quantile analysis). Despite the significant reduction in the number of observations, the border sample presents similar characteristics with respect to the full sample.

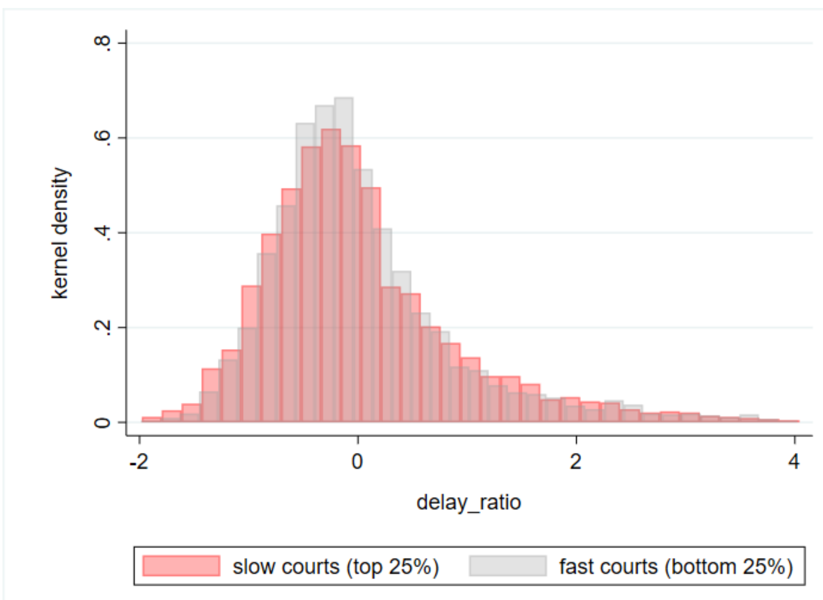
[19] A similar index was firstly developed by Clark and Merryman (1976) to estimate the average turnover of stock inventories, which is used especially in cross-country comparisons, to estimate the length of proceedings when actual data are not available. See also Palumbo et al. (2013).

Table A.1: Descriptive statistics

	Full sample			Border sample		
	Mean	SD	N	Mean	SD	N
Reserve price	473.0	630.2	44,492	482.3	640.8	24,147
N. of bidders	28.95	40.83	44,580	30.57	42.55	24,169
Contract value	402.4	559.0	41,016	409.2	590.4	22,208
Final value	414.5	589.3	33,182	417.6	543.6	17,929
Length jud. procedures	918.0	324.1	36,458	898.2	312.6	20,126
Time delay	0.877	1.018	45,028	0.862	1.012	24,421
Cost overrun	0.0930	0.183	30,376	0.0922	0.173	16,398
Winning discount	0.157	0.0960	40,224	0.161	0.0995	21,766
Contract length	222.1	147.9	45,028	226.6	149.7	24,421

Notes: Reserve price is the price set by the CA (thousands €); N. of bidders is the number of suppliers participating in the auction; Contract value is the amount of the contract as it is awarded to the winning firm (thousands €); Final value is the total amount paid by the CA to the supplier (thousands €); Length jud. procedures is the average lifetime of court proceedings (in days); Time delay is the difference between the actual delivery date and the delivery date foreseen in the contract as a percentage of the foreseen delivery date; Cost overrun is the difference between the final amount and the contract value as a percentage of the contract value; Winning discount is the difference between the reserve price and the contract value, as a percentage of the reserve price; Contract length is the delivery time to complete the job (in days).

Figure A.1: Time delay: distribution across the border



Notes: the figure in grey shows the residual distribution when the average length of judicial procedures is below or equal to the 25^{th} percentile level. The red figure plots the residual distribution when the average length of judicial procedures is above or equal to the 75^{th} percentile level.

To conclude this section about the data, we present a graphical test to verify the effect of court inefficiency on procurement delays. The figure plots the residuals of an auxiliary regression of time delay on a set of fixed effects, which control for the size of the contract in value terms (at 500,000 euro levels), the average year between the initiation of the contract and the due date, the region of the CA, and the border dummies. Standard errors are two-way clustered at the province and border level. The figure shows that the distribution of time delay of contracts in municipalities located in the bottom 25% slowest courts is characterized by fatter tails than the corresponding distribution in municipalities located in the top 25% fastest courts.

II Estimation approach and main estimates

This section provides some additional details on our estimation approach. It reports the point estimates and confidence intervals for all the quantile regressions that we presented in a graphical form in the main text.

II.1 An extension of Koenker (2004) to quantile models with multiple fixed effects

Our estimation procedure extends the penalized quantile regression framework for longitudinal data originally introduced by Koenker (2004) to a more general case with an arbitrarily large set of fixed effects ($n > 1$).

Consider for example a simplified version of our baseline quantile function (CQF) conditional on τ_k for $q = 1, ..., Q$:

$$Q_{Y_{ipt}}(\tau \,|L_{kt}) = L'_{kt}\beta(\tau) + \delta_b(\tau) + \theta_t(\tau)$$

where, as in equation 3, Y_{ipt} is the response vector of the variable time delay identifying the contract i issued by the municipality $p(k)$ within the county k at time t. L_{kt} is the main covariate, measuring the length of judicial procedures (our proxy for the strength of contract enforcement) and varying at the court k-time t level. The element δ_b defines border fixed effects while θ_t captures unobserved time heterogeneity.

We recall that in quantile settings with longitudinal data, each coefficient associated with a fixed effect captures the distributional shift of that unobserved heterogeneity on the outcome variable for each individual. Ideally, if the number of contracts for each cell defined by *border-time* were to be large, it would be possible to recover the distributional shifts $\delta_b(\tau)$ and $\theta_t(\tau)$ for each individual. However, this would require the existence of sufficiently large variation at the individual level. When the number of observations for each individual is relatively modest, estimating a τ-dependent distributional individual effects is unrealistic and not informative.

Building upon Koenker (2004), we thus model our quantile function by capturing border and time-specific heterogeneity with non-quantile dependent coefficients, where only parameters associated with L_{kt} are permitted to be conditional on quantiles τ:

$$Q_{Y_{ipt}}(\tau \,|L_{kt}) = L'_{kt}\beta(\tau) + \delta_b + \theta_t$$

The problem to be solved is as follows:

$$(\hat{\beta}_\tau, \hat{\delta}_b, \hat{\theta}_t) = \arg\min_{\beta_\tau, \delta_b, \theta_t} \sum_q \sum_i \sum_p \sum_t [w_q \rho_{\tau_q}(Y_{ipt} - L'_{kt}\beta(\tau_q) - \delta_b - \theta_t)] \tag{5}$$

The estimator thus minimizes a weighted sum of Q ordinary quantile regression objective functions with τ_q-specific weights. The component $\rho_{\tau_q}(u)$ is a piecewise linear quantile check function of the absolute error term (in the spirit of Koenker and Bassett (1978)), which weights positive and negative terms asymmetrically. The larger the value of τ_q, the more over-prediction ($u > 0$) are penalized compared to under-prediction ($u < 0$):

$$\rho_\tau(u) = u(\tau - \mathbf{1}(u < 0))$$

The term w_q is the weight assigned to the influence of quantile τ_q on the individual fixed effects parameters, δ_b and θ_t. Thus, the estimation is the solution of a minimization problem of a weighted sum of q objective functions, where the vectors of parameters $\hat{\beta_\tau}, \hat{\delta_b}, \hat{\theta_t}$ are assumed to be independent of τ and can be interpreted as pure location shift on the conditional quantile response (i.e., individual fixed effects are common to all quantiles).

In applications where the incidental parameter problem is an issue (e.g., the number of fixed effects concerning the number of observations per group is high), the shrinkage of the individual fixed effects to common values through ℓ_1 penalty might help to control the introduced variability due to the parameters δ_b and θ_t. [20] Equation 6 is thus transformed as follows:

$$(\hat{\beta_\tau}, \hat{\delta_b}, \hat{\theta_t}) = \arg \min_{\beta_\tau, \delta_b, \theta_t} \sum_q \sum_i \sum_p \sum_t [w_q \rho_{\tau_q}(Y_{ipt} - L'_{kt}\beta(\tau) - \delta_b - \theta_t)] + \lambda \sum_b |\delta_b| + \lambda \sum_t |\theta_t| \quad (6)$$

where the vectors of individual-specific fixed effects coefficients are penalized by the LASSO parameter λ.

II.2 Coefficient estimates and standard errors

We employ the R-package *RQPD* for obtaining estimates of the penalized fixed-effect model, as designed in equation 6. The estimation problem may appear difficult to solve as the number dimensions characterizing the model of interest is quite large. However, as argued by Koenker (2004), despite the high number of fixed effects, the problem in equation 6 can be successfully solved by taking advantage of the sparsity of the design matrix. In adopting *RQPD*, we set the LASSO parameter λ equal to 1 and define the Q-vector of weights (w_{τ_q}) as $1/Q$.

We also denote border groups b as our main individual-unit of reference in the command *RQPD*. We thus obtain bootstrapped standard errors in the spirit of Bose and Chatterjee (2003), which implies replicating our estimation R times by assigning unit-exponential weights to observations belonging to the same border group (b) rather than to each distinct observation, with $R = 200$. We then obtain Z-statistics as the ratio of the coefficients and the bootstrapped standard errors.

II.3 Details on the Main Estimates

- Table A.2: baselines estimates, with and without firm fixed effects, represented in Figure 4;
- Table A.3: estimates on the sample restricted to Ordinary Statute regions, with and without firm fixed effects. The graphical representation corresponds to the left panel of Figure 5;

[20] Koenker (2004) suggests the inclusion of a LASSO penalty ℓ_1, which offers statistical and computational advantages over more conventional Gaussian ℓ_2-penalties

- Table A.4: estimates on the sample restricted to road or building works (OG1 or OG3 procurement auctions), with and without firm fixed effects. The graphical representation corresponds to the middle panel of Figure 5;
- Table A.5: estimates on the sample restricted to first price auctions, with and without firm fixed effects. The graphical representation corresponds to the right panel of Figure 5;
- Table A.6: baseline estimates, with and without firm fixed effects, including province fixed effects in all specifications.

Table A.2: Time delay: baseline estimates, with and without firm fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
				Time delay (ratio)		
Tau 0.1	-0.173 ***	-0.160 ***	-0.181 ***	-0.155 ***	-0.151 ***	-0.129 ***
	(0.028)	(0.028)	(0.034)	(0.037)	(0.035)	(0.038)
Tau 0.2	-0.151 ***	-0.142 ***	-0.180 ***	-0.129 ***	-0.130 ***	-0.145 ***
	(0.028)	(0.028)	(0.034)	(0.038)	(0.036)	(0.038)
Tau 0.3	-0.114 ***	-0.091 ***	-0.124 ***	-0.118 ***	-0.089 **	-0.113 ***
	(0.028)	(0.028)	(0.034)	(0.038)	(0.036)	(0.038)
Tau 0.4	-0.076 ***	-0.065 **	-0.104 ***	-0.088 **	-0.065 *	-0.074 **
	(0.028)	(0.028)	(0.034)	(0.037)	(0.035)	(0.037)
Tau 0.5	-0.044	-0.026	-0.064 *	-0.060	-0.045	-0.063 *
	(0.028)	(0.028)	(0.035)	(0.037)	(0.035)	(0.036)
Tau 0.6	-0.008	-0.003	-0.048	-0.018	-0.015	-0.071 **
	(0.028)	(0.028)	(0.035)	(0.037)	(0.034)	(0.036)
Tau 0.7	0.046	0.043	-0.036	0.043	0.036	-0.014
	(0.028)	(0.029)	(0.035)	(0.037)	(0.033)	(0.035)
Tau 0.8	0.092 ***	0.085 ***	0.019	0.067 *	0.074 **	0.040
	(0.028)	(0.029)	(0.035)	(0.037)	(0.033)	(0.035)
Tau 0.9	0.142 ***	0.134 ***	0.089 **	0.174 ***	0.165 ***	0.081 **
	(0.028)	(0.029)	(0.035)	(0.037)	(0.033)	(0.035)
Observations	20,078	20,078	20,078	17,491	17,491	17,491
Fixed effects						
Border	Yes	No	No	Yes	No	No
Region	Yes	No	No	Yes	No	No
Year	Yes	No	No	Yes	No	No
Region-year	No	Yes	Yes	No	Yes	Yes
Border-year	No	No	Yes	No	No	Yes
Firm	No	No	No	Yes	Yes	Yes
Controls						
Contract value	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Quantile regressions with multi-way fixed effects. Dependent variable: time delay (ratio). Independent variable: Length judicial procedures. Each column denotes the inclusion of a different set of fixed-effects (see bottom panel). ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A.3: Time delay: Ordinary statute regions, with and without firm fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
				Time delay (ratio)		
Tau 0.1	-0.204 ***	-0.191 ***	-0.148 ***	-0.148 ***	-0.148 ***	-0.148 ***
	(0.035)	(0.037)	(0.040)	(0.027)	(0.026)	(0.024)
Tau 0.2	-0.185 ***	-0.161 ***	-0.134 ***	-0.134 ***	-0.134 ***	-0.134 ***
	(0.035)	(0.037)	(0.041)	(0.027)	(0.027)	(0.025)
Tau 0.3	-0.132 ***	-0.090 **	-0.058	-0.058 **	-0.058 **	-0.058 **
	(0.035)	(0.037)	(0.041)	(0.028)	(0.027)	(0.025)
Tau 0.4	-0.077 **	-0.066 *	-0.032	-0.032	-0.032	-0.032
	(0.036)	(0.037)	(0.041)	(0.028)	(0.027)	(0.025)
Tau 0.5	-0.027	0.007	0.018	0.018	0.018	0.018
	(0.036)	(0.037)	(0.041)	(0.028)	(0.027)	(0.025)
Tau 0.6	0.017	0.023	0.051	0.051 *	0.051 *	0.051 **
	(0.036)	(0.038)	(0.042)	(0.028)	(0.027)	(0.025)
Tau 0.7	0.1 ***	0.099 ***	0.112 ***	0.112 ***	0.112 ***	0.112 ***
	(0.036)	(0.038)	(0.042)	(0.028)	(0.027)	(0.025)
Tau 0.8	0.153 ***	0.183 ***	0.158 ***	0.158 ***	0.158 ***	0.158 ***
	(0.036)	(0.038)	(0.043)	(0.028)	(0.028)	(0.025)
Tau 0.9	0.273 ***	0.294 ***	0.239 ***	0.239 ***	0.239 ***	0.239 ***
	(0.036)	(0.038)	(0.043)	(0.028)	(0.028)	(0.025)
Observations	15,417	15,417	15,417	15,417	15,417	15,417
Fixed effects						
Border	Yes	No	No	Yes	No	No
Region	Yes	No	No	Yes	No	No
Year	Yes	No	No	Yes	No	No
Region-year	No	Yes	Yes	No	Yes	Yes
Border-year	No	No	Yes	No	No	Yes
Firm	No	No	No	Yes	Yes	Yes
Controls						
Contract value	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Quantile regressions with multi-way fixed effects, restricted to "Ordinary statute" regions sample. Dependent variable: time delay (ratio). Independent variable: Length judicial procedures. Each column denotes the inclusion of a different set of fixed-effects (see bottom panel). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.4: Time delay: road or building works, with and without firm fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
				Time delay (ratio)		
Tau 0.1	-0.167 ***	-0.156 ***	-0.143 ***	-0.143 ***	-0.143 ***	-0.143 ***
	(0.034)	(0.034)	(0.030)	(0.017)	(0.020)	(0.017)
Tau 0.2	-0.137 ***	-0.119 ***	-0.118 ***	-0.118 ***	-0.118 ***	-0.118 ***
	(0.034)	(0.034)	(0.030)	(0.018)	(0.020)	(0.017)
Tau 0.3	-0.096 ***	-0.076 **	-0.071 **	-0.071 ***	-0.071 ***	-0.071 ***
	(0.034)	(0.034)	(0.030)	(0.017)	(0.020)	(0.017)
Tau 0.4	-0.061 *	-0.048	-0.045	-0.045 **	-0.045 **	-0.045 ***
	(0.034)	(0.034)	(0.030)	(0.018)	(0.020)	(0.017)
Tau 0.5	-0.017	-0.002	-0.011	-0.011	-0.011	-0.011
	(0.034)	(0.034)	(0.031)	(0.017)	(0.020)	(0.017)
Tau 0.6	0.020	0.023	0.011	0.011	0.011	0.011
	(0.034)	(0.034)	(0.031)	(0.017)	(0.020)	(0.017)
Tau 0.7	0.078 **	0.092 ***	0.033	0.033 *	0.033 *	0.033 *
	(0.034)	(0.034)	(0.031)	(0.017)	(0.020)	(0.017)
Tau 0.8	0.176 ***	0.179 ***	0.116 ***	0.116 ***	0.116 ***	0.116 ***
	(0.034)	(0.034)	(0.031)	(0.017)	(0.020)	(0.017)
Tau 0.9	0.208 ***	0.247 ***	0.193 ***	0.193 ***	0.193 ***	0.193 ***
	(0.034)	(0.033)	(0.031)	(0.017)	(0.020)	(0.017)
Observations	15,417	15,417	15,417	15,417	15,417	15,417
Fixed effects						
Border	Yes	No	No	Yes	No	No
Region	Yes	No	No	Yes	No	No
Year	Yes	No	No	Yes	No	No
Region-year	No	Yes	Yes	No	Yes	Yes
Border-year	No	No	Yes	No	No	Yes
Firm	No	No	No	Yes	Yes	Yes
Controls						
Contract value	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Quantile regressions with multi-way fixed effects, restricted to road or building works (OG1 and OG3) sample. Dependent variable: time delay. Independent variable: Length judicial procedures. Each column denotes the inclusion of a different set of fixed-effects (see bottom panel). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.5: Time delay: First price auctions, with and without firm fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
				Time delay (ratio)		
Tau 0.1	-0.186 ***	-0.156 ***	-0.141 ***	-0.167 ***	-0.154 ***	-0.115 ***
	(0.034)	(0.035)	(0.039)	(0.041)	(0.041)	(0.037)
Tau 0.2	-0.175 ***	-0.137 ***	-0.124 ***	-0.152 ***	-0.115 ***	-0.088 **
	(0.034)	(0.035)	(0.039)	(0.041)	(0.041)	(0.038)
Tau 0.3	-0.140 ***	-0.095 ***	-0.065 *	-0.125 ***	-0.081 **	-0.049
	(0.034)	(0.035)	(0.039)	(0.040)	(0.041)	(0.037)
Tau 0.4	-0.091 ***	-0.053	-0.048	-0.090 **	-0.042	-0.022
	(0.034)	(0.035)	(0.039)	(0.040)	(0.041)	(0.037)
Tau 0.5	-0.043	-0.010	-0.011	-0.057	-0.014	-0.005
	(0.034)	(0.035)	(0.039)	(0.041)	(0.040)	(0.037)
Tau 0.6	0.000	0.025	0.033	-0.010	0.045	0.031
	(0.034)	(0.035)	(0.039)	(0.041)	(0.040)	(0.037)
Tau 0.7	0.059 *	0.079 **	0.072 *	0.071 *	0.094 **	0.099 ***
	(0.034)	(0.035)	(0.040)	(0.042)	(0.041)	(0.037)
Tau 0.8	0.143 ***	0.157 ***	0.144 ***	0.117 ***	0.121 ***	0.131 ***
	(0.034)	(0.035)	(0.040)	(0.042)	(0.041)	(0.037)
Tau 0.9	0.223 ***	0.260 ***	0.218 ***	0.225 ***	0.241 ***	0.187 ***
	(0.035)	(0.035)	(0.041)	(0.043)	(0.041)	(0.038)
Observations	14,914	14,914	14,914	12,905	12,905	12,905
Fixed effects						
Border	Yes	No	No	Yes	No	No
Region	Yes	No	No	Yes	No	No
Year	Yes	No	No	Yes	No	No
Region-year	No	Yes	Yes	No	Yes	Yes
Border-year	No	No	Yes	No	No	Yes
Firm	No	No	No	Yes	Yes	Yes
Controls						
Contract value	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Quantile regressions with multi-way fixed effects, restricted to first price auctions sample. Dependent variable: time delay. Independent variable: Length judicial procedures. Each column denotes the inclusion of a different set of fixed-effects (see bottom panel). ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table A.6: Time delay: province fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
				Time delay (ratio)		
Tau 0.1	-0.181 ***	-0.160 ***	-0.181 ***	-0.157 ***	-0.151 ***	-0.129 ***
	(0.029)	(0.028)	(0.034)	(0.039)	(0.035)	(0.038)
Tau 0.2	-0.165 ***	-0.142 ***	-0.180 ***	-0.136 ***	-0.130 ***	-0.145 ***
	(0.030)	(0.028)	(0.034)	(0.040)	(0.036)	(0.038)
Tau 0.3	-0.121 ***	-0.091 ***	-0.124 ***	-0.118 ***	-0.089 **	-0.113 ***
	(0.030)	(0.028)	(0.034)	(0.040)	(0.036)	(0.038)
Tau 0.4	-0.089 ***	-0.065 **	-0.104 ***	-0.095 **	-0.065 *	-0.074 **
	(0.030)	(0.028)	(0.034)	(0.039)	(0.035)	(0.037)
Tau 0.5	-0.049	-0.026	-0.064 *	-0.069 *	-0.045	-0.063 *
	(0.030)	(0.028)	(0.035)	(0.039)	(0.035)	(0.036)
Tau 0.6	-0.015	-0.003	-0.048	-0.030	-0.015	-0.071 **
	(0.030)	(0.028)	(0.035)	(0.039)	(0.034)	(0.036)
Tau 0.7	0.034	0.043	-0.036	0.035	0.036	-0.014
	(0.030)	(0.029)	(0.035)	(0.039)	(0.033)	(0.035)
Tau 0.8	0.081 ***	0.085 ***	0.019	0.066 *	0.074 **	0.040
	(0.030)	(0.029)	(0.035)	(0.039)	(0.033)	(0.035)
Tau 0.9	0.129 ***	0.134 ***	0.089 **	0.159 ***	0.165 ***	0.081 **
	(0.030)	(0.029)	(0.035)	(0.039)	(0.033)	(0.035)
Observations	20078	20078	20078	17491	17491	17491
Fixed effects						
Border	Yes	No	No	Yes	No	No
Region	Yes	No	No	Yes	No	No
Year	Yes	No	No	Yes	No	No
Region-year	No	Yes	Yes	No	Yes	Yes
Border-year	No	No	Yes	No	No	Yes
Firm	No	No	No	Yes	Yes	Yes
Province	Yes	Yes	Yes	Yes	Yes	Yes
Controls						
Contract value	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Quantile regressions with multi-way fixed effects, including province fixed effects. Dependent variable: time delay. Independent variable: Length judicial procedures. Each column denotes the inclusion of a different set of fixed-effects (see bottom panel). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

III Alternative Outcome Measure: Cost Overruns

In Table A.7, we present our baseline quantile estimates, where the dependent variable is the cost overrun. As mentioned in the main text, the national law and regional regulations introduce multiple thresholds determining procedures to handle cost overruns: essentially, the higher the share of the cost overrun relative to the original contract value, the more complex the renegotiation process is. These thresholds induce a non-smooth distribution of the cost overrun outcome variable, making it less amenable to be studied with our quantile approach. For the top quantiles, the estimates in Table A.7 are qualitatively similar to those presented earlier for time delay: slower courts induce more overruns when these are already high. However, the sign of the estimates is not clear for bottom quantiles, as it differs across specifications.

Table A.7: Cost overrun: baseline estimates

	(1)	(2)	(3)	(4)	(5)	(6)
				Cost overrun (ratio)		
Tau 0.1	0.000	0.001	0.011 ***	0.011 ***	0.011 ***	0.011 ***
	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)	(0.002)
Tau 0.2	-0.003	-0.003	0.003	0.003	0.003	0.003
	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)	(0.002)
Tau 0.3	-0.005 *	-0.003	0.002	0.002	0.002	0.002
	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)	(0.002)
Tau 0.4	-0.006 **	-0.007 *	0.002	0.002	0.002	0.002
	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)	(0.002)
Tau 0.5	-0.006 **	-0.005	0.000	0.000	0.000	0.000
	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)	(0.002)
Tau 0.6	-0.003	-0.004	0.000	0.000	0.000	0.000
	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)	(0.002)
Tau 0.7	0.002	0.001	0.006 ***	0.006 **	0.006 **	0.006 ***
	(0.003)	(0.004)	(0.002)	(0.003)	(0.003)	(0.002)
Tau 0.8	0.016 ***	0.016 ***	0.019 ***	0.019 ***	0.019 ***	0.019 ***
	(0.003)	(0.004)	(0.002)	(0.003)	(0.003)	(0.002)
Tau 0.9	0.022 ***	0.019 ***	0.016 ***	0.016 ***	0.016 ***	0.016 ***
	(0.003)	(0.004)	(0.002)	(0.003)	(0.003)	(0.002)
Observations	20078	20078	20078	17491	17491	17491
Fixed effects						
Border	Yes	No	No	Yes	No	No
Region	Yes	No	No	Yes	No	No
Year	Yes	No	No	Yes	No	No
Region-year	No	Yes	Yes	No	Yes	Yes
Border-year	No	No	Yes	No	No	Yes
Firm	No	No	No	Yes	Yes	Yes
Province	Yes	Yes	Yes	Yes	Yes	Yes
Controls						
Contract value	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Quantile regressions with multi-way fixed effects. Dependent variable: cost overrun (ratio). Independent variable: Length judicial procedures. Each column denotes the inclusion of a different set of fixed-effects (see bottom panel). ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

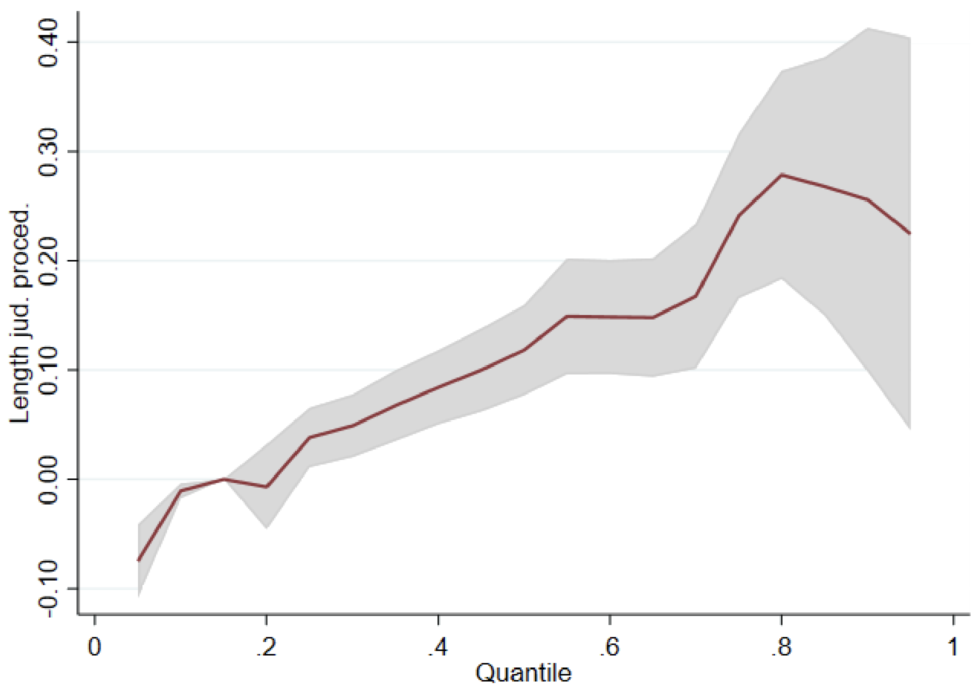
IV Alternative Estimators

We run two alternative specifications to test the robustness of our main results. We first perform a set of standard quantile regressions aimed at verifying the simple distributional relationship between time delay and our proxy for court efficiency. This specification does not contain border fixed effects required to reach identification via the spatial discontinuity approach. Therefore, coefficients may not be consistent. The second test attempts to mimic the quantile regression with multi-way fixed effects in an OLS setting. Results from both these exercises align with those from our main specifications, which is reassuring for different reasons. First, it suggests that reverse causality and omitted variables in the potentially inconsistent model do not play a significant role. Second, it proves that a large number of fixed effects in the main specifications do not lead to overfitting or the incidental parameter problem. Third, it validates the new quantile estimator that we employ to produce the core results.

IV.1 Standard quantile regressions

A standard quantile regression exercise is performed by regressing time delay on the length of judicial proceedings at each 0.05 quantile. Results presented in Figure A.2 are qualitatively similar to our baseline estimates.

Figure A.2: Standard quantile regression: coefficients and confidence intervals



Notes: The figure plots coefficients and 95% confidence intervals of a battery of quantile regressions performed at every 0.05 quantile within the [0.05, 0.95] interval. Dependent variable: time delay (ratio). Independent variable: Length judicial procedures. Border sample. Observations: 20,320.

IV.2 OLS with quantile partitioned interactions

We estimate an alternative specification based on simple OLS models to estimate the non-linear causal effect between court inefficiency and procurement delay. To capture the effects of L_{kt} on the quantiles of Y, we interact the independent variable with several dummies $I_{Y(\tau)}$, reflecting the quantile τ of Y to which each observation belongs.

$$Y_{ipt} = \sum_{\tau=0.1}^{0.9} \beta_\tau (L_{kt} \times I_{Y(\tau)}) + \zeta_i + \delta_{bt} + \gamma_{rt} + \varepsilon_{ipt} \tag{7}$$

The model includes ζ_i contract level controls, as well as region-year (γ_{rt}) dummy variables. We cluster standard errors at the border-court level. The results presented below confirm our baseline estimates and point toward a significant diverging effect of poor court efficiency on time delay. OLS estimates show significant negative coefficients until the sixth quantile, whereas we observe a positive coefficient between the seventh and the ninth quantile. Comparing quantile baseline estimates and results obtained from OLS regressions, we observe significantly higher OLS coefficients on the upper part of the time delay distribution. Column 2 reports results for an OLS regression that includes firm fixed effects. Results corroborate the findings obtained by quantile methods.

Table A.8: Time delay: OLS estimates, with and without firm fixed effects

	(1)	(2)
	Time delay (ratio)	
Tau 0.1	-0.166***	-0.145***
	(0.0108)	(0.0197)
Tau 0.2	-0.143***	-0.122***
	(0.0109)	(0.0195)
Tau 0.3	-0.119***	-0.0977***
	(0.0108)	(0.0197)
Tau 0.4	-0.0933***	-0.0716***
	(0.0108)	(0.0194)
Tau 0.5	-0.0627***	-0.0414**
	(0.0108)	(0.0195)
Tau 0.6	-0.0231**	0.000306
	(0.0108)	(0.0195)
Tau 0.7	0.0285**	0.0507**
	(0.0109)	(0.0196)
Tau 0.8	0.113***	0.136***
	(0.0110)	(0.0192)
Tau 0.9	0.316***	0.339***
	(0.0119)	(0.0192)
Observations	20,031	12,504
Fixed effects		
Region-year	Yes	Yes
Border-year	Yes	Yes
Firm	No	Yes
Controls		
Contract value	Yes	Yes

Notes: OLS with multi-way fixed effects. Dependent variable: time delay (ratio). Independent variable: Length judicial procedures. Each column denotes the inclusion of a different set of fixed-effects (see bottom panel). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.