Court Enforcement and Firm Productivity: Evidence from a Bankruptcy Reform in Brazil

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Abstract

Financial reform designed to improve creditor protection is often encouraged as a way to increase credit access for firms in developing countries. In this paper, I show that when court enforcement works poorly, financial reform is ineffective in fostering both credit access and the productivity of firms. In the empirical analysis, I exploit variation in the quality of court enforcement across Brazilian judicial districts and use a panel of manufacturing firms. I find that, after the introduction of a major pro-creditor bankruptcy reform, firms operating in districts with efficient court enforcement experienced substantially higher increase in capital investment and productivity than firms operating in districts with poor court enforcement. I provide evidence that this effect is due to a higher probability of external funds being used to finance investment in new technologies. To show that the results are not driven by district-level omitted variables, I use an IV strategy based on state laws establishing the geographical boundaries of judicial districts.

(*Keywords:* Credit Constraints, Financial Reform, Technology Adoption, Court Congestion *JEL:* G33, O16)

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1 Introduction

There is a consensus among economists and policymakers that financial frictions are a major barrier to firm investment and thus to economic development (Banerjee and Duflo, 2005; World Bank, 2005). By limiting access to external finance, they can prevent firms from adopting more advanced technologies. In addition, they can hinder the reallocation of capital towards more productive projects, decreasing aggregate productivity (Hsieh and Klenow, 2009).[1]

Weak protection of creditors is an important source of financial frictions (La Porta et al., 1997; Demirgüç-Kunt and Maksimovic, 1998; Djankov et al., 2007). In the context of bankruptcy, for example, creditor protection is measured by the effective rights to recover claims from financially distressed firms afforded to creditors by national laws and law enforcement institutions (La Porta and López-de Silanes, 2001). In an attempt to improve firms' access to external finance, emerging economies such as Brazil, China, and Russia have recently introduced new bankruptcy laws increasing the legal protection of creditors. One aspect often overlooked when assessing the potential benefits of these reforms is that, to be effective, they need proper and timely enforcement by courts. Judicial enforcement, however, is seldom well-functioning in many developing countries, where courts in charge of bankruptcy cases are characterized by limited expertise and long delays (Dakolias, 1999; Djankov et al., 2008). In such cases, even an otherwise desirable improvement in bankruptcy rules can prove ineffective.

In this paper I assess empirically the extent to which the effects of financial reform depend on the quality of court enforcement. I focus my analysis on Brazil for two reasons. First, it undertook in 2005 a major bankruptcy reform that substantially increased creditors' chances of recovering their claims when a firm is liquidated. Second, Brazilian judicial districts are highly heterogeneous in terms of efficiency. In some districts, cases are closed within time frames comparable to those in the US. In others, the functioning of courts is undermined by the large number of pending cases. Crucially, Brazilian laws do not allow creditors or firms to choose the district in which to file a bankruptcy case. Therefore, when the new bankruptcy law went into force, the efficiency of local courts became a key determinant of the ability of both creditors and firms to reap the benefits of the reform.

To guide the empirical analysis I propose a simple model of heterogeneous firms in the style of Melitz (2003), in which firms face a fixed cost for technology adoption. I assume that firms must borrow to pay the fixed cost and that the maximum amount they can borrow depends on two parameters: one captures the strength of creditor protection

[1]See also: Banerjee and Moll (2010); Buera et al. (2011) and Caselli and Gennaioli (2011). Midrigan and Xu (2010) and Moll (2010) show that, under certain conditions, firms can internally finance their way out of borrowing constraints relatively fast.

afforded by national legal rules and is the same for all firms; the other captures the quality of court enforcement and varies across judicial districts. The model has two main qualitative implications when a reform that strengthens creditor protection is introduced. First, firms operating in districts that have better court enforcement benefit more from the reform in terms of access to external financing, investment in the more advanced technology, and productivity. This effect is heterogeneous across firms: those in the middle of the pre-reform productivity distribution benefit more. Second, because firms investing in the more advanced technology increase their labor demand, the reform increases wages relatively more in districts that have better court enforcement. Higher wages drive out of the market the least productive firms and reallocate labor to the more productive ones.

I test the implications of the model using a differences-in-differences (diff-in-diff) strategy: I exploit the Brazilian bankruptcy reform introduced in 2005 as an external source of time variation, and the congestion of courts across judicial districts as a source of cross-sectional variation. Data on the judiciary comes from monthly reports that judges and the administrative staff of Brazilian courts submit to the National Justice Council (CNJ). This dataset contains unique, detailed information at the court and judge levels for all Brazilian judicial districts. The outcome variables at firm level are from two surveys of manufacturing firms produced by Brazilian Institute of Statistics (IBGE) that cover the years from 2000 to 2009.[2]

The baseline diff-in-diff results are consistent with the predictions of my model. First, firms operating in (one standard deviation) less congested judicial districts experience higher increase in capital investment (4.4%) and firm productivity (1.9%) after the introduction of the reform. The size of these effects is substantial: a one standard deviation in court congestion explains approximately 20% of the average increase in capital investment and 10% of the average increase in total factor productivity (TFP) in the years under study.[3] I further show that these effects correlate with a higher probability of external funds being used to finance investment in new technologies. Consistent with the model, most of the effect of court congestion on investment and productivity is driven by middle-to-large firms. Furthermore, I find a larger increase in average wages in districts with less congested courts. Finally, in these districts, small firms are more likely to exit the market.

Brazilian laws establish that bankruptcy cases must be filed in the judicial district where the headquarters of the firm is located. A first challenge to my identification strategy is that firms might initially decide where to locate their headquarters based on the

[2]The surveys are: the Annual Industrial Survey (PIA), from which I use yearly data from 2000 to 2009, and the Survey of Technological Innovation (PINTEC), that is produced every 2/3 years and for which I use the waves of 2000, 2003, 2005 and 2008.

[3]These are percentages of the average increase in capital investment and TFP observed at the firm level between the pre-reform years (2003–2005) and the post-reform years (2006–2009), respectively, 23% and 19.8%.

quality of court enforcement. I tackle firm selection by showing that court enforcement is not a significant determinant of firm location in the pre-reform period. A second challenge involves the fact that districts with better enforcement might also have other desirable characteristics driving the main results. I show that my diff-in-diff results are robust to controlling for initial conditions at the judicial district level such as average household income, number of banking agencies, and population. Exploiting the panel dimension of the firm-level dataset, I also verify that the diff-in-diff results are not driven by different background trends across districts.

To address endogeneity I also propose an instrumental variable (IV) strategy that corroborates the diff-in-diff results. The IV strategy is based on state laws that, starting from the 1970s, established minimum population size requirements for municipalities to become independent judicial districts.[4] Crucially, jurisdiction over municipalities below these minimum requirements was assigned to the courts of a territorially contiguous municipality that met the requirements. I construct the instrument as follows. First, I restrict my sample to those municipalities that, given their population, could have been an independent judicial district. Second, I compute for each of these municipalities the total population of their neighboring municipalities below the minimum requirements at the time the state laws were introduced. This is a measure of the initial extra jurisdiction assigned to judges by state laws that only depends on characteristics of neighboring municipalities. I use this as an instrument for current court congestion. Because the population of a subsample of neighbors can also determine firm-level outcomes through market size effects, I control for the total population of all neighboring municipalities, regardless of whether they were above or below the minimum requirements.

Existing work on the relation between legal protection of creditors and judicial efficiency has exploited cross-country differences (Djankov et al., 2003; Claessens and Klapper, 2005; Safavian and Sharma, 2007). These differences are likely to correlate with other unobserved country characteristics, such as the investment climate or the level of political accountability, that also affect financial development and the other outcomes under study. Research work using within-country data – which mostly relies on across-state variation – has focused on the effect of enforcement quality and not on its interaction with financial reform. For example, using loan-level data from a large Indian bank, Visaria (2009) finds that the introduction of specialized tribunals increases loan repayment and lowers the cost of credit for firms.[5] Lilienfeld-Toal et al. (2012) use Indian data to show theoretically and

[4]State laws on judicial organization in Brazil have been used also by Litschig and Zamboni (2012) to construct their instrument for the physical presence of judicial institutions.

[5]I do not find that firms that operates in districts with bankruptcy courts experienced a larger increase in investment and productivity in the aftermath of the reform. A possible explanation is that, in comparison to India, specialized courts in Brazil tend to be more congested than normal civil courts (see also De Castro (2009)).

empirically that when credit supply is inelastic, the existence of specialized courts reduces access to credit for small firms and expands it for big ones.[6] Another stream of empirical literature studies the aggregate effects of bankruptcy reform, but without focusing on the role of the judiciary in shaping these effects. For example, Araujo et al. (2012) analyze the effect of the Brazilian bankruptcy law reform on the financing decisions of publicly traded Brazilian firms using as control group publicly traded firms in neighboring countries.[7] The main contribution of my paper to the existing literature is that, to the best of my knowledge, I provide the first empirical evidence on the interaction between financial reform — in particular, bankruptcy reform — and court enforcement at the micro level.

On the theoretical side, several papers in the bankruptcy literature tackle the judicial system's role in shaping bankruptcy outcomes – for better or worse. Gennaioli (2012) proposes a model in which the ability of courts to properly enforce contracts depends on their ability to verify actual states of the world, e.g. the return of a given project. When these states are not easily verifiable, courts' verification generates enforcement risk in financial transactions. Gennaioli and Rossi (2010) show how judicial discretion can lead to an efficient resolution of financial distress, but only in a reorganization framework that offers strong creditor protections. Ayotte and Yun (2009) stress the potential negative effects of judicial discretion when judges are not trained or do not have the necessary experience to effectively run the bankruptcy procedure. Ichino et al. (2003) show that judicial enforcement can lead to different outcomes in similar firing cases made under the same national laws, especially when such laws leave a wide range of possible interpretations to judges.

The rest of the paper is organized as follows. In section 2 I describe the Brazilian bankruptcy reform and how the efficiency of the judicial system influenced its impact on creditors. Section 3 presents the data on the Brazilian judiciary. In section 4, I present a model of heterogeneous firms which delivers qualitative predictions on firm level outcomes. Section 5 describes the firm-level data used to test these predictions. In section 6, I present the identification strategy and the empirical results.

[6] Other papers using within-country variation in judicial variables are Chemin (2012), which studies the impact of judicial reform on the lending and investment behavior of small firms in India; Jappelli et al. (2005), which exploits variation across Italian judicial districts to establish a relation between judicial efficiency and bank lending; and Laeven and Woodruff (2007), which studies how the quality of the legal system at the state level affects firm size in Mexico.

[7] See also: Gamboa-Cavazos and Schneider (2007) for Mexico and Rodano et al. (2011) for Italy.

2 The Brazilian Bankruptcy Reform and the Role of the Judicial System

Until 2005 bankruptcy in Brazil was administered under Law 7,661, in force since 1945 (hereafter the "old law"). The old law was particularly unfavorable towards secured creditors, banks that provide loans guaranteed by collateral. In most developed countries, including the US, secured creditors have the right to repossess the collateral when a firm defaults on its debt. In Brazil, instead, the collateral proceeds are pooled together with the rest of the firm's assets and then used to repay creditors in an order established by law. Under this framework, secured creditors were put at a strong disadvantage by two characteristics of the old liquidation procedure:[8] successor liability and first priority given to labor and tax claims.

Successor liability implied that, in liquidation, the debts of a firm were passed on to the purchasers of the firm or of its business units. This dampened the value of financially distressed firms, which had to be discounted for the known debt, the costs of due diligence, and the risk associated with possible unknown debts. Successor liability did not apply when firm assets were sold separately instead of jointly (e.g., a single loom versus an entire textile plant), creating strong incentives to sell assets piecemeal, which further reduced the proceeds from liquidation.

The second characteristic of the old law was that labor and tax claims had first priority in the order of repayment. Only afterwards came secured creditors. The absolute priority rule required that labor and tax claims had to be paid off in full before anything was given to secured creditors. As a consequence, their probability of recovery was minimal once an official bankruptcy procedure was initiated.[9]

In June 2005 Brazil introduced a new bankruptcy law (Law 11,101) inspired by chapters 7 and 11 of the US bankruptcy code. The conflict of interest between the fiscal authority and the banking sector – the former interested in maintaining its priority on secured creditors, the latter pushing to reverse it – lead to high uncertainty about the wording of the final draft. In this sense, the exact content of the new rules could hardly be anticipated until the end of 2004.[10]

One of the objectives of the new law was to increase creditors' recovery. To this

[8] I focus here on the liquidation procedure, representing on average 97% of bankruptcy procedures in Brazil from 1992 to 2005.

[9] This had the additional negative effect of lowering the incentive for secured creditors to bring financially distressed firms to court in the first place. They delayed the filing for a firm's bankruptcy as long as possible, and only did so when the firm's debt situation was already unsustainable. Thus, firms entering official bankruptcy were usually in particularly bad shape, reducing even further the probability that creditors would recover any of their claims.

[10] The final wording of the law was only revealed at the end of 2004 after several passages through the two houses. The new law was applicable only to bankruptcy cases filed after it entered in force.

end, the new law introduced several important innovations (see Table 1 for a detailed description). In this paper I focus on three of them: (i) removal of successor liability when selling business units or the entire firm as a going concern, (ii) introduction of a cap of 150 monthly minimum wages[11] per employee on labor claims, and (iii) priority of secured creditors' claims over tax claims. The first point states that claims remain liabilities of the debtor and are no longer passed on to the purchasers (art. 141). This increased the value of distressed firms when sold in full or by business units. The second point was introduced to avoid fraud, because it was not uncommon for the management personnel of firms in financial distress to fix unreasonably high salaries for themselves before entering into bankruptcy, knowing they would enjoy the same priority as their employees. The third point was introduced to increase the protection of banks – those providing credit guaranteed by collateral. If the first point increased the potential value of firms in financial distress – and therefore the recovery rate of creditors – the second and third points reduced uncertainty about the bankruptcy outcome for secured creditors, allowing them to evaluate ex-ante their likely recovery on each loan. In fact, under the new law, a bank can estimate the present discounted value of the collateral necessary to guarantee full recovery if the firm were to go bankrupt.[12]

A survey of legal professionals promoted by the World Bank for the Doing Business Database suggests that the average recovery rate – expressed in cents per claimed dollar that creditors (be they workers, the tax authority, or banks) are able to recover from an insolvent firm – was 0.2 cents on the dollar in Brazil at the time the reform was passed. This is a negligible fraction of outstanding claims when compared, for the same year, with the US (80.2), India (12.8), and China (31.5). Figure 1 shows the pattern of the recovery rate in Brazil from 2004 to 2012. According to the World Bank, the recovery rate had a discrete jump about two years after the introduction of the reform, going from 0.4 in 2006 to 12.1 cents on the dollar in 2007. This pattern is consistent with the legal changes introduced by the bankruptcy reform, especially the removal of successor liability, which aimed to increase the value of firms sold in bankruptcy. However, the level attained is still far from that of the US. Table 1 compares the old Brazilian law, the new law and the US law. It shows how the gap with the US in terms of legal rules has been drastically reduced with the introduction of the new law. Why is therefore the gap in terms of recovery rate still so wide?[13] Part of the explanation lies in the efficiency of the judicial system. In the

[11]In 2005, the national monthly minimum wage was 300R$, corresponding to around 110 US$.

[12]As an example, take a firm with 10 employees asking for a loan worth US$ 200,000. The bank knows there are, at maximum, labor claims worth in total US$ 200,000 (assuming US$ 20,000 per employee) with priority over its own claim in case of default. As a consequence, for a loan worth US$ 200,000, a bank will ask a collateral worth at least US$ 400,000 at the time of maturity.

[13]One option is that it simply takes some time to digest the new rules and that Brazil will eventually catch up with the US in terms of efficiency of its bankruptcy process. But the data seems to tell a different story. After the reform the recovery rate stabilized at around 17 cents on the dollar, and it is

US, bankruptcy cases are closed in an average of 1.5 years. My data show that in Brazil, this take between 5 and 6 years.

Brazil is an ideal laboratory to study how much the efficiency of the judicial system can affect the impact of a major legal reform. Brazilian laws establish that bankruptcy cases must be filed in the civil court that serves the area where the debtor's headquarters is located. Unlike the US — where forum shopping is a diffuse practice, in particular for big reorganization cases (e.g., Eisenberg and LoPucki, 1999) — Brazilian judges tend to consider a firm's headquarters to be the location where most of the economic activity of the firm takes place.[14] This definition of a firm's headquarters makes the relocation for judicial purposes an extremely expensive process. In addition, under the old bankruptcy regime, the negligible recovery rate of creditors made the costs associated with relocation outweigh the benefits of a more favorable judicial environment. Finally, the vast majority of bankruptcy cases in Brazil are cases of liquidation, where the management has no power to choose the bankruptcy venue.

Second, Brazil is an ideal laboratory because it offers vast cross-sectional variation in judicial variables that can be exploited. Brazil is divided into more than 2,500 judicial districts, and each district has at least one court of first instance that handles civil cases, including bankruptcy.[15] Moreover, courts proceed at vastly different speeds across the country. Judicial data presented in this paper suggests that closing a civil case can take less than a year in some districts, and up to 30 years in others.

Slow courts are likely to have had a negative effect on the incentive to lend under both the old and the new law. The key assumption here is that for secured creditors (that in the pre-reform period expected to recover nothing regardless of court speed), the quality of court enforcement became a critical factor in determining their chances of recovery only in the post-reform period.[16]

2.1 Judicial Efficiency and Firms' Location

Is the efficiency of civil courts a key determinant in where firms choose to initially establish their headquarters? Even if forum shopping is not allowed or is extremely costly

not in an increasing pattern.

[14]This practice became widespread after cases such as the one of Grupo Frigorìfico Arantes, a large company (1.6 billions R$ of annual revenues) located in Ribeirão Preto (state of Sao Paulo) that filed for bankruptcy in the middle of the Amazonian Forest at the civil court of the tiny municipality of Nova Monte Verde that, with only 8 thousands inhabitants and at 750 km from the capital Cuiabá does not even have a permanent judge.

[15]Bankruptcy cases are a small fraction of the cases for which civil courts function as courts of first instance. They usually share the judge's desk with tax disputes, car accidents, divorce cases etc.

[16]The crucial role of the judiciary in the enforcement of the new law has been highlighted by academics (Araujo and Funchal (2005)), practitioners (Felsberg et al. (2006)), and the Brazilian Central Bank (Fachada et al. (2003)).

in Brazil, firms might decide to establish their headquarters in districts with better enforcement because, for example, in such districts it is easier to get a line of credit.[17] This is a potentially important selection problem: if all firms tend to locate where courts are more efficient, then the aggregate costs of judicial inefficiencies are negligible. As a first check to the data, in the empirical section I show that judicial districts characterized by different degrees of court efficiency are not systematically different in terms of the number of manufacturing firms registered under their jurisdiction in the pre-reform period. If anything, districts where courts are more congested register a higher number of manufacturing firms in 2000, the initial year. I also show that from 2000 to 2009 there is very little firm mobility across districts: about 1.5% of firms changed judicial districts every year on average. As an additional robustness test, in the empirical section I show that the main results hold if I restrict the sample to those firms that did not change location after the introduction of the bankruptcy reform, or to single-plant firms, those for which the possibility of forum-shopping would be more costly.

3 Data on the Brazilian Judiciary

In section 2 I argued that the new law made court speed a key determinant of creditors' and firms' ability to reap the benefits of the reform. In this section, I document the vast cross-sectional variation (subsection 3.1) and the high persistence over time (subsection 3.2) of court congestion across Brazilian judicial districts.

3.1 Description of Judicial Variables

Data on the Brazilian judiciary come from *Justiça Aberta*, a database produced by the Brazilian National Justice Council (CNJ).[18] The dataset covers all courts and judges working in the Brazilian judiciary, and the data are collected monthly through a standard questionnaire filled out by the judges or the administrative staff of each court. Data at the court level[19] include the type of court (civil, criminal, etc.), the year of creation, the administrative staff available, the number of cases pending at the end of each month, the number of new cases filed per month, the number of hearings per month, and the number of cases sent for review to higher courts per month. Data at the judge level include, for each court in which the judge worked during the last month, the number of

[17] The relationship between firm location and court enforcement quality is not straightforward: firms might want ex-ante to locate where enforcement is faster because it is easier to get credit, but ex-post, at least in the case of liquidation, the optimal location might be the one where justice is slower, because it is easier to postpone debt repayment.

[18] All judicial data used in this paper can be downloaded from www.cnj.jus.br.

[19] Data is available for both courts of first and second instance. I focus my analysis on courts of first instance.

days worked, the number of cases closed and the number of hearings. The database allows me to match judges with courts, and courts with judicial districts. Brazil is divided in 2,738 judicial districts, which are the smallest administrative division of the judiciary. A judicial district can correspond to a single municipality, or can encompass a group of them. Using official documentation provided by state tribunals, I map each judicial district to the municipalities it includes.[20]

The measure of court enforcement quality I am interested in is court speed in closing bankruptcy cases. Because data on case length by type of case is not available, I follow the existing literature on judicial productivity (Dakolias, 1999) and use as a proxy the backlog per judge, defined as the number of pending cases in a court at the beginning of the year over the number of judges working in that court over the year.[21] I compute this measure at the judicial district level using only civil courts, those that deal with bankruptcy cases. For those judicial districts that have two or more civil courts, I take a weighted average of court congestion, using as weights the total number of open cases in each court.

Finally, 12 judicial districts[22] have courts that specialize in bankruptcy cases. Where these courts exist, the judicial district is assigned their measure of court congestion.[23] Out of the 8,621 courts of first instance initially recorded in the database (which include not just civil courts, but also criminal courts and courts specialized in various types of cases, from tax evasion to child protection), I select 4,126 civil courts and the corresponding 5,276 judges that deal with bankruptcy cases.[24] After taking averages across courts within districts where more than one court handles bankruptcy cases, I am left with data on 2,507 judicial districts, 92% of all of those existing in Brazil.[25]

Table 2 shows descriptive statistics of the main judicial variables. The unit of obser-

[20]Because the geographical identifier for Brazilian firms is the municipality in which they operate, matching judicial districts to municipalities is essential in order to merge data on the judiciary with data on firms.

[21]Unfortunately, the data do not allow me to observe the work practice of judges. In particular, I can not observe whether judges start working on cases in the order they enter into the court or whether they give priority to new cases. Coviello et al. (2011) show that work practices, and in particular how workers deal with pending tasks, can influence worker's productivity. Coviello et al. (2012) apply this reasoning to the case of Italian judges and find that "task juggling" negatively affects the speed at which they close cases.

[22]These are Belo Horizonte, Brasilia, Campo Grande, Curitiba, Fortaleza, Juiz de Fora, Novo Hamburgo, Porto Alegre, Rio de Janeiro, São Paulo, Uberaba, and Vitoria.

[23]Specialized courts employ judges with a sound understanding of bankruptcy procedures. They might play a different role independently of their productivity. I explore the role of courts specialized in bankruptcy more extensively in the empirical section.

[24]I can classify these courts by type as follows: 2,306 civil courts, 1,797 general courts ("vara unica", these are the only tribunal available in these judicial districts and deal with all types of cases), and 23 courts specialized in bankruptcy law.

[25]The missing 8% comprises mostly judicial districts located in remote areas, e.g., the Amazonas. It is possible that these districts have no permanent judge, and those judges that visit them from time-to-time are less likely to fill out the CNJ questionnaires.

vation is the judicial district, and all data refers to 2009. Each judicial district in Brazil has, on average, 1.6 civil courts, and each court has an average of 2.2 judges and 12.7 administrative staff members. The congestion rate is the sum of pending and new cases filed over a year, divided by resolved cases, and it can also be interpreted as the number of years necessary to solve all currently open cases at the current pace. Notice that the average congestion rate is 5.4, suggesting that at the current pace it will take, on average, slightly more than five years to close a bankruptcy case filed this year in a Brazilian court. Notice also the large heterogeneity in the cross-section. The congestion rate has a standard deviation of 4.9 and ranges from 0.6 to more than 30, suggesting that some judicial districts, judicial productivity is close to that of US standards (even though some of these outliers are remote places with few cases), while in others it can take more than 30 years for a case entering the court today to reach resolution. Figure 5 (upper graph) show a map of the state of São Paulo where judicial districts are separated in four quartiles of court congestion rate.

Interestingly, courts specialized in bankruptcy law (descriptive statistics reported in Table 3) tend to be slower in case resolution than normal civil courts and to have a higher rate of appeal, i.e. more cases sent to higher courts for revision. The average congestion rate of bankruptcy courts is 8, and their initial backlog per judge is more than 2000 cases larger than that of all other courts. In addition, their appeal rate is on average 20%, meaning that one case out of five is sent to higher courts for revision, while in normal courts just one case out of ten is. This is only suggestive evidence, since I can not observe whether different types of cases proceed at different speed within civil courts.

3.2 Judicial Variables over Time

How has judicial congestion behaved over time in Brazil? Has it been affected by the implementation of the bankruptcy reform? I check for trends at the state level, a higher level of aggregation than the judicial district, using data starting from 2004 (the first available year for data at state level). Figure 2 displays the strong stability of the ratio of pending cases per judge between 2004 and 2008 in Brazil as a whole. Figure 3 shows the same variable in four Brazilian states, one per quartile of the distribution of court congestion at the state level: São Paulo, Rio Grande do Sul, Rondonia, and Paraná. Notice the large heterogeneity in the level of court congestion across states and its persistence over time within each state.

To estimate how court congestion has affected the impact of the reform implemented in 2005 it would be optimal to use pre-reform data on the judiciary. Unfortunately, with the exception of two states, data on the pre-reform period are available only at a higher level of aggregation (state-level), while data at the judicial district level covers all Brazilian judicial districts starting from January 2009. Figure 2 shows that judicial variables are very stable over time at the state level. The fact that no clear trends are observed at a more aggregate level does not prove that the same trends hold at a finer level. To check trends at the judicial district level, I therefore use data on the two Brazilian states, Mato Grosso do Sul and Rondonia, for which data is available in the official documentation of state tribunals starting from 2004.[26] Figure 4 shows the scatterplots of the number of pending cases, new cases, and sentences (expressed in logs) for 2004 and 2009 for all judicial districts in these two states. Even though these states constitute a small fraction (5%) of all judicial districts, the figure suggests that there is little variation over time in the judicial variables even at this finer level.[27] More importantly, in section 6 I verify that i) the main results of the paper hold when using only judicial data from these two states and ii) using judicial data from 2004 or 2009 gives very similar coefficients on the main outcome variables. In the OLS regressions, I therefore use the cross-sectional variation in court congestion at the judicial district level in 2009 as a proxy of its pre-reform level. In section 6.4, I then propose an instrument that is predetermined with respect to the 2005 reform for all judicial districts.

4 A Model of Heterogeneous Firms with Financial Frictions

In section 2 I described a major financial reform introduced in Brazil in 2005 that, by increasing secured creditors chances of recovery, should have increased their ex-ante lending incentives. In section 3 I showed empirical evidence on the quality of court enforcement: data suggests that it is vastly heterogeneous across Brazilian judicial districts and relatively stable over time. This section provides a conceptual framework for understanding the effects of bankruptcy reform on two sets of firm-level outcomes: those directly affected by the reform, such as firm access to external finance and investment, and those indirectly affected by the reform, such as technological upgrade, productivity, wages, and exit. To this end, I present a simple model of heterogeneous firms in which access to credit affects firms' technological upgrade through a fixed cost of innovation that can not be financed with internal funds. My theoretical framework builds on Melitz (2003) and Bustos (2011), by adding financial frictions that depend on both national bankruptcy rules and efficiency of local courts.

[26]The original documentation can be downloaded from www.tjro.jus.br and www.tjms.jus.br.

[27]The pairwise correlation between these variables in 2004 and 2009 is between 85% and 90%.

4.1 The Basic Setup

There are two dimensions of heterogeneity across firms, both exogenously determined. The first is each firm's initial productivity level (φ). This can be thought of as the quality of the management or the potential of the project that each firm wants to carry out. The second dimension of heterogeneity is the location in which each firm operates, described here as a judicial district. I assume that, at least in the short term, there are infinite moving costs across judicial districts for both firms and workers.

The consumer utility function takes the C.E.S. form: $U = \left(\int_{\omega\in\Omega} y(\omega)^{\frac{\sigma-1}{\sigma}} d\omega\right)^{\frac{\sigma}{\sigma-1}}$, where $\sigma > 1$ is the elasticity of substitution across different varieties (identified by ω). Maximizing this utility function subject to the expenditure constraint $\int_{\omega\in\Omega} p(\omega)y(\omega)d\omega = E$, where E is the aggregate spending in this economy, gives the demand for a single variety:

$$y(\omega) = \left(\frac{p(\omega)}{P}\right)^{-\sigma} \frac{E}{P} \tag{1}$$

where $P = \left(\int_{\omega\in\Omega} p(\omega)^{1-\sigma} d\omega\right)^{\frac{1}{1-\sigma}}$ is the aggregate price index.

Each firm produces one of these different varieties in a single industry under increasing returns to scale. Labor is the only factor of production, and judicial-district-specific wages are the numéraire. Entrant firms draw their own productivity φ from a known distribution $G(\varphi)$. Entry is disciplined by fixed set-up costs (f_e), expressed in terms of labor, that guarantee a finite number of entrants. Once firms observe their productivity, they decide whether to stay and produce or to exit the market. In addition, in every period there is an exogenous probability of exit e for an unexpected bad shock. Those firms that stay in the market can produce their varieties using a low technology (L), which is a basic technology that features a relatively low fixed initial cost (f) and a constant variable cost (both expressed in terms of labor). They also have the option to switch to a high technology (H) that reduces their marginal labor cost but has a larger fixed cost. Production under different technologies is described by the following total cost functions (TC):

$$TC = \begin{cases} f + \frac{y}{\varphi} & \text{if technology } = L \\ \eta f + \frac{y}{\gamma\varphi} & \text{if technology } = H \ (\eta, \gamma > 1) \end{cases}$$

Firm profits are a positive function of productivity:

$$\pi = \begin{cases} (1-\tau)\bar{\pi}^L - f & \text{if technology } = L \\ (1-\tau)\bar{\pi}^H - \eta f & \text{if technology } = H \end{cases}$$

In this equation, $\bar{\pi}$ stands for firm operating profits net of variable labor costs. It is equal to $(\varphi\gamma)^{\sigma-1}\left(\frac{\sigma-1}{\sigma}\right)^{\sigma-1}\frac{E}{\sigma}P^{\sigma-1}$ in case the firm operates with the H technology. It is equal to $(\varphi)^{\sigma-1}\left(\frac{\sigma-1}{\sigma}\right)^{\sigma-1}\frac{E}{\sigma}P^{\sigma-1}$ in case the firm operates with the L technology. Since wages are the numéraire, firm profits are a negative function of the real wage $1/P$ in each judicial district. As in Restuccia and Rogerson (2008), corporate taxes $\tau > 0$ are levied on $\bar{\pi}$.

There is a unique productivity cutoff (φ^*) above which firms find it profitable to stay in the market. The cutoff is determined by the zero profit condition for a firm using the low technology: $\pi^L(\varphi^*) = 0$. There is another unique productivity cutoff (φ^h) above which firms find it profitable to switch to the high technology (pinned down by $\pi^H(\varphi^h) = \pi^L(\varphi^h)$).

I assume that the high-technology fixed cost can not be financed using internal funds, so that firms that find it profitable to switch to the high technology — those whose productivity is such that $\varphi \geqslant \varphi^h$ — need to borrow from financial intermediaries.

4.2 Financial Frictions

Financial intermediaries will lend to firms if they are guaranteed to recover their claims in case of default. This implies that financial intermediaries are willing to lend b as long as:

$$(1 - p\tau)\bar{\pi}^H - (1 + r)b \geqslant \left(1 - \frac{1}{\psi_j}\right)\bar{\pi}^H \tag{2}$$

The left-hand side of equation 2 represents firm profits after repaying taxes and the privately contracted debt b (on which it pays an interest rate r). The parameter p captures how pro-investors the bankruptcy rules are. The higher is p, the lower the share of firm profits that is appropriable by financial intermediaries — either because there are classes of creditors with priority over them (in this case, the government) or because the bankruptcy rules (such as those on successor liability) do not preserve the value of the firm in liquidation. A value of p close to 1 might capture the fact that the government has first priority over financial intermediaries or that the bankruptcy procedure is particularly inefficient in preserving the value of liquidated assets, or a combination of these factors.

The right-hand side of equation 2 represents the share of firm value [28] that can not be seized by courts.[29] The share $\frac{1}{\psi_j}$ is the judicial-district-level measure of the quality of court enforcement, which is assumed to be a negative function of ψ_j, the parameter capturing court congestion. One can think of ψ_j as the discount factor for the number of

[28] Operating profits net of variable labor costs before paying debt due to private financial intermediaries. Remember that in this setup all firms that want to borrow find profitable to switch to the high technology

[29] The ability of courts to seize defaulting firm's assets is a key parameter in several theoretical models with borrowing constrained entrepreneurs, such as: Caselli and Gennaioli (2011) and Cagetti and De Nardi (2006).

years that a bankruptcy case stays in court in a given district, and of $\frac{\bar{\pi}^H}{\psi_j}$ as the resulting value of a firm at the end of the bankruptcy process. I assume that there are $j = 1, ...J$ judicial districts in the country and that ψ_j varies across districts.

Under these assumptions, financial intermediaries will lend b as long as firm profits in the case of repayment are larger that the share of the firm value that can not be seized by courts in the case of default. The maximum amount of debt that a firm can obtain from financial institutions is therefore pinned down by equation 2 solved with equality. When $p > 0$ and assuming for simplicity that $r = 0$, this is given by:

$$\max\{b\} = \left(\frac{1}{\psi_j} - p\tau\right)\bar{\pi}^H \tag{3}$$

Equation 3 shows the complementary role played in this model by the two frictions that constrain firm borrowing: legal rules (p) and enforcement quality (ψ_j). Notice also how the two frictions operate at different geographical dimensions: p is the same for all judicial districts, while ψ_j varies across districts.

To adopt the high technology, firms must borrow at least enough to pay the fixed cost ηf. Firms are financially unconstrained as long as $\max\{b\} \geqslant \eta f$.[30] All firms whose productivity is such that $\max\{b\} < \eta f$ (but high enough that the adoption of the new technology is a profitable option) are instead financially constrained.[31] To find the productivity cutoff to be unconstrained (φ^u) I set $\max\{b\} = \eta f$. Figure 7 shows in red the optimal technological choice as a function of firm productivity.

4.3 Industry Equilibrium

The industry equilibrium is pinned down by the zero profit condition and the free entry condition. Free entry requires the fixed entry cost to be equal to the present value of expected profits, discounted by the per-period probability of exit due to a bad shock (e):

$$f_e = [1 - G(\varphi^*)]\frac{\tilde{\pi}}{e} \tag{4}$$

where $[1 - G(\varphi^*)]$ is the probability of survival, i.e., the probability of drawing a productivity higher than the exit cutoff productivity (φ^*).

When financial frictions play a role, expected profits are defined as follows:

$$\tilde{\pi} = p^u\tilde{\pi}^u + p^c\tilde{\pi}^c \tag{5}$$

[30] In this case, the value of the firm in the case of default is large enough to repay all debtors, because $\frac{\bar{\pi}^H}{\psi_j} \geqslant \tau\bar{\pi}^H + \eta f$.

[31] In the extreme case in which $\tau \geqslant \frac{1}{\psi_j}$, financial intermediaries will never recover anything at the end of the bankruptcy process.

where p^u is the probability of an active firm being productive enough to be unconstrained, while p^c is the probability of being constrained (both conditional on being active). Formally, $p^u = \frac{1-G(\varphi^u)}{1-G(\varphi^*)}$, and $p^c = \frac{G(\varphi^u)-G(\varphi^*)}{1-G(\varphi^*)}$. If the firm is unconstrained, then its expected profits are given by:

$$\tilde{\pi}^u = \int_{\varphi^*}^{\varphi^h} \pi^L(\varphi)\frac{g(\varphi)}{1-G(\varphi^*)} + \int_{\varphi^h}^{\infty} \pi^H(\varphi)\frac{g(\varphi)}{1-G(\varphi^*)} \tag{6}$$

Substituting in the expression for $\pi^L(\varphi))$ and $\pi^H(\varphi))$:

$$\tilde{\pi}^u = (1-\tau)\tilde{\varphi}^{\sigma-1}\left(\frac{\sigma-1}{\sigma}\right)^{\sigma-1}\frac{EP^{\sigma-1}}{\sigma} - f\left(\frac{G(\varphi^h)-G(\varphi^*)}{1-G(\varphi^*)}\right) - \eta f\left(\frac{1-G(\varphi^h)}{1-G(\varphi^*)}\right) \tag{7}$$

where:

$$\tilde{\varphi}^{\sigma-1} = \int_{\varphi^*}^{\varphi^h} \varphi^{\sigma-1}\frac{g(\varphi)}{1-G(\varphi^*)} + \int_{\varphi^h}^{\infty} \gamma^{\sigma-1}\varphi^{\sigma-1}\frac{g(\varphi)}{1-G(\varphi^*)}$$

Using the zero profit condition, I can rewrite equation (7) as:

$$\tilde{\pi}^u = f\left[\left(\frac{\tilde{\varphi}}{\varphi^*}\right)^{\sigma-1} - 1 - (\eta-1)\left(\frac{1-G(\varphi^h)}{1-G(\varphi^*)}\right)\right]$$

Notice that τ simplifies away in this new expression. Using the assumption that $G(\varphi)$ is a Pareto distribution with support equal to 1 and shape parameter k, one can solve for $\tilde{\varphi}^{\sigma-1}$:

$$\tilde{\varphi}^{\sigma-1} = \frac{k(\varphi^*)^{\sigma-1}}{\sigma-1-k}\left[\left(\frac{\eta-1}{\gamma^{\sigma-1}-1}\right)^{1-\frac{k}{\sigma-1}}(1-\gamma^{\sigma-1}) - 1\right]$$

Substituting this expression into the expected profits equation, I obtain the expression for expected profits for an unconstrained firm as a function of the model's parameters. If instead a firm is constrained, its expected profits are given by:

$$\tilde{\pi}^c = \int_{\varphi^*}^{\infty} \pi^L(\varphi)\frac{g(\varphi)}{1-G(\varphi^*)} \tag{8}$$

The expressions for expected profits capture that if a firm is unconstrained, it will always be able to choose the most profitable technology given its initial productivity. If a firm is instead constrained, it will work with a low technology even if it would be productive enough to switch to a more productive technology in an unconstrained world. Substituting equations (6) and (8) into equation (5), yields the expression for expected profits in an industry with financial frictions. This expression is then substituted into equation (4) to find the equilibrium cutoff productivities to stay in the market, switch to the new technology, and be unconstrained.

4.4 Model Qualitative Predictions

In this section, I analyze the impact of a bankruptcy law reform on firm-level outcomes according to the model. Because p captures how pro-investor bankruptcy legal rules are (the higher is p, the lower the pro-investor intent of the law), the reform is modeled as a decrease in p. The reform is assumed not to affect the congestion of courts (ψ_j). The following propositions are obtained by taking derivatives with respect to p of the cutoff productivities' equilibrium expressions.

Proposition 1: *A decrease in p lowers the cutoff productivity for being financially unconstrained. The size of this effect is inversely proportional to the level of court congestion.*

Proof: In equilibrium: $\frac{\partial \varphi^u}{\partial p} > 0$ and $\frac{\partial^2 \varphi^u}{\partial \psi_j \partial p} < 0$.

Proposition 1 states that firms operating in districts where courts are more efficient should benefit more from the reform in terms of access to external financing. As a consequence, in these districts, more firms can switch to the H technology after the reform.

Proposition 2: *A decrease in p implies an increase in the average wage in district j. The size of this effect is inversely proportional to the level of court congestion.*

Proof: This follows directly from Proposition 1 and the following assumptions of the model: the H technology has a larger fixed cost (expressed in terms of labor); labor is the only factor of production; and labor is immobile across districts in the short run.

Proposition 2 states that in districts where courts are more efficient, because more firms will use external finance to switch to the H technology, labor demand and wages will increase relatively more than in districts where courts are less efficient.

Proposition 3: *A decrease in p raises both the cutoff productivity to stay in the market and the cutoff productivity to adopt the H technology. The size of these effects is inversely proportional to the level of court congestion.*

Proof: In equilibrium, $\frac{\partial \varphi^*}{\partial p} < 0$ and $\frac{\partial \varphi^h}{\partial p} < 0$. In addition, $\frac{\partial^2 \varphi^*}{\partial \psi_j \partial p} < 0$ and $\frac{\partial^2 \varphi^h}{\partial \psi_j \partial p} < 0$.

Higher wages in districts where courts are more efficient drive up both the fixed cost of entry and the H technology fixed cost, because both are expressed in terms of labor. When the fixed cost of entry goes up, so does the productivity cutoff for staying in the market, forcing the least productive firms to exit. This will additionally increase the average effect on firm productivity. When the H technology fixed cost goes up, so does the productivity cutoff to upgrade technology. The reform should therefore reduce the number of firms that are financially constrained in two ways: for pre-reform constrained firms at the higher end of the productivity spectrum, the reform should relax their borrowing constraint, and for those at the lower end, the reform should make the high technology no longer a profitable option. Both effects will reduce the misallocation of resources in this economy.

5 Firm-level Data

This section describes the firm-level outcomes available in the data that I use to test the model's predictions. Data comes from two confidential surveys of firms constructed by the Brazilian Institute of Statistics (IBGE): the Annual Industrial Survey (PIA) and the Survey of Technological Innovation (PINTEC).

The PIA survey monitors the performance of Brazilian firms in the extractive and manufacturing sectors. I focus on the manufacturing sector as defined by the Brazilian sector classification CNAE 1.0 (sectors 15 to 37) and CNAE 2.0 (sectors 10 to 33). I use yearly data from 2000 to 2009. The population of firms eligible for the survey comprises all firms with more than 5 employees registered in the national firm registry (CEMPRE, the *Cadastro Central de Empresas*). The survey is constructed using two strata: the first includes a representative sample of firms having between 5 and 29 employees (*estrato amostrado*) and the second includes all firms having 30 or more employees (*estrato certo*). For all firms in the survey, the data are available both at the firm and at the plant levels (when firms have more than one plant). My unit of observation is the firm. For each firm I can observe the municipality where it is registered, which also identifies the competent jurisdiction for any legal case involving the firm. At the firm level, the survey includes information on the number of employees, the wage bill, revenues, costs, capital investment, and gross value added.

Figure 6 shows a map of Brazil with the location of firms in the PIA sample. It is clear from the map that the majority of firms are located in the south, south-east and north-east regions (especially on the coast), the regions where most of Brazil's economic activity takes place.

The second source of firm-level data is the PINTEC survey. This survey monitors the technological innovation of Brazilian firms. The first wave of the survey was conducted in 2000, followed by other three waves in 2003, 2005, and 2008. The interviewed sample is selected from firms with more than 10 employees that are registered in the national firm registry and that operate in the extractive or manufacturing sectors. I focus on three variables from the PINTEC survey: spending in technology, introduction of new products, and access to external finance. The variable *spending in technology* includes seven categories of spending (all registered in monetary values): spending in internal R&D, acquisition of external R&D, acquisition of other external knowledge (e.g., patents), acquisition of machineries and equipment, personnel training, marketing/advertising, and investment necessary to implement new product/processes in the production chain. About 52% of spending in technology comes from acquisition of new machineries and equipment and 20% from internal and external R&D; the remaining 28% comes from the other categories.[32] *Introduction of new products* is a dummy variable equal to one if the firm has introduced a new product in the time elapsed from the last survey. Finally, *access to external finance* is constructed as a dummy equal to one if the firm indicates having financed a positive amount of its investment in technology through financial institutions[33]. Table 4 displays the descriptive statistics of both the PIA and PINTEC variables.[34]

6 Empirics

In this section, I test the model's predictions. I focus on two sets of firm-level outcomes. First, I look at those that should be directly affected by the bankruptcy reform: firm access to finance and investment. Secondly, I look at those that should be indirectly affected by the bankruptcy reform: firm productivity — measured both in terms of labor productivity and TFP — as well as average wages and exit.

The core of my identification strategy is the differences-in-differences estimation presented in section 6.1. In section 6.2.1 I show the baseline results on capital investment. I show that the results are robust to controlling for a full set of initial conditions at the judicial district level and to using different samples of firms.[35] In section 6.2.2 I exploit sectoral heterogeneity in terms of dependence on external finance and asset tangibility to test whether the effect on investment runs through larger access to external finance.[36] In section 6.2.3 I provide evidence that, consistent with the model, firms operating under more efficient courts have higher chances to finance their investment in technology using external funds after the reform. They also invest more in technology and are more likely

[32]Specifically: 5% from training, 4% from acquisition of external knowledge, 8% from marketing and advertising, 11% from other costs associated with implementing new products/processes in the production chain.

[33]Financial institutions include both private banks and publicly funded programs of the BNDES, the Brazilian Development Bank.

[34]Notice that the average technological investment in the PINTEC sample is bigger than the average of capital investment in the PIA sample. This is due to two reasons: first, most of capital investment is considered technological investment under the PINTEC questionnaire. Second, and more importantly, the PINTEC sample is, on average, composed of bigger firms than the PIA sample.

[35]E.g., single-plant firms, firms above 30 employees that are selected with probability one in the survey, firms that do not change the location of their headquarters in the years under study.

[36]This is because the PIA survey does not provide information on loans taken by each firm. When I instead use data from the PINTEC survey my main outcome variable is firm access to external funds used to finance technological investment.

to introduce new products in the market. In section 6.2.4 I present the baseline results on firm productivity, wages and exit. In section 6.2.5 I look at heterogeneous effects across firm size distribution. In section 6.2.6 I show that the main results are not driven by different background trends across districts with different enforcement quality. Finally, because diff-in-diff estimation might suffer from district-level omitted variable bias, in section 6.4 I show that the main results hold when using an instrumental variable strategy.

6.1 Differences-in-Differences Strategy

I write a diff-in-diff model in which the congestion of courts in each judicial district (ψ_j) is the heterogeneous treatment to which firms are exposed at the time of the reform.

The baseline model is as follows:

$$y_{ijt} = \alpha_j + \lambda_t + \beta(\psi_j \times \text{post}_t) + \varepsilon_{ijt} \tag{9}$$

where:

$$\begin{aligned}
\psi_j &= \log\left(\frac{backlog}{judge}\right)_{j,2009} \\
\text{post}_t &= \begin{cases} 1 & \text{if year} > 2005 \\ 0 & \text{if year} \leqslant 2005 \end{cases}
\end{aligned}$$

In equation 9, y is an outcome of firm i operating in judicial district j at time t and $post_t$ is a dummy equal to 1 after 2005 and 0 otherwise. The interaction term between ψ_j — which captures court congestion[37] — and $post_t$ is meant to capture the treatment intensity of the reform. I add year fixed effects and judicial districts fixed effects to control for the two main effects of the interaction. Year fixed effects are meant to capture common aggregate shocks that each year hit all firms in the same way. Judicial district fixed effects control for characteristics of each judicial district that do not vary across time and might be correlated both with the congestion of their civil courts and the average performance of firms operating under their jurisdiction.

In a framework that combines firm-level data with district-level regressors, the adjustment of standard errors is a key issue when making statistical inference. This is because the error term might be composed by a location-year component along with the idiosyn-

[37] As discussed in section 3.2, here I use the cross-sectional variation in court congestion for 2009 — the first year for which data are available for all Brazilian judicial districts — as a proxy for court congestion in the pre-reform period. In Table 18 I show that, when limiting the sample only to those judicial districts for which data is available from the pre-reform period (the states of Rondonia and Mato Grosso do Sul), I get very similar coefficients on the main outcome variables using the 2004 or the 2009 level of court congestion.

cratic individual component (meaning: $\varepsilon_{ijt} = \eta_{jt} + \eta_{ijt}$). The judicial district fixed effects take out the average from η_{jt}, but its demeaned time variation might still be serially correlated. In order to correct the estimates for potential serial correlation within judicial districts, I use one of the solutions proposed by Bertrand et al. (2004) — to average the data before and after the reform and run equation 9 taking the dependent variables in first differences. I define the pre-reform years as the period 2003–2005 and the post-reform years as the period 2006–2009. These two periods are as comparable as possible in Brazil in terms of political and aggregate economic variables in Brazil.[38] I therefore estimate equation 9 as follows:

$$\Delta y_{ijt} = \alpha + \beta\psi_j + u_{ijt} \tag{10}$$

When estimating equation 10 I cluster standard errors at judicial district level to take into account correlation across firms within the same district.[39]

Estimating in first differences is the analog of taking out judicial district fixed effects. In fact, equation 10 also takes out firm fixed effects, implying that it also controls for time unvarying unobservable characteristics, not just at the judicial district level but also at the firm level.[40]

Fixed effects do not take care of district-level omitted variables that might correlate with court congestion and that, at the same time, might explain why firms benefited in different ways from the introduction of the reform. In Table 5, I check for systematic differences across judicial districts that are below and above the median level of court congestion. The table shows the average value of a set of covariates in the pre-reform period: average monthly household income, population, number of bank agencies, alphabetization rate of individuals above 10 years, number of manufacturing firms, agricultural share of GDP, and industry share of GDP.[41] Judicial districts above and below the median of court congestion are different in a number of dimensions and (surprisingly) similar in others. For example, I do not find statistically significant differences in terms of population, number of registered manufacturing firms, or number of bank agencies. In other words, there is no sign of initial selection of firms into districts with better court enforcement. Districts with more congested courts, however, seem to be on average richer

[38] The Lula's government has been in power from 2003 to 2010. Brazilian GDP experienced average growth of 3.3% a year between 2003 and 2005 and 3.7% a year between 2006 and 2009. Results are robust to different definition of the pre period (e.g. 2000-2005) or of the post period (e.g. 2006-2008)

[39] All results are robust to clustering at higher levels of aggregation with respect to the judicial district (e.g. micro-regions, macro-regions, state). This is because there might be spatial correlation across judicial districts.

[40] The relevant variation is within-firm between before and after the reform, ruling out compositional effects, namely better firms entering in better judicial districts after the reform. Notice also that firms are relatively stable in the same judicial district over time. In the 10 years panel of firms only 1.2 % of them changed judicial district.

[41] All covariates are measured in the year 2000, the year of the last Census before the reform.

(higher average household income per month), to have a more alphabetized adult population, and to have a local economy that relies more on industry than on agriculture. All of these are potential problems because a heterogeneous effect of the reform attributed to the congestion of courts might actually be due, for example, to poorer districts catching up in terms of per capita income at the same time that the reform was implemented.

To address this concern, I add to equation 10 a set of initial conditions at the judicial district level ($X_{j,t=0}$): average household income per month (which is strongly correlated both with alphabetization and industry share on GDP), population, and the number of bank agencies in each judicial district in the year 2000. Given that the number of controls I can insert in the model is limited, my estimates might still suffer from omitted variable bias. Thus, I propose an instrumental variable strategy in section 6.4. Finally, I add to the model a dummy identifying the existence of bankruptcy courts (δ_j) and sector fixed effects (σ_s) to control for different trends across firms operating in different industries[42] so that the final model I estimate is:

$$\Delta y_{ijst} = \alpha + \beta\psi_j + \gamma\delta_j + \xi' X_{j,t=0} + \sigma_s + u_{ijst} \tag{11}$$

6.2 Differences-in-Differences Results

6.2.1 Baseline Results on Capital Investment

Table 6 shows the main result of the paper, obtained from the OLS estimation of equation 11. In all columns, the outcome variable is growth in log capital investment computed as:

$$\Delta log(I_{ijst}) = \frac{1}{4}\sum_{t=2006}^{2009} \log(I_{ijst}) - \frac{1}{3}\sum_{t=2003}^{2005} \log(I_{ijst})$$

where i identifies the firm, j the judicial district, s the two-digit sector and t the year. The coefficient on court congestion is negative and significant in all specifications. A negative coefficient indicates that firms operating under less congested courts have experienced larger increases in investment since the reform was implemented. In column 1, where I include only court congestion — measured as number of pending cases per judge, in logs — as a regressor,[43] the estimated coefficient is - .048 (t = 3.72), implying that in judicial districts that are one standard deviation less congested, firms experienced 4.4% higher increase in capital investment. This is almost 20% of the average of $\Delta log(I_{ijst})$ observed in the data. In column 2, I only include the dummy for the existence of a bankruptcy court as a regressor. The coefficient is positive and significant. However, when I insert

[42]In all regressions, I exclude firms operating in CNAE sector 23: the oil-processing industry. Results, however, are not sensitive to their exclusion.

[43]Along with two-digit sector dummies.

both court congestion and the bankruptcy court dummy into the same regression (column 3), the coefficient on bankruptcy court becomes not statistically different from zero, while the coefficient on court congestion is stable and strongly significant.[44] In column 4, I add a full set of judicial district controls: average household income (in logs), total population (in logs), population density (units per squared kilometer), number of bank agencies (in logs), alphabetization rate of the adult population (in percentage points), number of manufacturing firms (in logs), and the industry share of GDP. All these controls are measured in the year 2000. Importantly, they do not affect the precision or the size of the coefficient on court congestion, which remains about .05 and strongly significant. Finally, I show that the results do not change when I restrict the sample to firms with at least thirty employees, those for which the survey covers the full population (column 4), to single-plant firms (column 5) and to firms that did not change location in the years after the reform (column 6). All the results presented in Table 6 are robust to using as independent variable a different measure of court congestion such as the congestion rate — the ratio of pending cases plus new cases, divided by the number of closed cases.[45]

6.2.2 Investigating the Financial Channel

In this section I test whether the effect of court congestion on investment found in section 6.2.1 respond to the mechanism emphasized by the model. The model suggests that after the reform secured creditors have an higher incentive to finance projects of firms operating under less congested courts. I test this mechanism by exploiting sector variation in terms of external finance dependence and asset tangibility.

If court congestion affects firm investment through the financial channel, then the effect should be larger for firms operating in more financially dependent industries. In column 1 of Table 7, I interact both court congestion and the bankruptcy court dummy with 2 dummies that identify firms operating in sectors above ($d_{ef=high}$) or below ($d_{ef=low}$) the median of dependence on external finance. The data on dependence on external financing at the sector level is from Rajan and Zingales (1998).[46] I estimate the following equation:

[44]In other words, it seems that court speed matters more than court specialization for firm investment. Previous research on debt recovery tribunals in India (Visaria, 2009) has shown that the introduction of specialized courts can decrease the cost of credit for firms by increasing the recovery probability of creditors. A key difference between the Brazilian and Indian experiences that may explain this discrepancy is that Brazilian specialized courts are not faster than normal civil courts. In fact, summary statistics in Table 3 show that specialized courts tend to be more congested than normal courts.

[45]Results available upon request.

[46]The authors construct a proxy of dependence on external financing for each industry at the ISIC three-digit level using a representative sample of US manufacturing firms from the Compustat database. Dependence on external finance of each firm is constructed as the ratio of capital expenditures minus cash flow from operations over capital expenditures. They use the industry median value across the 1980s as the proxy for each industry dependence on external finance. Although data comes from US firms, it has been widely used as a benchmark to capture the technological need for external finance of industries worldwide. See Ciccone and Papaioannou (2010) for possible biases coming from the use of industry

$$\Delta y_{ijt} = \beta_1(\psi_j \times d_{ef=high}) + \beta_2(\psi_j \times d_{ef=low}) + \gamma_1(\delta_j \times d_{ef=high}) + \gamma_2(\delta_j \times d_{ef=low}) + d_{ef=high} + \xi' X_{j,t=0} + u_{ijt} \quad (12)$$

The results show that the negative relationship between court congestion and firm investment is mostly driven by firms operating in more financially dependent sectors. The same result holds when adding judicial district controls.[47]

In column 3 of Table 7, I exploit variation in terms of asset tangibility at the sectoral level (in the corresponding US sector). The data on asset tangibility are from Braun (2003).[48] Previous work has shown that firms with larger tangible assets that can be repossessed by the investor in case of default suffer less from underdeveloped financial markets. Following this reasoning, Braun (2003) among others suggests that a financial reform should benefit more firms operating in sectors that rely less on tangible assets. However, when contract enforcement is so weak that even tangible assets are difficult to repossess (like in the pre-reform Brazil), then they do not make up a sufficient condition to sustain external finance. When this is the case, a financial reform that favors secured creditors is likely to benefit more firms operating in sectors that for technological reasons use more tangible assets that can also be used as collateral.[49] The results in columns 3 and 4 are consistent with this interpretation.[50] Capital investment after the reform increased more in sectors with higher asset tangibility, and the negative effect of court congestion on investment is mostly due to firms operating in these sectors. In Table 8 I use quartiles of dependence on external finance and asset tangibility to show how the negative effect of court congestion on investment increases the higher the quartile. Figure 8 displays the beta coefficients and 90% confidence intervals from this regression in a graph.

6.2.3 The Role of Technology

The model suggests that increase in access to finance allows firms to adopt more advanced technologies. In this section I test whether firms operating under less congested courts had higher chances to finance their investment on technology using external founds. To this end I use data from the PINTEC survey. The PINTEC survey contains detailed

characteristics from a benchmark country as a proxy for worldwide characteristics of that industry.

[47]The null hypothesis that the two coefficients are the same can not be rejected in 15% of the cases.

[48]The author uses Compustat data for the years 1986–1995 to compute the median asset tangibility of each ISIC three-digit sector in the US. Asset tangibility is defined as the ratio of net properties, plants, and equipments over the book value of total assets.

[49]In line with this reasoning, Gelos and Werner (2002) show that after an episode of deregulation in Mexico, firms' access to credit became more linked to the value of the real estate assets they could use as collateral.

[50]The specification is the same of equation 12.

information on firms' spending in technology as well as on how firms finance such spending. It does not contain information on the municipality in which each firm is located, just on the federal state. In this set of regressions, I therefore exploit variation at state level.[51] I estimate the following equation:

$$y_{izt} = \alpha_z + \lambda_t + \beta(\psi_z \times \text{post}_t) + \xi'(X_{z,t=0} \times \text{post}_t) + \varepsilon_{izt} \tag{13}$$

where y is a firm-level outcome of firm i operating in state z at time t, and $post_t$ is a dummy equal to 1 for year 2008 and 0 otherwise (this survey contains data for 2000, 2003, 2005 and 2008). The interaction term between the reform dummy and a measure of court congestion of state z at time t is meant to capture the treatment intensity of the reform. As in equation 9, court congestion is measured as the log of the number of pending cases per judge. I control on the right-hand side for the two main effects using year dummies (λ_t) and state fixed effects (α_z). To deal with initial conditions at state level I control in all regressions for GDP per capita.[52]

I focus on three outcome variables: use of external finance for spending in technology, spending in technology, and introduction of new products.[53] Table 9 displays the results of estimating equation 13 with OLS. The coefficient on the interaction of court congestion at state level with the reform dummy is negative and significant for access to external finance, log spending in technology and introduction of new products. The size of the coefficient on access to external finance implies that a firm operating in the 25th percentile of the distribution of log backlog per judge has an almost 1.3% higher probability of having access to external sources of financing for its technological investment after the reform than a firm in the 75th percentile of the distribution. This larger probability is about one third of the average increase registered in the data between before and after the reform: 9.4% of firms had access to external sources of financing for technological investment in 2005, 13.3 % in 2008. The coefficients remain negative and significant when I control for initial GDP per capita at state level interacted with the reform dummy.

6.2.4 Baseline Results on Productivity, Wages, and Exit

According to the model, the average productivity of firms should go up in less congested judicial districts after the reform. This happens for two reasons. First, in these districts, more firms can now adopt a more productive technology. Second, in these

[51] Even though the estimates are less precise in this case since they rely on the variation across 27 states, the advantage is that the data on pending cases and the number of judges at the state level is available from 2004, allowing me to use the pre-reform value in the explanatory variable.

[52] I do not had all controls at state level to avoid collinearity, given that I am exploiting a smaller variation in the independent variable in these regressions.

[53] Section 5 presents descriptive statistics.

districts, wages increase more, driving less productive firms out of the market.

In Table 10 I test these predictions. Notice that in all specifications I control for the existence of bankruptcy courts as well as for judicial district initial conditions and two-digit sector fixed effects. Columns 1 and 2 report the results for labor productivity and TFP. Labor productivity is measured as the log of firm value added per worker. TFP is estimated with the methodology proposed by Olley and Pakes (1996).[54] The coefficients on court congestion are negative and significant when the outcome is the increase in firm productivity measures. The size of the coefficients implies that a firm operating in the 25th percentile of the distribution of log backlog per judge has increased approximately 2.2% more in terms of labor productivity and TFP than a firm in the 75th percentile of the distribution of log backlog per judge. In column 3, I use as an outcome the growth rate of average wages. The coefficient on court congestion here is negative and significant, suggesting that districts with less congested courts experienced relatively higher increases in wages after the reform. Finally, in column 4, I test the effect on exit. Exit is a dummy coded one when the registration status of the firm switches from "active" to "not active." The model predicts a negative sign for this coefficient. Column 4 shows that this effect, on average, is not different from zero in the full sample. In section 6.2.5 I show that when looking at the heterogeneous effects across the firm-size distribution, the effect of court congestion on exit is negative and significant for small firms.

In Table 11, I test whether the effects on productivity, wages, and exit are stronger in more financially dependent sectors. The specification I run is the same as equation 12. The effect of court congestion on labor productivity, TFP, and wages is increasing with sector dependence on external finance. Court congestion is also more precisely estimated and always significant when interacted with the fourth quartile. To more clearly illustrate this result, Figure 9 shows the beta coefficients (and 90% confidence intervals) of the interaction between court congestion and the dummies for each of the four quartiles of external financial dependence.

6.2.5 Heterogeneous Effects across Initial Size Distribution

The model predicts that court congestion should affect firms across the pre-reform productivity distribution differently. In particular, firms in the middle of the pre-reform productivity distribution should be those benefiting more in terms of investment and productivity. Due to their higher labor demand, the reform should increase wages for all firms, and relatively more so in districts with better enforcement. Higher wages should drive out of the market firms in the lower tail of the pre-reform productivity distribution. Again, this effect should be stronger in district with better enforcement.

[54] Results are robust to the use of different methodologies to estimate firm productivity, such as the one proposed by Levinsohn and Petrin (2003).

To test these predictions, I estimate the effect of court congestion interacted with each quartile of the firm-size distribution in the pre-reform period. As in Melitz (2003), in my model there is a one-to-one mapping between size and productivity.[55] As a proxy of firm initial productivity, I use the value of their assets in the pre-reform period.[56] My prior is to find stronger effects for mid-size and large firms with respect to small firms for variables that the model predicts should have heterogeneous effects across firms (e.g., capital investment and productivity), to find similar effects for all firms for variables that the model predicts should move due to general equilibrium effects (e.g., wages), and to find larger effects on small firms when looking at exit. I run the following equation:

$$\Delta y_{ijt} = \alpha + \sum_{q=1}^{4} \beta_q(\psi_j \times \text{size}_q) + \sum_{q=2}^{4} \nu_q(\text{size}_q) + \gamma\delta_j + \xi' X_{j,t=0} + \sigma_s + u_{ijt} \tag{14}$$

The coefficients of interest in regression 14 are the β_qs on the interaction between court congestion and firm-size quartile dummies (size_q). Table 12 displays the results of estimating equation 14 with OLS. Column 1 shows that the effect of court congestion on capital investment is negative and significant for firms in the second, third, and fourth quartiles. In particular, the stronger effect of court congestion is on firms in the third quartile of the pre-reform size distribution. The coefficient on the interaction of court congestion with the third quartile dummy is -0.110 (t=3.19), twice as large as those on the interactions with the other quartile dummies. I get a similar pattern, though with less precisely estimated coefficients, when the outcomes are labor productivity and TFP. In column 4, when I test the effect on wages, I find negative and significant effects in the first, third, and fourth quartiles. Finally, in column 5, I show that court congestion has a significant impact on exit for firms in the first quartile, indicating that less productive firms are more likely to exit after the reform in districts that have better enforcement.

6.2.6 Effects over Time and Background Trends

A standard concern with diff-in-diff estimation is that the results are capturing different pre-existing trends between firms that operate under more versus less congested courts.[57] Because my sample includes multiple years, I can check whether court conges-

[55]This comes from the fact that the ratio of any two firms' output (or revenues) only depends on the ratio of their productivity levels.

[56]To construct size quartiles, I use the residuals of a regression of log tangible assets at the firm level on two-digit sector dummies. This accounts for differences in the minimum scale of firms that could exist across sectors.

[57]The diff-in-diff regression framework relies on the key assumption of common trends across groups. With random assignment, this assumption is always verified because firms have the same probability of being assigned to any group. In absence of random assignment, it is possible that each group follows a different time trend due to its own characteristics.

tion predicts firm-level outcomes before the bankruptcy reform was implemented. I do a falsification test in which I assume that the reform was implemented in 2002 instead of 2005. I then test whether court congestion has more predictive power on firm-level outcomes when I pick the right or the wrong timing of the reform.[58] Table 13 shows the results. In Panel A I report the coefficients on court congestion when the outcomes are growth in investment, labor productivity, TFP, and wages between the years 2000–2001 and 2002–2004. In Panel B I report the coefficients on court congestion for the same outcomes between the years 2003–2005 and 2006–2009. All regressions include industry fixed effects as well as initial conditions and the dummy for the existence of bankruptcy courts.

The coefficients on capital investment, labor productivity, and wages on court congestion are not significant when I assume that the reform was implemented in 2002. The coefficient on TFP instead is negative and significant at 5% (though half the size of the corresponding coefficient in Panel B). With the exception of capital investment, I can not rule out that these pairs of coefficients are statistically different. I therefore take a second approach and interact court congestion along with all the controls with a full set of year dummies in the following regression:

$$\begin{aligned} y_{ijt} &= \alpha_i + \lambda_t + \sum_{t=2000}^{2009} \beta_t(\psi_j \times \lambda_t) + + \sum_{t=2000}^{2009} \gamma_t(\delta_j \times \lambda_t) \\ &+ \sum_{t=2000}^{2009} \xi'(X_{j,t=0} \times \lambda_t) + u_{ijt} \end{aligned} \tag{15}$$

Figures 10 plots the β_ts on the interactions between log backlog per judge and year dummies along with the 90% confidence intervals of the coefficients when the outcome variables are firm labor productivity (upper graph), TFP (middle graph), and average wages (lower graph). When the outcome is labor productivity, the β_ts are not statistically different from zero in all of the interactions with year dummies from 2000 to 2005, but they become negative and statistically different from zero for (almost) all the years after the reform, from 2006 to 2009. The break in the trend of the β_ts and their significance is even stronger when the outcome is TFP. As for wages, the coefficients are positive until 2005 — indicating more congested districts used to have higher wages — and then decline starting from 2005. Figures 10 is a robust sanity check that the data are picking the right timing of the reform and that the results presented in Table 10 are not driven by background trends.

As an additional robustness check, I test whether these trends are driven more by high

[58] Verhoogen (2008) used a similar strategy.

financially dependent sectors versus low financially dependent sectors. I run a regression similar to equation 15 but with a triple interaction of court congestion, industry dummies, and a full set of year dummies (along with all the cross-products):

$$\begin{aligned} y_{ijt} &= \alpha_i + \sigma_s + \lambda_t + \sum_{t=2000}^{2009} \beta_t(\psi_j \times \sigma_s \times \lambda_t) + \sum_{t=2000}^{2009} \mu_t(\psi_j \times \lambda_t) \\ &+ \sum_{s=1}^{S} \nu_s(\psi_j \times \sigma_s) + \sum_{t=2000}^{2009} \gamma_t(\delta_j \times \lambda_t) + \sum_{t=2000}^{2009} \xi'(X_{j,t=0} \times \lambda_t) + u_{ijt} \end{aligned}$$

Figure 11 shows, as an example, the βs on the triple interaction across time for two industries: an industry with low financial dependence, "Food and Beverages Manufacturing" (representing 19.6% of manufacturing GDP), and an industry with high financial dependence, "Electrical Machineries, Equipment and Supplies Manufacturing" (representing 2.7% of manufacturing GDP). The time-trends of the βs for the two sectors are similar in the pre-reform period. After 2005, however, the βs on the triple interaction with the high industry with high financial dependence become negative and significantly different from zero while the βs on the triple interaction with industry with low financial dependence remain not different from zero.

6.3 Preliminary Investigation of Aggregate Implications

In this section I address the following question more systematically: what would the performance of Brazilian firms have been if the level of court congestion had been different at the time of the bankruptcy reform?

I analyze the average increase of firm-level outcomes under four counterfactual scenarios. In each scenario, the level of court congestion is set at a different level for all courts. First, I set it at its average in the first quartile of court congestion (1,257 pending cases per judge), then at its average in the fourth quartile of court congestion (9,807 pending cases per judge), then at its minimum value among all courts (30 pending cases per judge), and finally at its maximum value among all courts (17,273 pending cases per judge).[59]

Table 14 displays the results of the estimation under the four counterfactual scenarios. The first column shows the average increase of each outcome variable between the pre-reform years (2003–2005) and the post-reform years (2006–2009). Columns 2 to 5 show the average increase of each outcome variable predicted by the model had the level of court congestion been different at the time of the reform.

[59] Data on the judiciary is winsorized at 1% level to control for extreme observations. As a consequence, minimum and maximum values of court congestion are, more precisely, the 1st percentile and the 99th percentile of backlog per judge.

The first row shows the results for capital investment. The average increase in capital investment between 2003–05 and 2006–09 was 23% for the firms in the sample. Had courts been on average less congested, say as congested as the average court in the first quartile, the average increase in capital investment, according to the model, would have been 29%, or 6% higher than observed. Had courts been on average more congested, say as congested as the average court in the fourth quartile, the average increase in capital investment would have been 17.8%, or 5.2% lower than observed. Columns 4 and 5 display the results for the extreme cases in which all courts are set to the level of congestion of the more and the less congested courts. The table shows also the results when the same exercise is done with value added, value added per worker, and TFP.

6.4 Instrumental Variables Strategy

In this section I present an instrumental variable strategy that corroborates the main diff-in-diff results. Diff-in-diff coefficients might be biased due to endogeneity. One possible source of endogeneity is given by judicial-district-level covariates correlated with court congestion that are unobservable (e.g., the level of trust, the accountability of local political institutions) and might affect firm-level outcomes. The stability of diff-in-diff coefficients when adding judicial-district controls suggests that omitted variables might not be a major concern. Another possible source of endogeneity is reverse causality. Table 5 shows that districts with more congested courts tend to be more industrialized. If the reform fosters firm growth and labor demand relatively more in less congested districts, this increase in industrial activity could also affect the congestion of civil courts (i.e., the independent variable). If firm-level outcomes affect positively court congestion, then the β coefficient estimated with OLS in the diff-in-diff model is biased upward. For example, the estimated β coefficient on court congestion when the outcome is the increase in firm capital investment is around -.05. If the OLS coefficient is biased upward, the "true" effect of court congestion on capital investment might be still negative but larger in absolute value.

6.4.1 Relevance of the Instrument

In the framework of this paper, a valid instrument must explain part of the variation in court congestion across judicial districts and must have no effect on firm-level outcomes other than through court congestion.

To construct my instrument I exploit the state-level rules on the administrative boundaries of judicial districts. From the 1970s, and mostly after the approval of the 1988 Federal Constitution, Brazilian states started to introduce laws to organize their judicial machineries. These laws established the minimum requirements that a municipality had to satisfy in order to form a judicial district. These requirements were usually expressed in terms of observable characteristics of each municipality, like the number of inhabitants, the area in square kilometers, the number of voters in the last election, or a combination of these. For example, Law 3,731 (1979) of the state of Bahia established that a municipality must have a population of at least 20,000 and an area of at least 200 km^2 to form a judicial district. A municipality whose population or area were below these thresholds at the time the law was implemented had to merge with the judicial district of a neighboring municipality whose population and area were above these thresholds.[60] It was only the administration of the judiciary that was merged: all other administrative and political prerogatives granted by the federative system remained with the municipal government.

In several cases, these rules were likely constructed with an eye toward the juridical division already in place. However, not all pre-existing cases could be accommodated, and, at least for some municipalities, these laws led to the creation of new judicial districts or the dismantling and merging of others. When this happened, the courts of municipalities above the thresholds received all judicial cases produced in neighboring municipalities below the thresholds. In other words, for each municipality above the thresholds, the size and population of its neighboring municipalities at the time the law was passed determined the amount of extra workload for its judges. I exploit this initial shock in the workload of courts as an instrument for today's court congestion (the endogenous independent variable). I measure this extra initial workload as the total population of neighboring municipalities below the thresholds at the time state laws on the formation of judicial districts were implemented.

6.4.2 Exclusion Restriction

For the exclusion restriction to hold, the population of neighboring municipalities below the thresholds to become an independent district should not affect firm-level outcomes, conditional on observables, other than through court congestion. The size and population of neighboring municipalities can certainly affect firm performance. Highly populated neighboring municipalities tend to be more vibrant economic centers and potentially more interesting markets than uninhabited ones. In addition, more densely populated areas might attract the the most skilled workers and foster innovative activities.

The key for the exclusion restriction to work is that the instrument must be independent from firm-level outcomes "conditional on observables." I include in all IV regressions the total population of *all* the neighbors of each municipality, both those below and those above the thresholds established by law. The exclusion restriction relies on the fact that there is no additional relevant information for firm-level outcomes brought by the popu-

[60]Alternatively, a municipality below the thresholds could have formed a judicial district with other neighboring municipalities that also did not fulfill the requirements.

lation of neighbors below the thresholds established by law that is not already brought by the total population of all neighbors, apart from the information that run through the way judicial districts were established.

6.4.3 First Stage Results

In all the following regressions, I restrict the sample to those municipalities that are the seat of a judicial district, because these are the only municipalities that could potentially be independent. The dependent variable is the log of pending cases per judge. The instrument is defined as:

$$Z = log\left(\sum_{n=1}^{N_m^{below}} \text{pop}_{t=\text{year law}}\right)$$

where:

$$N_m^{below} = \text{subset of neighbors of municipality } m \text{ below min.req. at time } t = \text{year law}$$

I focus on the population in the year in which the law was passed (or the closest year available).[61] Out of the 2,519 judicial districts for which I have data on court congestion, I am left with 1,210 municipalities that are the seats of judicial districts, that have available population data for the time of the law, and that have at least one manufacturing firm operating.

The baseline first stage regression is:

$$\log\left(\frac{backlog}{judge}\right)_{mj} = \alpha + Z + log\left(\sum_{n=1}^{N_m^{all}} \text{pop}_{t=\text{year law}}\right)_m + \varepsilon_m \tag{16}$$

Table 15 presents the results. In all of the specification I weight each municipality by the number of manufacturing firms operating in it. In the first column I regress court congestion on the instrument alone. This coefficient tells us that municipalities with more populated neighbors below the thresholds today have more congested courts.[62] In column 2, I present the results of estimating equation 16, i.e., I add as a control the log population

[61] Obviously, I exclude neighboring municipalities that do not belong to the same state.

[62] Explaining why the supply of judges in each judicial district on average does not respond in an effective way to the larger number of cases brought by additional municipalities is beyond the scope of this paper. One possible explanation is that there is a systematic lack of administrative organization when decisions encompass more than one municipality. Note that judicial districts (*comarcas*) are a fictitious territorial division that has no other purpose than the administration of justice. It is also possible that there is a fixed cost in terms of caseload that the local government of each municipality brings with itself due to the cases against, or started by, the local government itself. Local governments, in Brazil, are, in fact, some of the major litigants in civil courts.

of all the neighbors of each seat of a judicial district. In columns 3 and 4, I add controls for observables. The coefficient on the instrument is relatively stable and strongly significant in all specifications. Importantly, the F-test for the excluded instrument indicates that the instrument is not weak when compared to standard critical values (Stock and Yogo, 2002).[63]

As an additional robustness check, I show that not every subsample of neighboring municipalities conveys information on the congestion of the judiciary. To prove this, I perform the following test. For each seat of a judicial district, I generate a random sample of its neighbors. I allow any of the neighbors, no matter whether above or below the thresholds, to be selected into this sample. For each seat, the size of the sample is set equal to the number of neighbors below the thresholds at the time the law was passed. This allows me to create a version of the instrument in which the same number of neighbors for each municipality is chosen by random assignment instead of following the actual rules established by each state. The aim of this exercise is to see whether the instrument has more power than its random counterpart in explaining court congestion. In Table 16, I show the results. Column 1 repeats the regression run in column 3 of Table 15. In column 2, I use the random counterpart of the instrument. The coefficient on this random counterpart, reassuringly, has no explanatory power on court congestion.

6.4.4 Second Stage Results

The two-stage least-squares (2SLS) coefficients come from the following equation estimated with OLS:

$$\Delta y_{imt} = \beta \hat{\psi_m} + \xi' X_m + \varepsilon_{imt} \tag{17}$$

where:

$$\hat{\psi_m} = \hat{\rho} Z_m + \hat{\xi}' X_m \tag{18}$$

where ψ_m is court congestion – the endogenous regressor – in municipality m (and m is the seat of a judicial district), Z_m is the instrument, and X_m is a set of controls including the total population of all the neighbors of municipality m.

Table 17 shows the results of estimating equation 17 and compares them with the estimates obtained from an OLS regression on the same sample. The 2SLS coefficients

[63] One important caveat to the IV strategy is that in all weighted regressions, I exclude the municipality of São Paulo. Unweighted IV results are not sensitive to the inclusion of this municipality, but when weighting by number of firms, the inclusion of São Paulo – about 10% of firms registered in Brazil – affects sign and significance of the results.

on capital investment and TFP growth are negative and significant (though TFP is only marginally significant). The point estimates tend to be larger in absolute value than the OLS estimates on the same sample: -.142 versus -.037 for investment and -.062 versus -.015 for TFP, suggesting that the "true" negative effect of court congestion could be larger than what suggested by the OLS estimates. However, restricting the sample to those firms for which the instrument is available make all the estimates less precise, and the difference between OLS and 2SLS coefficients is not statistically significant. Finally, note that when I run the OLS regression restricting the sample to those firms for which the instrument is available, I do not get the negative and significant result on wages.

7 Conclusions

In this paper, I empirically assess how the quality of court enforcement shapes the impact of a financial reform on firms. To identify this effect, I exploit the introduction of a pro-creditor bankruptcy reform and the variation in court congestion across Brazilian judicial districts. To deal with district-level omitted variables, I propose an instrument based on the geographical boundaries of judicial districts. The IV strategy confirms the main OLS results: firms operating under less congested courts experienced larger increase in investment and productivity after the introduction of the reform. These findings are consistent with a simple general equilibrium model in which heterogeneously productive firms must borrow to finance their investment in technology adoption. This is the first paper that studies the relationship between legal protection of creditors and the quality of court enforcement at the micro level. Unlike previous literature, I focus on firm productivity as an outcome.

The main result of the paper is that in Brazil inefficient court enforcement has substantially limited the potential benefits of a major financial reform. This result has relevant implications because a large fraction of Brazilian manufacturing firms operates under inefficient courts. About half of the firms in my sample operate under courts where the average length of a civil case is more than four years, and more than 10% operate under courts where the average length of a civil case is above ten years. In this framework, the aggregate effects of judicial inefficiencies are substantial. A simple quantification suggests that if all civil courts operated at the level of efficiency observed in the first quartile of court congestion, the country would have experienced a 2.5 percentage points larger increase in manufacturing value added during the years under study.[64]

Even though the magnitude of these results is specific to the Brazilian case, the lack of an efficient, fair, and predictable judicial system is a common problem among developing countries (Dakolias, 1999). At the same time, several developing countries have recently

[64]Unfortunately, it is hard to give a plausible estimate of the corresponding costs.

introduced, or are about to introduce, reforms of their investor protection laws. China and Russia introduced new bankruptcy laws inspired by the US code in the last decade. A similar reform of bankruptcy is under discussion in India. The results presented in this paper suggest that inefficient courts can undermine the benefits of such reforms. Moreover, the costs related to inefficient enforcement of credit recovery cases are likely to soar in the years to come should the current financial crisis contaminate the financial sectors of developing countries.

This paper also informs the debate on the sequencing of economic reforms in developing countries. Caselli and Gennaioli (2008) advocate that financial reforms — like bankruptcy law reforms — should come first[65] because they favor the reallocation of resources to their more talented users. This paper makes the case that this is true as long as judicial institutions in place are able to enforce financial reforms. I show that an efficient judiciary is a necessary precondition for the private sector to benefit from these reforms. According to Hay et al. (1996), changing the way the judicial system works takes longer than modifying legal rules. Many developing economies, however, tend to simply "follow the rich ones and introduce elaborate bankruptcy procedures" that their courts can hardly enforce (Djankov et al., 2008). Finding the right balance between promoting necessary changes in legal rules and investing to make the judicial institutions in charge of enforcing them more efficient is one of the major challenges faced by governments in developing countries.

[65]In particular, before deregulation, intended as the removal of set-up costs.

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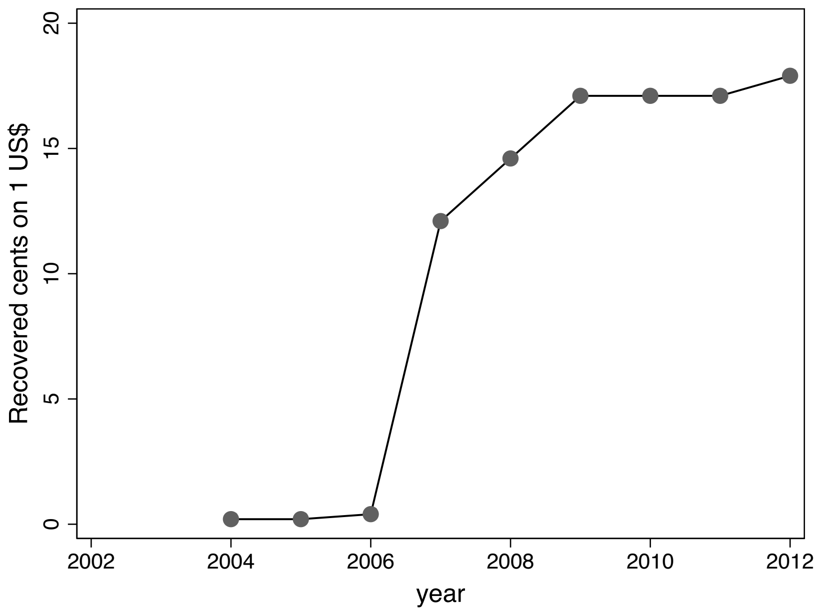
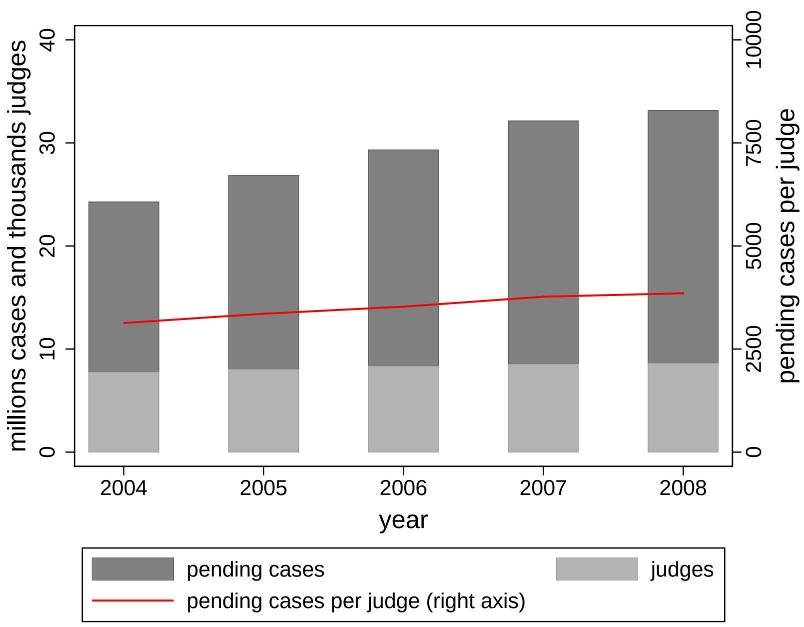
Figures and Tables

Figure 1: Recovery Rate in Brazil over time



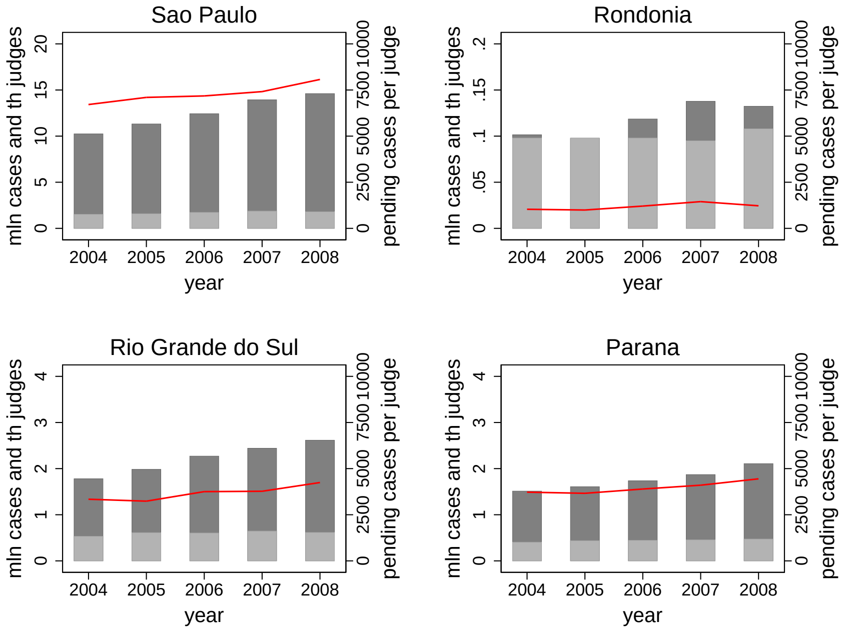
Notes: Data from the World Bank Doing Business database.

Figure 2: Trend in Pending Cases per Judge in Brazil



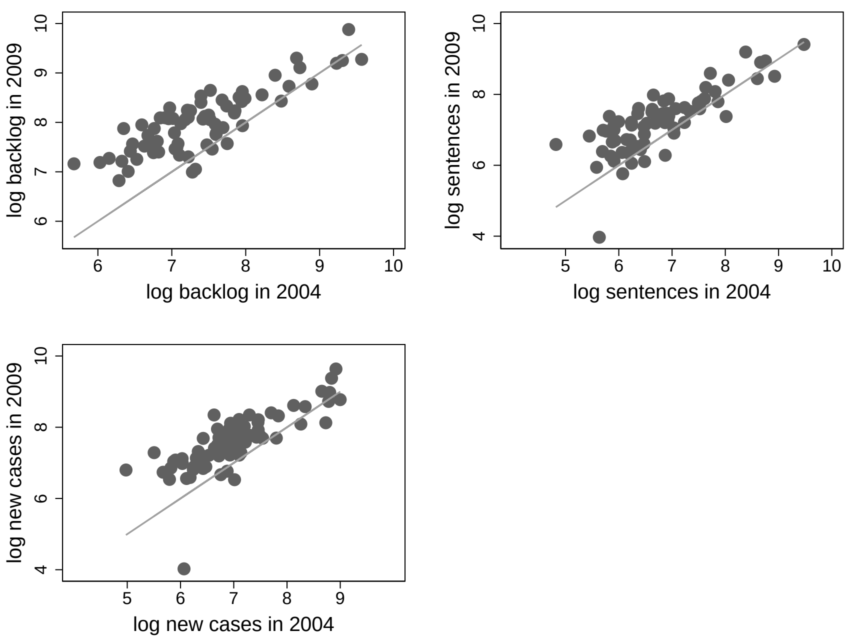
Notes: Data from the Conselho Nacional de Justica (CNJ).

Figure 3: Pending Cases per Judge across Federal States



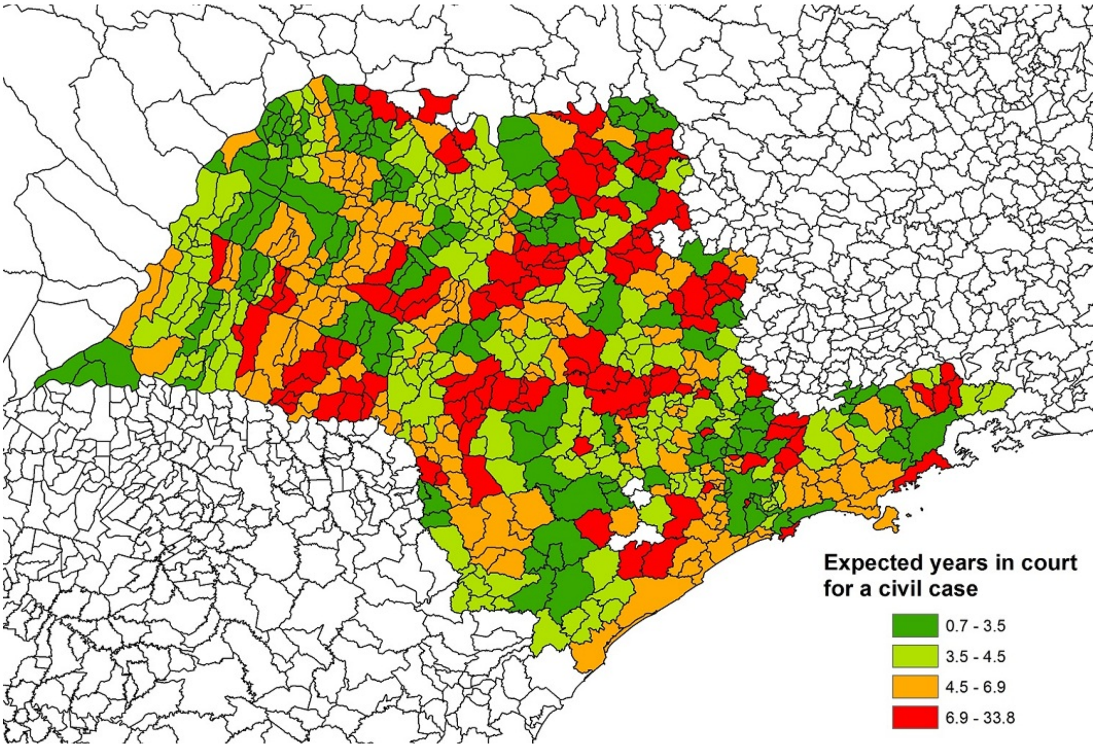
Notes: Data from the Conselho Nacional de Justica (CNJ).

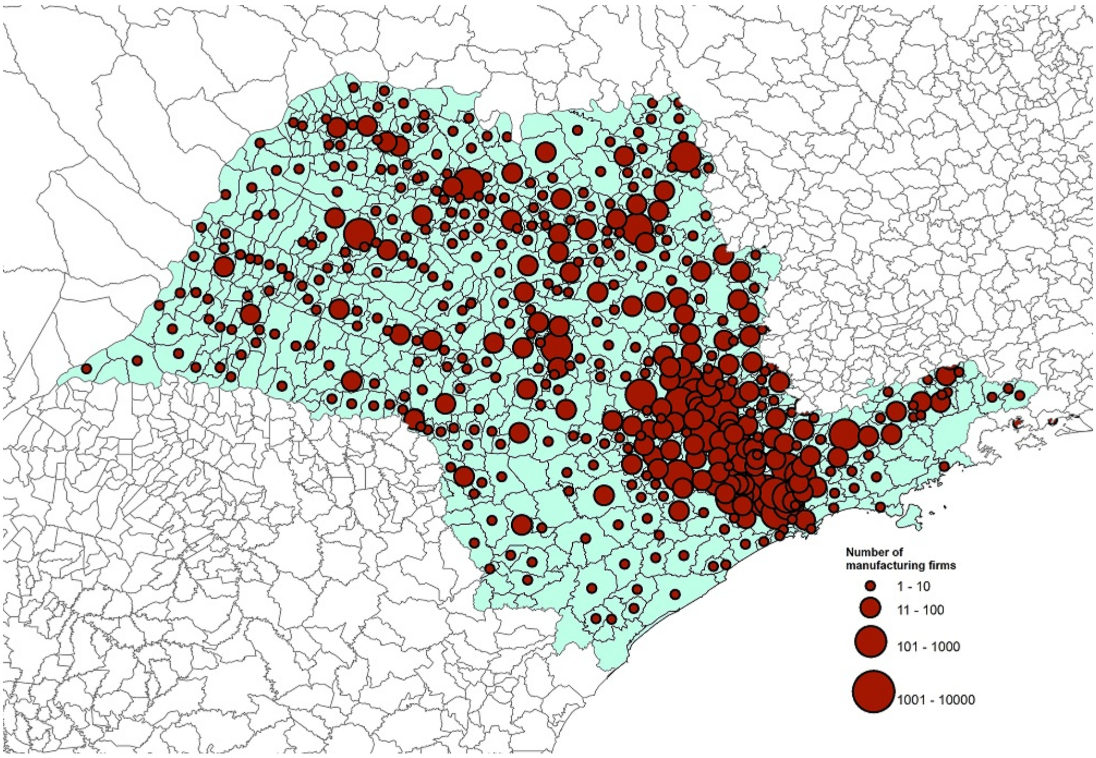
Figure 4: Comparing Judicial Variables in 2004 and 2009



Notes: Data from the Conselho Nacional de Justica (CNJ) and State Tribunals of Rondonia and Mato Grosso do Sul.

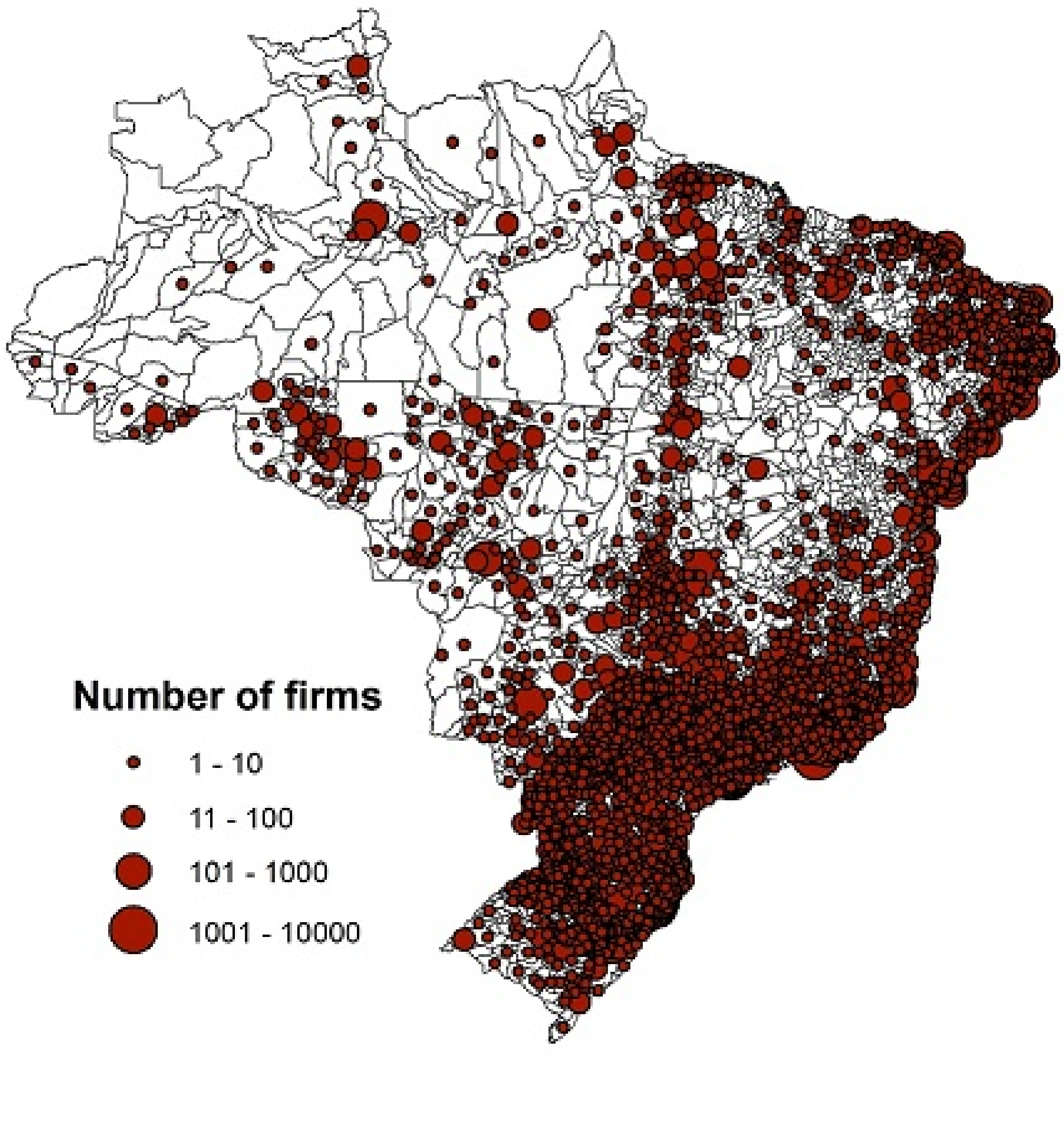
Figure 5: State of São Paulo: Congestion of Civil Courts (upper graph) and Location of Firms (lower graph)





Notes: Data on court congestion comes from the Conselho Nacional de Justica (CNJ), data on firm location comes from the Annual Industrial Survey (PIA) and refers to the year 2005.

Figure 6: Location of Firms across Brazil



Notes: Data on firm location comes from the Annual Industrial Survey (PIA) and refers to the year 2005.

Figure 7: Profit Functions and Productivity Thresholds

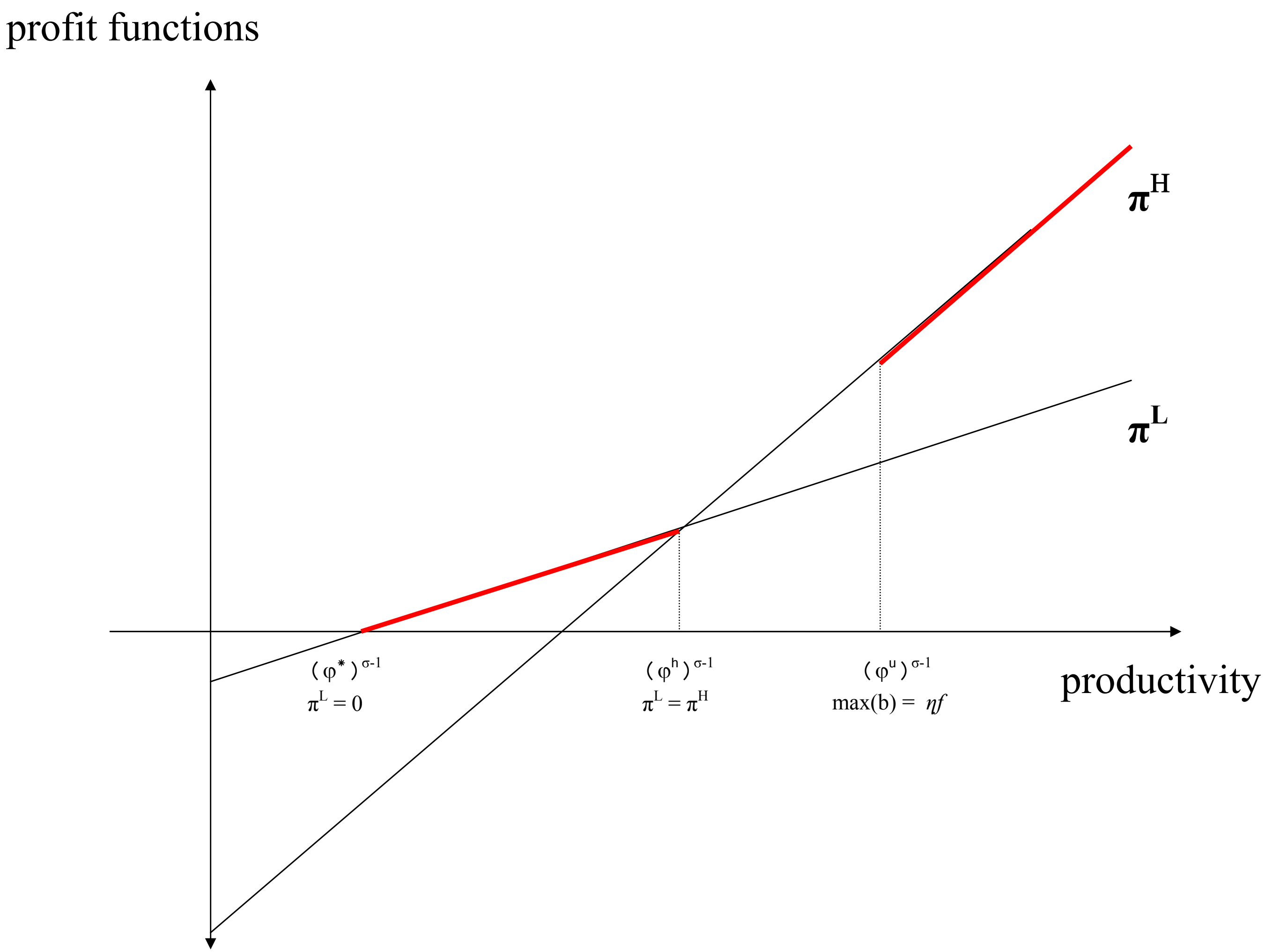
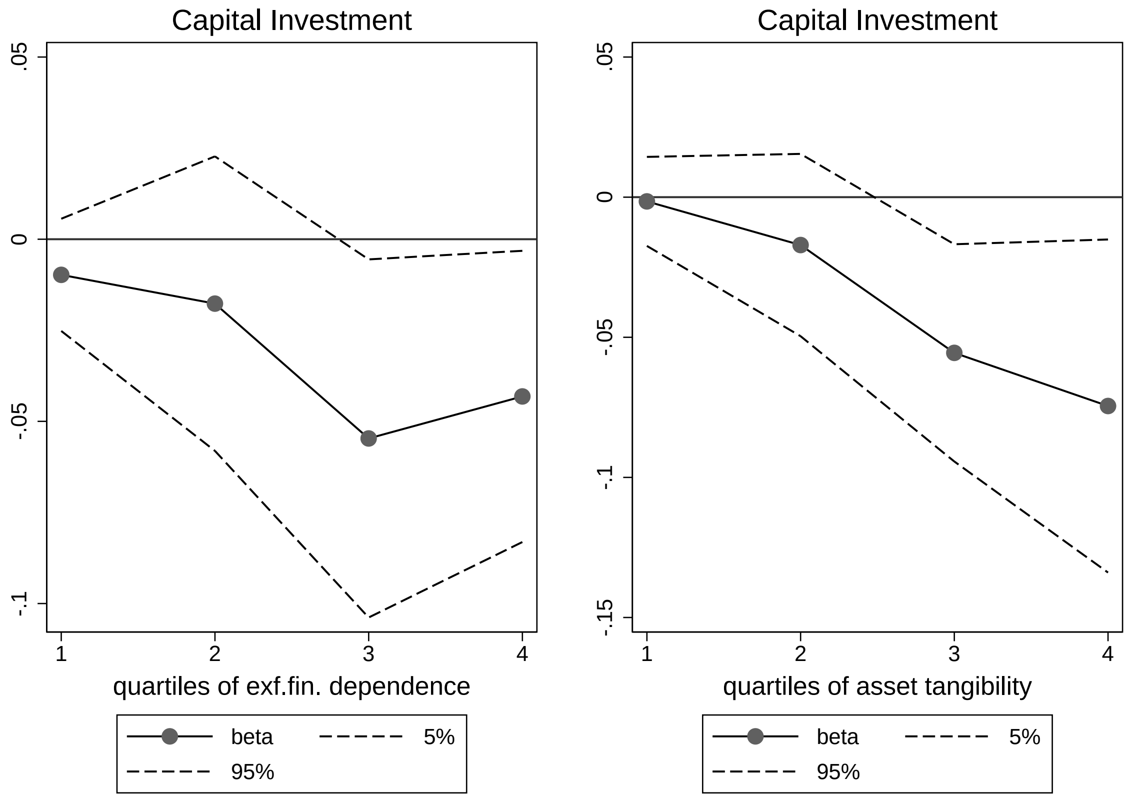
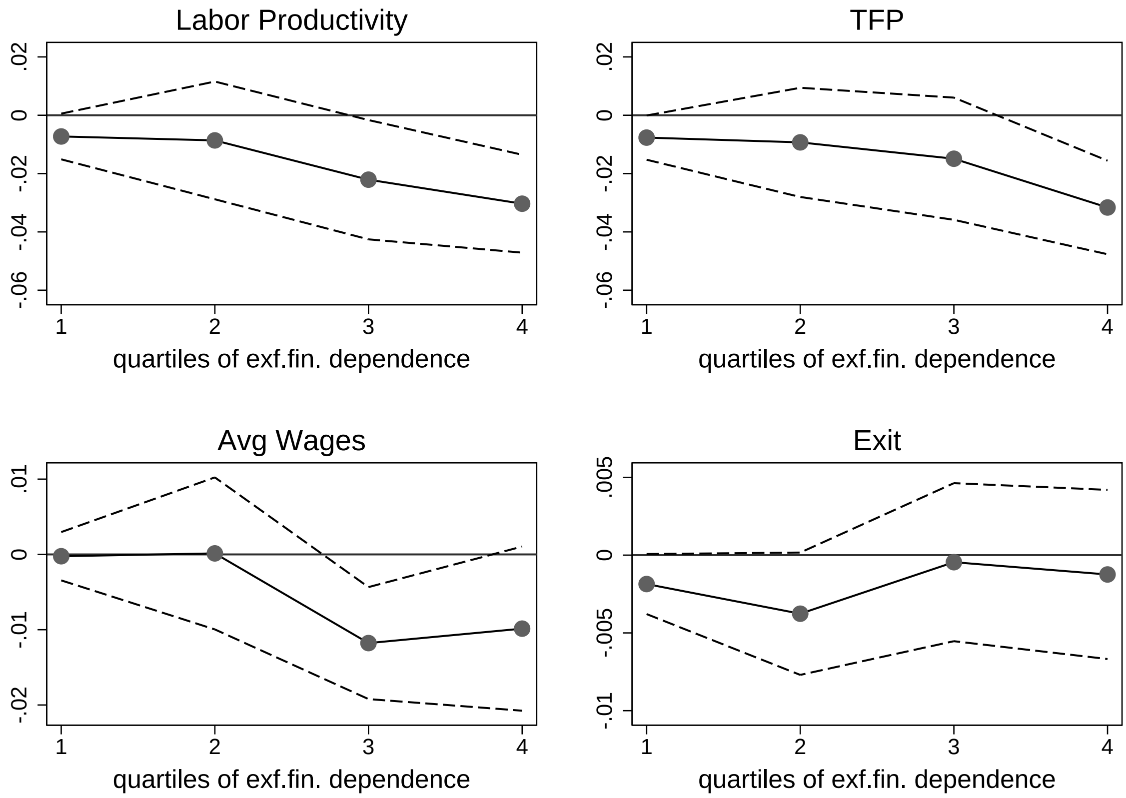


Figure 8: The Effect of Court Congestion on Investment across Quartiles of Financial Dependence and Asset Tangibility



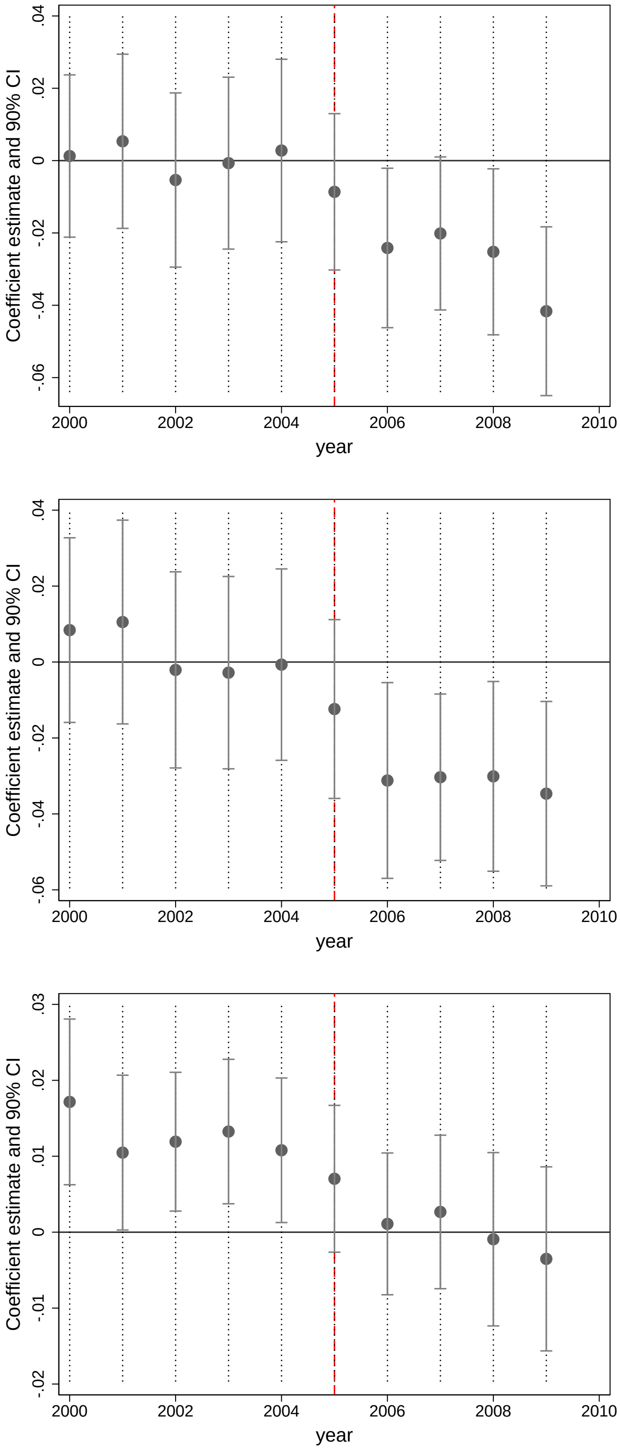
Notes: Dots are beta coefficients and dashed lines are 90% confidence intervals of two regressions where the dependent variable is growth in capital investment and the independent variable is court congestion interacted with quartiles of sector financial dependence (left figure) and asset tangibility (right figure). Both regressions include all the controls at judicial district level used in Table 6.

Figure 9: The Effect of Court Congestion on Firm-Level Outcomes across Quartiles of Financial Dependence



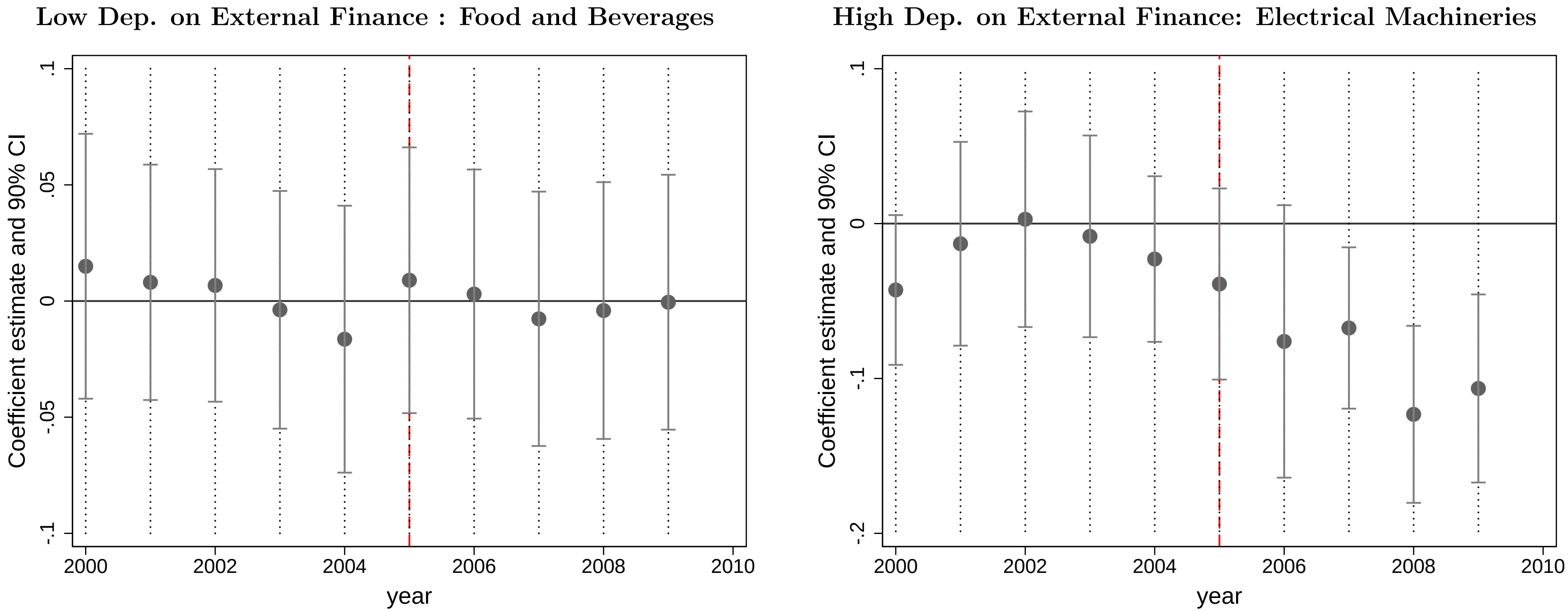
Notes: Dots are beta coefficients and dashed lines are 90% confidence intervals of two regressions where the dependent variables are labor productivity (upper left), TFP (upper right), average wages (lower left) and exit (lower right). The independent variable is court congestion interacted with quartiles of sector financial dependence. All regressions include all the controls at judicial district level used in Table 6.

Figure 10: The Effect of Court Congestion on Labor Productivity, TFP and Wages over Time



Notes:The figure depicts the coefficient estimates and the 90% confidence intervals from three regressions where the dependent variable is, respectively: Labor Productivity (upper graph) , TFP (middle graph) and average wages (lower graph). The explanatory variable is the log of backlog per judge in courts dealing with bankruptcy cases interacted with a full set of year dummies. The dashed red vertical line indicates the year when the bankruptcy reform was implemented. All regressions include the full set of controls present in Table 6 interacted with year dummies as well as firm and year fixed effects. Standard errors used to construct confidence intervals are clustered at judicial district level.

Figure 11: Effect of Court Congestion on Labor Productivity over Time and across Industries



Notes: The figure depicts the coefficient estimates and the 90% confidence intervals from a regression where the dependent variable is Labor Productivity. The main explanatory variable is courts' congestion (measured as log backlog per judge) in courts dealing with bankruptcy cases interacted with 23 sectoral dummies (each identifying a different financial dependence parameter) and a full set of year dummies. Results are plotted for two sectors: Food and Beverage Manufacturing and Electrical Machineries, Equipment and Supplies Manufacturing. The dashed red vertical line indicates the year when the bankruptcy reform was implemented. The regression includes include the full set of controls present in Table 6 interacted with year dummies as well as firm and year fixed effects. Standard errors used to construct confidence intervals are clustered at judicial district level.

Table 1: Comparing Brazilian Old Law, New Law and US Bankruptcy Code

	Brazil Old Bankruptcy Law (1945)	Brazil New Bankruptcy Law (2005)	US Bankruptcy Code
Liquidation	- Secured creditors are not entitled to the proceeds from the sale (or the simple appropriation) of the collateral securing debt. All collateral proceeds are pooled together with the assets of the debtor and distributed to creditors according to their ranking of priority. The sale of the assets itself can only take place only after the general list of creditors is compiled by the judge		Chapter 7: Secured creditors are outside the priority ordering: they have a legally enforceable right to the collateral securing their loans or to the equivalent value, a right which cannot be defeated by bankruptcy. Firms' collateralized assets are not pooled with other assets in order to pay the other creditors.
	- Unlimited labour and tax claims have priority over secured creditors.	- Gives secured creditors priority over tax claims, impose a cap of 150 minimum wages for each claim on previously unlimited labour claims. This avoid tunneling and strategic bankruptcy (remember that owner is considered a worker for Brazilian regulation).	
	- Successor liability for labour and tax claims in case of selling business units. If a distressed firm sell units to recapitalize, the liabilities for labour and tax claims are transferred to the new buyer. Price had to adjust for the due diligence to find out all the outstanding debt, the risk of unknown debt and the costs associated with paying the known debt.	- Eliminate successor liability when selling business units or the full business as a going concern. Article 141 establishes that tax, labour and social security claims remain as liabilities of the debtor and are no longer passed on to the purchasers in liquidation. The *lei complementar* 118/2005 amends the tax code and eliminate the successor liabilities when assets are sold from a debtor's estate.	- Bankruptcy courts can order that assets of the bankruptcy estate are sold "free and clear" of liabilities either pursuant art 363(F) of the bankruptcy code (liquidation) or as a part of a Chapter 11 restructuring plan (section 1141(c)). However, problems might come for claims that have not yet arise at the time of the sale
Judicial reorganization	- Available in the form of a 2 years installment plan only applicable to unsecured debt (*concordata (suspensiva or preventiva)*. Most of these installment plans ended up in liquidation, since suppliers (the usual unsecured creditors) had low incentives to keep providing inputs to a distressed firm.	- Automatic Stay: debtor is protected by the court from legal action from other creditors for a period of 180 days (time to present a restructuring plan), otherwise bankruptcy is started. - Creditors' committees: the three classes of creditor (labor/secured/unsecured) can discuss and approve or refuse the restructuring plan. If one class do not approve the judge has the power to impose the plan anyway (crawdown). - Debtor in possession financing: creditors providing new liquidity post-bankruptcy enjoy absolute priority - Successor liability: removed successor liability for labour and tax claimsw when selling business units, branches, or isolated productive units.	Chapter 11: - Automatic stay: all litigation against the debtor are stayed (put on hold) until they can be resolved in bankruptcy court. Debtor has usually 120 days to propose a plan. If the plan is not approved the firm is liquidated under chapter 7. - Creditors' committee: debtor's plan must be confirmed by the three classes of creditors (labor/secured/unsecured). If one class votes against the plan can be confirmed anyway by the judge (cramdown power) provided there is no discrimination against that class. - Debtor in Possession Financing: new lenders enjoy first priority. - Successor liability as in chapter 7
Out-of-court Restructuring	Not available	Introduces the *Recuperacao extrajudicial*: - only possible for: secured and unsecured creditors (not for labour and tax credit). - bankruptcy stay imposed by the court is not available. - need consent of 60% of creditors in each class (value of their debt). - possibility of cramming down non-consenting creditors (as long as non discrimination).	Pre-agreement that allows to contemporaneously file for chapter 11 and file a reorganization plan to short the procedure.

Notes: The table compares salient aspects of the Brazilian old bankruptcy law in force until 2005, the Brazilian new bankruptcy law in force since 2005 and the US bankruptcy code. The table is divided in 3 parts, each for a different possible outcome of the bankruptcy process: liquidation, judicial reorganization and out-of-court restructuring. The purpose is to show the main legal changes introduced with the new Brazilian law and compare them to the rules in force in the US. This is by no means an exhaustive description of the content of these laws.

Table 2: Summary Statistics of Judicial Variables, All Courts

Variable	Mean	Std. Dev.	Min.	Max.	N
Courts per judicial district	1.6	1.9	1	37	2495
Judges per court	2.2	3.5	0.1	96.9	2495
Staff per court	12.7	22	0	378.6	2495
Congestion rate:(pending+new)/sentences	5.4	4.9	0.6	33.7	2490
Backlog per judge: pending/judges	2890	2873	30	17273	2495
Appeal rate: appeals/sentences	0.1	0.2	0	1	2490
Clearance rate: sentences/newcases	1	0.7	0.1	6.1	2483

Notes: The table shows summary statistics of judicial variables from *Justiça Aberta*, a database produced by the Brazilian National Justice Council (CNJ). Data covers 92% of Brazilian judicial districts and refers to 2009.

Table 3: Summary Statistics of Judicial Variables, Bankruptcy Courts

Variable	Mean	Std. Dev.	Min.	Max.	N
Courts per judicial district	1.9	1.8	1	7	12
Judges per court	3.3	4.1	1	15.3	12
Staff per court	13.4	18.8	1	71	12
Congestion rate:(pending+new)/sentences	8	9.2	0.6	33.7	12
Backlog per judge: pending/judges	5318	7291	135	17273	12
Appeal rate: appeals/sentences	0.2	0.4	0	1	12
Clearance rate: sentences/newcases	1.8	1.9	0.2	6.1	12

Notes: The table shows summary statistics of judicial variables from *Justiça Aberta*, a database produced by the Brazilian National Justice Council (CNJ). Data covers only judicial districts with courts specialized in bankruptcy: Belo Horizonte, Brasilia, Campo Grande, Curitiba, Fortaleza, Juiz de Fora, Novo Hamburgo, Porto Alegre, Rio de Janeiro, São Paulo, Uberaba and Vitoria. Data !refers to 2009.

Table 4: Summary Statistics of Firm-level Variables

PIA survey	Mean	Std. Dev.	N
Number of Workers (in logs)	3.74	1.27	291,258
Value Added (in logs)	13.56	2.03	290,593
Value Added per Worker (in logs)	9.82	1.32	290,415
TFP, Olley and Pakes methodology(in logs)	6.7	1.13	265,307
Capital Investment (in logs)	11.8	2.47	134,803
Average Wage (in logs)	9.19	0.65	291,162
Exit dummy	0.06	0.24	305,498
PINTEC survey			
Number of Workers (in logs)	4.5	1.43	49,122
Access to external finance for tech investment (dummy)	0.12	0.32	49,874
Technological investment (in logs)	12.84	2.24	20,527
Introduction of new product since last survey (dummy)	0.3	0.46	49,874

Notes: The table shows summary statistics of firm level variables. Data comes from the Annual Industrial Survey (PIA) and the Survey of Technological Innovation (PINTEC) and, for confidentiality reasons, it was accessed in the IBGE offices of Rio de Janeiro. Minimum and maximum values of these variables is not reported to comply with the confidentiality rules of the IBGE. Data refers to years from !2000 to 2009.

Table 5: Comparing Judicial Districts

	Averages in 2000		
	Low congestion districts	High congestion districts	p-value
Household income per month (in R$)	747	893	0.00
Population (in units)	84297	95106	0.53
Number of bank agencies (in units)	9.2	9.4	0.93
Alphabetization rate of adult population	84%	89%	0.00
Number of manufacturing firms	66	85	0.29
Agricultural share on GDP	20%	14%	0.00
Industry share on GDP	18%	23%	0.00

Notes: The table shows summary statistics of covariates at judicial district level. Low (high) congestion judicial districts are those below (above) the median of pending cases per judge. Data on all covariates refers to year 2000. Sources are: Census (household income per capita, alphabetization rate, population), IPEA data (agricultural and industrial share on GDP) and ESTBAN Brazilian Central Bank (number of bank agencies). All data can be downloaded at: www.sidra.ibge.gov.br/, www.ipeadata.gov.br/ and www.bcb.gov.br/.

Table 6: Firm Investment

Dependent variable: change in log (capital investment)	(1)	(2)	(3)	(4)	L⩾30 (5)	single-plant firms (6)	no change in headquarters (7)
$\log\left(\frac{backlog}{judge}\right)_j$	-0.048***		-0.045***	-0.049***	-0.053***	-0.042*	-0.047***
	[0.013]		[0.016]	[0.018]	[0.017]	[0.025]	[0.018]
bankruptcy court $_j$		0.066***	0.019	0.080	0.070	0.013	0.075
		[0.021]	[0.035]	[0.058]	[0.056]	[0.083]	[0.060]
log (Avg household income) $_{j,2000}$				0.147*	0.199**	0.253**	0.147*
				[0.084]	[0.087]	[0.117]	[0.086]
log (population) $_{j,2000}$				0.012	0.035	0.038	-0.000
				[0.031]	[0.034]	[0.040]	[0.032]
Population density $_{j,2000}$				0.098**	0.128***	0.080	0.117**
				[0.048]	[0.047]	[0.061]	[0.048]
log (N bank agencies) $_{j,2000}$				-0.041	-0.061	-0.036	-0.031
				[0.043]	[0.045]	[0.057]	[0.044]
Alphabetization rate $_{j,2000}$				-0.815	-1.110**	-2.476***	-0.886*
				[0.495]	[0.525]	[0.672]	[0.509]
N of manufacturing firms $_{j,2000}$				-0.002	-0.003	-0.003	-0.002
				[0.002]	[0.002]	[0.003]	[0.002]
Industry share of GDP $_{j,2000}$				-0.052	-0.169	-0.183	-0.032
				[0.156]	[0.169]	[0.214]	[0.159]
Constant	0.622***	0.218***	0.595***	0.330	0.062	0.781	0.488
	[0.104]	[0.015]	[0.130]	[0.694]	[0.729]	[0.947]	[0.713]
Observations	17,922	17,922	17,922	17,862	15,486	10,332	17,569
Adjusted R-squared	0.008	0.008	0.008	0.008	0.009	0.009	0.008
N judicial districts	1215	1215	1215	1175	1138	1001	1164

Notes: Standard errors clustered at judicial district level reported in brackets. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include 2 digits industry fixed effects. Data at firm level is from the PIA survey. The dependent variable is defined as follows: $\Delta\log(I) = \frac{1}{4}\sum_{t=2006}^{2009}\log(I_{ijst}) - \frac{1}{3}\sum_{t=2003}^{2005}\log(I_{ijst})$ where i identifies firm, j the judicial district, s the 2 digits sector and t the year.

Table 7: Firm Investment, Effects by Financial Dependence and Asset Tangibility (Median)

Dependent variable: change in log (capital investment)	(1)	(2)	(3)	(4)
$\log\left(\frac{backlog}{judge}\right)_j \times 1(\text{ext.fin.} = \text{high})$	-0.059***	-0.065***		
	[0.022]	[0.021]		
$\log\left(\frac{backlog}{judge}\right)_j \times 1(\text{ext.fin.} = \text{low})$	-0.032	-0.035*		
	[0.020]	[0.020]		
1(ext.fin. = high)	0.281	0.295		
	[0.227]	[0.224]		
$\log\left(\frac{backlog}{judge}\right)_j \times 1(\text{asset tang} = \text{high})$			-0.071***	-0.074***
			[0.019]	[0.018]
$\log\left(\frac{backlog}{judge}\right)_j \times 1(\text{asset tang} = \text{low})$			-0.016	-0.022
			[0.021]	[0.021]
1(asset tang = high)			0.434**	0.403*
			[0.205]	[0.206]
bankruptcy court $_j$ × 1(ext.fin. = high)	-0.026	0.034		
	[0.047]	[0.068]		
bankruptcy court $_j$ × 1(ext.fin. = low)	0.063	0.123*		
	[0.040]	[0.065]		
bankruptcy court $_j$ × 1(asset tang = high)			-0.008	0.050
			[0.043]	[0.064]
bankruptcy court $_j$ × 1(asset tang = low)			0.048	0.101
			[0.044]	[0.069]
judicial district controls		yes		yes
Constant	0.457***	0.334	0.366**	0.267
	[0.167]	[0.356]	[0.175]	[0.357]
Observations	17,922	17,862	17,922	17,862
Adjusted R-squared	0.008	0.008	0.008	0.008
N judicial districts	1215	1175	1215	1175

Notes: Standard errors clustered at judicial district level reported in brackets. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include 2 digits industry fixed effects. 1(ext.fin. = high) is a dummy equal to 1 if the firm operates above the median of dependence on external finance in the pre-reform period. Same reasoning applies for asset tangibility. Judicial district controls include: average household income (source: Census 2000), log of total population (source: Census 2000) and log number of bank agencies (source: Central Bank of Brazil). All controls are for year 2000. $\Delta \log(I) = \frac{1}{4}\sum_{t=2006}^{2009} \log(I_{ijst}) - \frac{1}{3}\sum_{t=2003}^{2005} \log(I_{ijst})$ where i identifies firm, j the judicial district, s the 2 digits sector and t the year.

Table 8: Firm Investment, Effects by Financial Dependence and Asset Tangibility (Quartiles)

Dependent variable: change in log (capital investment)	(1)	(2)	(3)	(4)
$\log\left(\frac{backlog}{judge}\right)_j \times 1(exf_q = 1)$	-0.010	-0.010		
	[0.009]	[0.009]		
$\log\left(\frac{backlog}{judge}\right)_j \times 1(exf_q = 2)$	-0.015	-0.018		
	[0.024]	[0.025]		
$\log\left(\frac{backlog}{judge}\right)_j \times 1(exf_q = 3)$	-0.050*	-0.055*		
	[0.030]	[0.030]		
$\log\left(\frac{backlog}{judge}\right)_j \times 1(exf_q = 4)$	-0.039	-0.043*		
	[0.025]	[0.024]		
$\log\left(\frac{backlog}{judge}\right)_j \times 1(tang_q = 1)$			-0.001	-0.002
			[0.010]	[0.010]
$\log\left(\frac{backlog}{judge}\right)_j \times 1(tang_q = 2)$			-0.010	-0.017
			[0.020]	[0.020]
$\log\left(\frac{backlog}{judge}\right)_j \times 1(tang_q = 3)$			-0.056**	-0.056**
			[0.025]	[0.024]
$\log\left(\frac{backlog}{judge}\right)_j \times 1(tang_q = 4)$			-0.068*	-0.075**
			[0.035]	[0.036]
bankruptcy court $_j$	0.030	0.085	0.029	0.079
	[0.028]	[0.058]	[0.031]	[0.060]
judicial district controls		yes		yes
Constant	0.224***	0.100	0.283***	0.146
	[0.062]	[0.313]	[0.052]	[0.308]
Observations	17,881	17,821	17,881	17,821
Adjusted R-squared	0.008	0.007	0.008	0.008
N judicial districts	1215	1175	1215	1175

Notes: Standard errors clustered at judicial district level reported in brackets. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. $1(exf_q = 1)$ is a dummy equal to 1 if the firm operates in the first quartile of dependence on external finance in the pre-reform period. Same reasoning applies for asset tangibility. All regressions include a set of dummies identifying quartiles of pre-reform dependence on external finance and asset tangibility. Judicial district controls include: average household income (source: Census 2000), log of total population (source: Census 2000) and log number of bank agencies (source: Central Bank of Brazil). All controls are for year 2000. $\Delta\log(I) = \frac{1}{4}\sum_{t=2006}^{2009}\log(I_{ijst}) - \frac{1}{3}\sum_{t=2003}^{2005}\log(I_{ijst})$ where i identifies firm, j the judicial district, s the 2 digits sector and t the year.

Table 9: Firm Technological Investment and Access to External Finance

Dependent variable indicated in columns	access ext. finance (1)	access ext. finance (2)	log(*Tech I*) (3)	log(*Tech I*) (4)	New product (5)	New product (6)
$\log\left(\frac{backlog}{judge}\right)_z \times 1(year = 2008)$	-0.019***	-0.012***	-0.220***	-0.151***	-0.074***	-0.058***
	[0.003]	[0.003]	[0.047]	[0.045]	[0.010]	[0.012]
$1(year = 2008)$	0.144***	0.208***	2.158***	2.767***	0.626***	0.779***
	[0.026]	[0.033]	[0.404]	[0.488]	[0.092]	[0.130]
log (GDP per capita) $_{z,2000} \times 1(year = 2008)$		-0.014***		-0.132**		-0.033*
		[0.004]		[0.058]		[0.019]
Constant	0.146***	0.146***	12.592***	12.592***	0.299***	0.299***
	[0.005]	[0.005]	[0.021]	[0.022]	[0.010]	[0.010]
Observations	49,808	49,808	20,493	20,493	49,808	49,808
Adjusted R-squared	0.011	0.011	0.060	0.060	0.012	0.012
N states	27	27	27	27	27	27

Notes: Standard errors clustered at state level reported in brackets. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include year and state fixed effects. All data at firm level comes from the PINTEC survey and it is available for years 2000, 2003, 2005, 2008. $1(year = 2008)$ is a dummy equal to 1 if year is 2008 and indicates here the post reform period. At state level GDP per capita is used instead of average household income as a control.

Table 10: Firm Labor Productivity, TFP, Wages and Exit.

Dependent variable indicated in columns	$\Delta \log\left(\frac{VA}{L}\right)$ (1)	$\Delta \log(TFP)$ (2)	$\Delta \log(wage)$ (3)	$\Delta exit$ (4)
$\log\left(\frac{backlog}{judge}\right)_j$	-0.023***	-0.023***	-0.011**	-0.001
	[0.007]	[0.007]	[0.005]	[0.002]
bankruptcy court $_j$	-0.003	0.005	0.001	0.008
	[0.037]	[0.036]	[0.020]	[0.008]
(Avg household income) $_{j,2000}$	0.037	0.039	-0.032***	-0.024***
	[0.027]	[0.025]	[0.011]	[0.008]
log (N bank agencies) $_{j,2000}$	-0.030*	-0.027*	-0.001	-0.008*
	[0.016]	[0.015]	[0.007]	[0.004]
log (population) $_{j,2000}$	0.019	0.013	-0.008	0.008**
	[0.013]	[0.013]	[0.005]	[0.003]
Constant	0.274**	0.257**	0.522***	0.028
	[0.138]	[0.131]	[0.071]	[0.036]
Observations	35,831	33,246	35,866	37,585
Adjusted R-squared	0.004	0.004	0.007	0.010
N judicial districts	1507	1490	1507	1531

Notes: Standard errors clustered at judicial district level reported in brackets. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include 2 digits industry fixed effects. All data at firm level comes from the PIA survey. The oucomes $\Delta log(y)$ where $y = \frac{VA}{L}, TFP, wages$ are defined as $\Delta \log(y) = \frac{1}{4}\sum_{t=2006}^{2009} \log(y_{ijst}) - \frac{1}{3}\sum_{t=2003}^{2005} \log(y_{ijst})$ where i identifies firm, j the judicial district, s the 2 digits sector and t the year. The variable exit is a dummy equal to 1 if the firm is registered as "not active" in the data.

Table 11: Firm Labor Productivity, TFP, Wages and Exit, Effects by Financial Dependence and Asset Tangibility (Quartiles)

Dependent variable indicated in columns	$\Delta \log\left(\frac{VA}{L}\right)$ (1)	$\Delta \log(TFP)$ (2)	$\Delta \log(wage)$ (3)	$\Delta exit$ (4)
$\log\left(\frac{backlog}{judge}\right)_j \times 1(exf_q = 1)$	-0.007	-0.008*	-0.001	-0.002*
	[0.005]	[0.005]	[0.002]	[0.001]
$\log\left(\frac{backlog}{judge}\right)_j \times 1(exf_q = 2)$	-0.009	-0.010	-0.002	-0.004
	[0.012]	[0.011]	[0.006]	[0.003]
$\log\left(\frac{backlog}{judge}\right)_j \times 1(exf_q = 3)$	-0.020*	-0.013	-0.012***	0.001
	[0.012]	[0.012]	[0.004]	[0.003]
$\log\left(\frac{backlog}{judge}\right)_j \times 1(exf_q = 4)$	-0.032***	-0.034***	-0.014**	-0.003
	[0.011]	[0.010]	[0.007]	[0.003]
$1(exf_q = 2)$	-0.044	-0.026	0.029	0.015
	[0.111]	[0.110]	[0.050]	[0.025]
$1(exf_q = 3)$	0.127	0.055	0.107***	-0.001
	[0.098]	[0.098]	[0.035]	[0.024]
$1(exf_q = 4)$	0.207**	0.226**	0.105**	0.020
	[0.090]	[0.091]	[0.053]	[0.024]
bankruptcy court $_j$	0.001	0.012	0.004	0.007
	[0.040]	[0.039]	[0.019]	[0.009]
(Avg household income) $_{j,2000}$	0.036	0.039	-0.032***	-0.024***
	[0.027]	[0.025]	[0.011]	[0.008]
log (N bank agencies) $_{j,2000}$	-0.030*	-0.030*	-0.001	-0.008*
	[0.016]	[0.015]	[0.007]	[0.004]
log (population) $_{j,2000}$	0.020	0.016	-0.007	0.008**
	[0.014]	[0.013]	[0.006]	[0.003]
Constant	0.138	0.116	0.423***	0.032
	[0.134]	[0.128]	[0.056]	[0.033]
Observations	35,665	33,101	35,700	37,400
Adjusted R-squared	0.004	0.004	0.006	0.010
N judicial districts	1507	1490	1507	1531

Notes: Standard errors clustered at judicial district level reported in brackets. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. $1(exf_q = 1)$ is a dummy equal to 1 if the firm operates in the first quartile of dependence on external finance in the pre-reform period.

Table 12: Effects by Initial Firm Size

Dependent variable indicated in columns	$\Delta \log(I)$ (1)	$\Delta \log\left(\frac{VA}{L}\right)$ (2)	$\Delta \log(TFP)$ (3)	$\Delta \log(wage)$ (4)	$\Delta exit$ (5)
$\log\left(\frac{backlog}{judge}\right)_j \times 1(size_q = 1)$	-0.039	-0.006	-0.011	-0.011*	-0.005**
	[0.027]	[0.009]	[0.010]	[0.006]	[0.003]
$\log\left(\frac{backlog}{judge}\right)_j \times 1(size_q = 2)$	-0.057**	-0.011	-0.007	-0.005	-0.003
	[0.025]	[0.010]	[0.008]	[0.005]	[0.003]
$\log\left(\frac{backlog}{judge}\right)_j \times 1(size_q = 3)$	-0.110***	-0.042*	-0.034	-0.016***	0.007
	[0.035]	[0.023]	[0.023]	[0.005]	[0.005]
$\log\left(\frac{backlog}{judge}\right)_j \times 1(size_q = 4)$	-0.056**	-0.024	-0.015	-0.016**	-0.000
	[0.024]	[0.015]	[0.015]	[0.006]	[0.004]
$1(size_q = 2)$	0.199	0.088	0.008	-0.049	-0.033
	[0.246]	[0.101]	[0.093]	[0.041]	[0.024]
$1(size_q = 3)$	0.446	0.289	0.188	0.051	-0.090**
	[0.355]	[0.209]	[0.176]	[0.049]	[0.038]
$1(size_q = 4)$	0.009	-0.058	-0.107	-0.011	-0.055*
	[0.248]	[0.122]	[0.142]	[0.050]	[0.030]
bankruptcy court $_j$	0.020	0.006	0.011	0.010	0.018**
	[0.068]	[0.041]	[0.039]	[0.017]	[0.009]
(Avg household income) $_{j,2000}$	0.061	0.025	0.025	-0.027**	-0.029***
	[0.064]	[0.028]	[0.027]	[0.012]	[0.007]
log (N bank agencies) $_{j,2000}$	-0.052	-0.007	-0.008	-0.007	-0.007
	[0.040]	[0.018]	[0.018]	[0.007]	[0.004]
log (population) $_{j,2000}$	0.044	-0.001	0.000	-0.003	0.005
	[0.033]	[0.015]	[0.014]	[0.006]	[0.003]
Constant	0.116	0.311*	0.263*	0.461***	0.111***
	[0.415]	[0.165]	[0.148]	[0.079]	[0.039]
Observations	16,271	27,756	25,978	27,775	28,560
Adjusted R-squared	0.004	0.009	0.005	0.008	0.007
N judicial districts	1151	1404	1392	1404	1417

Notes: Standard errors clustered at judicial district level reported in brackets. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. $1(size_q = 1)$ is a dummy equal to 1 if firm is in the first quartile of firm size in the pre reform period. Size is measured as log book value of assets in the pre-reform period in deviation from the 2 digits sector average.

Table 13: Falsification Test

Dependent variable indicated in columns	$\Delta\log(I)$ (1)	$\Delta\log\left(\frac{VA}{L}\right)$ (2)	$\Delta\log(TFP)$ (3)	$\Delta\log(wage)$ (4)
Panel A year reform = 2002				
$\log\left(\frac{backlog}{judge}\right)_j$	0.017	-0.008	-0.013**	-0.002
	[0.022]	[0.006]	[0.006]	[0.003]
judicial district controls	yes	yes	yes	yes
Constant	0.919**	0.260*	0.140	0.552***
	[0.413]	[0.153]	[0.154]	[0.064]
Observations	14,581	28,285	26,351	28,495
Adjusted R-squared	0.009	0.005	0.004	0.005
N judicial districts	1097	1428	1409	1431
Panel B year reform = 2005				
$\log\left(\frac{backlog}{judge}\right)_j$	-0.050***	-0.023***	-0.023***	-0.011**
	[0.015]	[0.007]	[0.007]	[0.005]
judicial district controls	yes	yes	yes	yes
Constant	0.480	0.274**	0.257**	0.518***
	[0.331]	[0.138]	[0.131]	[0.075]
Observations	17,862	35,831	33,246	35,866
Adjusted R-squared	0.008	0.004	0.004	0.005
N judicial districts	1175	1507	1490	1507
t-stat on the difference	2.52	1.63	1.08	1.54

Notes: The table presents results of estimating regression 11 in two time periods: 2000 to 2004 in Panel A (assuming the reform was introduced in 2002) and 2003 to 2009 in Panel B (assuming the reform was introduced in 2005). Standard errors clustered at judicial district level reported in brackets. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include 2 digits industry fixed effects and a dummy identifying the existence of bankruptcy courts. Judicial district controls include: average household income (source: Census 2000), log of total population (source: Census 2000) and log number of bank agencies (source: Central Bank of Brazil). All controls are for year 2000.

Table 14: Average Growth Rates under Different Counterfactual Scenarios

	avg in data	**predicted under counterfactuals**			
	between 2003-05 and 2006-09	level of congestion assumed for all courts:			
		avg in 1st quartile	avg in 4th quartile	least congested court	most congested court
	(1)	(2)	(3)	(4)	(5)
$\Delta \log(I)$	23.0%	29.0%	17.8%	45.8%	7.8%
difference counterfactual/observed:		6.0%	-5.2%	22.8%	-15.2%
$\Delta \log(VA)$	28.6%	31.2%	26.1%	38.7%	21.6%
		2.5%	-2.5%	10.1%	-7.0%
$\Delta \log\left(\frac{VA}{L}\right)$	26.6%	28.9%	24.3%	35.8%	20.2%
		2.3%	-2.3%	9.2%	-6.4%
$\Delta \log(TFP)$	19.8%	22.2%	17.4%	29.3%	13.2%
		2.4%	-2.4%	9.5%	-6.6%

Notes: The table shows the average percentage change observed in the data for the main firm level outcomes (column 1) and compare it with the average percentage change predicted under different counterfactuals (columns 2 to 5). Under each counterfactual the level of court congestion of all courts is set to a unique value: the average level in the first quartile (column 2) the average level in the fourth quartile (column 3), the level observed in the less congested court (column 4) and the level observed in the more congested court (column 5). Under each average percentage change predicted by counterfactuals I report the difference in percentage points between the actual change observed in the data and the predicted change.

Table 15: First Stage

Dependent variable: $\log\left(\frac{backlog}{judge}\right)_{mj}$	(1)	(2)	(3)	(4)
log (Pop. neighbors below req.) $_m$	0.156***	0.178***	0.206***	0.190***
	[0.032]	[0.032]	[0.032]	[0.034]
log (Pop. neighbors) $_m$		0.137***	0.106***	0.047*
		[0.020]	[0.020]	[0.025]
neighbors controls			yes	yes
municipality controls				yes
Constant	6.697***	4.812***	3.220***	3.448***
	[0.339]	[0.429]	[0.657]	[0.682]
Adjusted R-squared	0.018	0.055	0.104	0.114
N municipalities	1210	1210	1210	1210
F-stat on the instrument:	23.18	31.08	42.27	31.86

Notes: The table presents results of the first stage regression. The unit of observation is the municipality. All regressions are weighted by the number of firms operating in each municipality in the initial year. The instrument is the total population of neighboring municipalities that were below the minimum requirements in terms of population size in the year in which the law on judicial organization was introduced in each state. The sample is limited to those municipalities that could have potentially be an independent judicial district given their population size. Controls at municipality level include: population, area and average household income. Neighbors controls include: area and average household income. Robust standard errors reported in brackets. Significance levels:*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 16: Robustness Check on First Stage

Dependent variable: $\log\left(\frac{backlog}{judge}\right)_{mj}$	(1)	(2)
log (Pop. neighbors below req.) $_m$	0.178***	
	[0.032]	
log (Pop. random sample) $_m$		-0.021
		[0.026]
log (Pop. neighbors) $_m$	0.137***	0.137***
	[0.020]	[0.026]
Constant	4.812***	6.903***
	[0.429]	[0.280]
Adjusted R-squared	0.055	0.029
N municipalities	1210	1210

Notes: The table presents results of the first stage regression in column 1. In column 2 I run the same first stage regression using as instrument the population of a random sample of neighbors of the same size (in terms of number of neighbors) that the actual instrument. In column 3 I add both the actual instrument and the randomly constructed instrument in the same regression. The unit of observation is the municipality. All regressions are weighted by the number of firms operating in each municipality in the initial year. The sample is limited to those municipalities that could have potentially be an independent judicial district given their population size. Robust standard errors reported in brackets. Significance levels:*** p<0.01, ** p<0.05, * p<0.1.

Table 17: OLS and IV Results Estimated on the Same Sample

Dependent variable and estimator indicated in columns	$\Delta\log(I)$ OLS (1)	$\Delta\log(I)$ 2SLS (2)	$\Delta\log(TFP)$ OLS (3)	$\Delta\log(TFP)$ 2SLS (4)	$\Delta\log(wage)$ OLS (5)	$\Delta\log(wage)$ 2SLS (6)
$\log\left(\frac{backlog}{judge}\right)_{mj}$	-0.037**	-0.142**	-0.015***	-0.062*	-0.004	0.012
	[0.015]	[0.072]	[0.006]	[0.036]	[0.002]	[0.015]
log (Pop. neighbors $)_{mj}$	0.026**	0.005	-0.004	-0.011	-0.017***	-0.015***
	[0.011]	[0.017]	[0.004]	[0.007]	[0.002]	[0.003]
Constant	0.205	1.329*	0.370***	0.830**	0.537***	0.375**
	[0.207]	[0.774]	[0.080]	[0.362]	[0.033]	[0.152]
Observations	13,118	13,118	23,658	23,658	25,520	25,520

Excluded instrument in IV regressions: log (Pop. neighbors below req.) $_m$

Notes: Robust standard errors reported in brackets. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include 2-digits industry fixed effects and are run only on municipality that are seat of a judicial district ($m = j$).

Table 18: Robustness Using Data for Rondonia and Mato Grosso do Sul

Dependent variable indicated in columns	$\Delta\log(I)$ (1)	(2)	$\Delta\log\left(\frac{VA}{L}\right)$ (3)	(4)	$\Delta\log(TFP)$ (5)	(6)	$\Delta\log(wage)$ (7)	(8)
$\log(backlog)_{j,2004}$	-0.398*		-0.093**		-0.130***		-0.014	
	[0.227]		[0.042]		[0.042]		[0.018]	
$\log(backlog)_{j,2009}$		-0.546**		-0.098**		-0.143***		-0.018
		[0.243]		[0.045]		[0.046]		[0.023]
Constant	3.078	4.343**	0.985***	1.047***	1.173***	1.315***	0.415***	0.449**
	[1.867]	[1.946]	[0.342]	[0.362]	[0.325]	[0.364]	[0.139]	[0.183]
Observations	179	180	640	642	588	590	641	643
Adjusted R-squared	0.072	0.075	0.030	0.030	0.026	0.026	0.008	0.008
N judicial districts	37	38	60	61	59	60	60	61

Notes: The table presents the results obtained estimating equation 11 only for the judicial districts of Rondonia and Mato Grosso do Sul. For these two states judicial data at district level is available starting from 2004 from the official documentation of state tribunals. In this table I check whether using measures of court congestion (in this case: the average number of pending cases in a district) from 2004 or 2009 significantly change the main results. Robust standard errors reported in brackets. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include 2-digits industry fixed effects and state fixed effects.